

M. Spanggs, "Comment" on Seafaring in the Pleistocene

Robert G. Bednarik

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. Dyrberg 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

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.

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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