KEYWORDS: ***Pleistocene – Palaeoart – Symbolism – Rock art – Portable art – Art origins***

THE EARLIEST EVIDENCE OF PALAEOART

Robert G. Bednarik

Abstract. A comprehensive review of evidence of very early palaeoart covering all continents reveals significant misconceptions in the dominant models of 'art' origins. The traditional preoccupation with predominantly zoomorphic, figurative traditions of south-western Europe is examined, as well as the closely related concept of an endemic cave art of the Upper Palaeolithic period. The existence of much earlier non-utilitarian traditions is demonstrated, including bead making and pigment use in the Lower Palaeolithic, and the widespread uniformity of Middle Palaeolithic palaeoart traditions is noted. The review of this global Pleistocene evidence suggests that the oldest and symbolically most sophisticated palaeoart is that of Asia rather than Europe.

Introduction

The question of the beginnings of art have long been recognised as being crucial to our understanding of the origins of human language, human consciousness, human culture, as well as the eventual development of modern human cognition. More importantly still, that question is thought to be intimately related to the formulation of past and present human concepts of reality. In this sense, the entire framework of our epistemology is ultimately predicated on the development of non-utilitarian human culture, and its interaction with our faculties of perception (Bednarik 1994a). The processes responsible for these developments remain very poorly understood. This is at least in part due to biased models archaeology has provided. In particular, throughout the twentieth century, the topic of art beginnings was entirely dominated by just one model: that relating to the 'Upper Palaeolithic' rock art and portable art of Europe, particularly south-western Europe (I use terms such as 'Palaeolithic' in the traditional sense, for the sake of communication, without endorsing them; cf. Bednarik 2003a).

Only very recently has this model come under sustained and coherent criticism, particularly with the promotion of earlier art evidence from other continents (Bednarik 1994b, 1994c), and the appearance of explanations of taphonomic nature to account for the composition of the surviving evidence (Bednarik 1994d, 1995a, 1995b). Other recent currents of thought have also become very important and are considered in this paper.

To explore the possible scenarios of cognitive hominid evolution, a variety of evidence has been proposed to have relevance. The perhaps most pertinent corpus of evidence at our disposal in this quest is the body of very early palaeoart, and any other 'non-utilitarian' evidence that may provide clues to early hominid cognition. This 'other' evidence may include manuports suggestive of non-utilitarian functions (e.g. tiny crystals, fossil casts and the like), or technologies that seem to have required certain minimum mental or cognitive capacities (e.g. seafaring). Of particular importance, however, are beads and pendants: not only does their skilled production require sophisticated techniques, and their use the availability of cordage and knotting (both of which are also required for seafaring), beads are a form of symbolic artefact that can only assume cultural relevance in a complex social system of symboling and of value concepts (Bednarik 1997a).

Claims for extremely old rock art (in excess of 30 000 years BP) have been made for almost all continents, the notable exception being North America, besides Antarctica where there is no rock art at all. I will summarise the evidence of 'art' beginnings as it stands for each continent, of what has either been claimed to represent particularly early use of symbolism, or what in my view might be worth considering in such a context. I will in each case consider petroglyphs as well as pictograms, engraved portable art, sculpted portable art, and evidence that has been suggested to be the result of non-utilitarian activities.

THE EVIDENCE

North America

Dorn and Whitley (1984) have obtained a series of cation-ratio minimum 'dates' from Coso Range (California) petroglyphs ranging up to about 11 500 years BP, but numerous writers have rejected the method's reliability (Bednarik 1988a; Bierman and Gillespie 1991; Bierman et al. 1991; Watchman 1989, 1992). More recently detailed scrutiny of Dorn's work has raised new questions (Beck et al. 1998), and Dorn himself has effectively withdrawn all his results (Dorn 1996a, 1996b, 1997).

Similarly, the datings at Salton Sea (Lake Cahuilla), California (Turner and Reynolds 1974), and at Long Lake, Oregon (Ricks and Cannon 1985), have been questioned and could not be sustained. Loendorf's (1986) attempt to date what he thought to be a rock painting at the petroglyph site

Rochester Creek, Utah, has been refuted (Bednarik 1987a). Early petroglyphs at Mud Portage, Lake-of-the-Woods, Canada (Steinbring et al. 1987), have been shown to be between 5000 and 9000 BP. Nevertheless, final Pleistocene petroglyphs may well exist in North America (Bed-narik 1988b; Parkman 1992). Dating information for Ame-rican petroglyphs has recently been provided by Tratebas (1994), for paintings by Russ et al. (1990), Chaffee et al. (1993) and Hyman et al. (1999).

There are several purported Pleistocene portable art objects from North America, but most have been exposed as fakes. The only exceptions (apart from beads from the Jones-Miller site in Colorado) seems to be a mineralised sacrum from Tequixquiac, Mexico, which has been mod-ified to look like an animal head (Bahn 1991: Pl. 18a); and the numerous limestone plaques from the Clovis layer of the Gault site, Texas, which bear 'geometric' engrav-ings (Collins 2002; Collins et al. 1991, 1992; Robertson 1999). So far, at least 134 specimens have come to light at this site, but the provenience of many is not secure (D. C. Wernecke, pers. comm.). Nevertheless, eighteen good examples are clearly from the Clovis deposits, and they represent some of the most important palaeoart the Americas have yielded (Fig. 1). Other examples are less well authenticated, but a bone with an engraving of a rhi-noceros from Jacob's Cave, Missouri, has been suggested to be of the final Pleistocene (Bahn 1991: 92). However, it would need to be explained why an animal that did not exist in the Americas would have been depicted (pers. comm. S. W. Edwards).

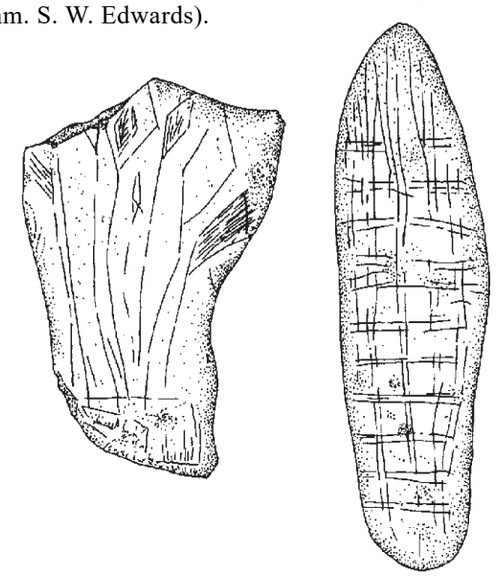

Figure 1. *Engraved limestone plaques of the Clovis, Gault site, U.S.A. (after Collins et al. 1991).*

South America

The principal claims of Pleistocene antiquity for South American rock art refer to the important sandstone shelter Toca do Boqueirao do Sítio da Pedra Furada, Piauí, in north-eastern Brazil, where human occupation traces seem to extend beyond 40 000 years BP (Guidon and Delibrias 1986; Parenti 1993). However, it is unlikely that any of the extant paintings in this site could be older than the final Holocene (Bednarik 1989). Older paintings may have existed, and at least some of the pigment traces reported from the floor deposit seem authentic. At Toca do Baixao do Perna I, another of Guidon's sites, the numerous red paintings are at least 10 000 years old (Bednarik 1989: 105). They occur immediately above a thick layer of charcoal. A fragment of a pigment ball that showed signs of having been worn as an ornament was found at the site, providing an AMS radiocarbon date of 15 250 ± 335 years BP (Chaffee et al. 1993).

'Archaic' petroglyph traditions occur also in South America, including in southern Piauí. The motifs are heavily patinated or weathered and often occur togeth-er with accumulations of extremely archaic-looking stone tools, for instance in Brazil (Bednarik 1989) and Bolivia (Bednarik 1988c, 2000a). Their motif range, and that of early petro-glyph sites in North America, is typically non-figurative and resembles that of archaic petroglyphs of other continents (Bednarik 1987b). Crivelli and Fernández (1996) have reported a series of linear petroglyphs on the bedrock of Cueva Epullán Grande, western Argentina, under sediment approxi-mately 10 000 years old, and petroglyphs on the walls of this cave include cupules. Also in the eastern foot-hills of the Andes, but in Bolivia, lies Inca Huasi, on whose quartzite dyke I have found the apparently oldest petro-glyphs I have seen in South America, again sets of cupules (Fig. 2). Although undated, circumstantial evidence suggests an early Holocene or final Pleisto-cene antiquity (Bednarik 2000a). Cupules and other petroglyphs at further Bolivian sites have been dated to the second half of the Holocene.

Figure 2. Early cupules on quartzite, Inca Huasi, Bolivia.

Asia

There have been several claims relating to Upper Palae-olithic rock paintings in central India, championed especially by Wakankar (1983); similar claims from Siberia (Okladnikov 1977); and claims of portable engravings from the early Upper Palaeolithic of China (e.g. You 1984) and South Korea (Sohn 1981). An examination of many Asian claims of Palaeolithic art has invalidated the overwhelming majority of them (Bednarik 1992a, 1993a, 1993b, 1994c; Bednarik et al. 1991; Bednarik and You 1991; Bednarik and Devlet 1993).

In Siberia, finds of portable art have been reported from about twenty sites (Abramova 1990; Bednarik 1994c). I have argued that the mere depiction of a mammoth does not constitute proof of Pleistocene antiquity of the art in Siberia (Bednarik 1993c; cf. Steelman et al. 2002 for confirmation), although the Mal'ta plaque (Bednarik 1992a)

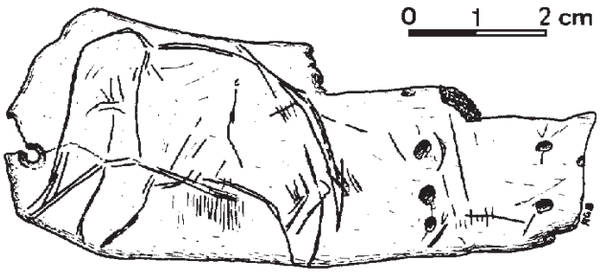

Figure 3. *Perforated ivory plaque with engraving of an apparent mammoth image, Mal'ta, central Siberia.*

might be around 14 000 years old (Fig. 3). However, nearly all known Asian (as well as eastern European) graphic art of the Pleistocene is 'non-figurative' (Bednarik 1993d), a key issue that has so far largely been ignored. Siberian portable art includes the probably oldest presently known iconic sculpture, an animal head from Tolbaga, thought to be possibly 35 000 years old (Fig. 4). Siberian claims of

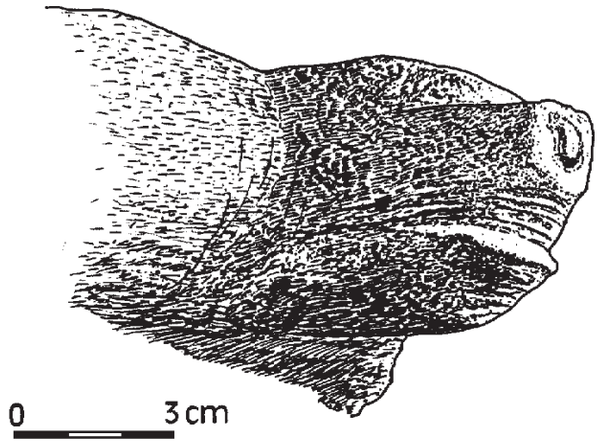

Figure 4. *Sculpture resembling the head of a bear on a vertebra of the woolly rhinoceros, Tolbaga, southern Siberia.*

Pleistocene rock art, however, have been seriously questioned. A few painted motifs among the many thousands of pictograms and petroglyphs on the upper Lena, Siberia, were identified as being Palaeolithic by Okladnikov (1959: 22–41; cf. Okladnikov and Saporoshskaya 1959), a finding that is frequently cited in the literature (e.g. Abramova 1962; Ksica 1973, 1984). Yet there is no evidence for this dating (Bednarik 1992b; Bednarik and Devlet 1993). Much the same can be said about rock art in central Asia, where we have seen various frequent claims for great antiquity rebutted by subsequent analysts. Examples are some thirty sites on the Kalguty River of the Ukok Plateau in south-western Gorniy Altai (Molodin and Cheremisin 1993, 1994) and the petroglyphs of Delger-Muren and Tes (Novgoro-dova 1983), both refuted by Kubarev (1997) who showed that all known central Asian rock art west of China is either of the Bronze Age or younger. Similarly, Jasiewicz and Rozwadowsji (2001) showed that some of the presumed oldest rock art of central Asia, at Zaraut-Kamar Rockshelter in Uzbekistan, is most probably a recent historical site.

In neighbouring China there are many examples of dating rock art to the Ice Age by perceived animal species (Gai 1986: 415-24; Li 1992; Liu 1991; You 1984; Chen 1991: 126; cf. Tang 1993 and Wang 1984) and there is even a claim for Tertiary rock art. At the present time, no rock art in China has been shown to be of the Pleistocene. No portable art from the Chinese Pleistocene was known until 1991, except the material from the Upper Cave of Zhoukou-dian: haematite lumps, perforated teeth, pebbles and shells, and five tubular bone sections with parallel cut marks (Bednarik and You 1991). In 1991, a masterfully engraved piece of antler was reported from a limestone cave north-east of Beijing, Longgu Cave in Hebei Province (Bednarik 1992c). Being about 13 065 years old, the object remains the only known specimen of art from the Chinese Pleistocene (Bednarik and You 1991: Figs 2–4). The same paper also reported the discovery of a stone pendant at Shiyu wenhua, from a dated final Middle Palaeolithic or very early Upper Palaeolithic context (Fig. 5).

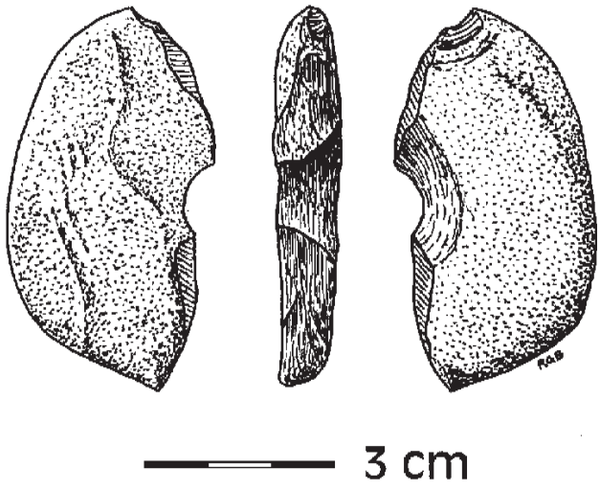

Figure 5. *Stone pendant from the Shiyu site, Shanxi Province, China.*

The only known evidence of Pleistocene art in Japan comes from the cave of Kamikuroiwa, where engraved natural pebbles were found in a layer dated to about 12 000 BP (Fig. 6). Some of the marks have been interpreted as depicting breasts and skirts (Aikens and Higuchi 1982). In addition there are a few apparently non-utilitarian stone objects known from the Japanese Palaeolithic, including a perforated specimen (Bednarik 1994c).

Marked ostrich eggshells have been reported from four central Indian sites (Kumar et al. 1988), which are among over forty recorded sites of ostrich eggshell in India. Radiocarbon dating of the shells places them roughly between 25 000 and 40 000 years BP. The markings on 45 of the 46 known specimens are attributable to mycorrhizal micro-organisms (Bednarik 1992a). Similar markings occur on

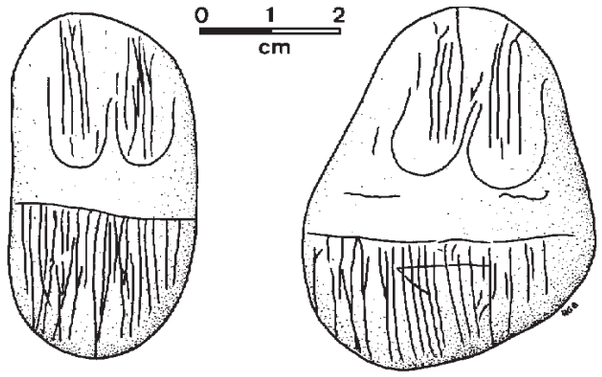

Figure 6. *Two engraved pebbles of the Incipient Jomon of Kamikuroiwa rockshelter, Ehime Prefecture, Japan.*

Siberian ivory and Chinese and European bone finds. The remaining specimen of Indian ostrich eggshell is from Patne and bears a 'non-figurative' pattern that was engraved with a stone tool, as its microscopic study demonstrates (Bedna-rik 1992a). It is thought to be 25 000 years old (Fig. 7).

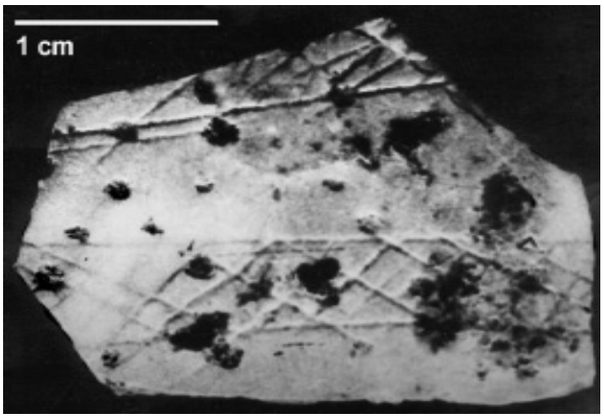

Figure 7. *Engravings on ostrich eggshell fragment, Patne, western India, early Upper Palaeolithic.*

The Upper Palaeolithic of India has also yielded three ostrich eggshell beads, two from Bhimbetka III A-28 and one from Patne (Bednarik 1997a). The carved and polished bone object found in the Belan valley, Uttar Pradesh, has been described as a 'mother goddess' (e.g. Misra 1977: 49). It is, however, not a female figurine, but a damaged bone harpoon of the early Upper Palaeolithic (Bednarik 1993b) (Fig. 8).

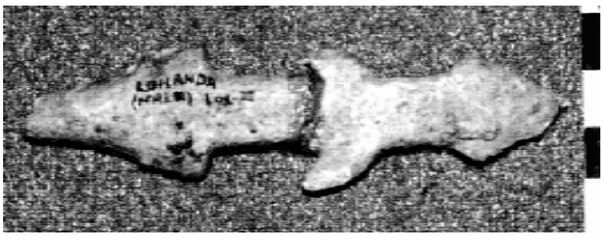

Figure 8. *Carved and polished bone object from Lohanda Nala, Belan valley, India, formerly regarded as female figurine, but in fact a harpoon point.*

Turning next to the claims for a Palaeolithic antiquity of rock art in India, we find that Wakankar's (1975, 1983) notion of the precedence of the green dynamic paintings, which he considered to be of the Upper Palaeolithic, has been negated by Tyagi (1988). Most contemporary researchers have great doubts that any Indian rock paintings are of Pleistocene age (e.g. Misra 1977; Neumayer 1983, 1993; Bednarik 1993b; Chakravarty and Bednarik 1997). Until 1990, petroglyphs were only known from the north and south of the country. The Raisen petroglyphs (Bednarik et al. 1991) are of unknown age, but are totally repatinated and coated with a silica skin and resemble the archaic petroglyphs of other continents. Some of the Bhimbetka quartzite cave petroglyphs were covered by in situ Lower Palaeolithic occupation strata (Bednarik 1992b, 1994b, 1994c) and they are of the Acheulian (Fig. 9), being therefore the oldest currently known rock art

Figure 9. *Cupule and meandering groove on boulder in Acheulian layer, Auditorium Cave, Bhimbetka, India.*

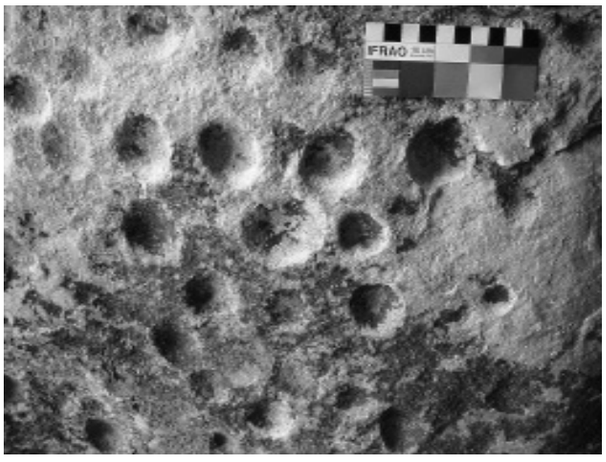

Figure 10. *Some of the more than 500 Palaeolithic cupules in Daraki-Chattan, India, thought to be of the Acheulian or Middle Palaeolithic.*

in the world (Bednarik 1993b). A large number of cupules in Daraki-Chattan (Fig. 10), a quartzite cave near the Chambal valley, has been suggested to be of either Acheulian or Middle Palaeolithic age (Kumar 1996), a claim that is being evaluated by an international commission at the present time (Kumar et al. 2003). Striations on a wear facet of one of a series of haematite pebbles from the Lower Acheulian of Hunsgi, Karnataka, were apparently the result of use of the pebble as a crayon, to mark a hard rock surface (Bednarik 1990a). Another find of relevance is the suite of six quartz crystal prisms (Fig. 11) from the Lower Acheulian of Singi Talav, Rajasthan, which are much too small to have served as stone tool material (d'Errico et al. 1989).

