Seafaring in the Pleistocene

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Archaeological data from Wallacea (Indonesia) and elsewhere are summarized to show that the history of seafaring begins in the Early Pleistocene, and that this human capability eventually led to Middle Palaeolithic ocean crossings in the general region of Australia. To understand better the technological magnitude of these many maritime accomplishments, a series of replicative experiments are described, and the theoretical conditions of these experiments are examined. The proposition is advanced that hominid cognitive and cultural evolution during the Middle and early Late Pleistocene have been severely misjudged. The navigational feats of Pleistocene seafarers confirm the cultural evidence of sophistication available from the study of palaeoart.

In January 1957, seven years after commencing his research on Flores (Verhoeven 1968, 395; see Verhoeven 1952; 1953; 1956; 1958a,b,c; 1959; Verhoeven & Fuchs 1959; Verhoeven & Heine-Geldern 1954), the late Dr Theodor Verhoeven discovered the island's first reported remains of Stegodontidae at an exposure near the abandoned village Ola Bula, on the Soa plain of central Flores (Hooijer 1957; Verhoeven 1958a). The previous month, the Governor of Flores had shown him a large fossilized bone found by the Radja of Boawae, Joseph Dapangole, on a hunting trip. A few years earlier, a similar fauna had been located on southern Sulawesi (Heekeren 1957). In March 1957, Verhoeven also found stone flakes and blades eroding from the fossiliferous deposit at Ola Bula (Verhoeven 1968, 400). After notifying the Indonesian authorities of these finds, he was joined in his search by A.M.R. Wegner and A.S. Dyhrberg from the Museum Zoologici Bogoriense, and a collection of fossil bones and stone tools they assembled over three days was sent to Dr Hooijer in Leiden for a more detailed examination. Henri Breuil recognized among these initial finds a number of typical Lower Palaeolithic stone implements (Verhoeven 1958a, 265), while von Koenigswald initially assigned Verhoeven's finds to the Middle Pleistocene.

In mid-1963, Verhoeven succeeded in demonstrating the contemporaneity of the Flores fossil remains with the artefacts, when he excavated similar artefacts in the thin fossiliferous stratum at the site Boa Leza (Verhoeven 1968). The condition of the finds in the silty upper part of this layer showed that they had not been subjected to fluvial repositioning: edges were sharp and fresh, and some of the skeletal remains were articulated. Moreover, this concurrence of the *Stegodon*-dominated megafauna and archaic stone tools was not limited to a single site; Verhoeven demonstrated the same association also at nearby Mata Menge, where he excavated in 1965 (Verhoeven 1968). In 1968, while in Europe, he teamed up with Professor Johannes Maringer of the Anthropos-Institut, Germany, and the two excavated together later in the same year with three large teams at Boa Leza, Mata Menge and Lembah Menge. Maringer confirmed the validity of all of Verhoeven's crucial observations, and their collaboration led to a series of publications about the early pre-history of Flores (Maringer & Verhoeven 1970a,b,c; 1972; 1975; 1977; Maringer 1978).

In the meantime, Verhoeven worked briefly also on Sumba and Timor (Fig. 1), and in August 1964 he succeeded in discovering Stegodontidae in the north of West Timor (Verhoeven 1964). Von Koenigswald compared some of the typologically Lower Palaeolithic surface stone tools from Timor to those from the Java Trinil Beds (Verhoeven 1968, 402), but in subsequent decades there were no serious attempts to follow up this work (Glover & Glover 1970; see

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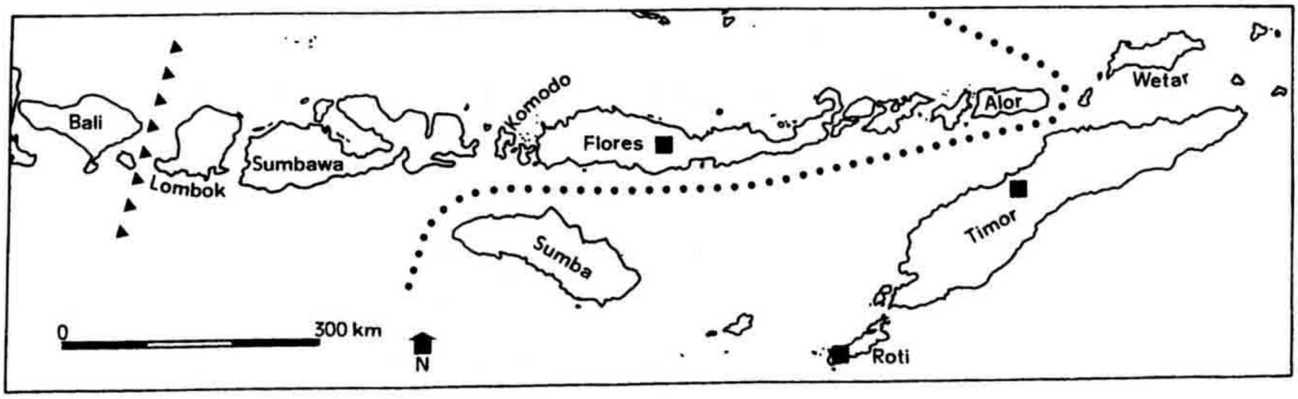

Figure 1. *Map of Southern Wallacea (Nusa Tenggara), Indonesia. The presumed dividing line between the Eurasian and Australian continental plates is shown. Wallace's biogeographical line runs between Bali and Lombok. The locations of known hominid occupation evidence of the Lower and Middle Pleistocene are indicated.*

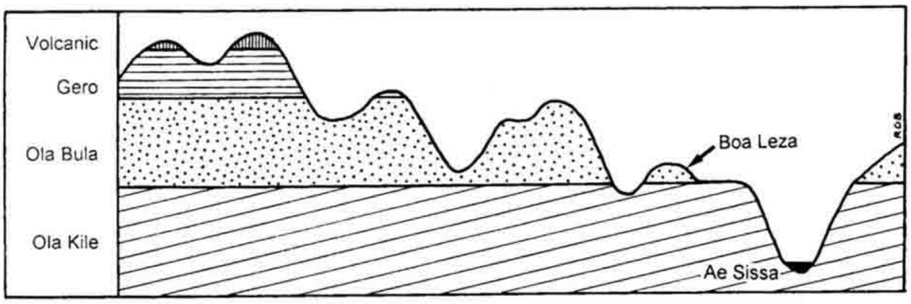

Figure 2. *The principal sediment facies forming the geological sequence in the Ola Bula region of the Soa Basin, central Flores, Indonesia.*

Bednarik 2000). In East Timor, too, no Pleistocene presence of humans was demonstrated before 2001 (Glover 1969; 1971; 1986), even though such a possibility had been implied by early Portuguese work in East Timor (Almeida 1953; Almeida & Zbyszewski 1967).

The Soa plain on Flores consists of four distinctive rock facies (Ehrat 1925; Hartono 1961), schematically illustrated in Figure 2. These are dissected to various degrees by deep fluvial erosion of the Late Pleistocene. The sloping volcanic Ola Kile deposit is overlain by the horizontal Ola Bula Formation, a facies of poorly consolidated mudstone layers averaging about 80 metres thick at some sites, 120 metres at others. The fossiliferous band, usually measuring from one to three metres thick, occurs in its lowest part, just above a distinctive white tuffaceous sediment forming its base. The overlying Gero limestones, up to 40 metres thick, were (according to early research: e.g. Maringer & Verhoeven 1970a) formed at or slightly below sea level, as shown by their fossil foraminifera. (Note, however, that Morwood *et al.* 1999, suggest that the fossil fauna indicates freshwater conditions.) These are in turn capped by a comparatively recent volcanic deposit. The fossiliferous layer consists of two definable horizons, a lower sandy component indicative of some water transport, and an upper silty component lacking evidence of fluvial movement of bones and stone tools. In both these deposits, the stone tools and fossilized bones occur together, sometimes in very close proximity, even in direct contact (Fig. 3).

Von Koenigswald eventually estimated the age of this deposit to between 830,000 and 500,000 years (von Koenigswald & Ghosh 1973), based on the geology, the palaeontology and the presence of tektites (Ashok Ghosh pers. comm. 1996). He favoured an age of 710 ka (von Koenigswald & Ghosh 1973). After Maringer's death in 1981, Sondaar (1984; 1987) and others undertook palaeomagnetic analyses of two sections in 1991–92, one at Mata Menge and one at Tangi Talo (Sondaar *et al.* 1994). At the first site, what appears to be the Matuyama-Brunhes reversal to normal polarity (780,000 BP) occurs just 1.5 m below the fossiliferous stratum, which is in agreement with von Koenigswald's favoured age estimate. A subsequent application of fission-track analysis of zircons suggested a slightly greater age for this deposit, of between approximately 880,000 and 800,000 years (Morwood *et al.* 1998). A major Indonesian–Australian research program is currently under way at over ten sites in the region, using a variety of analytical methods to explore the circumstances of the early hominid settlement, and of the relevant sedimentation conditions. Secure datings of stone-tool-bearing sediments have so far become available from Boa Leza, Mata Menge, Koba Tuwa and Ngamapa, and all fall between 750,000 and 850,000 years BP (Morwood *et al.* 1999). Although the error margins associated with these fission-track results are substantial and our ongoing research may result in 'fine-tuning', in the present context only the general order of magnitude of antiquity is relevant. It is, however, amply evident from the massive overburden of rock facies (up to 150 metres) that these results are entirely reasonable, and they are in complete agreement with all other dating evidence so far considered. The status as tools of the flint artefacts from the fossiliferous bed in the lower Ola Bula Formation has not been questioned by any of the numerous archaeologists who have examined them, and close to one thousand implements have so far been recovered (Fig. 4). The best-explored site so far is Boa Leza, where the shore of a former lake is being excavated. The sedimentary rock has been formed primarily from volcanic silt with a very low sand fraction, heavily cemented by amorphous silica. Some of the *Stegodon* remains occur in near-articulation, and are occasionally found together with large hammerstones. The discovery of a minute flint spall in a sediment sample during grain-size analysis suggests that artefacts were made or retouched in the immediate vicinity. The artefacts are mostly made of good-quality sedimentary silicas and show no fluvial wear. They are the only angular material in a sediment that was deposited by very slow-moving water, and except for a few heavy hammerstones they are the only large detrital mineral matter in the entire facies.

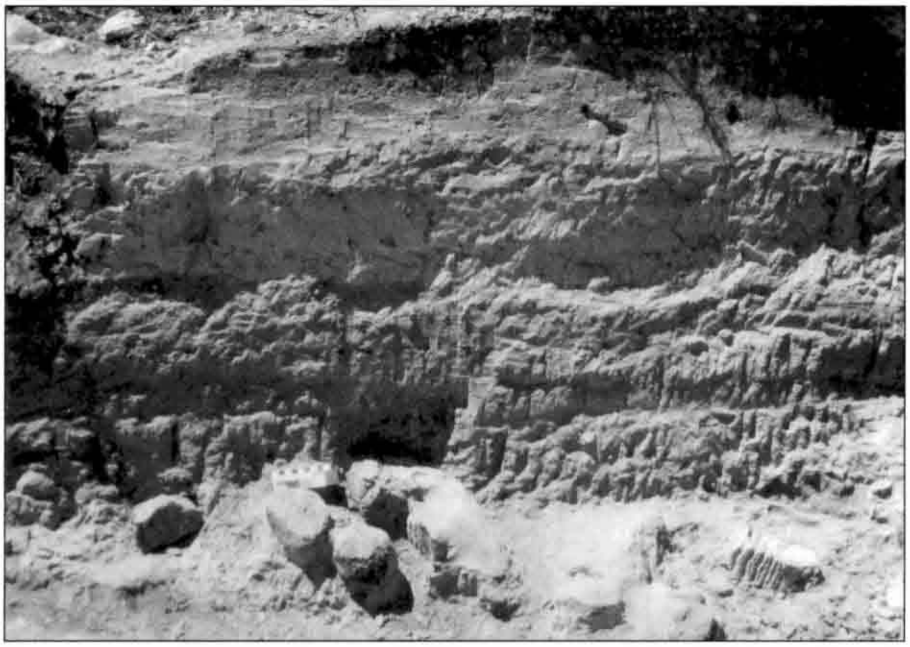

Figure 3. *Two* Stegodon *molars (one near the 10 cm scale, one on the right) on the floor of the Indonesian–Australian Boa Leza excavation in solid siltstone, August 1998. Stone tools were recovered from the same level and from the same area on the same day.*

In-depth research into the Pleistocene human occupation of Timor commenced only in December 1998, after the discovery of a major jasper quarry in southern Roti in March of that year (Bednarik 1998a). Fieldwork in Timor located several Pleistocene sites in the island's western half (East Timor having become politically stable only recently), and in 1998/99 began to focus on the Weaiwe valley near Atambua. There, a sequence of Pleistocene sediments occurs above estuarine clay deposits containing a great abundance of marine shells and snails. This demonstrates an uplift of well over 300 metres. The Weaiwe Formation, a calcite-cemented Pleistocene conglomerate, has now yielded remains of Stegodontidae from six sites (Bednarik 1999a; Bednarik & Kuckenburg 1999), and solid evidence of human presence in the fossiliferous stratum occurs at two, Motaoan and To'os (Bednarik 1999a). During its formation, the Pleistocene occupation layer was located close to sea level, which could itself have been much lower at the time. Since then it has been lifted well over 300 metres, although Timor, being in the 'outer arc', is considered tectonically less volatile than the islands of the 'inner arc' of Nusa Tenggara (formerly the Lesser Sunda Islands).

Context of the origins of seafaring

The current Indonesian–Australian work has so far confirmed the occurrence of undisputed stone tools together with the *Stegodon*-dominated fauna at six localities on Flores: Koba Tuwa, Mata Menge, Boa Leza, Ngamapa, Kopu Watu and Pauphadhi; while Ola Bula, Dozu Dhalu, Dozo Sogola, Tangi Talo and Nagerowe have produced only fossil materials so

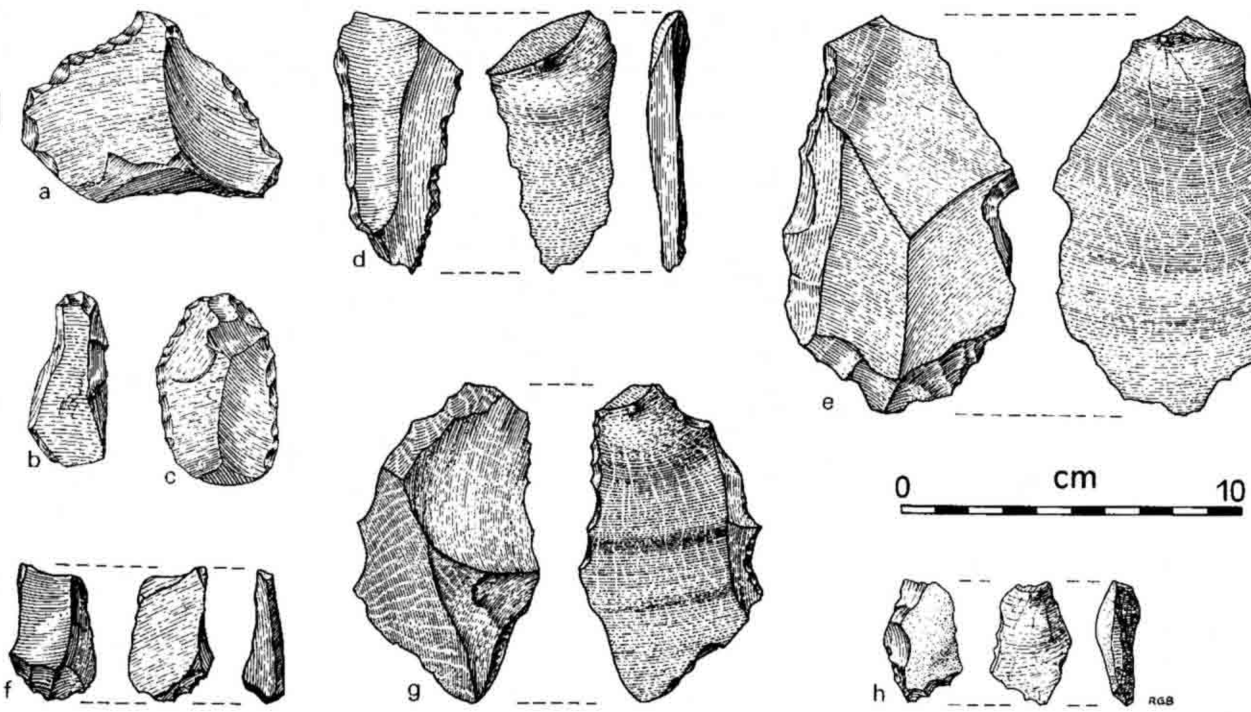

Figure 4. *Stone tools of the final Early Pleistocene of the Soa Basin, central Flores, thought to be between 750,000 and 850,000 years old: Mata Menge (a–e, g) and Boa Leza (f, h). Specimen f was excavated in the immediate vicinity of the* Stegodon *remains in Figure 3.*

far. The deposits from Tangi Talo, attributed to the Jaramillo subchron by Sondaar *et al.* (1994), may indicate the absence of hominids at 900,000 years ago, a date squarely confirmed by Morwood *et al.*'s (1998) fission-track date of 900,000±70,000 BP. Hominid presence has been dated through stone tools to between 750,000 and 850,000 BP at four of the six human occupation sites. One may expect some minor adjustments in these findings, but it seems soundly demonstrated that *Homo erectus* (or another as yet unknown hominin; currently *erectus* is the only available candidate) was sufficiently well-established on the island of Flores by 800,000 BP or so to leave behind numerous major deposits of stone tools. At Timor, similar stone-tool technology coincides with a similar fauna in a presumed Middle Pleistocene sediment. There the link between the cultural and the palaeontological evidence is made even stronger by the *in situ* recovery of a large shell fragment with signs of massive impact and extensive burning at To'os in the Weaiwe valley (Bednarik 1999a).

Flores is separated from Bali, the furthest extension of the Asian mainland during the Pleistocene (at times of low sea level), by two other islands, Lombok and Sumbawa (as well as several smaller islands which may have been connected to the larger ones at times of lower sea level). The lack of any former landbridge between Bali and Lombok was initially recognized by Wallace (1890). While this was based primarily on biogeographical observations, it is supported by the continuing uplift in the 'inner arc' of the Indonesian archipelago, which amounts to at least several hundred metres over the past million years in this tectonically active subduction zone (Hantoro 1996). Despite the incredibly rich mainland fauna of both extant and fossil terrestrial eutherians that can be found as far east as Bali, few of them ever reached the islands of Nusa Tenggara, or southern Wallacea (Bednarik & Kuckenburg 1999, 108–9). Some, such as the dog, pig and macaque, were probably carried by humans, while small mammals, mostly Muridae but including *Trachypithecus auratus*, probably crossed unaided, perhaps on floating vegetation (Diamond 1977). Proboscideans, however, crossed to numerous of the islands of Wallacea (Hooijer 1957; Verhoeven 1958c; 1964; Glover 1969; Groves 1976; Hantoro 1996) and to the Philippines (von Koenigswald 1949), where they underwent speciation and dwarfism. Elephants are superb long-distance swimmers. They have been observed to swim for 48 hours in herd formation across African lakes, and in one reported case swam a distance of 48 km at sea and at a speed of 2.7 km/h (Johnson 1980). In swimming great distances, individuals may tow others to allow them to rest. Their buoyancy is helped by digestive gases in their intestines and their habit of travelling as a herd would facilitate the success of a founding population upon landfall.

Hominids, however, lacked the trunks and swimming ability of elephants. Even deer, hippos, tapirs and pigs, four of the most capable terrestrial long-distance swimmers, never colonized Wallacea unaided. Some researchers have suggested that there may have been a landbridge across Lombok Strait (e.g. Groves 1995). This attempt to save the Bartstra *et al.* (1991) model of rapid Wallacean and Australian settlement just 50,000 years ago is highly implausible. Not only has the Strait acted as a biogeographical filter, preventing crossing by species not capable of swimming in excess of 30 kilometres (Bednarik & Kuckenburg 1999), but the local area is subject to rapid uplift (up to 1000 m/my) and Nusa Penida, an island in the Strait, consists entirely of recent coral limestone rising from the sea. Thus the implication is that the hominid settlement of Flores was preceded by at least two, but possibly three crossings of sea barriers. This conclusion is essential, particularly in view of the evidence that hominids subsequently also reached Timor and Roti, i.e. the southernmost point of the 'outer arc' of the archipelago. As this is separated from the 'inner arc' by a deep graben it would be tectonically absurd to look for a former landbridge between Alor and Timor: the Strait of Ombai is over 3000 metres deep. Thus it seems that a hominin of the final Early Pleistocene, most probably *Homo erectus*, was the world's first seafarer.

This simple realization presents several conundrums to mainstream archaeology. It seems widely agreed (e.g. Noble & Davidson 1993; 1996) that seafaring ability, particularly when it is used for the successful colonization of new lands, involves the skilled and standardized use of communication, presumably language or speech. If this is so, the Wallacean evidence implies the use of a form of symbolism almost a million years ago. This is in stark contrast to current dogma, particularly in some schools of Pleistocene archaeology, which favour the short-range model (or discontinuity model: d'Errico & Nowell 2000) of cognitive evolution. Language, 'art', social systems, self-adornment and self-awareness, blade-tool technology, skilled hunting, shelter construction, forward planning, human interment, or any form of perceived 'modern human behaviour' are the exclusive preserve of that very pinnacle of human evolution, 'anatomically modern humans' (see Tobias 1995 for a pertinent critique of this latter concept). They are said to have appeared towards the Late Pleistocene in one small region of Africa, and became widely disposed only during the last forty millennia of the Pleistocene. This model cannot accommodate seafaring ability before 50,000 BP without sustaining severe damage (Chase & Dibble 1987; Davidson & Noble 1989; Noble & Davidson 1993; 1996; Gamble 1993; Stringer & Gamble 1993; Byers 1994; Mellars 1996; d'Errico & Villa 1997; Mithen 1998); hence the insistence that these 'modern humans' reached Southeast Asia perhaps 60,000 years ago (of which there is no evidence), invented watercraft (ditto) and sailed at once to Sahul (Pleistocene Australia).