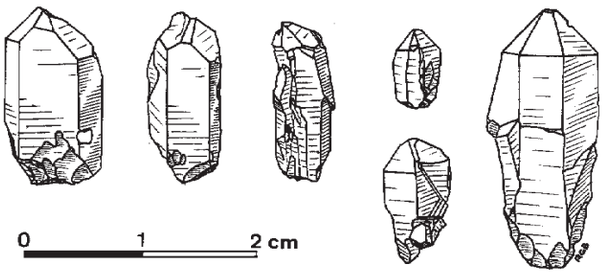

Figure 11. *Tiny quartz crystal prisms from the Lower Acheulian, Singi Talav, near Didwana, Rajasthan.*

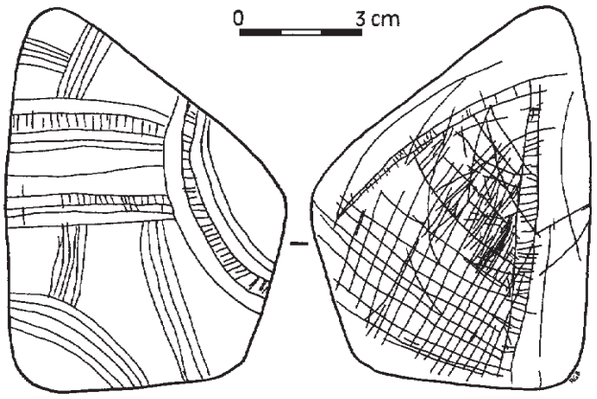

Figure 12. *Epi-Palaeolithic engravings on both faces of a limestone cobble from Urkan e-Rub, Israel.*

The Levantine region has yielded a variety of portable art of the Pleistocene. An engraved limestone cobble from the late Palaeolithic site of Urkan e-Rub II (Fig. 12), Israel, is between 14 500 and 19 000 years old (Hovers 1990). It features complex non-iconic arrangements. An older limestone pebble from Hayonim Cave also bears engravings on both faces, but it is of the Aurignacian and 29 000 to 27 000 years old (Belfer-Cohen and Bar-Yosef 1981; Bar-Yosef and Belfer-Cohen 1988). Its markings include a motif that has been interpreted as depicting a horse. From the same site and horizon, Layer D, come also five gazelle scapulae, each engraved with a series of notches (Davis 1974). Of similar age is a gazelle metatarsal from Ksar Akil, bearing five sets of linear incisions (Tixier 1974; Mellars and Tixier 1989). Three engraved fragments of bone points have been excavated at Ohalo II, on the shores of the Sea of Galilee, and appear to be about 19 000 years old (Rabinovich and Nadel 1994). One of them was found with a human burial. Finally, there are two decorated Kebaran bone artefacts, one an awl from Jiita II in Lebanon (Copeland and Hours 1977), the other an incised radial fragment from Kharaneh IV in Jordan (Muheisen 1988).

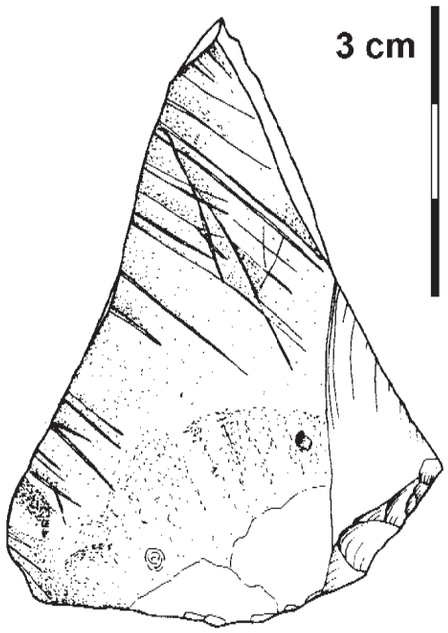

Figure 13. *Engraved stone tool of the Middle Palaeolithic, Qafzeh Cave, Lower Galilee, Israel (after Hovers et al. 1997).*

Much earlier art-like finds from the region are the Middle Palaeolithic engraved stone tool (Fig. 13) from Qafzeh Cave, c. 100 000 years old (Hovers et al. 1997), and the engraved cortex piece from Quneitra, which is only about half that age (Goren-Inbar 1990; Marshack 1996). Much earlier still is the basaltic tuff pebble containing scoria clasts excavated in an Acheulian occupation layer at Berekhat Ram, Golan Heights (Goren-Inbar 1986; Goren-Inbar and Peltz 1995) that is dated to between 233 000 BP and 470 000 BP (Feraud et al. 1983). The pebble has the natural shape of a female human torso, head and arms (Fig. 14), and it bears artificial markings (Marshack 1997; d'Errico and Nowell 2000). Another Acheulian site of the region, Gesher Benot Ya'aqov,

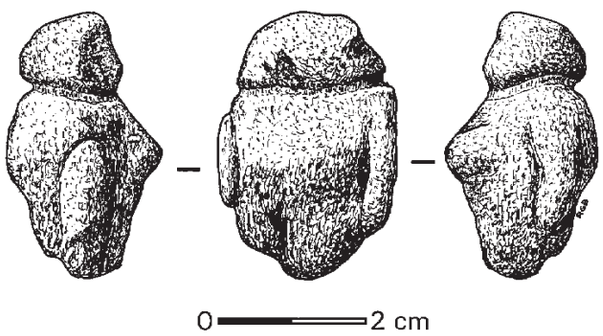

Figure 14. *Naturally shaped scoria pebble bearing engraved lines. Acheulian, Berekhat Ram, Israel.*

yielded two perforated crinoid fossils and a number of very small quartz crystals (Goren-Inbar et al. 1991), which in view of similar Acheu-lian finds elsewhere are of interest. In particular, disc beads of that period have been reported from Africa and Europe as well.

Non-figurative rock engravings in caves at Mount Carmel have been suggested to include Palaeolithic marks, but in view of the many false claims of this type elsewhere this requires specialist appraisal (Ronen and Barton 1981). At the upper end of the time scale, towards the end of the Pleistocene, the Levant has provided a good number of art-like objects, particularly stone objects, although at least one engraved ostrich eggshell fragment has also been reported (Goring-Morris 1998). A series of proto-sculptures has been described from the Natufian layers of el-Wad Cave, Mt Carmel (Garrod and Bates 1937; Weinstein-Evron et al. 1993), Kebara Cave (Turville-Petre 1932: 276), Wadi Hammeh 27 (Edwards 1991: Fig. 9.2), Upper Besor 6 (Goring-Morris 1998) and a few other sites. This material is generally of the last two or three millennia of the Pleistocene. The earliest rock art so far identified in Saudi Arabia might possibly be of a similar age, consisting of cupules and archaic petroglyph motifs at the Shuwaymas 1 site, south-west of Hail (Bednarik 2002a).

Australia

The persistent claims of the precedence of western European art are particularly hard to understand when one considers the long-standing expectation that some Australian rock art would be shown to be extremely old (e.g. Basedow 1914). While it is almost self-evident that a great deal of Australian rock art, perhaps a higher percentage than in any other continent, is of the Pleistocene, there have been several false claims made and credible dating evidence remains scarce. Leaving aside claims based on perceived styles and the supposed depiction of extinct animal species, which are in any case based on subjective and untestable evidence, there have been three specific Pleistocene age proposals that turned out to be false: at Olary, Devil's Lair and Jinmium.

Of the four earliest minimum dates reported from South Australian petroglyphs in the Olary region, which range from about 36 000–45 000 BP, three were radiocarbon dates, secured from organic inclusions under rock varnish covering the petroglyphs (Dorn et al. 1992). The fourth, a 'cation-ratio' determination, was based on an always controversial and now discredited method, but recently even the radiocarbon dates have all been withdrawn by the researcher who presented them (Dorn 1996a, 1996b, 1997; cf. Beck et al. 1998).

A series of six limestone pieces from Devil's Lair in south-western Australia, described and widely accepted as engraved plaques (Dortch 1976, 1984), apparently of the Pleistocene, have been found to consist of naturally marked clasts (Bednarik 1998). However, a naturally perforated marl pebble from the same site has been used as a pendant (Bednarik 1997b), as has a small bird bone fragment (Bednarik 1998). Another small cave in coastal Western Australia, Mandu Mandu Creek Shelter, has yielded a series of perforated marine shells about 32 000 years old (Morse 1993).

A third false claim of Pleistocene art from Australia was made concerning the cupule panel at the Jinmium rock-shelter, Northern Territory, said to be between 58 000 and 75 000 years old on the basis of thermoluminescence dating (Fullagar et al. 1996). This was rejected by several Australian rock art specialists even before publication (Rothwell 1996), and subsequently refuted by more detailed dating (optically stimulated luminescence and radiocarbon) of the site's sediments, which indicated that the rock art was of the Holocene (Gibbons 1997; Roberts et al. 1998). On present indications, Australia was only settled around 60 000 BP (Roberts et al. 1993). As in most other continents, some cupules are regarded as being extremely old in Australia (Bednarik 1993f), but the Jinmium panel occurs on a type of sandstone that experiences rapid exfoliation. More credible is the minimum dating estimate for one of the petroglyph traditions in Malangine Cave, South Australia (Fig. 15), which was derived from uranium-series analysis, suggesting an age of well over 28 000 years (Bednarik 1999).

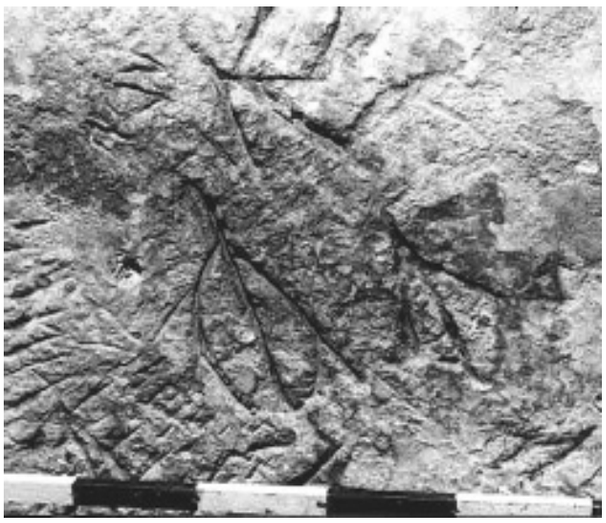

Figure 15. *Karake-style petroglyphs carved into the ceiling of Malangine Cave, near Mt Gambier, South Australia. They were covered by a speleothem layer of 15 to 20 mm thickness yielding a U/Th age estimate of about 28 000 years BP.*

Other credible age estimations were recently presented for Pilbara petroglyphs, ranging up to the same magnitude, and it is clear that older petroglyphs exist in the region (Bednarik 2001a, 2002b) (Fig. 16).

Despite the wealth of portable palaeoart in Australia, very little has so far been dated to the Pleistocene. Striated haematite occurs in abundance from the continent's earliest known occupation levels onwards (Jones 1985; Roberts et al. 1990; Thorne et al. 1999). Of interest are the so-called 'cylcons', often decorated cylindrical-conical stone objects found in the Darling River basin, because they might possibly date from the Pleistocene.

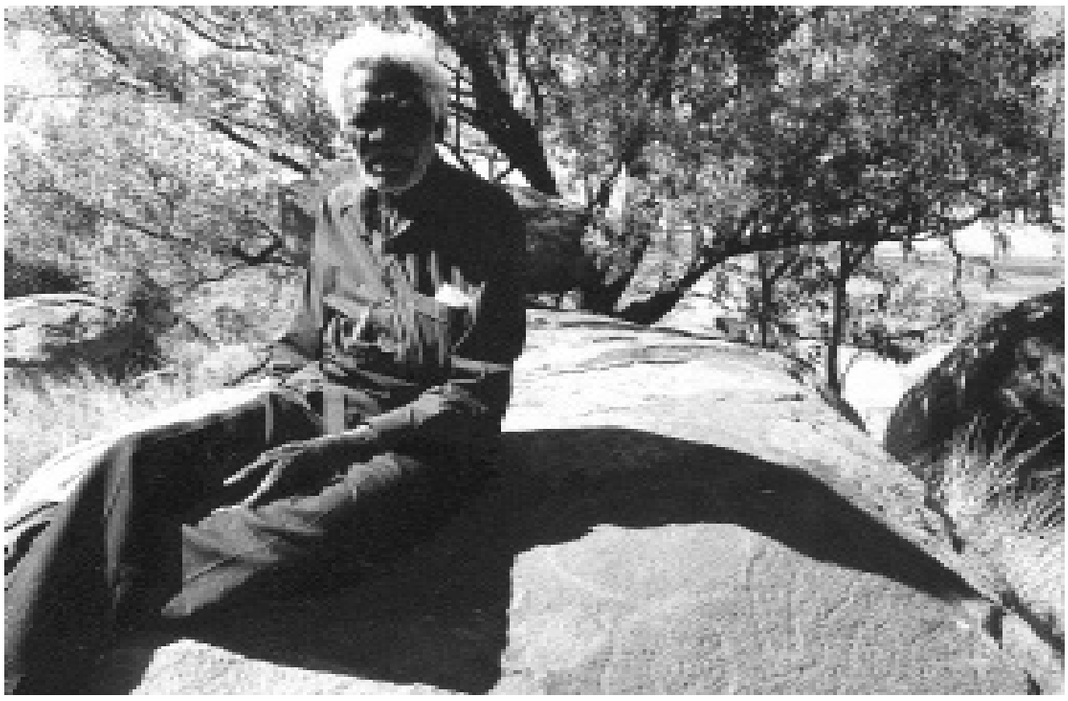

Figure 16. *Senior Traditional Custodian Monty Hale seated next to some of the oldest scientifically analysed petroglyphs of Australia, forming a circulinear pattern on granite in the eastern Pilbara.*

Africa

From the African Pleistocene, figurative portable art has been reported only from the Middle Stone Age (MSA) of Apollo 11 Cave, Namibia (Wendt 1974), thought to be 26 000–28 000 years old (Fig. 17).

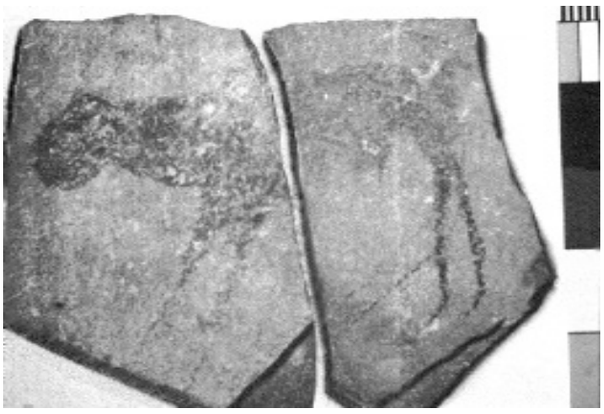

Figure 17. *Zoomorphic pictogram on stone slab from the MSA of Apollo 11 Cave, Namibia.*

Older bone objects with serrations or notches are known from the MSA of several sites: Klasies River Mouth, South Africa (Singer and Wymer 1982), Border Cave, South Africa (Beaumont et al. 1978; Grün and Beaumont 2001) and again Apollo 11 Cave (Wendt 1974). A wooden fragment with longitudinally engraved lines comes from a Middle Pleistocene deposit at Florisbad, Orange Free State (Volman 1984). Engraved ostrich eggshell fragments from the Howieson's Poort phase of Apollo 11 Cave are perhaps in excess of 83 000 years old (Miller et al. 1999), and such finds have also been reported from the MSA of Diepkloof Shelter in the south-western Cape (Beaumont 1992; Bednarik 1994b) where they might be about the same age (Feathers 2002). The fragment of a circular ostrich eggshell pendant from the Cave of Hearths at Makapansgat is also of similar antiquity (Mason 1988). Several other African sites have yielded apparent body ornaments of comparable ages, including the four deliberately perforated quartzite flakes from Debenath, Nigeria; the shell bead from Oued Djebanna, Algeria; and the bone pendant from Grotte Zouhra, Morocco (McBrearty and Brooks 2000: 521). While this African material provides some belated evidence refuting White's (1995) pronouncements about the origins of such behaviour, hundreds more apparent beads and pendants of the Lower Palaeolithic have been available from Europe for over 150 years (Bednarik 1997a, 2001b).

Evidence of ochre use in Bambata and Pomongwe Caves in Zimbabwe (Jones 1940; Cooke 1963; Klein 1978) is thought to be up to 125 000 years old. Stone fragments bearing ochre markings come from the MSA sites Pomongwe Cave (Fig. 18) and Nswatugi (Walker 1987). The extensive mining evidence in Lion Cavern, South Africa (Beaumont and Boshier 1972; Beaumont 1973), includes a radiocarbon date of about 43 200 BP. Apparent use of iron pigments has been widely recorded in the MSA (Beaumont et al. 1978; Clark 1988; Inskeep 1962; Klein 1978; Knight et al. 1995; Singer and Wymer 1982; Walker 1987). It includes notched (Hollow Rock Shelter, south-western Cape), carefully drilled (Klasies River Mouth Shelter 1A) and heavily striated specimens (Klasies River Mouth Cave 1) (Singer and Wymer 1982; Knight et al. 1995: Figs 3–6). A ground haematite fragment from the MSA of the Howieson's Poort site bears

a series of eighteen notches (Stapleton and Hewitt 1928), two other haematite pieces with notches on their edges were found in the MSA of Hollow Rock Shelter (Evans 1994).

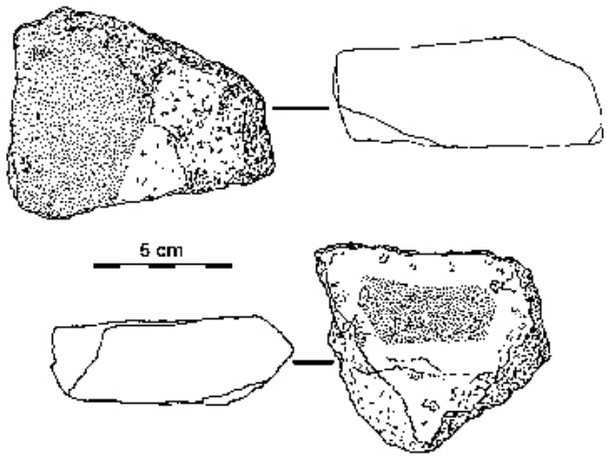

Figure 18. *Two stones with paint residues from Pomongwe Cave, Matopos, Zimbabwe, final Pleistocene (after Walker 1987).*

Two lumps of red volcanic tuff (Oakley 1981: 207), previously identified as ochre (Leakey 1958), were recovered in the much earlier Developed Oldowan levels of Olduvai BK II, Tanzania. Their significance remains uncertain, however. Some of the most extensive early evidence of haematite use comes from Wonderwork Cave, in the northern Cape region of South Africa. Every level of the excavation has produced an abundance of ochre fragments, occurring together with Acheulian bifaces and exotic quartz crystals (Beaumont 1990, 1999; Binneman and Beaumont 1992; Bednarik 1994b). The substantial occupation sequence has been suggested to extend to 800 000 or 900 000 years BP. Of particular importance are two ironstone slabs bearing engraved sub-parallel lines which appear to be between 260 000 and 420 000 years old (Imbrie et al. 1984; Beaumont in press) and are thus among the earliest engravings known. Well-dated evidence of very early pigment use comes from two recent studies. First, more than seventy red ochre pieces, weighing together some five kilograms, were excavated at the site GnJh-15 in the Kapthurin Formation, Kenya. They are more than 285 000 years old (McBrearty 2001: 92). Twin Rivers, Zambia (Barham 2002), has yielded at least 306 pigment pieces of specularite, haematite, limonite, ochrous sandstone and manganese dioxide. Three per cent of these show signs of modification by grinding or rubbing, vindicating the interpretation of the isolated previous Indian evidence from Hunsgi. The age of Barham's specimens is safely bracketed between 270 000 and 170 000 years. The African evidence of early pigment use is therefore currently more numerous and better dated than the sporadic occurrences known from the same time interval in Eurasia.