The alternative model, less popular among archaeologists and palaeoanthropologists, favours a long-range, gradual cognitive development, which began perhaps three million years ago (Bednarik 1998b) and led to important changes 900,000–800,000 years ago, with the use of mineral pigment and the collection of 'exotic' objects (crystals, fossil casts: d'Errico *et al.* 1989; Bednarik 1990a). Haematite and other ochreous minerals occur at many Lower Palaeolithic sites in all three Old World continents (Bednarik 1992a; McBrearty 2001), often bearing abrasion facets with distinctive striations (Barham 2002). In at least some cases these were presumably used in applying colour to rock surfaces (e.g. at Hunsgi, India: Bednarik 1990b). The making of excellent wooden artefacts is amply documented (Jacob-Friesen 1956; Howell 1966, 139; Wagner 1990; Belitzky *et al.* 1991; Thieme 1995; 1996; 1997), and eventually, still in the Lower Palaeolithic (notably the Acheulean), the production of beads and pendants (Bednarik 1997a), petroglyphs (at Auditorium Cave: Bednarik 1995a; and Daraki-Chattan, safely dated to the Acheulean) and 'proto'-iconographic palaeoart (Goren-Inbar 1986; Bednarik 2001a; 2002). Prismatic blade stone tools, burins and backed knives appear at the transition from Lower to Middle Palaeolithic industries (Rust 1950; Garrod & Kirkbridge 1961; Copeland 1978; Hours 1982), and the following Middle Palaeolithic period provides ample evidence of human burials (such as those at La Ferrassie), haematite use, palaeoart (Bednarik 1992a), bone harpoons (Narr 1966, 123; Brooks *et al.* 1995; Yellen *et al.* 1995; Bednarik 1997b, 36), mining and quarrying (Bednarik 1995b), and other forms of evidence indicating cultural complexity. Some conspicuously universal features of late 'Lower Palaeolithic' and 'Middle Palaeolithic' cultures suggest the existence of cultural contact across much of the Old World, which contrasts with models of cultural and genetic isolation. These include the use of iron oxides/hydroxides, the production of cupules and line petroglyphs, the collection of crystals and other unusual objects, the use of beads and pendants, the widespread production of a very distinctive engraving tradition, and, in a late phase of this technology, an art based on a surprisingly uniform repertoire of geometric markings (Bednarik 1990/91). Rather than attributing these and various technological uniformities (which occur across physically different groups, like Neanderthals and modern humans) to independent cognitive evolution, I find it more likely that the human population of most of the Old World, despite significant technological and ethnic differences and occasional isolation, experienced sufficient genetic and cultural exchange to facilitate a certain level of cultural uniformity. In most parts of the Old World this evidence is in stark contrast to the scenario of a sudden replacement of human populations.

Since the first colonization of Nusa Tenggara by hominids, more recent Pleistocene seafarers have undertaken even more daring sea crossings. The best known is perhaps the journey leading to first landfall in Sahul, which on current evidence is suggested to have occurred in the order of 60,000 years ago (Roberts *et al.* 1990; 1993; but cf. Allen & Holdaway 1995; the much greater TL dates reported in Fullagar *et al.* 1996 are attributable to misinterpretation of data). Since southern Wallacea was apparently settled much earlier than any other part of the archipelago, the seafarers who achieved a successful colonization of Australia probably set out from Timor or Roti. Their essentially Middle Palaeolithic technological mode (cf. Foley & Lahr 1997) continued on in Australia for the rest of the Pleistocene, and with this level of technology, numerous further sea crossings were achieved, resulting in the establishment of viable human populations on various islands in the region before 33,000–27,000 BP, including the Monte Bello Islands (today 120 km from Australia), Gebe Island (west of New Guinea), New Ireland (east of New Guinea) and Buka Island (180 km from New Ireland) (Allen *et al.* 1988; Wickler & Spriggs 1988; Bellwood 1987; 1996; Lourandos 1997). In contrast to the sea crossings in Nusa Tenggara (which includes Timor), during which with any Pleistocene sea level the target shore remained in sight, the destination would not have been visible for much of the journey on these much more recent crossings, including that to Australia. The lack of visual contact may have remained a barrier for many hundreds of millennia after crossings to visible targets commenced. Some at least of the later crossings were apparently also made in the opposite direction; for instance the cuscus, a Sahulian marsupial, was probably taken to the Moluccas by watercraft (Bellwood 1996).

No physical evidence of Pleistocene seafaring has ever been reported, nor have we any credible depictions of watercraft in Pleistocene art. Direct archaeological evidence of navigation goes back to between 8000 and possibly 10,500 years ago (Bednarik 1997b,c), in the form of Mesolithic paddles, canoes and a purported reindeer antler rib from a skin boat of the Ahrensburgian (Zeist 1957; Arnold 1966; Clark 1971; Ellmers 1980; McGrail 1987; 1991; Bednarik & Kuckenburg 1999). Watercraft and paddles of the middle of the Holocene are also known from two Japanese sites (Aikens & Higuchi 1982, 124; Ikawa-Smith 1986), but most of this evidence is from the western seaboard of Europe. Indirect evidence of seafaring, in the form of insular obsidian on the mainland, comes from Franchthi Cave in Greece, being only marginally older at 11,000 BP (Perlès 1979; Renfrew & Aspinall 1990). The same has also been suggested for the western Mediterranean, but with inadequate proof (e.g. d'Errico 1994). Very much earlier sea crossing and island colonization is indicated by Mousterian tools on Kefallinía, west of Greece (Kavvadias 1984; Warner & Bednarik 1996), and by the presence of *in situ* Clactonian-like stone tools in Middle Pleistocene sediments on Sardinia (Martini 1992; Bini *et al.* 1993; Sondaar *et al.* 1995). Crete was occupied during Palaeolithic times, as indicated by the human remains of modern type with preserved archaic features (Facchini & Giusberti 1992, 200). In Japan, Palaeolithic seafaring is demonstrated by the remains of four humans at Okinawa (Baba 1998) and by transported obsidian at Kozushima (Anderson 1987). In North America the earliest evidence is provided by the two femora fragments and one humerus from Arlington Springs on Santa Rosa Island (reportedly 13,000 years old). In comparison to the seafaring evidence in the seas of Indonesia, New Guinea and Australia, however, most of the finds from Europe and elsewhere are comparatively recent. The only other evidence of Lower Palaeolithic occupation of an island not previously connected to the mainland is from Sardinia, which at lower sea level was joined to Corsica.

Navigation capability was apparently first developed between one million years and 800,000 years ago in Southeast Asia, possibly as a local adaptation to gain access to off-shore marine resources in an ecologically volatile island environment. Humans entrusted themselves for the first time to an artefact that harnessed the forces of nature: the buoyancy of a floating object, and the currents, waves and winds at sea. This event determined the direction of human development right up to the present time, as it led to improvements in the skilled application of cultural systems to utilize natural ones. Ultimately this 'domestication of natural systems' resulted in the unsurpassed seafaring skills of the Polynesians, but also in the technology-based ascent of human culture generally. Hence if the impetus of the technological evolution from which human culture derives is attributed to a specific development, it is not something that took place around 50,000 years ago; it was in all probability the first successful navigation of the Lombok Strait.

By about 850,000 BP, an adequate number of males and females to establish a new population apparently had travelled to Flores, probably from Sumbawa. This demands earlier crossings by hominids, most likely from Bali via Lombok to Sumbawa, although the lesser possibility of migration via Sulawesi still needs to be considered. This first geographical and technological Rubicon crossed by the human genus, most probably at the Lombok Strait, almost certainly demanded the use of sophisticated communication, most probably in verbal form (speech), or some other suitable mode of language. Chronologically it coincides roughly with the introduction of material evidence suggestive of symbolic behaviour (Bednarik 1990a; 1992a; 1995a; 1998b), which reinforces the notion of a major cultural watershed at about that time. Symbolizing abilities acquired an archaeologically visible status, and can perhaps be assumed to have become a major cultural influence. It also coincides roughly with the human colonization of Europe, which may have occurred via the Strait of Gibraltar rather than by land. Such contact is implied by several factors: the complete lack of finds from the Early and probably most of the Middle Pleistocene in eastern Europe; the identical trajectories of the Acheulean industries in the Maghreb of Africa and in Iberia and the rest of western Europe; the absence of an early Acheulean or other early Lower Palaeolithic in eastern Europe; the early use of disc beads and haematite on both sides of the western Mediterranean; the sophistication of the Tan-Tan Acheulean quartzite figurine and the Erfoud manuport from Morocco (Bednarik 2001a; in press); and by the evidence of Acheulean navigation on the huge former Fezzan Lake in Libya (Bednarik 1999b; 2001b; Werry & Kazenwadel 1999). On that basis it would appear that the first Europeans were mariners, and that early contact between the two continents was by sea until late in the Middle Pleistocene. That is what the present evidence and the geographical distribution of the earliest occupation sites in Europe seem to suggest.

Replicative experiments

We lack any form of direct physical evidence that would tell us how any of the many Pleistocene seafaring feats were accomplished. The obvious source of ethnographic information, Australia, provides no answers, as all watercraft observed there in the last two centuries would be unsuitable for lengthy sea journeys (Massola 1971; Jones 1976; 1977; 1989; Flood 1995; Bednarik & Kuckenburg 1999). Indeed, this raises the question why these nautical skills would have been lost in coastal Australia, unless the material used in the ocean-going craft was not readily available there. Every commentator on the initial settlement of Australia, from Birdsell (1957; 1977) to the present, seems to agree that the most likely craft were bamboo rafts (e.g. Thorne 1980; 1989), and bamboo occurs only as small pockets of relatively thin-stemmed species in northern Australia (Jones 1989). This may well explain the absence of large, sea-going rafts in Aboriginal Australia.

Although we know that humans reached Australia in Middle Palaeolithic times (Roberts *et al.* 1990; 1993; Thorne *et al.* 1999), we have in fact no evidence about any aspects of this first landfall: where and when it occurred, at what sea level, where the sailors originated, how many there were, what their vessel was like, how they survived. Did they barely manage the trip, were they swept out to sea against their intention, or were these expeditions well equipped, completing the journey with relative ease? Mainstream archaeology cannot ever answer any of these questions, and if they interest us we need to find alternative methods to arrive at credible models. There are basically two approaches available to us. One is to use a carefully designed program of replicative experiments; the other is an intensive study of the technology available to these people, from a pragmatic perspective, and the integration of such knowledge in practical experiments where possible. I have been involved in both of these approaches for well over 30 years, replicating stone and bone implements, making fire, producing petroglyphs, beads and pendants, working wood, bamboo, skins, fibres and resins, and butchering with stone tools (e.g. Bednarik 1997a). This has usually included detailed microscopic studies of the resulting objects (e.g. microwear), by-products or surface markings. In contrast to Semenov (1964), whose pioneer work in this field concerned particularly Upper Palaeolithic technologies, I have most frequently focused on what are understood to be Middle and Lower Palaeolithic technologies. The most ambitious archaeological replication project I have attempted concerns some of the early sea journeys in the Indonesian and Mediterranean regions.

In principle, I perceive two types of replicative work: *product-targeted* and *result-targeted*. The easier procedure is the former, in which one copies an archaeologically-demonstrated physical result (e.g. an artefact) so as to determine what has to be done in order to arrive at the known product. If only the result of a particular strategy is known, however, and not the physical means by which that result was achieved, the approach is necessarily more complex. One begins by deconstructing the phenomenon to identify as many variables as possible, and then constructs multiple scenarios to account for all known and quantifiable variables so as to test each within a framework of probability. The greater the number of variables or determinants one manages to account for in this fashion, the greater the confidence that the most probable scenario can be identified. It is clear that both these replicative approaches involve uncertainties, but these can be minimized by rigour, and the procedure is still accessible to falsification and thus scientific: one can refute a result by demonstrating a more parsimonious explanation, either of the data available, or by providing additional data. The problem with this approach is that the most logical, most economic and most sensible course of action is not necessarily the one taken by the prehistoric people whose activity remains we examine. In matters concerning survival, however, that may not introduce as much uncertainty as it might in aspects involving greater individual choice.

A research program addressing questions of Pleistocene navigation is currently under way, with the purpose of creating probability scenarios for the Pleistocene crossings of sea barriers in eastern Asia and in the Mediterranean. Among these are Lombok Strait at >800,000 years ago, the Timor Sea at >60,000 years ago, the strait between Elba and Corsica *c.* 300,000 years ago, that between Andikíthira and Crete *c.* 50,000 years ago, and the Strait of Gibraltar. A series of international expeditions, commenced in 1996, is engaged in result-targeted replication experiments, supplemented where possible by product-targeted replication (Bednarik 1997b,c,d; 1998a; 1999a; 2001b). Rafts have been built with the aid of Palaeolithic stone tool replicas, equipped entirely with materials that would have been available to Pleistocene seafarers at the particular time in question. The purpose of these expeditions is to acquire the data required to construct a scientifically-based (i.e. testable) probability framework that can generate the most rational explanations of how very early maritime navigation may have been achieved.

The first sea-going Pleistocene-style raft built and sailed in modern times was the *Nale Tasih 1*, constructed between August 1997 and February 1998, and dismantled after sea trials without attempting a sea crossing. This vessel was 23 m long and weighed about 15 tonnes plus load, and it carried a crew of eleven. Constructed as a pontoon raft, it was launched by about 400 men who lifted and carried it into Oeseli Lagoon, southern Roti, on 14 February 1998. Split vines (*rattan, Calamus* sp.) and palm fibres (*gemuti*) were used to lash 550 bamboo stalks together. Three rain-proof shelters were constructed from *lontar* palm (*Borassus sundicus*) leaves, and the vessel carried a fire box over which food was boiled in buckets made from palm leaves (*haik*). Fire was made by drilling softwood with hardwood. The raft also carried 170 stone tools, modelled on Middle Palaeolithic types. For experimental purposes the vessel was equipped with two sails of woven palm leaves, rigged on A-frame masts (Fig. 5).

During sea trials in March 1998 the craft was found to be too heavy, and the El Niño effect made it unlikely that a successful crossing of the Timor Sea would be possible. *Nale Tasih 1* was beached for destructive testing (including complete sectioning of a pontoon for the removal of a 30-cm sample), and totally dismantled for inspection of all components. Materials and design were both critically analyzed, various materials were found to be defective, and the performance of different types of bamboo was established. This work led to the design of *Nale Tasih 2*, a bamboo raft that was very significantly lighter and of an entirely different configuration, 18 m long, weighing only 2.8 tonnes plus superstructures and payload (Fig. 6). Built near Kupang, Timor, by only eight men in three months, it performed superbly, effortlessly carrying equipment, supplies and a crew of five. Single-masted and of very simple design, rigged and tied together by forest vines, this vessel crossed from Kupang harbour to the south coast of Melville Island near Darwin in 13 days during December 1998. The shoreline at the presumed time of first landfall in Australia roughly 60,000 years ago is the margin of the continental shelf, which was crossed after only six days. A variety of conditions were encountered on the journey, ranging from calms to heavy

Figure 5. *The* Nale Tasih 1, *a 23-m bamboo raft launched on 14 February 1998, moored in Oeseli Lagoon, Roti.*

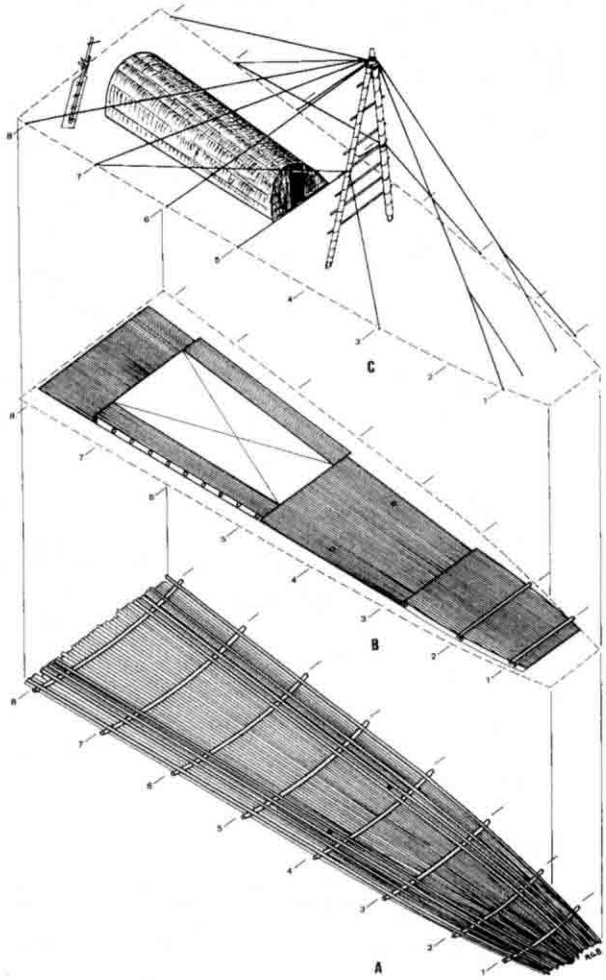

Figure 6. *Exploded view of the* Nale Tasih 2, *showing arrangement of raft body (A), deck (B), and superstructures (C).*

tropical storms with 5-m-high waves. The latter tested the vessel to its very limits, and helped greatly in determining breaking strains of materials and studying the design under stress conditions. Various design adjustments were made at sea using some of the 65 stone tools carried on board, at times under perilous conditions (Bednarik & Kuckenburg 1999). Drinking water was carried in two hollow mangrove tree trunks; food consisted primarily of fish caught with harpoons of Middle Palaeolithic design (such as those from Katanda and Ngandong: Yellen *et al.* 1995; Narr 1966, 123), supplemented by *pottok*, palm sugar and fruit. Upon arrival in Australia, the raft was in a better state than when it had left Timor, owing to design improvements made at sea, and both the vessel and its crew were in such condition that they were perfectly capable of repeating the journey. The raft had travelled almost 1000 km without an escort.

Since this instructive experiment, further replicative work has continued, particularly in working Balinese timbers with chert tools. The first attempt to cross Lombok Strait on an even more primitive raft, in March 1999, had to be abandoned about half way, when it became apparent that the treacherous currents of the Strait were forcing the raft too far north and we would have missed the Lombok coast. This experiment was repeated with *Nale Tasih 4* in February 2000, when twelve men crossed Lombok Strait successfully on a raft as basic as possible, devoid of sail or steering, and propelled solely by twelve crude wooden paddles fashioned with stone tool replicas of the local Lower Palaeolithic (Fig. 7). By this stage our cumulative experience permitted the design of rafts to proceed by empirical means derived

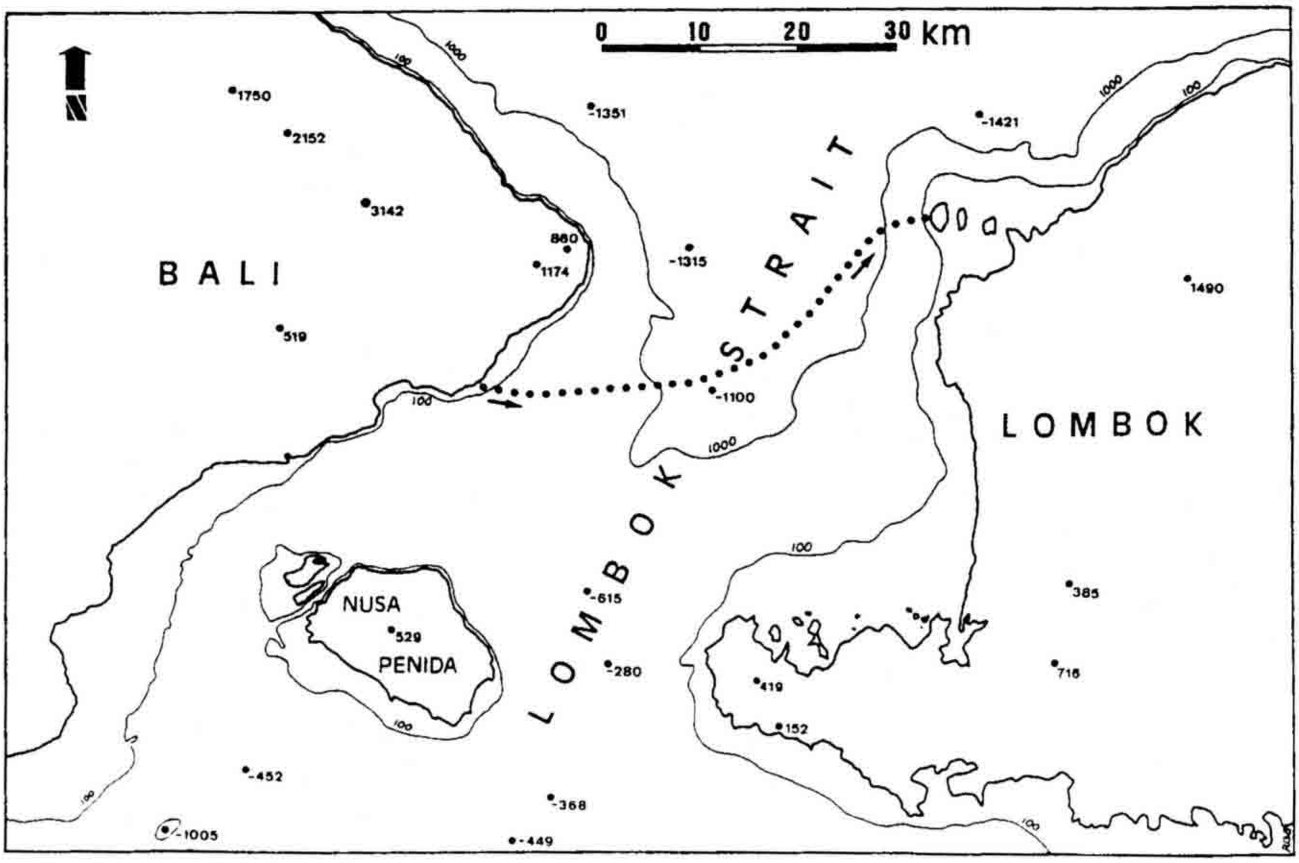

Figure 7. *The route taken by the* Nale Tasih 4, *from the east coast of Bali to the Gillies Islands, just off northwestern Lombok, attempting to replicate the first sea crossing by a human colonizing party.*

from years of testing (Bednarik 2001c). Similar experimentation has commenced in the Mediterranean where so far I have constructed two experimental vessels, one from inflated animal skins and one from cane (Bednarik 1999b). Both vessels were made in Morocco, entirely with Lower Palaeolithic-type implements, and sea-trialled. Further experiments are in preparation.