Two engraved fragments of ochre bearing geometric markings have recently been excavated from the MSA of Blombos Cave, South Africa (d'Errico et al. 2001). They were found in 1999 and 2000 respectively and are apparently at least 73 000 years old (Henshilwood and Sealy 1997). The engraved geometric markings comprise linear patterns and borders (Fig.19). Crisscrossing lines forming a diamond lattice bordered by 'enclosing' lines are reminiscent of the patterns engraved on numerous Upper Palaeo-lithic portable finds from Asia, which may define a distinctive marking strategy of great longevity and distribution.

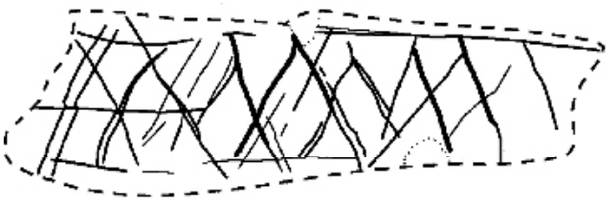

Figure 19. *The engraved pattern on one of the Blombos Cave ochre fragments, MSA, lower Late Pleistocene, South Africa.*

The earliest palaeoart evidence from Africa includes the proto-figurine from Tan-Tan, southern Morocco, a modified manuport from a Middle Acheulian layer (Bedna-rik 2001c). Its recent discovery confirms the authenticity of the similar Berekhat Ram specimen, also a proto-sculpture of this period. Importantly, the Tan-Tan figurine bears microscopic traces of a bright-red pigment, which is currently the earliest evidence of applied colouring material (Fig. 20).

Figure 20. *Natural stone object with anthropic groove markings and traces of red paint residue, Middle Acheulian, from Tan-Tan, Morocco.*

The Tan-Tan object also raises the question of the relevance of a probably natural anthropomorphous dolomite piece from Mumbwa Caves, Zambia, found in the remains of an apparent windbreak structure (Barham 2000: 137, 140). Another find of interest from the Moroccan Sahara, the manuport from Erfoud Site A-84-2, a Late Acheulian site,

was also found in such a possible dwelling site (Kuckenburg 2001). It is the fossilised fragment of a cuttlefish cast that has the distinct shape and size of a human penis (Bednarik 2002c). Of significance are also the Acheu-lian ostrich eggshell beads from El Greifa site E, Libya, which at about 200 000 years are among the oldest known beads (Ziegert 1995; Bednarik 1997a).

Oddly, no African rock art has so far been securely shown to belong to the Pleistocene, although a few such claims have been made concerning northern Africa. Those concerning Saharan rock art have been refuted by Muzzolini (1990), while a claim from Upper Egypt (Huyge 1998) remains to be tested (cf. Huyge 2002; Watchman 2002; Whitley and Simon 2002). Similar postulations for Tanzanian rock paintings (Anati 1986) are without basis. However, the issue of the earliest cupules in Africa may soon be clarified. Peter Beaumont has very recently reported finding extremely early cupule sites in the Korannaberg region of the southern Kalahari (Beaumont in press). Like those in India they occur on heavily metamorphosed and thus particularly weathering-resistant quartzite. They appear to be either of the MSA or earlier, which brings to mind two other finds. One is the grid pattern on a Fauresmith grindstone Laidler (1933) excavated at the Blind River mouth in East London, South Africa, which is thought to be in the order of 400 000 years old (Bednarik 2002d). The other is the grooved and pecked phonolite cobble from Olduvai FLK North 1 in Bed 1, Tanzania (Leakey 1971: 269), which bears what appears to be a cupule on each side (Fig. 21).

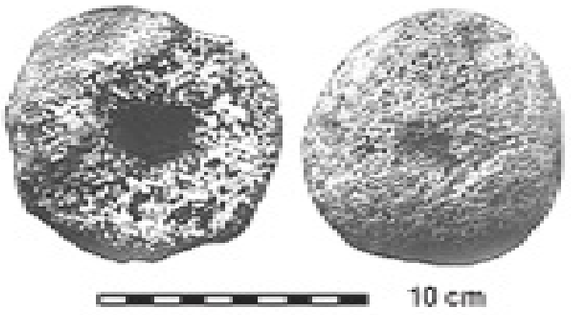

Figure 21. *Cobble with apparent cupules on each side, Olduvai (after Leakey 1971).*

Its Plio-Pleistocene age might render a utilitarian explanation for this artefact more plausible (Bednarik 2002d), but it should not be overlooked that the earliest known 'palaeoart' object is the water-worn jasperite cobble found in the level 3 bone breccia at Makapansgat (South Africa), which is older still. It was brought into the cave from some distance away, either by australopithecines (Dart 1974) or perhaps by very early hominids. It bears several natural markings that give it the appearance of a head (Fig. 22). As we lack any other suggestions that Australopithecus recognised the iconic qualities of such objects, the significance of this find remains tentative. However, and particularly in view of the recent discovery of *Kenyanthropus platyops,* that does not warrant its exclusion from discussions of possible traces of early cognition. A recent microscopic analysis resulted in the reconstruction of much of the object's long history, and confirmed that the extraordinary red stone was carried into the cave 2.5–3 million years ago (Bednarik 1998b).

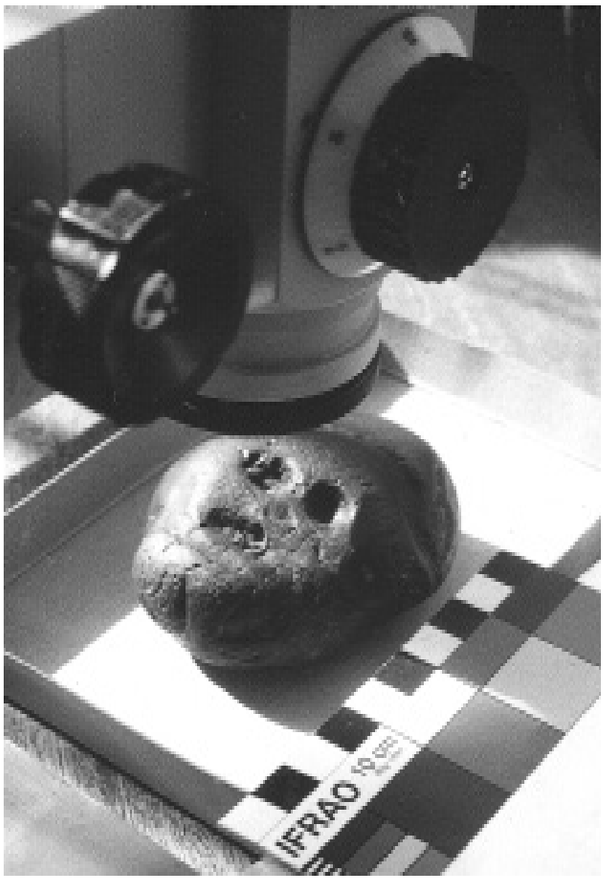

Figure 22. *Red jasperite cobble with distinctive natural markings, a manuport taken into Makapangat Cave in the late Pliocene and deposited with australopithecine remains.*

The evidence Africa has so far yielded provides some tantalising glimpses, and it is clear that this continent can be expected to provide much more very early evidence relating to the origins of non-utilitarian practices by hominids as the search continues.

Europe

Despite the qualifications that apply to all claimed datings of the Upper Palaeolithic rock art of Europe (Bednarik 1996a), it is clear that this magnificent art corpus is between roughly 32 000 and 10 500 years old. This parietal art, together with the portable art of the same time span, is arguably the most thoroughly studied palaeoart. The Palaeolithic rock art of Europe has been claimed to occur at about 300 sites across Europe (Bouvier 1993 lists 291, plus several recently discovered sites). However, the attribution of most of these sites to the Upper Palaeolithic is only on the basis of style, an inadequate form of dating. Since the stylistic basis of dating this art has been refuted by the reliable dating particularly of Chauvet Cave, it is essential that each presumed Pleistocene rock art site of

Europe be reviewed in that light (Bednarik 1995d). Their Palaeolithic attribution needs to be tested, since even that of famous sites such as Lascaux is being reconsidered (Bahn 1994). Of the sites Bahn and Vertut (1997) list, several certainly are either not of the Pleistocene or they lack any form of rock art (e.g. Bednarik 2002e; Steelman et al. 2002). The oldest safely dated evidence of this rock art tradition is at Chauvet Cave, France (Clottes et al. 1995), being about 32 000 years BP.

Of the numerous claims falsely attributing European rock art and portable art to the Pleistocene, or describing natural markings as such palaeoart, few have so far been examined scientifically. For instance all claims made concerning Palaeolithic rock art in Germany (e.g. Hahn 1991; Conard and Uerpmann 2000) have had to be rejected (Bednarik 2002e). Some specimens feature natural surface deposits or discolouration caused by chemical reduction of iron salts (Geißenklösterle), some thought to be exfoliated fragments of rock art were made on already spalled clasts, and numerous presumed engravings were identified as taphonomic grooves occasioned by quartz grains embedded in the fur of cave bears (Hohle Fels). The stag image from the Kleines Schulerloch in Bavaria (Birkner 1938: Pl. 13) and the zoomorph in the Kastlhänghöhle (Bohmers 1939: 40) have long been rejected in this context (Bosinski 1982: 6; Freund 1957: 55), while claims of Pleistocene rock art in Jenö Hillebrand, Hungary (Kozlowski 1992: 41), Mladec Cave and Býci Skála (Oliva 1996: 120, 129, Fig. 2) have yet to be examined. In Austria, petroglyphs at two sites were attributed to the Pleistocene with only stylistic justification, some are natural markings and the others are only a few centuries old. A series of open air sites on the Iberian Peninsula has been proposed to be of Palaeolithic age, based again on perceived style only (Bahn 1995), but studies in two valleys (Côa in Portugal, Agueda nearby in western Spain) have severely questioned these postulates (Bednarik 1995c, 2000b). Substantial efforts to demonstrate the claimed Pleistocene age of the Côa petroglyphs have failed to provide tangible evidence (Aubry et al. 2002), and direct dating, geology, palaeozoology and even archaeology all imply that most are of Historical age.

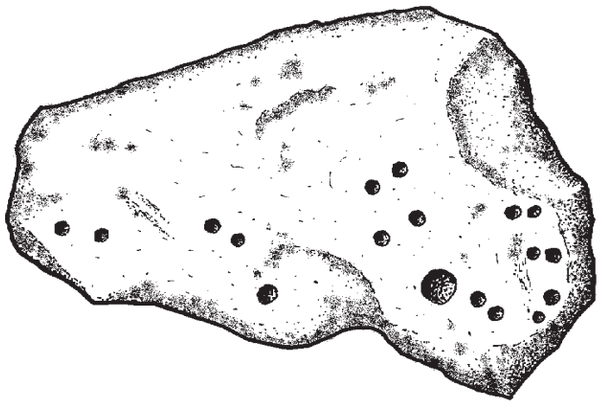

Figure 23. *Limestone slab bearing eighteen cupules, sixteen of which are arranged in pairs. They were fashioned before the slab was placed over a Neanderthal infant burial. La Ferrassie, France.*

The earliest known rock art of Europe, however, is not of the Upper Palaeolithic, it consists of a set of eighteen cupules found on the underside of a limestone slab placed over the burial of a Neanderthal child at Le Ferrassie (Peyrony 1934) (Fig. 23). Peyrony also thought to recognise a motif consisting of patches and irregular bands on a limestone block with brown, bluish and black paint traces, excavated from the Mousterian of Le Moustier. Further apparently non-utilitarian evidence occurs in the form of portable objects, even from the Lower Palaeolithic period. Mousterian examples are engravings and apparently artificial notches on bone remains from such Mousterian sites as La Quina (Martin 1907-10), Petit-Puymoyen, abri Lartet, abri Suard (Débenath and Duport 1971), Peyrere 1 or Noisetier Cave (d'Errico and Allard 1997) and La Ferrassie

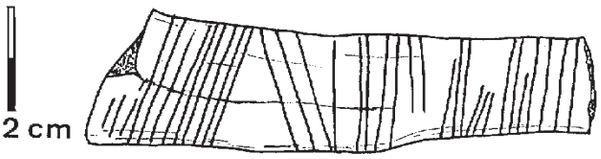

Figure 24. *Bone fragment with series of engravel lines, Mousterian, La Ferrassie, France.*

(Capitan and Peyrony 1921) in France; Cueva Morín (Freeman and González Echegaray 1983) and Lezetxiki (Baldeon 1993: 25-6) in Spain; Bacho Kiro, Bulgaria, (Marshack

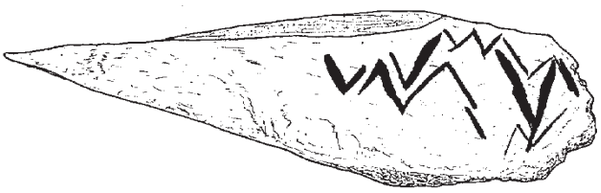

Figure 25. *Bone fragment with engraved zigzag patterns, Middle Palaeolithic, Bacho Kiro, Bulgaria.*

1976); Tagliente rockshelter, Italy (Leonardi 1988); as well as from French Charentian sites (Bouvier 1987). A serrated bone fragment made with stone tools has been reported from the Mousterian of Schulen, Belgium (Huyge 1990),

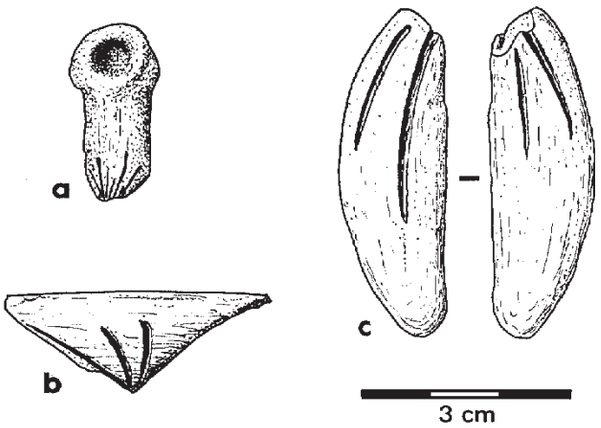

Figure 26. *Engravings on (a) modified phalanx, (b) bone splinter, and (c) horse canine. Micoquian, Prolom 2, Crimea, Ukraine.*

and the Crimean cave Prolom 2 yielded several engraved Micoquian specimens (Stepanchuk 1993) (Fig. 26). Non-figurative Mousterian markings have also been reported on stone, at several sites in Italy (Leonardi 1988) and Hungary (Vértes 1964, 1965). Of particular interest is the well analysed schist plaque with about 43 incised sub-parallel lines, c. 50 000 years old, from Temnata Cave, Bulgaria (Crémades et al. 1995), one of the best examples of Mousterian palaeoart (Fig. 27). The cuts on a bone artefact from the last Interglacial at a German site, Taubach, may also be anthropic (Kuckenburg 1997).

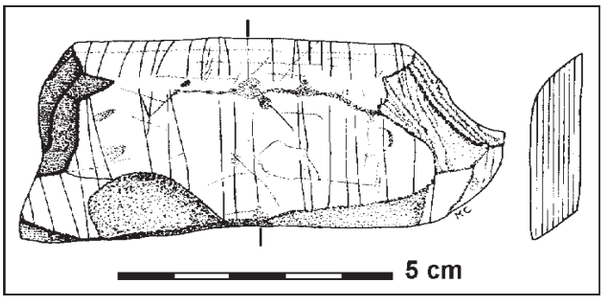

Figure 27. *Engraved schist plaque from Temnata Cave, Bulgaria, Mousterian (after Cremades et al. 1995).*

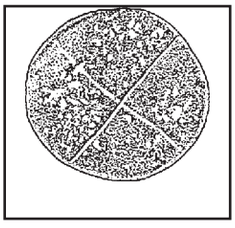

Figure 28. *Engraved nummulite, Tata, Hungary, Mousterian.*

A unique object of the Mousterian is the silicified nummulite from Tata, Hungary. This circular disc is divided into two halves by a natural fracture that is visible on both sides of the semi-translucent fossil. It is crossed at right angles by engraved lines on both sides, thus forming crosses within the circular outline of the object (Fig. 28). The conceptualisation underpinning the simple act of creating a complex pattern by such action is itself rather complex (Bednarik 1995b: 613).

Much older than the Middle Palaeolithic engravings are those from Bilzingsleben, Germany, which occur on fragments of bone, ivory and stone and are roughly 350 000 years old (Behm-Blancke 1983; Mania and Mania 1988; Bednarik 1993e). Of importance is the lasermicroscopic study of the principal Bilzingsleben palaeoart objects by Steguweit (1999) which shows unambiguously that their engravings are intentional markings. A similarly marked forest elephant bone is from Stránská skála, Czech Republic (Valoch 1987), but its anthropic nature remains uncertain. Highly relevant are the three engraved bone fragments from gravel pit Oldisleben 1, Thuringia, Germany, found with a Micoquian industry and Eem fauna (Bednarik in prep.). Among them are an apparently iconographic image and a scapula fragment with a distinctly intentional set of twenty engraved parallel lines, arranged in two sets in precisely the same manner as those on Bilzingsleben No. 1 specimen (Fig. 29). Also clearly anthropic and intentional are the more than twenty oblique notches arranged in two distinct rows on a probably Lower Palaeolithic mammoth tusk fragment

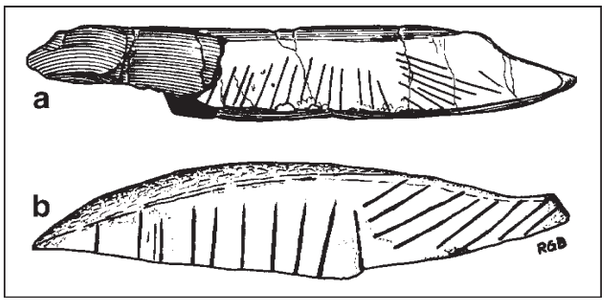

Figure 29. *Engraved bone fragments from Bilzingsleben (a) and Oldisleben 1 (b), Germany, found only 10.5 kilometres apart.*

(Moog 1939). This object of Middle Pleistocene age found at Wyhlen, Germany, may even be notational in character, but all my efforts to locate the specimen have so far remained fruitless (Fig. 30).