Discussion

It must be emphasized that I do not suggest that the raft on which first landfall in Australia or Lombok was made resembled any of the *Nale Tasih* versions. The purpose of the project is to determine the minimum conditions necessary for each Pleistocene crossing, which essentially means that the circumstances of severity have to be progressively increased to the point when a successful crossing becomes clearly impossible. In a logical sense I am therefore *not trying to cross* sea barriers, I am trying to find out *how they cannot be crossed* — much in the same way as refutation operates. Therefore the experimental rafts are not actual replicas, as should be obvious; they merely provide building stones within an overall project of acquiring data. Specific implements used and many technological aspects are, however, replicative, or very closely so, and the end result of my experiments should be a close definition of the conditions under which the initial crossings did occur.

Until 2005, when this work is expected to be complete, it would be premature to discuss its results in any detail — even though the empirical understanding of ocean-going raft technology that has been acquired already is most substantial. Some fundamental issues should, however, be clarified. In particular, I would like to take issue with the notion that Pleistocene colonizations might have been accidental, that the seafarers had no intention of departing from their homeland; that they may have been swept out to sea by swollen rivers or caught up in strong ocean currents; or that they drifted to Australia on naturally accumulated vegetation matter. Having sailed all six 'Pleistocene' rafts of modern times, my most important finding is that the Middle and Lower Palaeolithic seafarers were technologically and cognitively far more advanced than archaeology has ever thought possible. Hundreds of cultural skills ('culture' *sensu* Handwerker 1989; Bednarik 1990a) and forms of knowledge are essential to construct a raft of adequate design and size to carry the minimum number of colonizers required, and their essential supplies. Without such a vessel, no colonization was possible, and I submit that such a craft was not built by mere accident.

What we need to ask is why scholars advocating the short-range model of human cultural evolution (or the 'discontinuist approach', as it is called by d'Errico & Nowell 2000) tend to find it necessary to explain away such incredible accomplishments. Such dismissal parallels the efforts of others who deny pre-modern humans the ability to communicate, to use symbols, to hunt effectively, to construct shelters and so forth. Hominids can now be traced back about 7 my, from *Sahelanthropus tchadensis* through *Kenyanthropus platyops* at the half-way mark. At Laetoli, even Australopithecines walked fully erect and rather like modern humans 3.6 my ago (Leakey 1981), while almost 3 my ago, hominids at Makapansgat probably recognized the 'staring eyes' in a jasperite cobble and carried it a long distance into a cave (Bednarik 1998b). Are we to believe that hominids did not progress at all until the Late Pleistocene? We need to ask why some archaeologists find it so difficult to accept evidence of gradual evolution — or of technological, cognitive or intellectual sophistication before the final Pleistocene in France.

It appears that *Homo erectus* was the greatest colonizer in the phylogenetic history of the primates, and also the greatest achiever in a cultural sense. The capacity to domesticate natural systems and energies, for complex communication, for symbolling, presumably for perceiving constructs of reality, i.e. all the traits fundamentally defining modern humans, were first developed by *Homo erectus*, and what *Homo sapiens sapiens* has added to this cultural capital is much less significant, seen in a proper historical perspective. Once these capacities had been initiated, all subsequent developments were logical outcomes of what had been set in motion by *Homo erectus*.

Acknowledgements

I express my gratitude to Bob Hobman, Peter Rogers, Emmanuel Littik, Jacobus Zakawerus, Fachroel Aziz, Eben Unu, Mark Pidcock, Peter Welch, Silvia Schliekelmann, Alan Keohane, Abdeslam El Kasmi, Mohammed Boumahdi, Mohammed Habibi, Georgina Pye, Richard Rudgley, and to the 38 crews and the many builders of the four *Nale Tasih* rafts. The results presented here would not have been possible without them, and the several hundred helpers of the First Mariners Project. Thanks are also due to the members of the Indonesian–Australian Soa Basin research team, to Paul Sondaar, Martin Kuckenburg and to the late Ashok Ghosh.

This paper is dedicated to the memory of Theodor Verhoeven, the greatest archaeological investigator of Wallacea, whose pioneer work was largely ignored in his lifetime.

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Author biography

Robert G. Bednarik is the President of the International Federation of Rock Art Organizations (IFRAO), Chairman of the AURA Congress and Secretary of the Australian Rock Art Research Association (AURA). He edits three scientific journals and two series of academic monographs. He has produced about 1000 publications, nearly half of which have appeared in refereed scientific journals, and he has conducted extensive research in all continents. His primary interest is the origin of hominid cognition.

Comments

From Mike Morwood, Archaeology, School of Human Environmental Studies, University of New England, Armidale, NSW 2351, Australia.

The mainstay of this paper is that there are archaeological sites of Lower Pleistocene age in the Soa Basin of Flores, an Indonesian island located midway between the Asian and Australian continental shelves; and that the associated hominids would have had to undertake at least two sea crossings to reach the island. Some of the details concerning the regional geology and archaeology provided need revision, but the claims made for Flores are substantially correct.

As outlined in the paper, archaeological and palaeontological research in the Soa Basin by Verhoeven (e.g. 1968) and an Indonesian–Dutch team (e.g. Sondaar *et al.* 1994) indicated that the island had been colonized in the Middle Pleistocene by *Homo erectus*. These findings were generally judged inconclusive, however, because of doubts about the status of the 'artefacts', their stratigraphic association with the fossils, and the age of the strata (Allen 1991; Bellwood 1985, 66). More recent work by our Indonesian–Australian team has not only confirmed the previous findings and claims, but has also provided much more contextual and geochronological data (O'Sullivan *et al.* 2001).

From 1997 to 2001 we undertook mapping and dating of all major geological strata in the Soa Basin (almost 1000 km^{2}); systematic surveys to record palaeontological and archaeological sites; and excavations at five of these sites — Mata Menge, Boa Lesa, Dozu Dhalu, Pauphadhi and Tangi Talo. A total of 30 fossil sites were located in the Ola Bula Formation ranging in age from 900,000 to 700,000 years BP.

The earliest, Tangi Talo, with a highly endemic fauna of pygmy *Stegodon sondaari*, giant tortoise and komodo dragon, appears to represent a mass natural death site, in fact an extinction event, and predates any evidence for hominids (van den Bergh 1999). Other fossil sites are located upsection and accumulated in localized drainage channel or beach deposits usually sealed in by layers of tuffaceous silt. The fossils comprise the full-size *Stegodon trigoncephalus florensis*, komodo dragon, crocodile and rodents *(Hooijeromis nusatenggara)*. None of these fossil sites older than 840,000 BP contains stone artefacts (e.g. Dozu Dhalu, Sagala, Ola Bula). In contrast, all the younger sites, with the same depauperate range of fossil species, contain *in situ* stone artefacts (Morwood *et al.* 1999).

Presumably, on the basis of age, *Homo erectus* made the stone artefacts in the Ola Bula Formation, while our excavations in West Flores have shown that premodern hominid occupation of the island continued into the Upper Pleistocene right up to the arrival of fully modern people (Morwood *et al.* 2002): this was no transient occupation of the island.

There are two points worth emphasizing. Firstly, the impoverished range of fauna evident on Flores from the Lower Pleistocene to the beginning of the Holocene clearly shows that sea crossings were a major and continuing biogeographical obstacle to the migration of Asian land animals to the island. Basically, rodents, *Stegodon* and hominids were the only such animals to reach the island during this time, while even excellent island colonizers such as deer, pigs and macaques had to await human assistance to reach Flores in the Holocene (van den Bergh *et al.* 2001).

Secondly, long-term parallels between the stone artefact technological sequences on Flores and Java over an 840,000 year time span indicate consistent social (and genetic?) exchange between Flores and continental Asia — the scenario of a pregnant woman crossing from Bali to Lombok/Sumbawa (then to Flores) on a log will not suffice, nor will accidental crossings on natural rafts, or temporary land bridges across the Wallace Line (cf. Groves 1996; Smith 2001).

My purpose in commenting here is to indicate that the foundation data used in this paper to argue the case for long-term and gradual development of hominid symbolling capacity over a minimum of 840,000 years, and possibly much more, is rock solid: I do not have the knowledge, expertise or wit to comment on other archaeological data or inferences used to support the argument, but can say with absolute confidence that archaeological sites of Lower Pleistocene age do occur on the Wallacean island of Flores; and that hominids managed to reach the island when most other Asian animals could not. The most logical explanation is that watercraft capable of making sea crossings at least 25 km in length were in use by 840,000 BP. There also seems general consensus that language use is a prerequisite for the logistics and planning required to construct watercraft (e.g. Davidson & Noble 1992). Maybe Bednarik is right.

From Michael Rowland, Cultural Heritage Branch, Environmental Protection Agency, PO Box 155, Brisbane Albert Street, QLD 4002, Australia.

There can be little question of Robert Bednarik's commitment, over a period of more than thirty years, in attempting to understand the technological, cognitive and cultural development of hominids. His publication output (over 1000 entries) in several languages is formidable. I am impressed by his attempts at replicative experiments on a range of materials to which he alludes in this article (p. 47ff.) and in particular to watercraft, having myself made a small contribution (see Rowland 1995). Readers might also find Bednarik's web site, The First Mariners Project, at http://mc2.vicnet.net.au/users/mariners/ to be of interest.

Bednarik's article should be judged solely on its merits, but this is extremely difficult to do when one is aware of the fuller content of papers of his own that he cites here and others that he does not. In these we find a good deal of repetition (which may not be a major problem) but also evidence of the driving force behind his ideas (which seems to me to be a problem). Although subdued, there are residuals of his ideas in the current article. For example, in the final paragraph he concludes

> *Homo erectus* was the greatest colonizer in the phylogenetic history of the primates, and also the greatest achiever in a cultural sense . . . and what

> *Homo sapiens sapiens* has added to this cultural capital is much less significant, seen in a proper historical perspective.

Is there anyone that would actually accept such a view?

Also, take this from a 1998 paper not cited in the current article:

> Only a few decades ago the initial landfall in Australia, then still thought to have occurred during the Holocene, was considered to have been the result of accidental drift, of individuals having been washed out to sea helplessly, perhaps clinging to some log or floating vegetation. The absurdity of this desperate scenario was symptomatic of a neocolonialist, Eurocentric attitude to alien societies, a form of epistemology that still determines attitudes to, and interpretations of, archaic *Homo sapiens* populations. Concepts of relative primitiveness dictate our Darwinist thinking, as if Pleistocene hominoids had been simple organisms exercising no control whatsoever over their individual destinies (Bednarik 1998c, 14).