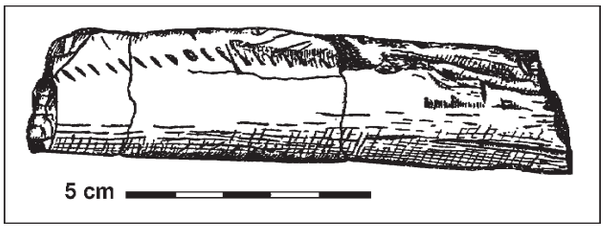

Figure 30. *Engraved ivory fragment from Wyhlen, south-western Germany, Middle Pleistocene (after Moog 1939).*

An engraved bone fragment from the Acheulian of Sainte Anne I, France, bears ten similar short cuts along an edge (Raynal and Séguy 1986). A Middle Acheulian handaxe from l'Observatoire, Monaco, bears linear, deeply cut markings on its cortex that appear to be artificial (de Lumley 1976: Fig. 12.5). Close examination, however, shows these grooves to be the result of entirely natural processes. Relevant are also a striated haematite pebble of the Acheulian from Beçov, Czech Republic (Marshack 1981); several faceted pieces of limonite among the seventy-five found at Terra Amata, France (de Lumley 1966; cf. Wresch-ner 1985); while an apparently shaped slab of ochre reported from Ambrona, Spain (Howell 1966) appears to be of red sandstone (L. Barham, pers. comm.).

Perforated small objects which may have been used as beads or pendants have been reported from European Palaeolithic sites for more than 150 years—in fact they include the oldest such specimens in the world, from the Acheulian of France (at Saint-Acheul)—to the Mousterian (at Fontmaure) and right through to the Upper Palaeolithic. Tens of thousands of such objects have been published, and while a proportion of them has been naturally perforated, most, including some Lower and Middle Palaeolithic specimens, are clearly artefacts (Bednarik 1997a). D'Errico and Villa (1997) have shown that a few of these many bead-like finds bear natural perforations, which is of little relevance as an object does not necessarily have to have an artificial perforation to have been used as a

bead. Wear traces of the type I have described (Bednarik 1998a) are more important in the identification of beads, and certain types of beads cannot be mimicked by nature, they are always anthropic products (e.g. ostrich eggshell beads and perforated teeth).

Traditional interpretation of the evidence

The traditional model of art origins is almost entirely based on European evidence of the Upper Palaeolithic, and perceives art as a phenomenon initially arising in Europe, and most particularly in south-western Europe. There are some simple reasons for the development and persistence of this view. Perhaps most importantly, the relatively large number of sites of the perceived Pleistocene traditions is interpreted as indicating cohesive cultural entities, while the iconic quality of some of the motifs of this art corpus, particularly of zoomorphs, is seen as a sign of artistic sophistication.

There are, however, also more subtle underpinnings of the paradigm that has practically dominated all discussions of art origins. The Pleistocene art of south-western Europe was discovered at a time when colonialist ideology still determined scholarly thought patterns. Subsequent to its controversial acceptance by the archaeological establishment a century ago, it was still possible for a fake hominid fossil to be eagerly accepted as evidence that humans first evolved in Britain. While rational 'amateurs' like Eugène Dubois had long realised that the human cradle was not to be found in Europe, colonialist metaphysics permitted the suppression of Dart's Australopithecus for decades, until the counter evidence became simply overwhelming in the middle of the 20th century and the focus shifted to Africa.

In palaeoart studies, however, the Eurocentric paradigm remains in control, and with good reason. A major 'industry' has developed around it, connected with tourism, education, publishing, heritage management, national pride, even ethnic identity, as if the Palaeolithic artists could validly be seen as the ancestors of modern nations or ethnic groups. Archaeology, always a highly political pursuit (Kohl and Fawcett 1995), has created a mythology permeating all levels of education and intellectual conditioning. Its perpetuation is secured by the fact that the reputation and influence of the academic experts of Palaeolithic art depends on maintaining the dogma. The specialists of Palaeolithic art derive their positions within the hierarchy primarily from a mysterious deeper understanding of the art, whose finest manifestation is the ability of estimating the age and 'cultural attribution' of a specimen from its 'style'. This ability derives from such sources as the tenets laid down by earlier scholars, a close knowledge of the art corpus and related literature, and some intuitive factors that have never been quantified and rarely subjected to a form of testing (Bednarik 1995d). On the few occasions when the latter have been challenged the reactions have been unsatisfactory. For instance the introduction of 'direct' rock art dating techniques has led to personal attacks of archaeometrists and to the description of scientific methods such as 'blind testing' as 'unethical' (Zilhão 1995).

In recent decades this paradigm found a new lease of life in the 'African Eve' notion of a culturally, technologically and cognitively superior new 'species', which replaced all other humans and then developed art, speech and complex culture in south-western Europe. In this origins myth, cultural sophistication is implied to have largely been disseminated from Europe to the rest of the world. To survive, it needs to reject evidence for human sophistication prior to the Aurignacian, and it needs to correlate 'modern' behaviour with 'modern' physical features of humans. Scholars relying on maintaining this paradigm are finding it increasingly difficult to reject contrary data, especially as its only support, genetic claims about divergence times based on unknown mutation rates and population sizes, is dubious (Barinaga 1992; Templeton 1993, 1996; Ayala 1996; Brookfield 1997; Pennisi 1999; Strauss 1999).

This orthodox model ignores most of the Lower and Middle Palaeolithic evidence listed above; its consideration is discouraged and its dissemination stifled in order to preserve the archaeological dogma. (The present paper has been submitted to two journals previously and was deemed unacceptable without major alterations.) According to it, 'modern' behaviour—which includes the skilled working of non-lithic materials (bone, ivory etc.), blade tool technology, navigation, 'art' and body decoration, speech, shelter construction, advanced hunting techniques, clothing, cordage and underground mining—is the exclusive preserve of recent humans, typified by the undated Cro-Magnon specimens. Yet all of these behaviour traces can be found in Middle Palaeolithic contexts and most in Lower Palaeolithic ones, at least outside of Europe. Indeed, the African Eve model itself lacks any evidential support from archaeology (Bednarik 1995b; Bednarik and Kuckenburg 1999). There is unambiguous evidence in archaeology that the perceived divisions between populations of specific physical characteristics are independent of cultural, technological and presumed cognitive divisions. In numerous parts of the world, including south-eastern Australia, the Iberian Peninsula, the Levant and central Europe, populations of 'modern' and 'archaic' characteristics occurred in the same time intervals, and they often shared essentially identical tool kits, even decorative objects. Moreover, there are numerous finds of reportedly intermediate hominids, claimed to display both archaic and 'anatomically modern' characteristics, including those from Mladec Cave, Krapina, Starosel'e, Rozhok, Akhshtyr', Romankovo, Samara, Sungir', Podkumok, Khvalynsk, Skhodnya, Lagar Velho, Crete, Narmada, Jinniushan, and several more Chinese sites. A sapienisation process occurred apparently in many regions outside of Africa, or alternatively, the presumed two populations interbred extensively (which also refutes the genetic hypotheses). Anatomically modern humans occur in Mousterian contexts, e.g. in Ukraine and Russia (Roginsky et al. 1954; Yakimov 1980), and Neanderthals possessed Upper Palaeolithic technology, even in France. Cultural and perceived palaeoanthropological divisions certainly do not coincide. Therefore the cultural model implicit in the Replacement Theory lacks any sound archaeological foundation. The notion of the 'replacement' of an 'inferior' population needs to be severely

questioned, it probably reflects the historically contingent ideologies of commentators much more than any aspect of the human past (Bednarik and Kuckenburg 1999).

An alternative interpretation

Two main factors in the final refutation of the replacement model, however, are the evidence of Middle and even Lower Pleistocene maritime colonisation (see Bednarik 1997c for bibliography), and the application of taphonomic logic to the issue. The far-reaching effects of the early seafaring evidence have been discussed in detail elsewhere (e.g. Bednarik and Kuckenburg 1999; Bednarik 2003b), here it will suffice to note that the idea that a cultural quantum jump was suddenly generated by a 'modern' intrusive population in western Europe is becoming an absurdity. It does not even fit with the archaeological evidence from central to eastern Europe, according to which both 'modern' humans and Upper Palaeolithic technology developed directly in situ. There is no archaeological evidence of a 'superior technology' spreading northwards through northern Africa in the mid- or upper-Late Pleistocene. The Aurignacian was not imported from the Levant, it is an indigenous European development most likely from such traditions as the late central European Micoquian, Mousterian, Szeletian, Bohunician and Olschewian, or the eastern European Streletsian. East of the Rhine there is considerable evidence of technological and cultural continuity from Middle to Upper Palaeolithic traditions (Bednarik 1995d). The Périgord-centric perspective of this time period, the first half of the Würm glacial, does not facilitate a balanced and realistic perception. Western Europe was never a great centre of cultural innovation, it seems to have been a peripheral region throughout the Pleistocene. Particularly the Iberian Peninsula has apparently hosted a variety of relict traditions, even in the Holocene. Indeed, the artistic sophistication evident in the French, and much later Spanish, cave art is really an oddity, quite out of step with other global trends of the Late Pleistocene.

Technologies long established elsewhere took a long time to reach western Europe. Barbed bone harpoons, for instance, were made in Africa and east Asia many tens of thousands of years before they made their debut in Europe, during the Magdalenian. The earliest decorated pottery of Japan is twice as old as that of Europe, and the earliest ground stone axes of Sahul (Greater Australia) are six times as old as their first European counterparts. More relevantly, the Palaeolithic art of Eurasia east of the Rhine seems to have been almost entirely free of graphic figurative depiction, consisting instead of much more complex designs. If one excludes the few examples that are more appropriately considered as bas reliefs (such as the anthropomorphs from Molodova V, Ukraine, and Kostenki I, Russia; Abramova 1962) or that are doubtful (such as the rabbit-like engraving from the latter site, or the iconic elements Marshack [1989] discerns in the markings on the mammoth tusk tip from Kirillovskaya, Ukraine, which I have examined and regard as non-iconic), the confirmed iconic figures in the 'Palaeolithic' graphic art of eastern Europe and Asia are limited to the undated paintings in Kapova Cave (Boriskov-ski 1984: 226) and Ignatiev Cave (but note that Steelman et al. 2002 have dated a 'Palaeolithic' 'mammoth' figure in that cave to 7370 ± 50 BP) and two 'mammoth' engravings, one each from Mal'ta and Bereliokh, Siberia, and perhaps one figure from Hayonim Cave. Instead of iconic (to the European eye figurative) elements, graphic Pleistocene art seems to have consisted almost entirely of 'geometric' arrangements: in about 97% of the total area of Eurasia, graphic Palaeolithic art, where it does occur, seems almost entirely restricted to geometric or non-iconic marks. Of particular interest are the numerous 'geometric signs' on portable objects from Russia (Marshack 1979), Ukraine, Siberia and India (Bednarik 1994c). They are best exemplified at Eliseevichi, Mezin, Kirillovskaya and Mezherich (but also occurring, less pronounced or in smaller numbers, at Patne, Mal'ta, Afontova, Kavkaz, Balinkosh, Klinets, Timonovka, Suponevo, Novgorod-Severskaya, Avdeevo and Gagarino), in the first Palaeolithic art discovered in China, and in several engraved objects from the Levant (especially the Urkan e-Rub II plaque and an Upper Besor 6 ostrich eggshell fragment). The same pattern is found much earlier in southern Africa (Blombos Cave) and may later have extended to North America, where it occurs in the Clovis tradition. Preliminary indications are that these traditions begin in the Lower Palaeolithic and continue right through to the end of the Pleistocene, but this issue has never been examined because of the discipline's preoccupation with western Europe.

Seen in a greater perspective, some distinctive stylistic traits can be discerned in these works, and the first hypothesis I propose here is that the traditions characterised by them are culturally more complex than those of prominent, more or less 'naturalistic' (in the sense of Western-conditioned perception) animal profile figures, such as those of the classical Franco-Cantabrian traditions.

The simplistic view that such animal figures are cogni-tively more sophisticated than the often highly complex 'geometric patterns' of these eastern sites is easily refuted. If we separate art works into three-dimensional figurative, two-dimensional figurative and non-figurative genres, we see that the first is the least complex and the last the most complex. This is because in the first art genre, referent (the object depicted, the signified) and referrer (the art motif) are cognitively relatable by direct visual resemblance of certain characteristics. In graphic figurative art, the referent is related to the art motif through the projection of certain of its characteristics onto a two-dimensional plane, so the perception of its relationship to the referrer involves a decoding process requiring certain cognitive faculties. In entirely non-figurative arts as well as those that use highly 'stylised' versions of iconicity it is impossible to know the referrer, unless one has direct access to the cultural conventions in question. Moreover, in the last-named art form, concepts or ideas involving no figuratively definable referents can readily be 'depicted'. It is therefore clearly the most sophisticated art genre, and can communicate unlimited numbers of ideas, in rather the same way as written characters.

Discussion

This separation can be correlated broadly with the main stages of human evolution. The Makapansgat cobble would seem to indicate an early hominid ability to detect at least some aspects of iconicity, even if only at a 'reflexive' level (Bednarik 1998b). It would then be reasonable to consider that subsequent hominids developed the capacity to detect iconic properties of natural objects (such as the Tan-Tan and Berekhat Ram pebbles). A predilection for abstracting three-dimensional likeness to graphic image apparently developed more recently, perhaps preceded by an ability to replicate two-dimensional imagery, such as phosphenes (Bednarik 1987b), fossil imprints (Feliks 1998) and, in some traditions, eventually tracks. The use of non-iconic markings to form complex patterns of communicable meaning seems to originate in Africa or Asia, on present evidence. This last art form, the most sophisticated, dominates in most of Upper Palaeolithic Eurasia. Even in the western European cave art, 'non-figurative' motifs far outnumber zoomorphs, and since they are almost certainly symbols of specific meanings, they are more semiotically complex than the usually favoured animal figures. An animal picture, by itself, communicates very little by comparison, but it has been much more likely to attract scholarly attention.

This is in itself an intriguing point: considering that the communicative value of a so-called Palaeolithic 'sign' is most likely more sophisticated and informative than that of an animal outline, why are scholars, who are surely meant to see beyond these 'aesthetic' superficialities, so preoccupied with the figurative component of this art? I do not seek to detract from the great artistic excellence of the Upper Palaeolithic animal figures, I am as much in awe of these masterworks as anyone else. But the scholar is meant to be objective enough to see that the semiotic potential of these pictures is rather limited. The rules of refutation force me to accept that I cannot, definitively, determine the species of the animal apparently depicted, because my opinion cannot be falsified. It only reflects my own cognitive and perceptive processes. The falsification of a proposition is not a democratic process, subject to a majority decision, and even what all the experts of Palaeolithic art collectively think was depicted in a picture does, in the final analysis, not amount to evidence (cf. Macintosh 1977 for a conclusive demonstration that alien researchers cannot identify zoomorphic motifs in rock art).

At first sight it would appear that the outstanding oddity of Pleistocene art is the rich Upper Palaeolithic figurative art corpus of south-western Europe, with its strong development of iconic graphic depiction, but there is in fact a more perplexing deviation from a simple evolutionary trend evident. Simple non-iconic markings appear in the late part of the Lower Palaeolithic, and they continue to be made during the Middle Palaeolithic. Over an enormous time span they seem to experience some change towards increasing complexity, but their range nevertheless remains remarkably consistent: parallel lines, convergent lines, radial motifs, zigzags or meanders, dot patterns, lattices, circles. Their wide distribution over the Old World suggests a near-global cognitive tradition that perhaps coincides with archaic *Homo sapiens* groups, and may even have been universal to them. This art form continues throughout the Middle Palaeolithic and is eventually taken to Australia by Middle Palaeolithic sailors, where it manages to survive into the Holocene. The only cohesive explanation so far proposed for this long-lived and near-global 'tradition', which culminates in a distinctive set of motif types, is the phosphene theory (Bednarik 1984, 1987b, 1990b; Hodgson 2000). This is also the only scientific theory so far proposed for art origins, in the sense that it is a fully falsifiable and thus testable proposition.

Irrespective of this interpretation of the existing record, it appears that by Upper Palaeolithic times, traditions of using non-iconic markings had become so sophisticated that they appear to have served for mnemonic, record-keeping or other exceedingly complex semiotic activities in Russia and Siberia. Their vestiges have so far attracted only cursory attention and these traditions remain profoundly unknown. At the same time, similarly complex traditions of 'geometric decoration' had evolved across Asia, for which only impoverished parallels can be discerned in south-western Europe. On the other hand, the very few iconic graphic depictions of Eurasia east of the Rhine, while indicating that this art form was available across the continent, seem to suggest that it was not widely used. But before we draw this conclusion we would be well advised to consider alternative interpretations. For instance, the apparently complete restriction of Upper Palaeolithic rock art in Europe to limestone caves is almost certainly a taphonomic phenomenon, and as such must not be used to formulate explanations without the extensive use of taphonomic logic (Bednarik 1994d, 1995a, 1995b). Similarly, even if these severe limitations imposed by taphono-mic logic did not apply, distribution of evidence would still be a function of research effort, which has massively favoured Europe, especially south-western Europe, for over a century.

This is the second hypothesis I propose. In Asia, for example, only two small regions have seen some level of concerted effort in this area, the Levant and the Irkutsk region. Both have yielded good evidence, but many parts of Asia have never been subjected to any serious attempt to locate Pleistocene art. In other words, frequency of evidence seems to be directly related to intensity of research work, qualified to a considerable extent by research biases introduced from Europe. In many cases such endeavours were guided by European ideals of 'what to look for'. Bearing in mind the exceptional nature of those 'ideals' this was clearly a misguided approach that can only have led to biases in data-gathering practices. For instance, the Pleistocene bone harpoon of Lohanda Nala in India was interpreted as a female figurine until I examined it, and many Chinese, even North American, investigators have been guided in their search for early art by the European paradigm. This was a direct result of the false models of Pleistocene art evolution emanating from south-western Europe.

The global development of Pleistocene art is very different from what has been perceived to have occurred in south-western Europe. But it will take a long time to eradicate this mythology, not just because it is so entrenched

in the published record as well as in the public's mind, but because there exists an influential academic structure that will resist the corrections I advocate. In a model of global art origins as demanded by the evidence I have listed, and by other factors related to this topic, the rock art and mobiliary art of south-western Europe are of peripheral significance, instead of occupying centre stage as the orthodox model would have it. Throughout the Pleistocene, Europe, a small and unimportant appendage of Asia, played a marginal role in the evolution of hominid cognition, and south-western Europe in particular was a cultural and technological backwater of the world, a geographical cul-de-sac remote from the main theatres of this evolution in eastern Europe, the Near East, southern Asia and parts of Africa. It is therefore not to be expected that the figurative art of the Franco-Cantabrian sites, which is no more than a taphonomic fluke, had a decisive influence on the major cultural currents that developed during the Pleistocene and especially towards its end. What I have tried to show here is that these major currents have been so inadequately studied to date that they remain largely misunderstood. Not only are the data hopelessly skewed by the false model of art origins, they are just as distorted by other factors, especially geographically uneven research efforts and the pronounced biases of researchers and research directions.

However, the most fundamental aspect of the topic of this paper and the strongest evidence that traditional archaeology can provide only unsatisfactory models of 'art origins' still has to be discussed. The material evidence listed in this paper is actually redundant in showing that this traditional model must be false. Taphonomic logic is an axiom-like principle capable of filtering out false and whimsical hypotheses in archaeology. It views archaeological populations of evidence categories as the surviving remnant of cumulative populations that have been subjected to continuous and perfectly systematic degradation selecting in favour of specific properties facilitating longevity: the greater the age of the evidence, the more distorted its distributional and compositional variables, until a point in time is reached at which all these variables become literally irrelevant to the interpretation of the aspect the phenomenon category in question is supposed to refer to. Or in other words: the further we go back in time, the more misleading traditional archaeological interpretations must be expected to be. For most archaeological evidence categories, the composition and distribution of the material evidence of the Pleistocene has little or no bearing on explaining the societies, cultures or even technologies in question. The reason for this is very simple: if taphonomic processes effect the loss of a certain portion of a phenomenon category per time unit, a point in time must be reached when all of the evidence above a certain age (the taphonomic threshold) should be exhausted. In reality this cannot occur, because the probability of survival of any evidence can never be nil. Therefore there will be a tiny remnant population, consisting of 'survival flukes' (e.g. rock paintings in deep caves), extending beyond the threshold time of the phenomenon category. Archaeology systematically misinterprets these specimens from a category's 'taphonomic lag period' (for explanations of this quantifiable form of logic, see e.g. Bednarik 1994d, 1995b: 630, 2001d) by regarding quantifiable variables as being culturally significant, when in fact they are largely or entirely attributable to taphonomy. For instance, the world distribution of hominid remains is not a map of hominid distribution, it is a map of the distribution of sedimentary and other preservation conditions favouring the survival of such remains, combined with the distribution of both hominids and research efforts to find their remains.

Taphonomic logic is capable of predicting accurately the type of evidence of palaeoart one should encounter so further one goes back in time. Such evidence should become progressively less common, until a point in time when it seems almost to disappear from the record. However, beyond that threshold it should still extend for a much longer period in the form of extremely rare specimens. With increasing age, specific art forms should occur in specific environments—such as figurines of calcite, bone and ivory in limestone caves and loess deposits, or rock paintings in deep caves. As one proceeds back further in time, one should encounter very rare specimens of particularly deterioration-resistant forms: deeply cut petroglyphs such as cupules on highly resistant rock types such as quartzite, stone figurines, haematite crayons and the like, i.e. the types of materials that were at the disposal of hominids and had the greatest prospects of surviving under fluke conditions.

It comes as no surprise that the kinds of quantitative and qualitative evidence taphonomic logic predicts is precisely the kind we are finding. The oldest single specimen of palaeoart in the world is a round cobble of jasperite. This is about as deterioration resistant as we can hope to find from that time. There are no surprises here. If we were to perpetuate the penchant of traditional archaeology to misinterpret the evidence, we could create from the above catalogue a model of how art-like production began, with stone figurines and quartz crystals and cupules. This is how archaeology translates data into models, *and it is the false way*. It merely demonstrates that Pleistocene archaeology must be expected to be wrong most of the time. The taphonomic interpretation of the above catalogue is *the precise opposite*: the evidence of figurines and crystals and cupules demonstrates that palaeoart *did not* begin with figurines and crystals and cupules. Until archaeologists appreciate why this is so and then apply this logic to all finds their discipline can only remain a 'consensus fiction' of the past (Bahn 1990: 75).

Conclusions

The two main pillars of the orthodox model are that art begins with the Aurignacian of Europe, and that Pleistocene rock art is an endemic cave art primarily of south-western Europe. Both these concepts are serious errors of fact, and their preservation has necessitated the explicit denial of the existence of Middle and Lower Palaeolithic palaeoart, as well as the systematic neglect of extra-European Pleistocene palaeoart. Yet all of the Pleistocene rock art of Australia should be regarded as essentially Middle Palaeolithic, and since this corpus is thought to be numerically much greater than that of

the Upper Palaeolithic art of Europe, it follows that we seem to have more surviving Middle Palaeolithic rock art than Upper Palaeolithic. Hundreds of scholars have been engaged in exploring the question of art origins, but the Pleistocene rock art of Australia has usually not attracted their attention, nor has the palaeoart of the rest of the world. The resulting spatial bias implicit in the Eurocentric paradigm, the continued failure to adopt taphonomic logic as the universal theory of archaeology in lieu of the debilitating uniformitarianism that has been its de-facto universal theory for almost two centuries, and the scandalous treatment of dating scientists when their results did not meet stylistic expectations are just three factors that speak for themselves. When we add to this the fact that nearly all of the countless fakes of Pleistocene art relate to the south-western European traditions, and when we consider that a good portion of what is still considered to be Palaeolithic art may be either fake or at least of Holocene age (consider, for instance, the most celebrated paintings of Lascaux, which appear to be of the Holocene), it should be self-evident that this field of study is indeed in crisis (Beltrán 1992; Bahn 1994; Bednarik 1996b).

There are other epistemological or heuristic issues to be considered. For instance, why is there not a single fake of Palaeolithic art known in Russia and Siberia, where portable art is as plentiful as in western Europe? The high concentration of fakes in one small region seems to be attributable to the same factor that has led to the largest number of false claims of Pleistocene age for rock art and portable art in the same region: an excessive preoccupation with the importance of the Upper Palaeolithic art of that region. These are fascinating topics for analysis.

In summary, the Palaeolithic art lobby has made several fundamental errors in interpreting the available empirical evidence. Some are of little concern in the present context, but the following need to be clarified here. First of all, it has assumed that sophistication in graphic art is indicated by figurative complexity, and particularly by 'naturalistic' depiction ('naturalistic' in the sense of Western perception, which can differ significantly from the perception of other people). Second, geographical discrepancies in research intensity are so severe that the published record is massively distorted, yet this has not been taken into account in either hypothesis building or in research design. Third, the dominant dogma of art origins has not only affected what we think about this topic, but also what we look for, find, and consider relevant; it has dictated research directions and priorities. Fourth, dominant paradigms in general archaeology have successfully censured, ignored and suppressed data about art traditions outside of Europe or preceding the Aurignacian in Europe, as well as data about other aspects of hominid sophistication before the advent of 'anatomically modern' humans (seafaring, for example). In cases where such data could not be explained away they were accepted as evidence of a 'running ahead of time' (Vishnyatsky 1994), a particularly pernicious argument in view of the taphonomic bias always embedded in the data.

However, the greatest systematic error has been the neglect of taphonomic logic, according to which most forms of palaeoart of the Pleistocene predate the taphonomic threshold of their respective phenomenon categories. It is for all practical purposes impossible, except by pure chance, to contrive valid explanations of any aspect of such evidence without the application of this form of logic. Until now the study of Pleistocene art has been conducted in the form of a game of chance, bereft of systematic procedure, and without a universal theory of how what happened in the distant past relates to what we perceive as the 'archaeological record' of it. Until the specialists of Palaeolithic art, whose prestige and influence derives from some apparently mythological powers of perception, understand and employ taphonomic logic, and replace this game of chance with the scientific framework of metamorphology (Bednarik 1995a), their interpretations will remain aspects of a belief system. Freeman (1994) has most perceptively noted the astounding similarities in the processes of validating Palaeolithic art 'sanctuaries' and religious shrines. His paper needs to be read by everyone believing in the mythological powers of perception of Palaeolithic art experts that enables them to know the age of a motif from its 'style'. In comparing the two forms of validation, Freeman concludes that 'these two manifestations of belief, reverence, and validation of experience have the same origin at a deeper structural level' (1994: 341). Until these belief manifestations are replaced by processes of falsification, blind testing and other scientific procedures, the pronouncements of the art experts have no more validity than those of Roman Catholic arbiters of holy shrines: they may well be valid, but that is not the issue. The issue is whether they are intrinsically falsifiable.

Robert G. Bednarik
International Federation of Rock Art Organisations (IFRAO)
P.O. Box 216
Caulfield South, Vic. 3162
Australia
E-mail: *robertbednarik@hotmail.com*

RAR 20-646

COMMENTS

The challenges of studying 'earliest' art

By WALTER BOWYER

Bednarik, in 'The earliest evidence of palaeoart', makes two important contributions in his persuasive argument to intensify palaeoart research outside of Europe and earlier than the Upper Palaeolithic. First, his paper provides a critical review that updates and reinforces earlier reviews (e.g. Bednarik 1994b; Bahn and Vertut 1997: 27–41) of the earliest known palaeoart. The evidence presented here

should make it hard to deny or dismiss the existence of art before the Upper Palaeolithic, and it adds to the growing clamour against the concept of language, art and consciousness appearing abruptly and without antecedents. As Bednarik argues, the current paradigm of Neanderthal replacement supports the paradigm of recent origins of symbolic behaviour. Both paradigms rest on controversial evidence. Given the history of thinking about human evolution, it is fair to ask if a subconscious need to award special status to *Homo sapiens sapiens* contributes to the appeal of these paradigms.

Second, Bednarik's paper raises again the importance of taphonomy. Important to any area of archaeology, taphonomy is especially critical in the study of palaeoart because of art's diversity. The earliest art probably was not preserved. Body painting, drawing on animal skins or wood, and even drawing with a stick in soft earth likely preceded rock art. Furthermore, studying the trajectory of art has proven very perilous because of taphonomy and the random factor inherent in rare finds. The most seemingly reasonable propositions of art evolution may be disproved by single discoveries (e.g. Chauvet Cave or the Pataud Shelter).

In passing, Bednarik makes a third important point upon which he has elaborated in earlier papers (e.g. Bednarik 1994b): our modern cognition is not an appropriate context for thinking about the invention of art. Dennett (1990) convincingly argued that human consciousness has evolved dramatically, even over short time scales. If we define consciousness as the mind's way of creating a world from sensations, the earliest artists lived in a world very different from our own.

Bednarik offers a radical re-evaluation of Franco-Cantabrian art, arguing that it has been overemphasised in studies of early human cognition. I agree with Bednarik that the aesthetic appeal of the art makes it difficult to study objectively. I also agree that the place of Franco-Cantabrian cave art in the worldwide pre-History of art is yet to be determined. Certainly, the cave art does not represent the origins of art. But was it the result of an independent, European development, or was it part of a much older and widespread tradition? Was cave art uncommonly sophisti-cated art for that period, or did humans in other parts of the world at that time produce equally sophisticated art that simply has not yet been found? These questions can only be answered with broader research effort.

I differ from Bednarik in his estimation of the importance of the Franco-Cantabrian art. The cave and portable art of the Cro-Magnons in the Upper Palaeolithic represent a spectacular florescence (or probably more accurately, a series of related florescences) that offers us an informative example of cognitive development. Although Breuil's emphasis on realism was misleading, the three-dimensional perspective, perfect proportions, and even perfect dimensions (Iakovleva and Pincon 1998) of the art were real technical achievements.

The animal figures in the Franco-Cantabrian Upper Palaeolithic probably are both icons and symbols at the same time, and so cannot be dismissed as less important than 'abstract signs'. Indeed, researchers are making more progress in finding patterns among the animal images than among the 'signs'. For example, the many papers of Brigitte and Gilles Delluc provide evidence for structure in the compositions (e.g. in Pair-non-Pair Cave in 1997), and Sauvet and Wlodarczyk (e.g. 1995) have used statistics to find 'rules and constraints that are probably semantic' among the figures. Perhaps it is easier to find rules among the icons than among the abstract signs because the recognisability of the icons facilitates the reformulation of the artists' taxonomy—a necessary first step before identifying the rules.

This is not to undermine Bednarik's concern about the subjective recognition of icons. In addition to Bednarik's objections, ambiguous and indeterminate figures may imply, as Lorblanchet (1989) argues, that Franco-Cantabrian Upper Palaeolithic artists occasionally ignored categories and broke down boundaries. Nevertheless, the iconic figures are easier to categorise than many of the abstract signs.

The achievements of the Franco-Cantabrian artists, the excellent preservation of the art, and the broad foundation already established by researchers guarantee that continuing study will be productive. A nuanced cultural relativism can most effectively be accomplished by building upon the rich corpus of research already performed and, at the same time, broadening research efforts to earlier times and a wider geography.

Did art originate as icons or as symbols? Icons, indices, and symbols are related by a hierarchy of increasing demands on cognition, and some researchers argue that art began in the form of icons. Furthermore, it seems, *a priori*, that icons require less cultural investment since a figurative image can be recognised in the absence of cultural conventions. On the other hand, symbols are easier to create than icons, at least in two-dimensional art. And as Bednarik amply demonstrates, symbols (or at least signs) occur much more frequently than iconic images in the early archaeological record. The Tan-Tan figurine described recently by Bednarik (2003c) appears to be an exception to this rule and, as such, may ultimately prove to be one of the most important finds of palaeoart.

Although continued critical inquiry into European Upper Palaeolithic art is necessary, the question of the *origins* of art will require more early evidence from around the world. Of course, as Bednarik discusses, the challenge of taphonomy increases as one looks earlier in the record. If Bednarik's paper influences researchers to look more seriously for art in the Lower and Middle Palaeolithic, it will have been successful indeed.

Professor Walter Bowyer
4015 Scandling Center
Department of Chemistry
Hobart and William Smith Colleges
Geneva, NY 14456
U.S.A.
E-mail: *bowyer@hws.edu*

RAR 20-647

State of origins: art, cognition and the uncertain mindedness of the first Australians

By ADAM BRUMM

I am no palaeoart expert. Consequently, I do not wish to be drawn into an extended discussion on the legitimacy of either (a) Bednarik's exhaustive summary of early palaeoart and symbolic activity from around the world, or (b) his argument concerning the differential 'sophistication' of iconic and non-iconic art (except perhaps to mention that whilst I agree non-iconic art is probably no less sophisti-cated in communicative potential than iconic art, I see no reason to suggest that it is any *more* sophisticated either— this seems to presume too much about the meanings and functions of different forms of visual imagery in ancient societies). Instead, in commenting on this paper I wish to focus on some of the wider implications of Bednarik's argument with respect to the present debate about the origins of modern human cognition and behaviour.

In this sense, Bednarik's paper presents a timely contribution to the search for the origins of what it 'means' to be human, a key element of which must be the universal propensity to create and communicate meaning through art. Bednarik's views are of particular significance in light of the recent discovery of the Herto fossils in Ethiopia (see Clark et al. 2003). If ancient people from the Middle Awash region were indeed engaging in mortuary practices and curating human skulls for symbolic purposes 160 000 years ago, then Bednarik's extensive summary of early palaeoart evidence from around the world must impress upon even the most ardent supporters of the 'orthodox model' that human ancestors before 50 000 years ago were not biologi-cally incapable of 'modern' behaviour and symbol use.

But let us suppose for a moment that Bednarik is wrong; that is, some form of as yet unknown neurobiological change, perhaps a 'neural rewiring' at the genetic level (e.g. Coolidge and Wynn 2001), took place amongst anatomically modern Africans around 50 000 to 40 000 years ago— igniting the 'dawn of human culture' (see Klein and Edgar 2002). According to proponents of the 'orthodox model' this key evolutionary event resulted in certain abrupt discontinuities in human behaviour; namely the sudden emergence of the modern human capacity for language and art, the many creative technological and economic advances of the so-called Upper Palaeolithic 'human revolution', and the radiation of cognitively and behaviourally modern people from out of Africa and into Eurasia and Europe where they replaced the non-symbol-using Neanderthals. Prior to this time our foraging ancestors are argued to have thought and behaved in ways little different to archaic hominin species, they adapted to the world through primarily biological rather than cultural means. If we take this 'orthodox model' to be the more correct version of the early human past in that it accurately reflects present archaeological, palaeoanthropological and genetic data, then what would be some of its wider logical implications?

Most obviously I think we must consider the question: if anatomically modern humans were present outside of Africa before this fundamental neurobiological transition supposedly took place, then were these early peoples cognitively and behaviourally *modern*? 'Orthodox model' theorists argue there was a timelag of tens of thousands of years between the appearance of modern human anatomical traits and the development of fully modern consciousness and behaviour. Bednarik of course would argue the opposite; in fact that symbolic behaviour predates the appearance of *H. sapiens* by several hundreds of thousands of years. One might therefore logically assume that if there were biologically modern humans outside of Africa before 50 000 years ago, then if the 'orthodox model' is to survive and retain credibility it must be argued that these peoples were cognitively and behaviourally *non*-modern; perhaps a relic population with an archaic or 'primitive' mentality much like earlier hominins.

This is part of the reason why the early luminescence dates of 60 000 – 55 000BP from Malakunanja II and Nau-walabila I in northern Australia, and the U-series and ESR determinations of approximately 62 000 years for the ochred human skeleton from Lake Mungo, have aroused world interest and attracted such intense scepticism and controversy. As Richard Klein (2000: 32), a vocal proponent of the 'orthodox model', bluntly concedes: 'The occupation of Australia by modern humans 60 ky ago would argue … against a radical behavioural shift roughly 50 ky ago'. Although the validity of these dates has been brought into question from several different quarters, population-genetics data would also corroborate a c. 60 000 BP time-frame for initial colonisation—indicating Australasians may have split off genetically from northern Eurasians at least 75 000 to 50 000 years ago (e.g. see Mountain and Cavalli-Sforza 1997: 715). According to the 'multiple dispersals' model postulated by Lahr and Foley (1994, 1998), the early luminescence dates and genetic data could imply there was a first successful dispersal of anatomically modern humans from a southern route out of Africa (when the Levantine corridor was inaccessible to Africans) and along the coast of southern Asia into the Australian region, probably between 70 000 and 60 000 years ago. This early range expansion occurred long before the final 'replacement' wave of cognitively and technologically 'superior' modern African *H. sapiens* into Eurasia and Europe between 50 000 to 40 000 years ago; a central tenet of the original 'African Eve' theory accused by Bednarik and others (e.g. Ingold 2002) of naturalising Western colonial history.

But if according to the 'multiple dispersals' or 'southern dispersal route' hypothesis the ancestors of the first Australians arrived in this continent long before the supposed 'dawn of human culture' in Africa roughly 50 000 to 40 000 years ago, then logically, could we consider these early travelers to have been fully modern humans in terms of cognition and behaviour? If we continue with the view that Bednarik is wrong, then *no*, we can not, because the first peoples of Australia simply could not have been a part of the African-based neurobiological 'revolution' towards modern humanity. In short, they could not have been 'truly

human' in the same sense that we consider ourselves to be today. Yet does the earliest archaeological record of Pleistocene Australia, mysterious though it is, but which as Bednarik mentions in this paper includes compelling evidence for ochre use, ritual burial and rock art, really seem the result of non-symbolic, non-thinking, non-*human* beings? Moreover, it is highly likely that a modern sense of 'mindedness' *must* have been possessed by the first Australians in order for them to have been able to make the difficult sea crossings into this continent in the first place (Davidson and Noble 1992).

Other evidence would suggest such novel behaviours have considerable antiquity and did not emerge abruptly during the Late Pleistocene. As mentioned, due to an early range expansion the *final* genetic separation of Australasians and Eurasians may have occurred relatively recently, perhaps only 75 000 to 50 000 years ago. However, minor genetic and morphological differentiation between these and other modern-day human populations may have begun as early as 200 000 to 130 000 years ago during Oxygen Isotope Stage 6 (OIS 6). One theory suggests that during this time a major population bottleneck in Africa was followed by a 50 000-year period of demographic expan-sion, contraction and the formation of regional-local populations, some of which became extinct (see Lahr and Foley 1998). In light of this data the universality of modern-day human symboling abilities, namely the capacity for art and language, would seem to indicate that a common genetic basis for the origins of modern human cognition and behaviour in Africa must lie during or before OIS 6 (cf. McBrearty and Brooks 2000). The recent Herto discoveries and also early art and bone tools from Blombos Cave and other Middle Stone Age sites in South Africa might strengthen this 'unorthodox' position; implying that whatever it 'means' to be a modern human must extend deeper into *non-European* pre-History than is currently argued. Precisely how deep, however, remains to be demonstrated.

Of course these are complex issues that are frustrated by a general lack of substantive archaeological evidence, and we may never have complete answers. But after Bednarik and others (e.g. d'Errico and Nowell 2000; McBrearty and Brooks 2000), and in light of the problematic Australian situation, it would appear somewhat more plausible to look at the origins of modern human behaviour and cognition as lying in gradual processes of development rather than a discontinuous evolutionary event (cf. Ingold 2002); in which case Bednarik's summary of early palaeoart evidence is of major importance.

After all, if we maintain the 'orthodox' view that being human is something our foraging ancestors had to abruptly and simultaneously converge upon during the so-called 'dawn of human culture' in Africa roughly 50 000 to 40 000 years ago, don't we run the risk of creating ridiculous scenarios in which for historical reasons (such as early dispersals from Africa) some anatomically modern peoples like Pleistocene Australians never really *became* human at all? Indeed, as Tim Ingold (2002) evocatively concludes,

> [w]e have not reached above our biology, and we never will. There never was any mighty moment in the past when the upper limits of nature were breached and our ancestors emerged onto the stage of culture, for the very idea of a division between nature and culture is, as I have shown, a Western conceit.