Or this from the very lengthy paper of 1997 selectively cited in the current article:

> 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 premature death.
>
> 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 (Bednarik 1997b, 17).

Bednarik also complains that the reason behind all this has never been explained to 'we the uninitiated'. In fact it appears he is not uninitiated but indeed has a superior knowledge base to many of the rest of us in the form of a 'scientific pre-History' versus an 'archaeological prehistory':

> [Which] 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 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 (Bednarik 1997b, 50, emphasis in original).

Under the heading of 'Epistemology and politics in archaeology' Bednarik continues:

> 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 . . . Never before has an entire hominid species been implicated in appropriating the achievements of a preceding *species*.
>
> This pattern of response is typical in archaeology, a discipline in which saving face is consistently considered more important than refutation and veracity (Bednarik 1997b, 45–6, emphasis in original).

According to Bednarik the navigational abilities of *Homo erectus* have been known for forty years but the information had been ignored:

> 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* (Bednarik 1997b, 46).

Morwood and his colleagues are currently attempting to confirm or negate the evidence for the presence of *Homo erectus* on Flores. It appears they have done that. *Homo erectus* appears to have been on Flores by at least 800,000 years ago. It is highly likely they got there by crossing water barriers. They may or may not have had language. The relationship between *Homo erectus* and modern human populations in the area remains uncertain. The facts remain, however, that there was a flourishing of people and watercraft throughout Mainland Southeast Asia, Wallacea, Melanesia and Australia at about 40,000 to 50,000 years ago. Art, ornamentation, symbolism, ritual burial, sophisticated architecture, land-use planning, resource exploitation and strategic social alliances flourished at this time. Population increased, intensified exploitation of small prey occurred, populations expanded to higher, colder latitudes, and so on. No doubt *Homo erectus* (or some other forms of hominids) developed some of the skills that were to flourish with *Homo sapiens sapiens* but there is a lot yet to be explained in getting from 800,000 years ago to 50,000 years ago.

The invention of watercraft introduced an entirely new contact/isolation mechanism in getting from mainland to islands into the course of human evolution and adaptive radiation. This would be one of many interesting foci of debate in this case.

I found the story of *Nale Tasih 2* and its 13-day trip to Australia interesting and would like to know more but as for the rhetoric, well enough said.

From Matthew Spriggs, School of Archaeology and Anthropology, A.D. Hope Bldg, Australian National University, Canberra, ACT 0200, Australia.

Shortly before his untimely demise in 1976 Eric Higgs posed to me the conundrum that when reindeer migrate in search of food we call that Nature, but when human hunters follow them, also to obtain food, we call that Culture. The dispersal of elephant-like Stegodons, Geochelene tortoises and large Varanid lizards across short sea gaps to Flores, and then on to Timor, at about 800,000 years ago is clearly a natural phenomenon. It relates to a major faunal turnover that occurs world-wide at that time, apparently to do with the start of extreme global climatic oscillations and generally lower sea levels than had occurred in the previous period (van den Bergh *et al.* 2001, 404). If we take account of these global events then the dispersal of *Homo erectus* across the sea gaps to Flores, and probably also to Timor is perhaps not as dramatic an event as Bednarik seems to suggest. Analysis within this wider framework might be useful, but, of course, as he seeks to demonstrate, the hominid dispersal was a paradigmatically cultural achievement.

Bednarik is perhaps the strongest contemporary champion of the cultural achievements of *Homo erectus*, and equally strong in minimizing those of *Homo sapiens*. I have no particular problem with this boosting of *Homo erectus*' intellectual and linguistic achievements. After all, given subsequent history the species can hardly speak up for itself. But I do have a problem with distortions of the intellectual faculties of particular groups of *Homo sapiens* seen in Bednarik's treatment.

For instance, there seems to be a somewhat wilful distortion of the history of archaeological research in the Wallacean region to run down the contribution of others involved in it (admittedly a typically *sapiens*-type activity). Thus regional (particularly anglophone) archaeologists are castigated in this paper, and other recent Bednarik publications (1997b,d) for ignoring the pioneering work of Verhoeven in Flores and Timor. In related vein, we are told that: 'In-depth research into the Pleistocene human occupation of Timor commenced only in December 1998'. The work referred to is of course by Bednarik himself. Both of these statements are untrue. On the first, Bednarik could have referred to two publications by Glover (1969; 1973) which he cites in earlier articles but not this one, both of which give due reference to Verhoeven's work. Glover (1973, 122–5) gave particular attention to Verhoeven's contribution. It also noted that Indonesian archaeologists had followed up on Verhoeven's findings in Indonesian Timor, as in 1970 did the celebrated palaeontologist Hooijer. On that occasion archaeologist Tegu Asmar had found

> a few flakes and core tools *in situ* in bone-bearing layers in Timor; and so we have from Flores and Timor, as from Celebes, evidence to show that man, possibly a fossil form of man, was present in the islands of Wallacea in the Middle to Late Pleistocene, and was contemporary there with a now extinct megafauna derived from Asia (Glover 1973, 125).

The first general monograph-length synthesis of Southeast Asian and Pacific archaeology, published in 1978, further referred to Verhoeven's work (Bellwood 1978). The reference to Hooijer and Asmar's research should be enough to suggest that in-depth research on Timor predates Bednarik's own work. But Glover himself had previously established a Pleistocene occupation of the island during very extensive PhD fieldwork in East Timor during 1966–67 (Glover 1972; 1986). The details of this have been pointed out recently by O'Connor (2002) in answer to another contentious piece by Bednarik (2000). I would only add that the date of 13,400 BP which Glover obtained from Uai Bobo 2 was reported in a 1969 paper which Bednarik had, as reported above, previously referred to in his own publications. He thus really has no excuse.

In relation to the replicative experiments reported on in this article, I would encourage Bednarik to follow up on his stated aim of finding out how sea barriers 'cannot be crossed' rather than how they can be. As he realizes, the fact that — knowing what a modern human knows about the world — we can use tools of similar form to those available 800,000 years ago to construct sophisticated rafts to our modern mental templates does not necessarily tell us very much. There is no evidence that sails existed on boats in this region prior to the Neolithic, on the basis of distances crossed and conditions likely to be encountered on early attested crossings. I would follow Bednarik's point in an earlier paper: 'if the crossing was humanly possible without a sail, then it ought to be undertaken without one' (Bednarik 1997b, 34). Thus *Nale Tasih 3* and *4* sound far more like the kind of vessel used to reach Australia than *Nale Tasih 2* could have been. Given Bednarik's ongoing search to find out how sea barriers cannot be crossed, one wonders on what basis he can be so certain that colonization cannot have been accidental, by being caught up in strong currents. After all, strong currents between Bali and Lombok necessitated the abandonment of *Nale Tasih 3* when 'the treacherous currents of the Strait were forcing the raft too far north and we would have missed the Lombok coast'. Where Bednarik's own accidental voyage would have finally fetched up we shall sadly never know. It also seems premature to rule out even the use of natural rafts of vegetation in colonization. If the aim is really as stated then surely an attempt needs to be made in such a 'craft' to see whether it is in fact impossible to colonize across short sea distances in this way. May the experiments continue.

From Iain Davidson, School of Human and Environmental Studies, University of New England, Armidale, NSW 2351, Australia.

Robert Bednarik has previously made a valuable contribution to the understanding of the archaeological history of hominins and humans. His catalogue of many of the claims for evidence of early symbol use (Bednarik 1992a) showed just how patchy the record was and allowed a sharper focus to the identification of suitable criteria for recognizing it (Chase & Dibble 1992; Davidson 1992). In much the same way, this article collects a variety of claims about early navigation, but does not settle the issue because each of them needs to be treated critically before it can be accepted. Cherry's (1990) reviews of navigation in the Mediterranean gave some idea of how this might be done, and reached a rather different conclusion from Bednarik, namely that there is no good early evidence for seafaring in the Mediterranean. It is true that Sondaar's claims are more recent than Cherry's latest review, so that it would have been good to have a treatment of them as thorough as Bednarik's review of Morwood's work in Flores. What we are left with is a bibliography of those cases that Bednarik thinks support his argument, but with little supporting evidence given for their inclusion on his list.

Bednarik also is to be congratulated on bringing the collected works of Verhoeven to a wider audience, but his criticism (elsewhere) of those of us who have not read these works in the original sits strangely with his own patchy use of bibliography. Thus, for example, he does not cite Cherry's paper. In various publications (e.g. Bednarik 1992a) he prefers to use his own line drawings of an object rather than photographs published in his own journal (Mania & Mania 1988) which show crucial evidence of chewing by carnivores, omitted in his drawings — despite his editing a paper which points out the importance of these toothmarks (Davidson 1990). He leaves it very unclear (at least in the version of the paper that I read) who is the archaeologist responsible for the data from Flores that he cites despite knowing full well that it is Morwood's work. He alludes to the Berekhat Ram modified object from its original publication — where it was far from clear that the pebble was modified — and omits the definitive publications that demonstrate the modifications (d'Errico & Nowell 2000; Marshack 1997). He does cite the d'Errico & Nowell article elsewhere, but says, unfairly, that it is an example of a dogmatic defence of a short time-scale, despite the fact that any scrutiny of d'Errico's work would show that he has shown remarkable open-mindedness on this subject (d'Errico & Villa 1997; d'Errico *et al.* 1998; d'Errico *et al.* 2001; d'Errico & Nowell 2000). Bednarik mentions the work of Jones (1989) and Thorne (1980; 1989) but omits to mention that they too experimented with watercraft.

He fails to cite my paper with Noble (Davidson & Noble 1992) which tried to grapple with some of the issues about language and watercraft, though he certainly knows the work. Instead, he cites other work of ours as suggesting that the issue about language is 'skilled and standardized use of communication', although Noble and I (1992) consistently stress the implications for language of the mental abilities implied by the building of a watercraft. And he does not deal at all with the challenge Foley (1991) set to Noble and me which led to our 1992 paper — the evident crossing of water barriers by primates colonizing the Americas. Accidental colonization by rafting on mats of vegetation still seems a good bet for those primates, as well as for the appearance of hominins in Flores (Davidson 2001; Smith 2001), particularly as the formation of such rafts may be one of the distinctive differences between the Indonesian archipelago (where they do form) and the Mediterranean (where, I suspect, they do not).

And here, I think, is the most important role for experiments in watercrossing of the type described by Bednarik. Of course it is possible to construct a huge boat (though on what grounds it can be called 'Pleistocene-style' is not mentioned — especially given that there are no rock-art depictions from the relevant period) which will make the voyage more successfully than some of the vessels used today by refugees. But as Bednarik admits, there were many aspects of the original effort that are fairly unlikely conditions at 800,000 years ago. In particular, there is little evidence at 800,000 years ago for cooperative efforts of any sort, less for groups of 400 hominins, little for shelter, little for fire, still less for its controlled use in a 'fire-box', little for cooking and none for the sorts of storage of food and water implied by the conditions Bednarik allowed himself. Of much more interest is the last raft which was 'as basic as possible' — although, characteristically, Bednarik gives no indication at all of what this was like. If we can take Bednarik's word here, this may have been a very simple platform, not unlike a natural raft of vegetation. On this, with minimal effective steering, he and his companions were able to drift from Bali to Lombok. It seems to me that this is the best evidence yet that not much may have been required in terms of technology for hominins sometimes to be lucky in making landfall. Presumably, many other times they were not, and presumably some times the numbers of individuals who crossed were not sufficient to establish an on-going population.

Yet again, I would interpret Bednarik's work very differently from him. I think he has come close to making an important contribution against his own argument — as I think he did in 1992. For both of these, I think we should be grateful to him.

From Ursula Mania, Forschungsstelle Bilzingsleben der, FSU Jena, Forstweg 29, 07745 Jena, Germany.

We welcome the trend perceptible among some authors confirming our idea that early humans acquired remarkable cognitive capacities in the course of their development. With the help of our more than 30-year-old excavation and successive, continuous research work of the camp site of Bilzingsleben in East Germany (approximately 350,000 years BP) we are able to present quite a number of finds and contexts proving that *Homo erectus* was able to create its own socio-cultural environment which was necessary to secure its survival in a warm-temperate climate.

Appropriate and conspicuous evidence is represented by the discovery of three dwelling structures, of fire-places set up in front of them, of different workshops serving various functions, of a standardized lithic industry, of deliberately selected raw materials used for the production of tools showing different functions, of a paved area quite different from activity zones found at the site, and last but not least, of deliberately engraved bone artefacts confirming even the existence of language.

We are confident that we can prove with these discoveries at Bilzingsleben that early humans had more intellectual/cultural capacities than are often conceded to them by some authors. Additional and valuable evidence is obtained by another Lower Palaeolithic site situated at the northern border of the Harz mountains. At Schöningen, around a hundred kilometres from Bilzingsleben, a remarkable discovery was made a few years ago: eight wooden spears and one throwing club were found dated to approximately the same age as Bilzingsleben. They constitute sufficient evidence that early humans, i.e. *Homo erectus*, cannot be regarded as passive hunters scavenging carcasses. They must have used these optimally-functioning wooden instruments in active hunt. In Bilzingsleben, for example, 30 per cent of the quarry were rhinoceros, and 11 per cent straight-tusked elephants. Here, too, wooden imprints of spears were discovered, though only badly preserved since embedded in calcareous layers. We do not regard scavenging the basis of early human subsistence.

I have gone into this detail in order to show how much evidence we have from the site of Bilzingslenben for the conspicuous cognitive abilities of early humans. The same is true of the site of Schöningen with the discovery of the spears.

The situation seems to be different with Bednarik's idea of early human (800,000 years BP) navigational capacities which he thinks enabled them to reach the Australian continent from the island archipelago. This idea is based on the assumption, which in the case of Bilzingsleben and Schöningen can be proved to be right, that early humans were able to control natural forces in the sense they needed and wanted. In the case of Bednarik's investigations the situation seems to be somewhat different. We think as long as no solid evidence is found in Australia that *Homo erectus* really lived there in the Lower Palaeolithic, it cannot be assumed that they reached it at this early time. We think there is no support for Bednarik's claim *Homo erectus* navigated to Australia as early as 800,000 years ago.

From G.A. Clark, Department of Anthropology, Arizona State University, Tempe AZ 85287-2402, USA.

Using the coarse-grained time–space grid of initial human colonization of Island East Asia, Bednarik establishes what he takes to be the minimal logical imperatives necessary to account for a sustained human presence in the region after *c.* 750,000 years ago, when the earliest convincing archaeological sites occur on Flores, Roti, and Timor. Even during maximum sea-level regressions, hominids (probably *Homo erectus)* must have crossed bodies of water >30 km wide, beyond the capabilities of even the most adept long-distance swimmers, thus implying technological solutions (i.e. rafts) and sophisticated forms of communication (i.e. language). The major implication is that the cognitive abilities of Lower and Middle Pleistocene hominids have been seriously underestimated.

I am in broad agreement with Bednarik's argument, his construal of the evidence in support of it, the implications for other colonizations (e.g. Europe: see Tobias 2000), and for language. I think our fixation on *modern* human origins has caused us to overlook or de-emphasize evidence for earlier cognitive evolution that calls into question our notions of 'modern humanness'. In my opinion, the essay turns on the alleged uniqueness of modern human language. What is it? How do we define it? When did it occur? Was its appearance an 'event' or a 'process' in evolutionary space–time?

My own (non-specialist) view is that language is an emergent property rooted deep in our evolutionary history as social primates (see e.g. Cheney & Seyfarth 1990; Hauser 1996; papers in Hauser & Konishi 1999). I think it probably evolved from something like the 'social chattering' of gelada baboons and/or the gestural 'language' of chimps, both of which appear to have rudimentary elements of syntax and grammar (see e.g. papers in Zimmerman *et al.* 1995; Strier 2000, 272–301). The fact that we can (with considerable difficulty) teach language to chimps, bonobos, and gorillas in laboratory contexts probably means that hominoid brains come 'hard-wired' with the requisite neural circuitry for language acquisition. This means that the last common ancestor of chimps and humans, somewhere back in the late Miocene, probably was also 'hard-wired' for language acquisition. In other words, it's a plesiomorphy (primitive retention) within the hominoid clade (more accurately, the epigenetic and developmental predisposition for language acquisition is a plesiomorphy).

Apes, of course, do not speak, nor can humans acquire normal, functional language if they are deprived of a social context in which to do it before a 'chronological window' at *c.* 11–12 years of age. We know this from studies of children who were isolated from contact with other people from birth to early adolescence (e.g. Rymer 1992). They never acquire language in later life, are in fact incapable of doing so. Arguments from cognitive neuroscience, psychology, non-human primate capacity, early hominid intelligence and the successful, long-term radiation of early hominids in the absence of large-scale genetic change all suggest that a 'language-like' communicative *repertoire* was present during the Middle, and probably Lower Pleistocene — that it goes back to the origins of *Homo* (*c.* 1.8 mya) and might even pre-date the ape–human split (*c.* 6–7 mya). Although modern language is symbolic, it almost certainly evolved from non-symbolic gestural and/or representational antecedents, and then subsequently exapted (i.e. gradually took on other functions). When this occurred is subject to debate, but it clearly was a 'process' (and not an 'event'), and almost certainly had nothing to do with either genetic superiority or the kind of simple-minded 'replacement' scenarios seen in modern human origins research. There is nothing radical in these observations. Darwin himself suggested that language was a gradually selected capability that emerged from more primitive forms of communication evident in animals (1871). It may well be that the only uniquely human characteristic of language is recursion (the ability to generate an infinite range of expressions from a finite set of elements), but even that might have arisen for reasons other than communication (e.g. computational systems outside the domain of language) (Hauser *et al.* 2002).

With respect to language and modern human origins, there is no question in my mind that Neanderthals had fully modern language. If 'art' is taken as an indicator of symbolic capacity equal to our own, and is used as a surrogate for language, it emerged when and where it did (in SW Europe after *c.* 25,000 years ago) not because it was 'imported' by modern humans from somewhere else, but because of social and demographic factors that selected for it in the European refugia during the pleniglacial maximum to the extent that it became visible archaeologically for the first time. This is basically Gamble's 'art as information' argument — the incidence of 'art' is a tangible indicator of the volume of information flowing through channels cut by alliance networks of various kinds (e.g. 1982; 1991). These alliance networks were under selective pressure, and emphasized, abandoned, strengthened, etc., according to context. One would expect 'art' to be most manifest materially under conditions of demographic stress. Demographic stress would have been most severe in the Franco-Cantabrian refugium during the pleniglacial maximum (21–13,000 years ago), when much of the rest of Europe was depopulated (Barton *et al.* 1994; Clark *et al.* 1996). Neanderthals probably produced 'art', but they were so thinly spread on the landscape that it never became visible archaeologically (at least so far . . .).

Hominoid ethology demonstrates unequivocally that the higher primates in general, and apes and humans in particular, have a genetic predisposition to incorporate a lot of learning into the behavioural *repertoire.* Symbolism is an important kind of learned behaviour, but it didn't 'just happen', as those who would equate its appearance with modern humans or the Aurignacian would have us believe. Symbolic behaviour clearly had adaptive significance. It increased the inclusive fitness of the individuals and groups that engaged in it over the evolutionary long term, but it probably arose in contexts very different from those in which it is manifest today. This means we'll only be able to detect it archaeologically long after it had become a significant part of human behaviour.

About veracity and audacity in archaeology: a reply from Robert G. Bednarik

Perhaps the most instructive aspect of this debate derives from Rowland's commentary, which might help illuminate the faint latent sentiments detectable in some of the above comments. Rowland barely responds to my article, citing instead out of context a series of passages from other publications. His intent is not made explicit.

I think that the objective value of an academic debate is determined by the heuristic potential it offers the readers. The most effective way of turning the present debate into a more meaningful learning experience is perhaps to broach some of the sensitivities Rowland touches upon. If veracity in archaeology were not important to me and I did not know that the discipline made lots of mistakes it would not matter to me, a scientist, whether any archaeologist 'accepted' my 'findings'. As it happens, archaeology has a history of bungling unmatched in other academic endeavours. Moreover, those who tried to correct mistakes in archaeology have invariably been treated badly. Once they were vindicated (usually posthumously) the establishment appropriated their ideas without acknowledging the hardships these pioneers had suffered at the hands of the discipline. Acknowledgment of their contribution is only apparent in the few cases that have become so well known that it was unavoidable: de Perthes, Fuhlrott, de Sautuola, Dubois, Dart, Heyerdahl, Leakey and Marshack come to mind.

Archaeology is a complex field with many specializations, all of which are in significantly better epistemological shape than Pleistocene archaeology. The simple reason for this is that, without understanding and employing a specific form of logic called taphonomic logic or metamorphology, it is impossible to derive valid interpretations from the raw data of Ice Age archaeology, except by pure chance. Since taphonomic logic as a formal method was introduced only recently (in this very journal in fact, albeit in embryonic form: Bednarik 1992b; cf. 1994), we must assume that most archaeological constructs of the Pleistocene are likely to be false. The severity of this condition is clearly a function of age: the older the evidence, the greater the probability of falsity. This conclusion is unavoidable, but we have yet to see it reflected in the model-building strategies of Pleistocene archaeology. The discipline is continuing without a valid universal underlying theory, in the same haphazard way it has floundered through the nineteenth and twentieth centuries. Interpretive synthesis is formulated essentially by a combination of personal authority and zealous group consensus. It is this second variable which Rowland refers to when he asks rhetorically, 'Is there anyone that would actually accept such a view?'