Adam Brumm
Department of Archaeology and Natural History
Australian National University
Canberra, ACT 0200
Australia
E-mail: *arbrumm@care2.com*

RAR 20-648

Cognitive challenges to taphonomy

By KALYAN K. CHAKRAVARTY

The question of art origins—and since rock art arguably constitutes the largest and earliest surviving body of palaeoart, of rock art origins— is inalienably connected, for Bednarik, with the question of cognitive beginnings of humanity. He has relentlessly demolished the theory of rock art origins in the limestone caves of south-western Europe, supposedly heralded by sophisticated figurative, zoomor-phic depictions in the Aurignacian period, and accompanied by a quantum cultural jump in the material and technologies of an intrusive Cro-Magnon population. He has also rebutted the theory of subsequent dissemination of rock art from south-western Europe to the entire world. He has shown this theory to be a hangover of colonial archaeological assumption of the coincidence of racial and cultural superiority of Europe. He has castigated exclusive claims of Palaeolithic age for rock art as coming out of a mythology rooted in dated uniformitarianism, stylistic confirmationism, confusion of non-anthropic with anthropic markings, and, out of needs of shoring up ethnic identities.

Bednarik has adduced evidence of Pleistocene portable art and rock art from all over the world to demonstrate that the Aurignacian in western Europe was really peripheral to Pleistocene palaeoart traditions, which began much earlier in eastern Europe, Near East, southern Asia, Africa or Australia. He has pointed out that the non-figurative, 'geometric' art in the rest of the world is older and more complex and sophisticated than the figurative art of south-western Europe. He has used the phosphene theory to explain that the non-iconic art is also more homogeneous all across the globe. He has shown, with magisterial command over the material, that the capacity to deliberate, set destinations and reach them, to design and create art, by modifying and enhancing nature, was not a monopoly of the European artist, but characteristic of the human cognitive system all over the world in the Pleistocene period. He has explained the sparse evidence of Palaeolithic rock art in the non-European world by the taphonomic logic that occurrence of art precedes its physical appearance, that the visible evidence represents a fraction of the evidence that has been destroyed, and that the survival of the evidence is a a factor of the environmental conditions, while its availability is a a function of the in-

tensity and propensity of research. He has done yeoman's service to the cause of understanding of human beginnings, by correcting the terms of Eurocentric discourse about rock art origins. He has conclusively proved that the rock art outside Europe is not a clumsy or superseded beginning, a mere stage in the progress to the perfection of European rock art, but a mighty and uncanny beginning, which needs to be thought back to its roots to be thought forward into the future.

Rock art appears to have represented that unity of life and art, in which art was at once man's way of experiencing and responding to life. Bednarik's article raises the problem of bridging the hiatus between experience and knowledge of life as depicted in rock art. In experiencing life, it was only partially understood by the rock art artist, and, in the process of acquiring its understanding, it is not really being experienced by the rock art researcher, and the latter's knowledge is inevitably doomed to arrive late on the scene of experience. The time-hallowed practice of retouching rock art in Australia, the continuing traditions of folk art on the floors, walls, household objects in rural India, or among indigenous communities of the world, attempts of rock art scholars like Lorblanchet to remake the rock art, the project of Bednarik to re-do the expeditions of ancient seafarers are in line with the mission of this article, which hermeneutically negotiates the distance between the rock art and its interpreter, by recapitulating, retelling, reliving the text and context of the art.

It is essential, as Bednarik insists, that games of chance give way, with respect to assessment of the cognitive roots of rock art, to an appreciation of the metamorphological and taphonomic processes associated with it. It is, however, necessary to ensure that the metamorphological and taphonomic logic are applied not only to the impact of physical factors like force, heat, pressure, moisture etc., but also to our own heuristic approaches to rock art, which are controlled by our own time and place, and by the debris of our historical prejudices and predilections. Taphonomy should be harnessed to bridging not only the evidentiary gaps but also the cognitive gaps that have widened between the rock artists and us. It has to set itself the task of recovering the blend of form and function, embellishment and necessity, beauty and utility, that may have been characteristic of most of the Pleistocene art and artefacts, which Bednarik terms as non-utilitarian within quotes. If the theory of phosphene universals and entoptic constants is valid, it should be possible to recollect and repossess the simultaneity of creative and pragmatic responses to similar ecological situations, which contributed to the making of rock art.

Bednarik has proposed direct dating approaches as scientific and falsifiable, as against stylistic dating, which was initially adapted in Eurocentric discourse and which he has dubbed as subjective, unscientific and nontestable. Bednarik agrees, however, that direct dating only offers an approximation to an exact date, though it is falsifiable and improvable. The direct dating approaches, moreover, often rest on the doubtful assumption of uniformity in physical processes. Once the definition of style is changed, it may become as good a tool for approximating a correct hypothesis for the age of Pleistocene rock art as any one of the direct dating methods, which have to be calibrated and are subject to several conditionalities. The permutations and combinations of materials and techniques, perspective, line and volume, light and shade, colour and location of a large range of rock art in similar biogeographic contexts should provide concrete clues to style, instead of evoking vague kinaesthetic gut reactions or intuitive imaginings. With growing understanding of the isomorphism of the verbal and genetic codes, the phosphene theory should provide a clue to the movement in the life of the form, to the manner in which the same figure or the same geometric pattern is conceived and modelled differently in different 'styles' in different times and periods. The stylistic aid, so honed and redefined, should be as repeatable and falsifiable as the direct dating methods.

One wonders whether the need and motive of restitution and redemption against Eurocentric theories, which are insistently and stridently present in Bednarik's article, may not result in inappropriate claims of priority or sophistication for Asian or Austronesian art. We have to be careful that the post-colonial reaction to colonial prejudices does not culminate in reverse claims of superiority of the colonised. It may be necessary to guard against the danger of veering to any extreme position to suggest concentration of iconic or non-iconic art in Asia or in Europe, or to accept the priority of non-iconic over iconic art, as a more complex and sophisticated art. As Bednarik himself observes, the archaic and the modern coexist in rock art. So do the iconic and non-iconic, the simple and complex, the concrete and abstract in all phases and areas of rock art. In fact, it is likely that the iconic and non-iconic were performing interchangeable functions in the history of art. Pictures have served as letters and letters have suggested pictures in the history of language. The iconic and figurative depictions in rock art, which appear mutually differentiated, tactile, concrete and 'haptic' from proximity, would appear undifferentiated, visual, abstract and 'optic' from a distance. Bednarik also speaks of the danger of using the perceived nature of Pleistocene traditions as evidence of cohesive cultural entities and cognitive sophistication. As he emphatically proves, cultural and perceived palaeoan-thropological divisions do not coincide. On this postulate alone, the Asiatic or the European traditions should not possibly stake claims to priority over each other, in view of taphnomic accidents in the survival of evidence or of gaps in human surveys.

The intensity of rock art research has indeed accounted for a hegemonic commodification of rock art in technologically advanced countries, as also for objectification and marginalisation of rock art in technologically backward countries. And yet, the consciousness industry, the society of spectacle, which have promoted rock art, have, to no small extent, accounted for its preservation and survival, whereas countries which have neglected the heritage tourism industry have been losing their rock art due to neglect and vandalising mega-developmental schemes. It is possible, using the Australian example, to use heritage as an instrument of rock art conservation.

The theory of quantum leaps in the making of rock art needs, no doubt, in deference to Bednarik's argument, to be rejected as applied specifically to south-western Europe. But, once we spurn, with Bednarik, the theory of uniformitarianism, we do probably need to accept moments of sudden illumination, in the making of rock art. In a multilinear rather than diffusionistic, monolinear model, such moments may have occurred as parallel and co-ordinate occurrences all over the globe. George Kubler (1962: 95) describes such occurrences as comparable with a forest fire in its leaping action across distances of the earth, where unconnected centres have blazed into similar activity. It is not like a slow, cumulative, glacial drift. Having conceded the fact that biological and cognitive divisions do not coincide, it is necessary to acknowledge the possibility that similar cognitive leaps may have been taken by the human species, irrespective of their biological divisions, in response to similar challenges, with similar developments in their neural or cortical hardware and encephalisation.

The taphonomic and metamorphological argument may have to acknowledge the possibility of scientific discoveries with regard to the process of human cognition, which will bring what is today regarded as unscientific within the domain of falsifiable and replicable scientific tests. Science may not be advanced enough today to yet encompass and comprehend all the steps in the creative process.

Dr Kalyan Kumar Chakravarty
Forest, Education, Culture and Election
Government of Chhattisgarh
Raipur
India
E-mail: *kkchakraborty@cg.nic.in*

RAR 20-649

Acheulian evidence

By STEPHEN W. EDWARDS

Robert Bednarik makes a persuasive argument that Lower and Middle Palaeolithic evidence for non-utilitarian or aesthetic behaviour has been missed or ignored by most prehistorians. I think his efforts to bring such material to light amount to a substantial contribution. However, he omits at least two classes of evidence that can make his case even stronger.

I find the author's philosophical assessment of the state of rock art studies as a scientific discipline thought-provoking and stimulating. I am not comfortable, however, with his portrayal of the motivations of investigators who adhere to the prevailing model of art origins.

Kenneth Oakley (1972: 40) deserves credit for his characterisation of Lower Palaeolithic people:

> We have no reason to infer that all Early Palaeolithic men had brains qualitatively inferior to those of the average man today. The simplicity of their culture can be accounted for by the extreme sparseness of the population and their lack of accumulated knowledge. A supposed hall-mark of the mind of *Homo sapiens* is the artistic impulse—but archaeological evidence suggests that this trait manifested itself almost at the dawn of toolmaking. Crystals of quartz were collected by Peking man many miles from his home, and one may presume that, partly at least, this was because their shape and appearance appealed to him. Some of the finer Acheulian handaxes are masterpieces of artistic craftsmanship, displaying perfection which exceeds bare technical necessity.

Oakley was impressed not only by quartz prisms collected at considerable distance and brought into the Choukoutien Lower Cave (Black et al. 1933; Pei 1931), but also by manuported fossils found with the Swanscombe cranial fragments in England (Oakley 1981: 14–16). He also described and figured (ibid.) two Acheulian handaxes from England, both knapped to display dramatically in their centres well-preserved fossil shells. One handaxe, from West Tofts, Norfolk, preserves a pelecypod valve in full-face view; the other, from the middle gravels at Swanscombe, a Cretaceous sand dollar (*Conulus* sp.). In the latter artefact the fossil occupies more than a third of the length of the piece (Fig. 1).

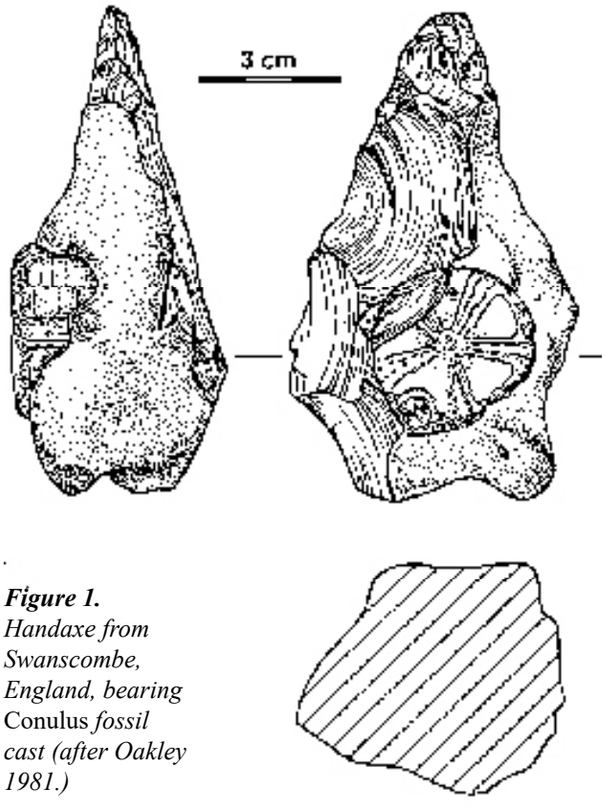

Figure 1. *Handaxe from Swanscombe, England, bearing* Conulus *fossil cast (after Oakley 1981.)*

Centring fossils in this way requires careful knapping. The parts of the bifaces closest to the fossils are minimally worked in order to avoid damaging them. A modern knapper will eschew flakes or cores that contain large steps or flaws, or hindrances like these fossils, which will hamper thinning the biface. If a maximally functional handaxe was the point of the work, saliently fossiliferous flakes or cores would probably not have been chosen.

Oakley stressed that many Acheulian handaxes were knapped to a level of refinement far beyond utilitarian

needs. This opinion has been repeated by many investigators, e.g. Schick and Toth (1993) and Edwards (2001). Jones (1981) showed that refinement of handaxes at Olduvai Gorge was strongly influenced by raw material: very simple basalt handaxes with few flake scars maintained a durable edge, while phonolite handaxe edges held up better if they had been more intensively knapped, thus producing a tool with more flake scars. Nevertheless, African Acheulian people often made highly refined, intensively flaked basalt handaxes, for example in the Middle Awash region of Ethiopia (Clark 1987), and such production is not a mere function of resharpening (Edwards 2001).

From the point of view of a knapper who has specialised in the production of refined bifaces in diverse raw materials (especially basalt) for twenty-seven years (Edwards 2000, 2001), these authors are certainly correct in their assessments. The refinement is particularly true of handaxes and cleavers of the late Acheulian. At Kalambo Falls in Zambia, for example (Clark 2001), knapping skill well beyond the attainments of most modern knappers was achieved by Acheulian hominids (Fig. 2). Handaxes of extraordinary symmetry, often with highly regularised flake-scar patterns, were produced in tough, demanding (but often quite colourful) quartzites. Typically, experienced modern knappers need one to two hours (or more) of concentrated work to replicate such artefacts (although simple bifaces characteristic of the earliest Acheulian can be made in a few minutes). Wynn (1979) assessed late Acheulian handaxes from Isimila, Tanzania, from the point of view of Piagetian cognitive analysis. He found that the tools imply operational intelligence akin to that of modern humans. Belfer-Cohen and Goren-Inbar (1994) reached similar conclusions.

It strikes me as pertinent that Acheulian handaxes, in pre-eminently durable stone such as quartzite or chert (flint), conform very well to Bednarik's 'taphonomic' model. To experienced modern knappers they suggest intelligence, skill, a strong aesthetic sense, appreciation of forms, logic, and perhaps ritual. The handaxes (and cleavers) are thus consistent with a portrayal of Lower Palaeolithic people involved in a broad range of non-utilitarian activities, evidence for few of which has survived. Essentially sculpted in stone, late Acheulian (and middle Acheulian, for that matter) bifaces should be recognised as primordial and outstanding rock art. I remember during my graduate training at the University of California, Berkeley, in the 1970s, the late Glynn Isaac cautioning us that Acheulian handaxes should not become the province of art historians. His point was well taken, yet these superb artefacts certainly have at least as legitimate a claim as the various indecipherable and minimally organised engraved lines on stone and bone cited in Bednarik's summary of palaeoart.

I suggest that many archaeology instructors since the 1960s, when knapping became popular, have inadvertently attempted—and failed—to falsify the proposition that later Acheulian bifaces manifest a truly human intelligence. All the students who failed in their knapping trials to attain late Acheulian skill were the subjects of a great collective experiment. It is perhaps more difficult to propose, in a way that makes it falsifiable, the idea that late Acheulian people made handaxes partly for non-utilitarian reasons. However, that difficulty does not render the idea unscien-tific. There is weight in its favour—that cruder handaxes serve perfectly well for cutting, chopping, scraping or digging; that historic primitive cultures prized analogous large bifaces for ritual-religious and aesthetic reasons; that modern knappers universally find pleasure in the forms of handaxes; that design traditions are evident in Acheulian sequences and are not necessarily determined by limitations of the raw material; and that the handaxe tradition persisted for more

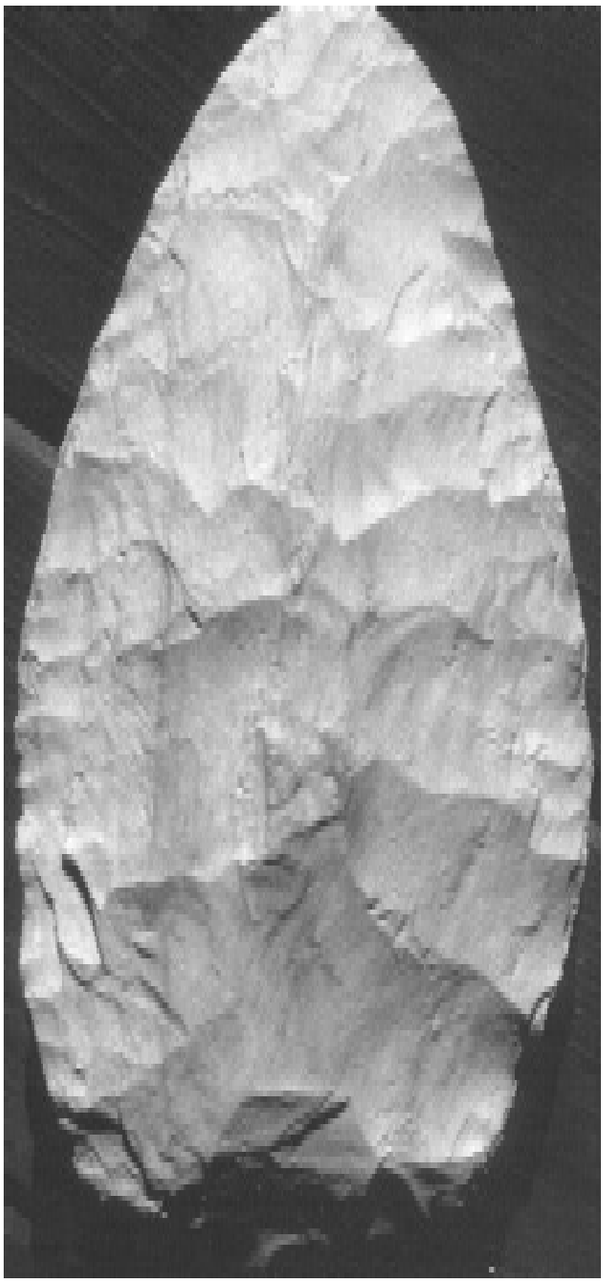

Figure 2. *Photograph of a fine elongate handaxe from the later Acheulian horizon 5 at Kalambo Falls, Zambia, approximately 26.5 cm long. The photograph is of a cast made by the Wenner-Gren Foundation for Desmond Clark and photographed with his permission.*

than a million years and clearly reached a zenith of formal development at the terminus of that period. Clear means of falsification may not be evident now; they may become evident in the future. Meanwhile it is illogical to equate a proposition supported by evidence, subject to scrutiny, open to rejection, and focusing scientific inquiry, with religious belief. On the contrary, it is important to consider such propositions because in combination with other evidence they may allow deeper insights and support new hypotheses that are more clearly falsifiable. Considering and listing such ideas and the possible evidence for them is somewhat akin to 'saving all the pieces' in ecology. You never know what seemingly humble species may turn out to be a keystone element.

Other aesthetic-technological achievements of the Lower and Middle Palaeolithic are relevant to the author's discussion. For example, the tiny 'Nubian'-type Levallois cores of eastern Africa (Clark 1988) are surprisingly fancy. Few modern knappers can replicate them.

A second class of information that might have been treated by the author is the growing evidence for canni-balism (or de-fleshing) and mortuary practices in the Lower and Middle Palaeolithic. Weidenreich (1939, 1943) made the case for *Homo erectus* at Choukoutien. Binford's (1981) attempt to reduce this evidence to the work of hyenas remains unconvincing. More recently, definitive stone-tool cut marks have been found on the Bodo cranium from Ethiopia (White 1986), on hominid bones from the Gran Dolina at Atapuerca in Spain (see White 2001), and on cranial material of hominids at Herto in the Middle Awash (Clark et al. 2003). The Herto remains show signs of mortuary practices involving diverse manipulations of the bones. They are associated with an archaeological assemblage at the transition of Acheulian to Middle Stone Age. While the purposes of cut-marks can be debated, it is important to recognise that the hominid taxa involved have been variously associated with traces of palaeoart discussed by Bednarik. The Bodo people made superb basalt handaxes. While I do not mean to imply that pre-Historic handling of any of these early hominid fossils involved palaeoart, I do think it is worthwhile to bear in mind that there is credible evidence that the makers of palaeoart of diverse kinds were involved with cannibalism (or de-fleshing) and/or mortuary ritual. These activities may shed light on palaeoart, and palaeoart may shed light on them.

The author presents a compelling case that the prevailing model of art origins is Eurocentric, even Perígord-centric, and that evidence from other regions and from earlier times has been missed or dismissed by most palaeoart experts. But his portrayal of researchers as people who need to prop up the prevailing myth of art origins to secure their positions of privilege in academia is unwarranted. By and large these are people who have examined a considerable amount of the evidence and honestly disagree with him, or who have simply not seen all the evidence.

Dr Stephen W. Edwards
Regional Parks Botanic Garden
c/o Tilden Regional Park
Berkeley, California 94708
U.S.A.
E-mail: *sts52b@value.net*
RAR 20-650

Toward a comprehensive paradigm

By JOHN FELIKS

Bednarik offers a revolutionary paper in which he proposes to replace the current Upper Palaeolithic-focused, Euro-centric model of art origins with one of a more up-to-date, scientifically objective nature. He provides insightful perspectives on how political environments in archaeology have effected interpretations of data, and, in particular, that involving the Pleistocene (i.e., how we evaluate the cognition of Neanderthals, *Homo heidelbergensis*, *Homo erectus* and others). The paper begins by first providing an overview of controversial earliest non-utilitarian artefacts etc., as well as some of the less ambiguous early artworks. According to Bednarik, much of this data has been ignored, discouraged from consideration, or otherwise stifled in various ways. This is a significant charge, and one that must be addressed by those in cognitive archaeology if the goal is to genuinely understand our ancient predecessors. I fully concur with Bednarik's proposal to create something more comprehensive than the Eurocentric model and offer support below; however, I have concerns that the new model (in its present form) contains within it certain exclusive elements that should be adjusted before proceeding further.

A new paradigm against previous exclusivities

Among other things, Bednarik proposes to bring an end to the long-established Eurocentric paradigm with all its focus on *Homo sapiens* and the Upper Palaeolithic. It is high time for this change, and I consider his arguments (not least his earlier-developed taphonomic logic applied to palaeoart) well nigh irrefutable. Bednarik also seeks to bring cognitive archaeology up-to-date by adopting the standard methodologies used in other sciences so that it may enjoy similar benefits. It must be noted that Bednarik has sought these changes for many years without commen-surate response, but I think, now, the timing is right, and the tide is about to turn in his favour.

Apart from the above-mentioned changes, which I enthusiastically support, there are a couple of issues that give me reason to pause. For instance, I believe that cognitive archaeology as a 'science' has certain rarely acknowledged special needs and should remain open to all means of knowledge acquisition, because even if it adopts scientific methods with the strictest blind testing etc., results would not be as conclusive as they are for other sciences. We must keep in mind that the other sciences tend to be replete with a never-ending supply of readily-accessible materials and/or living subjects for easy employment of real-time scientific methods. Cognitive archaeology, on the other hand, both needs and benefits from something

more because it depends on extremely sparse physical evidence (Feliks 1998: 129). Hypotheses in cognitive archaeology face another profound barrier to 'proofs', in that our ultimate goal is the understanding of long-dissipated mental processes and behaviours, which are inaccessible. So, unlike other sciences, where results of testing are directly applicable to that being tested, nearly everything we seek to know is accessible only through inference. Even employment of modern neuroscience, while offering substantial gains via 'falsifiable' hypotheses, still provides us with information by inference only (Feliks 2000). But all of this is 'the nature of the beast', and it just calls us to be more innovative. Therefore, I advocate retaining some of the 'old tools' while in the process of adopting modern scientific procedures. By the way, I have proposed useful analogical clinical testing possibilities with chimpanzees etc., in my original thesis (Feliks 1998: 111–12).

In general, count me in as supporting Bednarik's new paradigm but with the several non-exclusivity recom-mendations I will discuss below. In a paper where one is advocating the replacement of an entire long-standing political structure, I do not see the issue of exclusivity as minor or diversionary. If Bednarik views it this way, then I would have to perceive this new system as being not altogether different from that of the system with which he is presently engaging. After all, are not previous exclusivi-ties in large part responsible for inspiring Bednarik's present case? At this opportune time, I must point out that each of these new exclusive elements either ignores or marginalises my own contributions to this field. So, given the context of this paper, I will take the liberty of expounding on these points for inclusion in Bednarik's new system.

Already-made representations: reassessing Acheulian artefacts for iconic recognition

One of the most viable tools in archaeology is the reassessing of supposedly 'already understood' prior evidence. Bednarik, as always, makes excellent use of this tool. New finds such as the recently discovered Tan-Tan 'figurine' (Bednarik 2003), for instance, adds validation to the earlier discovered but not readily accepted Berekhat Ram 'figurine' and demonstrates the value of continuous reassessments in this field. Bednarik's interest in these two artefacts revolves around their 'iconic' nature. Significantly, the case he develops also vindicates prior contributions involving iconic recognition such as my reassessment of the West Tofts handaxe as featuring 'the earliest iconic image [fossil shell] "framed" by a human being' (Feliks 1998: 114–16, 129). But this artefact is missing from Bednarik's review of early palaeoart. The possibly deliberate 'framing' of this iconic image is as significant to cognitive archaeology as the minor engraved lines in the otherwise *naturally-formed* Berekhat Ram and Tan-Tan figurines. These three objects greatly support the case for Acheulian iconic recognition. They also support the 'natural representations theory' of priming the depictive mind by way of 'already-made representations' (Feliks 1998: 110–13). The interpretive history of the West Tofts handaxe is a classic example of Bednarik's central point about how paradigms influence interpretations of the evidence. Prior to my reassessment (inspired by Oakley 1973, 1981; Dissanayake 1988), the fossil inclusion in the West Tofts handaxe had been regarded of interest to its Acheulian maker merely as an 'interesting pattern'. Most academics just assumed or simply did not even consider that Acheulians could have recognised its 'iconic' nature.

Requesting more objective, non-exclusive terms so that the new paradigm is not simply a modernised repeat of the old

One of Bednarik's primary complaints regarding the Eurocentric paradigm is that it rejects any evidence that counters the current dogma, or that interpretations of evidence are made to fall in line with the current belief system. In the process of creating a new paradigm, I would request that Bednarik reduce employment of terms such as those interspersed throughout his paper that already border on predisposition, namely 'non-iconic', and 'non-figurative', because it is not established that the human markings to which he is applying these terms are, indeed, thus (although they very well could be). I have discussed this issue earlier (Feliks 1998, 2000) to no avail. And, just as Bednarik protests against the current system for ignoring certain evidence of Pleistocene cognition, I believe that use of these terms disregards the contributions of those who are making cases for iconic interpretations of geometric motifs. This is not a pedantic detail. Insistent use of these terms is setting up the newly proposed paradigm to be just as exclusive as the one it is intended to replace. Subjective interpretational assumptions, as these certainly are (not unlike those Bednarik claims for the Eurocentric palaeoart specialists), should not stand in so easily for more objective, purely descriptive, or open terms such as 'geometric', 'enigmatic', or 'unidentified'. Objective terms are preferred for the following three-tiered reason: (1) they are inclusive; (2) they keep open both iconic and non-iconic interpretations; (3) they impartially respect the work of researchers who fall into either category.

It is interesting that Bednarik continues to insist on a 'non-iconic' interpretation of nearly all geometric markings even though he comfortably focuses on the Berekhat Ram, Tan-Tan and Makapansgat objects for reason of their apparent iconic qualities. I do not believe he would make such a strong distinction were he not also invested in his 'phosphene theory'. As I had earlier observed with Hodgson (Feliks 2000: 23–4), it is possible that Bednarik believes a 'non-iconic' classification of geometric motifs is necessary in order for the phosphenes interpretation to be valid (i.e. no other possible interpretation should be in close proximity). I do not think it is necessary, or beneficial to our ultimate understanding, as everything in life is simultaneously something else on a different level anyway.

Dual existence: iconic interface between the worlds, Part 2 (a stronger case for both fossils and phosphenes)

Bednarik's 'phosphene theory' (Bednarik 1984, 1987b, 1990b, 1994b, 1995b etc.) and the 'natural representations',

and 'fossil depictions' theories (Feliks 1998) could each benefit from a re-examination of their similarities. (Without getting into nomenclature, I am including here other entoptic phenomena, as well, e.g. as per Lewis-Williams and Dowson [1988: 202]. I have, also, earlier promoted the compatibility of 'fossils' and 'entoptics' [Feliks 1995: 33–6, 1997a: 31–3, 1997b: 27–9, 1998: 120 Footnote].) A starting point is their shared identical geometries, which I have informally called 'dual existence' (Feliks 2000: 24). The basic proposal has been that observation of natural, physical-world geometric imagery (e.g. fossils) *substantiates* (via cryptomnesic memory, conscious reflection etc.) the similar inner-world phosphenes. In other words, by combining these theories, a stronger case is made for explaining the hominid motivation to artistically create geometric images.

It has recently come to my attention that there could be another level to this; Sacks speculates that there may be similar morphogenetic constraints affecting both 'entoptic' forms *and* organic/fossil forms (O. Sacks, pers. comm.; see also Sacks 1992: 289). If similar constraints, or perhaps even similar mechanisms work in each, then phosphenes may help explain the long-time human attraction to fossil collecting—physical evidence of which goes all the way back to the Acheulian—and may also be linked to issues of Acheulian and Mousterian iconic recognition. By extension, the very underlying mechanisms that create specific biological forms in the natural world may also work to internally guide creative human expression of identical forms. Combining the 'fossils' and 'phosphenes' theories could provide fertile ground for testable hypotheses.

The new paradigm should value all reasonable theories, 'testable' and 'untestable'

While I trust in Bednarik's general ability to retain a comprehensive overview, I would ask of all involved at this juncture what I have asked ever since I began my own independent work in this field in 1993; that we continue to recognise the value of multiplicity, the possibility that seemingly contradictory ideas can all be true simultaneously. We should not become too attached to the present focus on neuroscience as though, negating all others, it is now bringing us the correct or final word, for it too will pass over to an even more evolved paradigm. If we learn from the past, we can see that it is illusory to think that current cognitive peaks or methods, no matter how superior they seem at the moment, provide the final word on anything. Bednarik's testability/falsifiability of his 'phosphene theory' will certainly help to solidify the connection between Pleistocene artworks and the hominid brain, and this, in itself, will have profound impact; but *alone*, it cannot prove what the early artists' creative intentions or inspirations were. This is where multiplicity of ideas and input from everyone involved combine to create the most meaningful answers, promoting new 'testable' hypotheses. As I have said in many ways before, the most complete understanding of our ancient predecessors will necessarily involve all reasonable theories, both 'testable' and 'untestable' (Feliks 1998: 110, 129, 2000: 25). For the following reason, I stand by this belief even more now as we prepare to cross over a new threshold into stronger science.

Brainstorming: the value of 'untestable' theories in co-operative efforts

'Untestable' theories are of value if for nothing else than the sheer sake of inspiring others to produce testable theories, or inspiring others to discover ways to make 'untest-able' theories 'testable', or perhaps just revealing inherent 'test-abilities' not focused on by their originators simply because they had other things to do. (Composers could write out all manner of dynamic indications in their compositions, but do they have the time? Prolific artists could frame all their own paintings, but do they?) In cognitive archaeology as in any other aspect of life, those who inspire or appreciate play as valuable a role as those who create. One inspires, another proves the inspiration, or sets out to prove something other than the inspiration. It does not matter. These are co-operative, or ping-pong style interactive efforts. Either way, like in a group brainstorming session where all utterances contribute to the flow of thought resulting in every comprehension imaginable, inspiration is the motivating factor. Science history is replete with originally 'untestable' theories which have changed the course of human understanding. Hence, the 'destructive' (as opposed to critical/constructive) bad-mouthing that sometimes occurs in cognitive archaeology against well-argued theories because they are purportedly 'untestable' has to be thrown to the trash heap along with the more obvious biases condemned by Bednarik, as they discourage meaningful contributions from being heard by a broader audience, and stop up the process of interaction and growth.

Concluding thoughts

For those of strong Eurocentric convictions, I would point out that the changes Bednarik proposes in this paper are not heretical. They just represent the most recent instance of an occasional scientific metamorphosis. Appealing to Kuhn (1962), dynamic new paradigms follow on the heels of previous as a basic means of scientific progress. In my own view, and speaking generically, the new is not 'better' than the former, but is an essential, natural stage of the same organism (child/adult, caterpillar/butterfly). Several academic and independent scholars working in different areas have been priming this change for many years. But someone was needed who could step forward and take the bull by the horns. Who better than one with an immense corpus of work and a diversified background including experience and contributions in and on all levels of the discipline, from theoretical to experimental, from reforming scientific dating methods to challenging the seas in a bamboo raft?! I commend Bednarik for another reason. Although he pursues his own theories, he is, nonetheless, objective enough to consider and publish new work from independent scholars with differing ideas.

When the caterpillar metamorphoses into the butterfly, it does not become an entirely new creature, rather, it is the very same creature, albeit, in a startling different form. Make no mistake; the old beliefs have brought us to this

point, and have served us, as they should. Even problems with diversionary issues, such as an abundance of fake artefacts etc., were useful, in that they have taught us to develop better discernment (one of Bednarik's fortes). Now, we are at the beginning of a new era that calls for a whole new level of objectivity. Bednarik's paper is as good a turning point as could be imagined. It may seem like an abrupt turn to many; however, it offers cognitive archaeology a number of indisputably useful tools and the potential of some very exciting, and, as yet, unanticipated discoveries.

John Feliks
32619 Dover Avenue
GARDEN CITY, MI 48135-1669
U.S.A.
E-mail: *feliks@umich.edu*

RAR 20-651

Was ist Paläo-Kunst?

By LUTZ FIEDLER

Robert Bednariks Artikel über erstes Erscheinen von Kunst im Paläolithikum sollte zur Pflichtlektüre aller studierenden Archäologen werden. Eine vernünftige Revision der bisher dominierenden eurozentrischen Vorstellungen über das Auftreten und die Verbreitung von Kunst ist nach den vorliegenden archäologischen Fakten schon längst notwen-dig. Ich befürchte aber, dass unsere sehr traditionelle und affirmativ operierende Wissenschaft einige Zeit brauchen wird, um sich mit diesem neuen Modell vertraut zu machen und es zu akzeptieren. Es könnte im Gegenzug wahrschein-lich sogar mit Erfolg versucht werden, Bednarik nachzuwei-sen, dass er bei dem einen oder anderen Fundobjekt oder Fundort mit der zeitlichen Zuweisung oder auch der Interpretation einiger nicht-utilitärer Objekt irrt. Aber was würde das an der überzeugenden Fülle der vorgeführten Fakten und der daraus zu ziehenden Konsequenz ändern?

Die Tür zu einem Verständnis nicht figurativer Paläo-Kunst und deren sehr frühem Vorkommen vor allem in Asien aber ebenso in Afrika und Europa ist mit der vorlie-genden Arbeit jetzt endgültig aufgestossen. Doch wenn man den damit geöffneten Weg weiter beschreiten will, wird es sich als notwendig herausstellen, dass zu definieren ist, was Archäologen gegenwärtig unter Kunst verstehen und zukünftig verstehen sollen. Schon jetzt ist doch deutlich, dass die heutige europäisch-westlich geprägte Vorstellung von Kunst linear auf griechisch-hellenistische Ursprünge und vor allem auf Ideen der Renaissance begründet ist. Diese Vorstellungen sind nahezu unreflektiert in die Betrachtungen von paläolithischer Kunst in die Archäologie eingegangen. Weil es aber im Paläolithikum noch kein Kunstverständnis gegeben haben kann, das einem uns historisch vertrautem Kunstsystem aus Asien oder Europa glich, ist es schwierig oder sogar unmöglich zu sagen, ob es im Paläolithikum über-haupt eine Idee von Kunst gab. Wenn es keine urmenschliche Idee von Kunst gab, die unseren heutigen westlichen oder asiatischen Modellen wenigstens annähernd entsprach, dann kann es weder das Ziel, Kunst zu produzieren noch Kunst als Ergebnis gege-ben haben. So ist es wissenschaftlich weder korrekt noch objektiv, unsere zeitgemässe Vorstellung von Kunst in der Diskussion von prähistorischer "Kunst" zu verwenden. Auch die Erwartung, zwischen technisch nützlichen und sogenannten nicht-utilitären Artefakten wissenschaftlich unterscheiden und letztere als Kunst klassifizieren zu können, erweist sich bei näherer Betrachtung als naiv.

Eines der wichtigsten Elemente von Kultur ist, dass alle diesbezüglichen Handlungen und Produkte von Handlun-gen, die von Einzelnen oder Gruppen einer Gemeinschaft erzeugt werden, von ihnen selbst und den übrigen Mitglie-dern der Gemeinschaft verstanden werden. Das trifft sowohl auf solche Handlungen und Produkte zu, denen ein rein technischer oder ökonomischer Zweck unterstellt wird, als auch auf solche, denen unterhaltende, spielerische, gesell-schaftliche, magische, religiöse oder künstlerische Ziele unterstellt werden. In kulturellen Traditionen sind weder praktische Handlungen, noch künstlerische oder religiöse Aktivitäten nicht-utilitär oder frei von einem Ziel, Zweck oder Sinn. Was würden in ferner Zukunft Alien-Archäo-logen zu unseren heutigen Verkehrszeichen, Skiliften, Strandkörben, Bingo-Buden, Modeartikeln, Rosenkränzen, Kruzifixen, Reklametafeln oder Parkanlagen sagen: Kunst oder utilitäre Objekte? Diesen Dingen besitzen gemeinsam und jeweils in unterschiedlichen Anteilen Aspekte des Designs und Stils, des Rituellen, Kultischen und Auffordern-den sowie des Praktisch-Nützlichen. Ein heutiges Automo-bil kann ebenso wie ein altes Kriegskanu der Maori oder eine geschäftete Obsidianklinge aus Neukaledonien oder ein Atlatl aus Mexiko oder eine Narben-Tätowierung aus Kameroun oder eine Höhlenmalerei aus Südfrankreich stilistisch-formale, soziale, ökonomische, logistische und sogar magische Funktionen in sich vereinen. Der Gestalt-gebung wird dabei immer eine grosse Beachtung gegeben. Aber es kann nicht die Rolle der Archäologie sein, darin Kunst oder Nicht-Kunst zu erkennen.

Es ist demnach akzeptabel, wenn Archäologen den Begriff Kunst in Anführungszeichen benutzen, aber besser, wenn sie einfach nur von Darstellungen sprechen würden. Malereien und Plastiken aus dem Jungpaläolithikum Süd-westeuropas sind figurative Darstellungen. Rhythmische oder geometrische Ritzungen auf Stein oder Knochen können als nonfigurative Darstellungen bezeichnet werden, dagegen Feuerstellen, Faustkeile, Speere oder Behausungen als technisch-ökonomische Darstellungen. Selbstverständ-lich kann ein Objekt zugleich – beispielsweise – technische, magische, heraldische oder statusanzeigende Darstellung sein. Aber darum geht es hier nicht, sondern um generelle Darstellungsfähigkeit als Ausgang und Grundlage für frühe "Kunst". Denn mit diesem Begriff wäre es sehr viel einfacher, aber auch korrekter, über erste "Kunst" sowie deren Ursprünge zu diskutieren. Denn das Dargestellte ist das zuvor als realisierbar erkannte Gedachte, oder anders gesagt, es muss als Darstellbares vorstellbar sein. Es ist dabei sowohl abstrakt und neuronal im Kopf gespeichert und kann real in einer oder jeweils beliebig wiederholbaren Ausführung vertreten sein. Man kann sagen, eine Sache der Darstellung besteht sowohl symbolisch als auch wirklich.

Danach ist beispielsweise ein typischer Faustkeil des Acheuléen eine Darstellung des Gedachten; ein realisiertes Symbol (Fiedler 2003).