Scientific veracity is certainly not the outcome of some democratic process. Throughout its history we have seen examples of how all of archaeology was emphatically wrong while one single (non-archaeologist) dissenter turned out to be right. For instance, all commenting archaeologists claimed that the Côa petroglyphs in Portugal date from the Pleistocene. For a century archaeologists have maintained that they can determine the age of rock art in the caves of Franco-Cantabria by looking at its style. Yet it was the solitary claim that these stylistic constructs for the Upper Palaeolithic are often false (Bednarik 1995c) that turned out to be correct. Aurignacian art, we had been told for a century, was very simple and schematic, because art evolved from the primitive to the sophisticated. Yet the most sophisticated rock art we know from the Upper Palaeolithic, in Chauvet Cave, is also the oldest we have ever dated. The experts on this subject could not have flunked the test more decisively. I have shown, time and again, that archaeologists cannot securely distinguish between rock art and other rock markings, or between portable palaeoart and non-'art'. I have demonstrated that they often do not understand the falsifiable propositions they import from scientific disciplines, that they misuse and misquote them far too often (e.g. Bednarik 2002). But archaeology seems incapable of accepting, from the hundreds of similar experiences, that they indicate systematic structural flaws within the discipline and that it is essential to explore these if we are to achieve real progress. This is a much more important task in archaeology than the acquisition of more data, the training of more scholars or the securing of more funding. I have examined the epistemological defects of the discipline for decades, identified and described some of the most important ones, and I have even shown how they can be rectified. They include the lack of falsifiability, the operation as a belief system, the inappropriateness of uniformitarianism, the lack of real dialogue with indigenous interests, the antiquated epistemology, and particularly the absence of a valid scientific universal theory. For this effort I have attracted the ire of archaeologists who object to being corrected — most especially by non-archaeologists.

These are unpalatable issues, but is a discipline that is incapable of confronting them worthy of an academic existence? Some of my respondents cannot accept that the presence of early hominids on Flores was first demonstrated almost forty years ago (by a non-archaeologist), and that because it was not presented in English, subsequent commentators misunderstood these reports. In the mid-1990s I wrote a couple of rather frustrated papers about this, and it is most gratifying that within a few years Morwood took up the challenge and established a substantial and spectacularly successful research project in Flores. More recently, Spriggs with others followed me to Timor, and while I welcome his work and his interest, I am less enthusiastic about his belittling of my previous work there. This points to yet another issue in the discipline, the undignified clamouring for attention: many practitioners seem to think that their own work is undervalued, while at the same time disparaging that of their predecessors.

Spriggs sees one sentence in my article as a 'wilful distortion of the history of archaeological research' in the region to 'run down' the contribution of others, including presumably his own. Oddly enough, in his next sentence he castigates me for criticizing him and his colleagues for ignoring the work of Verhoeven. So according to his own observations I am critical of the work of some while praising or promoting that of others. Why should that attract disapproval? I use the same standard in judging the work of Verhoeven as I use in judging, for instance, Spriggs' work, and I find his performance, or Glover's, lacking when compared with Verhoeven's.

It is definitely not my wish to be unfairly critical, but my first consideration is the reader, and what he or she can gain from this debate. For that reason alone, and not to respond in a polemic mode of discourse, I will illustrate my criteria for judging research standards including Spriggs'. He accepts Glover's sole Pleistocene 'date' as proving Pleistocene human occupation in Timor. That date is from the basal layer of Uai Bobo 2 and it does not date any specific event. It was obtained from a composite sample comprising three types of material (charcoal, bone and seed-cases) 'scattered throughout Horizon I' (Glover 1971). Moreover, of the 5573 lithics from this small site, only one was excavated from below Horizon IV (about 8000 BP). With rich assemblages (up to 2269 lithics per layer) from the younger layers, this lowest singular flake looks totally isolated to me, and its sediment is overlain by two entirely sterile horizons. Shelter sediments of this kind are frequently disturbed, e.g. by animals ranging from rodents to termites, and I feel that more than one stone flake and more than one questionable carbon isotope analysis are required. Spriggs evidently accepts this evidence for a Pleistocene occupation hitherto not demonstrated, but I do not. Moreover, we know that carbon isotope results stated at one standard deviation have about one chance in three of being 'false', they are not numerical ages but statistical approximations.

Human Pleistocene presence was therefore not satisfactorily demonstrated in Timor prior to 1998 *in accordance with my standards of proof*, but I concede that it was according to *Spriggs' standards of proof*. We shall have to agree to disagree about what constitutes 'adequate proof'. Concerning the standard of Glover's work, I have already discussed this in some detail elsewhere (Bednarik 2000), and the work of 'celebrated palaeontologist Hooijer' (cited by me many times) is not relevant to 'in-depth research of Pleistocene human occupation'. Similarly, a passing mention of some unprovenanced stone tools does not, I submit, amount to such research. Again, Spriggs' standards and mine differ substantially. Insistence on certain minimum standards is no justification for attributing *ad hominem* motives, such as 'wilful distortion'.

Davidson, characteristically, chides me for failing to cite his and Noble's paper (Davidson & Noble 1992). Entitled 'Why the first colonisation of the Australian region is the earliest evidence of modern human behaviour', it argues essentially that the initial landing in Australia, presumed to have occurred about 40–60,000 years ago, is the earliest proof we have of language. Its key argument, that maritime navigation would not have been possible without effective communication, is certainly valid and has my support for reasons too complex to rehearse here. The problem, however, is that Davidson was not aware of the numerous earlier sea crossings demonstrated elsewhere. They included the Flores evidence, the validity of which has since resoundingly been confirmed by Davidson's close departmental colleague, Morwood. So by locking himself into the argument that seafaring demonstrates language use, Davidson effectively negated his main hypothesis, which was perhaps the most extreme form of short-range theory we have ever seen ('We propose that all human ancestors [prior to moderns] should be considered as apes, closer to chimpanzees than to humans': Davidson & Noble 1990). If seafaring proves the use of language, then language is a million years old, not 40–60,000 years as he claimed.

Davidson now argues that hominids might have reached Flores on mats of vegetation. I have published a detailed report of the Lombok Strait crossing in a major international journal and cited it in my paper (Bednarik 2001c), yet Davidson writes: '... characteristically, Bednarik gives no indication at all of what [the Lombok crossing] was like'. I have described in detail how one might succeed in crossing the Strait on a simple platform of bamboo, and the harrowing conditions of such an undertaking. My report recounts the severe exhaustion of the mariners, and how one of us lapsed into a coma. I have explained why past and present maritime conditions render the idea of a human breeding population crossing the Strait on natural vegetation matter totally absurd. I have clarified why certain crossings were only possible after specific technological thresholds had been reached, and what these were, and why.

Morwood focuses in his comment on his own current excavations in Flores and adds some fascinating new information, especially concerning the Late Pleistocene occupation. Readers will welcome this valuable material. Similarly, Mania discusses another corpus of evidence I have made good use of, showing the relative sophistication of Middle Pleistocene hominids in northern Germany. It is unfortunate that she misunderstood my article and thought that I advocate the presence of *Homo erectus* in Australia. I have of course never suggested this — not in this article or anywhere else. In fact I have developed elaborate arguments why *Homo erectus* is unlikely to have reached Australia, and I was the very first researcher to publicly reject the claims concerning Jinmium. Mania demonstrates that Anglophone archaeologists have no monopoly on misconstruing information presented in a language other than their own.

One of the present commentators engages the 'greater picture' I have tried to paint and does justice to the gravity of some of the universal issues I have broached. I find Clark's pertinent comments just as precisely articulated and eminently instructive as his previous work. He offers several decisive statements, each defining a key issue. Neanderthals, among Davidson's 'apes', had fully modern language and probably produced art-like work. The kind of simple-minded 'replacement' scenarios we have had to endure in recent years have no merit. In matters of human evolution, hominoid ethology and a variety of other disciplines need to be consulted more exhaustively — as does Darwin's ever-relevant work. Clark also reminds us, by his example, that in academic writing we need to focus on the data and the ideas, not on the perceived motives of our opponent. Whilst I fall short in matching his discipline in debate (for which I crave the indulgence of the readers), I do aspire to it, and I do hope that I have managed to clarify some important points about archaeology and the way it is being conducted. One minor point: Clark thinks that the 'art' of Neanderthals has not yet been found. There can be no doubt that the La Ferrassie cupules (Peyrony 1934) were made by these humans: they were found hammered into the underside of the large rock placed on a Neanderthal infant burial. And we should not forget that their archaic *sapiens* contemporaries produced identical rock art at around the same time in Australia, and their predecessors in India earlier still. As I have said before, it appears that we have far more Middle Palaeolithic rock art in the world than Upper Palaeolithic.

On the cognitive competence of hominids and related matters we have a spectrum of views, from Davidson's 'apes' to Clark's in most ways 'modern' Neanderthals. As I have said before, we do not need archaeologists to know that the abilities of Neanderthals were somewhere between those of modern humans and apes. We do not tolerate determinations by chemists that a given substance might have a pH of between 0 and 14; we expect considerably more precise formulations from them. If, after two centuries, archaeology still wavers between defining early humans as apes and as fully human creatures, we need to ask what causes such coarse resolution. I think that my investigation into the epistemology of this troubled discipline has shown that the reasons are an endemic failure of academic knowledge, certain language barriers and confirmationist modes of reasoning, combined with the cultivation of consensus and systematic denouncement of academic dissenters. To help illuminate the issue of hominid competence I have presented a series of falsifiable propositions about Pleistocene seafaring, a topic no researcher has previously investigated in any consistent fashion. In response, Davidson argues that some crossings can be made on natural mats of flotsam, though it remains a mystery why he claims that some other crossings in the same region must have involved watercraft. So why does he not demonstrate that he and a group large enough to form a colonizing population can cross Lombok Strait on a vegetation mat? That is what is required to test my hypothesis. What he does not know is that straits cannot be crossed by *drifting*; they have strong transverse currents that frequently change direction. Thus *any* carrier needs to be propelled by some means, and the crossing of any strait is fraught with difficulties. If any group can succeed in crossing Lombok Strait on naturally accumulated vegetation flotsam, I will at once withdraw *some* of my hypotheses about early seafaring. There would, however, remain an issue of logic: how would we explain that hominids were the sole large species that managed to cross from Bali on flotsam? The issue is not just *how* one can cross; we need also to explain the near-complete absence on the islands of eutherian species larger than rats. If hominids can cross on flotsam, then so can many other species. The desperate scenarios involving a pregnant woman on a log, natural rafts and mysterious former land bridges that only permitted human crossing, are all designed to save doomed hypotheses that should have no currency in serious scholarship.

Just over a year ago, thirteen gruesome bamboo rafts were washed up on a beach at Woleai, one of the 600 islands of Micronesia. They bore skeletal remains of some of their many sailors, all of whom had perished at sea. The only clue found to their point of departure was a faded identity card of a man from Bitung, Sulawesi, 1600 km away. The case of the skeleton-bearing rafts was investigated by the FBI and it is presumed that they were part of a flotilla sailed by refugees fleeing the religious and ethnic violence to the west. Whatever the case, it appears that the distance these rafts had travelled was not much greater than the distance we sailed on the *Nale Tasih 2*. We can assume that they had plastic containers and other modern paraphernalia, and rotting food was in fact still present on some of the rafts. Armchair archaeologists, who think that sea crossings are a piece of cake, really ought to try doing this on drifting vegetation flotsam.

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