Wenn prähistorische "Kunst" und gegenwärtige Kunst auf die Fähigkeit zur Darstellung zurückgeht, dann ist aber auch zu fragen, wo die Anfänge dazu tatsächlich liegen. Die Antworten darauf sind nicht mehr allein von der Archäologie oder Kulturanthropologie zu erwarten, sondern von der Ethologie. Denn wie die Anfänge von Kommunika-tion, Technik, Werkzeuggebrauch und traditionellem Sozialverhalten im Tierreich liegen, so liegen auch die Ursprünge von Darstellung und Kunst dort. Die imponie-rende Aufführung eines Chimpansen-Mannes, der äste-rüttelnd und kreischend vor seiner Gruppe erscheint, geschieht in Absicht und hat einen Zweck. Sie ist wirkungs-voll und richtig, wenn sie möglichst perfekt und ausführlich realisiert wird, weil sie dann von den übrigen Mitgliedern der Gruppe so verstanden wird, wie es der ursprünglichen Intention unterlag. Kann man den Menschenaffen dabei eine gewisse Absicht unterstellen, so ist selbstverständlich zu fragen, wo die Grenzen zur nicht bewussten Absicht liegen. Ist das Knurren eines Wolfes, des Scharren eines Büffels im Sand oder das Duft-Markieren eines Katers Absicht? Auf jeden Fall wird die jeweilige Darstellung von anderen Tieren als Mitteilung oder Botschaft verstanden. In dieser gezielten Funktion gibt es keinen Unterschied zu "künstlerischen" Darstellungen bildnishafter, plastischer, musikalischer, szenischer, kulinarischer oder architektoni-scher Art.

Wir haben uns daran zu gewöhnen, dass die angebliche Sonderstellung des Menschen ausserordentlich fragwürdig ist. Biologisch sind wir dem Tierreich verbunden und auch die Wurzeln der Kultur mit all ihren Teilaspekten der Technik, der Kommunikationsfähigkeit, der Traditionsbil-dung und Gruppenidentität, des sozialen Verhaltens sowie der Befähigung zur Darstellung – und damit zur Kunst – liegen dort und nicht im Jungpaläolithikum des europäi-schen Cro-Magnon-Menschen. Die von Robert Bednarik sehr zahlreich aufgeführten archäologischen Belege früher "Kunst" aus dem älteren Paläolithikum helfen uns jetzt, die Komplexität der Kultur des frühen Menschen besser zu verstehen.

Professor Dr Lutz Fiedler
Freiherr-vom-Stein-Str. 10
D-35085 Ebsdorfergrund
Germany

RAR 20-652

Lower Palaeolithic palaeoart, religion and protolanguage

By JAMES B. HARROD

Robert Bednarik has comprehensively marshalled the global evidence for palaeoart and demonstrated that despite great taphonomic loss there is still a large and growing body of art prior to modern *Homo sapiens sapiens*. This art ranges from the Australopithecine Makapansgat many-faced stone through the Oldowan, Acheulian and MSA/Middle Palaeolithic down into later Stone Age cultures across the world. He has convincingly demonstrated that by the later Lower Palaeolithic (around 500 000 BP and thereafter) there was a long-lived and near-global cognitive tradition of geometric marking motifs (cupules, meanders, lattices etc.) as well as stone sculptures, red ochre use, crystals and other exotic objects, and beads and pendants. The view that modern *Homo sapiens sapiens* was the first artist and symbol-maker is no longer tenable.

Archaeologists engaged in Palaeolithic excavations are now challenged not to relegate the world's legacy of palaeoart to the spoils heap of a now out-dated 'short chronology'. All the dusty museum drawers labelled 'atypical tools' might well be re-examined for evidence of palaeoart.

From my own perspective as a philosopher and prehistorian of religion I have relied on Bednarik's inventories and direct examination of objects as a starting point for theory building about the origins of art, religion and language during the Lower Palaeolithic.

With respect to the Oldowan survey, I would add Koobi Fora, FxJj1 #302, c. 1.88 MYA, a chopper core with four alternating flakes removed accidentally yielding an inner cleavage plane in shape of a diamond rhomboid. The object was evidently a curated manuport at the site where it was found. I have proposed an interpretation of the symbolic significance of FxJj1 #302 and the Olduvai FLK North cobble (Harrod 1992) and a reconstruction of Oldowan mimetic protolanguage corresponding to these two artefacts (Harrod 2002a).

While bifaces *per se* are not mentioned as palaeoart, I have proposed hypotheses for the symbolic significance of Acheulian bifaces and stone sculptures (Harrod 2002b, 2002c, 2003). Discussions of palaeoart in the Lower Palaeolithic need to account for characteristics of some bifaces that are not explicable except under a non-utilitarian (i.e., symbolic) interpretation. The recent discovery of an aesthetically beautiful biface placed in the hominid assemblage at Sima de los Huesos, dated more than 350 000 years ago is interpreted as possible evidence for Lower Palaeolithic mortuary rites (Carbonell et al. 2003). This lends further support to arguments for biface symbolism.

I see no reason not to consider in the Lower Palaeolithic and Middle Palaeolithic survey the hundreds of stone sculptures identified by Walther Matthes from sites such as Hamburg-Wittenbergen, Germany (Matthes 1964/1965; compare Hélène 1963). Associates of the Netherlands *Archaeo-logische Berichten* group have similar sculptures from across north-western Europe (e.g. Musch 1987; Beneken-dorff 1990). Their work confirms that of Matthes and other pioneers. I have documented some of this corpus at my website *OriginsNet.org* and I have seen many of these objects first hand. They have strong zoomorphic and anthropo-morphous representational qualities. Most bear evidence of intentional flaking; others may be 'found art' like that of the Makapansgat stone. These objects seem fully worthy of more scientific examination. Not to do so may be a grave loss for our understanding of human evolution.

Bednarik suggests that the Middle Palaeolithic/MSA cognitive tradition of marking signs might best be explained by the phosphene theory. While the sign corpus seems to match up to inventories of basic phosphenes or early childhood drawing motifs, I do not believe the phosphene theory is sufficient in itself to explain why hominid artists used this repertoire of marking motifs. Thus I have proposed that a subset of 'geometric' markings may have had a gestural significance as well as a visual and that they might actually have been 'gesture-movement forms'. The evidence seems to me to indicate that the marking motifs are iconic, rather than non-iconic, and constitute a protolan-guage. In other words, phosphenes were used in art because they were 'good to think'. They could be organised into a conceptual logic and thence into a protolanguage. I have generated a detailed decipherment of a geometric protolanguage that evolved in the Upper Palaeolithic Magdalenian (Harrod 1987a, 1987b). Bednarik's research on Palaeolithic marking motifs offers us rich opportunities for identifying possible symbolic operators and their significance.

I am suggesting that the evidence seems to indicate that 3-D iconic production and geometric marking motifs developed concurrently from the Oldowan and continued through all periods into the present with steadily increasing sophistication. There are many definitional and methodological issues here that are worthy of further discussion as we seek to advance the field of palaeoart studies.

Foremost, in this and other research articles, Robert Bednarik has established the pre-eminent, non-Eurocentric archaeological inventory of the world's palaeoart. He has examined and validated objects and has greatly expanded our knowledge base on art before modern *Homo sapiens sapiens*. He has performed a major service not only for the field of archaeology but also for all disciplines involved in the effort to understand human cultural evolution.

Dr James B. Harrod
22 Shelter Drive
Richmond, ME 043457
U.S.A.
E-mail: *jbharrod@midmaine.com*

RAR 20-653

Primitives in palaeoart and the visual brain: the building-blocks of representation in art and perception

By DEREK HODGSON

Bednarik rightly demonstrates how geometric marks predate representational depiction by a considerable margin. The evidence pointing to the universality and ancient derivation of these marks is compelling but, as emphasised, their significance has generally been neglected.

Although I am in total agreement with the notion that these marks are important epistemologically, it is the underlying reason why they were first created with which I take issue. Perception is held to be crucial to this debate, however, some of the most significant evidence, pertaining to *normal* perceptual correlates, seems not to feature in these discussions. The two main areas of disagreement centre on the purported greater complexity of these marks compared to representational depiction and the significance of phosphene theory as a viable explanation for the appearance of such marks in relation to perception.

The fact that the geometric marks of Lower/Middle Palaeolithic art of a similar caste are found throughout the world and during an enormously long time frame suggests the existence of a common predisposing factor. The very universality of these marks argues against a complex referential system as a feasible explanation as this would predict a considerable variation in motifs. What this does suggest is an evolutionary process whereby the first simple lines incrementally led to more complex geometric forms providing the graphic primitives for the eventual realisation of representational art. The important question to be answered is what were the factors that inspired hominids to produce such lines in the first instance.

Elsewhere (Hodgson 2000a, 2000b) I have argued that, as a consequence of the atypical stimulation of the early centres of the visual brain, phosphene experience may be elicited. But it is not the phosphene experience itself that acted as a template that was simply copied (Bednarik 1990b) or provided the originating stimulus for the realisa-tion of repetitive geometric forms (Lewis Williams and Dobson 1988), rather, phosphenes supply one of several clues as to how the visual system might function as part of the process of assembling the visual image itself (Ffytche and Howard 1999). In other words, the explicit representational image is composed of primitives to which we normally remain oblivious because of the process of 'chunking' as larger portions of this information are transferred to higher centres of the conscious brain. Most of the work in attaining this conscious image is carried out pre-consciously, but this does not mean that the effects of the lower- order mechanisms are not felt in one way or another. Many psychologists and neuroscientists have stressed the importance of primitives (akin to those found in palaeoart) as necessary building blocks to the realisation of the representational image (Marr 1981; Gibson 1979: 276; Treisman 1986; Ramachandran and Hirstein 1999; Gleitman et al. 1999: 227; Pinker 1998). It seems that these simple features have special status in the perception of form.

Recent work has shown that the early visual brain can process primitives even though the higher centres (of the conscious brain) do not register their presence (He and Macleod 2001) —yet a certain unspecified sense of arousal may still be sensed (Crick and Koch 1995: 122). This is because the earlier part of the visual cortex has a raised sensitivity to the presence of such lines. Zeki (1999: 201) has noted that these areas are more active when individuals view purely geometric forms (such as in Mondrian-like paintings involving straight lines and rectangles) than representational forms. This may explain the 'oblique effect' where it has been demonstrated that the primary visual

cortex (V1) tends to react more strongly to straight and horizontal lines as opposed to oblique ones (Furmanski and Engel 2000). Because of this individuals tend to show a greater behavioural sensitivity to horizontal and vertical lines than oblique orientations. At slightly later stages of the visual cortex (V2 and V3) this effect disappears as these areas respond equally well to both oblique and horizontal/vertical orientations.

The appeal of geometric forms to palaeoartists may reside in the fact that they disproportionately stimulate the early visual brain leading to an undefined sense of arousal. Feliks' (1998) notion that fossils may have been significant in this respect can be explained by this dynamic, as the repetitive geometry of these items will have served as an attractor because they will have similarly stimulated the early visual centres. In fact, wherever such repetitive geometric forms were to be found in the natural environment hominids would have been drawn to their intrinsic quality, e.g. concentric circles as ripples in water, rainbows, crystals, honeycombs, rock strata etc.

A more likely explanation for the appearance of these motifs, however, is to be found in the fact that in the de-fleshing of bone and making of tools scratch marks of various persuasions will have been produced. Some of these will have accidentally assumed the configuration of a regular pattern and therefore became significant in the way described. This scenario is more probable because it is proactive rather than simply passive in that the implement used to produce scratch marks will have been conveniently at hand, so that a repetition of the initial procedure could easily have been enacted.

These observations illustrate how geometric lines may have been derived from determinants where the 'meaning' did not centre directly on conscious awareness. Such marks may later have come to be consciously-mediated, as is typical of repetitive decorative motifs, but initially the underlying determinant would have resided in the early visual cortex's raised sensitivity to co-ordinated lines. The beauty of this approach is that it can explain both the universality and persistence of graphic primitives, while at the same time allowing for the eventual projection of cultural meaning when geometric forms became a more diversified phenomenon. In fact, this theory predicted that any palaeoart that came to prominence in the archaeological record that predated the earliest representational art would consist of the same or similar geometric forms. This prediction has recently been verified by the confirmation of the dates of the Blombos artefacts (Henshilwood et al. 2002), which evince the same kinds of patterns as found elsewhere in the Lower/Middle Palaeolithic. Personal ornaments of Neanderthals inscribed with regular spaced notches (d'Errico et al 1998) add credence to this model.

In conclusion, I am in agreement with the general view that early mark-making needs to be taken more seriously within a global context as an important index of early human cognitive ability. However, I continue to have reservations with regard to existing accounts as to precise derivation that do not take into account the means by which the visual cortex *normally* processes visual information.

Derek Hodgson
2 Belle Vue Street
York
North Yorks YO10 5AY
England
E-mail: *dhgson@email.com*

RAR 20-654

A paradigmatic shift

By GIRIRAJ KUMAR

'The earliest evidence of palaeoart', a paper by Robert G. Bednarik, marks a paradigm shift in rock art research from the European model of origins of art to the evolutionary model of the origins of art and its development. Originally there was only the European model for rock art research, and understanding of art origins was sought in the European Upper Palaeolithic art. It places naturalistic animal forms of rock paintings in the European caves at its beginning. This model was popularised by academics and influential institutions. Scholars started looking for the same kind of evidence in their countries and began classifying rock art more or less as this influential model demanded.

As the intensity of rock art research increased, systematic work in this field developed gradually. Evidence of human workmanship and early man's fascination for natural objects with appealing forms and features, from the Lower, Middle and Upper Palaeolithic periods, began appearing throughout much of the world. Interestingly, unlike the European model predicts, most of these finds were non-iconic in form. There were also objects modified only little by humans to enhance their natural form. They precede the European Upper Palaeolithic in age and show gradual evolutionary patterns.

Bednarik has done hard work in assembling this evidence. Most of what he cites is either little known or not known altogether by many scholars. He has also evaluated already reported evidence from the Lower Palaeolithic, particularly from Africa and Asia. This had been neglected by scholars as it did not fit in the European model of art origins. Thus, this European model has been an important factor hindering the scientific progress of rock art research for a long time.

Recently non-iconic forms of rock art (cupules and other forms of petroglyphs) have been discovered at Bhimbetka (Bednarik 1993f) and Raisen (Bednarik et al. 1991) near Bhopal in Madhya Pradesh, Daraki-Chattan in Chambal valley (Kumar 1996) and Bajanibhat in Alwar (Kumar and Sharma 1995) and Ajmer (Kumar 1998) districts of Rajasthan in the Aravalli Hills. They are archaic in character and the majority of them present no recognisable pattern of any kind. Petroglyphs at Bhimbetka were found in the excavation of Lower Palaeolithic sediments (Bednarik 1994c). In the excavation at Daraki-Chattan, hammer stones used for cupule production were obtained right on the bedrock at the

level where the sediment contained more Lower Palaeolithic and a few Middle Palaeolithic-tradition artefacts on quartzite (Kumar et al. 2003). Moreover, cupule-bearing slabs have also been found from the mid-levels of the cave sediments, which yielded stone artefacts representing a transitional phase from the Lower and Middle Palaeolithic (Kumar 2002). This evidence of non-iconic forms of early Palaeolithic art from India is in tune with the global pattern. Its antiquity is being tested scientifically by an Indo-Australian Commission through the EIP Project, a joint venture by the Rock Art Society of India and the Australian Rock Art Research Association (Bedna-rik 2001d, 2001; Kumar 2002; Kumar et al. 2003).

The excavations at Bhimbetka (Wakankar 1975: 14–16) and now at Daraki-Chattan (Kumar et al. 2003) have also provided evidence for an indigenous evolution of Palaeolithic and Mesolithic industries. This continuity appears to be reflected in the artistic manifestations of the humans. Hence, in order to accommodate these recent evidences an evolutionary model of the origin and development of rock art has been developed (Kumar 2000/01). This model is flexible and capable of accommodating not only the emerging new evidence of the early human creations, it can also cater for that expected in the future. More important is that it can be tested—it can be refuted.

On the basis of available evidence Bednarik puts forth a hypothesis that southern Asia in general and the Indian subcontinent in particular, rather than western Europe, can be assumed to have been a main theatre of the interaction and evolutionary dynamics. It is a logical hypothesis. To test this hypothesis, let us wait for more data and evidence by systematic research in different parts of the world, particularly in Africa, Asia and Australia. At present we have very limited data to fill the evolutionary gaps, which is obviously as the taphonomic principle predicts. Lack of systematic work is another reason. Whatever the available evidence of early Palaeolithic art globally at present, is not due to systematic research. Most of it was found by chance. The EIP Project is a first serious attempt in this direction. Future research will also decide the place of the Upper Palaeolithic naturalistic animal figures in the European caves and such evidence elsewhere in the evolution of human visual creations.

Let us develop a mechanism for sharing the latest information and experiences gained in this field quickly and effectively. In this respect the review of the available evidences of the early Palaeolithic art of the world by Bednarik is laudable. Let us not be biased in our views of human abilities but have an open mind. Future research can bring unbelievable wonders, such as the cupules on pure quartz, a very hard rock discovered on the outcrops of quartz rock in the Aravalli Hills valley at Ajmer in Rajasthan, India (Kumar and Bednarik 2002:).

Bednarik rightly suggests that belief manifestations are to be replaced by processes of falsification, blind testing and other scientific procedures.

Dr Giriraj Kumar
Faculty of Arts
Dayalbagh Educational Institute
Dayalbagh, Agra 282 005
India
E-mail: *giririajrasi@yahoo.com*

RAR 20-655

Eurocentrism and postcolonialism

By TALIA SHAY

Bednarik‘s article on the origin of palaeoart reminds me of a poetic book by Cuddihy (1974, 1987) who claims that the revolutionary ideas of Freud, Marx and other Jewish intellectuals, at the beginning of the 20th century derived from their personal struggle to conform to Protestant code of behaviour. Similarly, one can sympathise with Bednarik who lives and works outside Europe, and is sensitive toward Eurocentrism in rock art research. The author boldly claims that the predominant model that associates the origin of art with zoomorphic, figurative traditions of south-western European rock art dating to the Upper Palaeolithic period is one-sided. Bednarik’s disapproval of the above paradigm is based on both modern theories in archaeology, as well as on the existence of much earlier evidence found outside south-western Europe. In order to reinterpret the archaeological evidence, Bednarik firstly defines palaeoart as non-utilitarian activities including bead making, use of pigment and the like. Secondly, he applies modern archaeological theories, which imply alternative ways of writing about the past. These theories, which are implicitly applied in the article (e.g. Bhaba 1994; Hodder 2002; Wylie 2002), should, in my opinion, be explicitly indicated as they reflect both upon knowledge-producing enterprise in the discipline, as well as upon the social and ideological entanglements of archaeological inquiry. For example, the so-called postcolonial theory in archaeology that questions the monolithic definition of culture, which traditionally operates with a set of Eurocentric concepts at its core, could corroborate one of Bednarik’s hypotheses (see below). Based generally, therefore, on the above notions, Bednarik offers two hypotheses to be tested. The first refers to artistic ‘styles’ and proposes that traditions characterised by geometric and abstract designs are culturally more complex than those representing naturalistic, figurative forms. The second proposal suggests that the traditional paradigm associating the origin of art with south-western Europe is based on biased research. In order to test his hypotheses the author introduces a bulk of new data that date from the Lower Palaeolithic and on, and originated from outside south-western Europe. This new evidence, unlike the material from European cave art, is mainly non-figurative with complex geometric or abstract designs. Discussing these data Bednarik concludes that art, in the sense of symbolic activities, had first appeared in the Lower Palaeolithic, perhaps in Asia. Later these artistic traditions developed into geometric and abstract forms, which could communicate an unlimited number of

ideas. It is generally agreed, says Bednarik, that abstract style is more sophisticated than direct visual representations, such as the figurative European cave art. Although the author introduces ample data and defends his latter argument convincingly, one should also note that there are other cases (dated to much later periods) that have led scholars to the opposite conclusion about the development of artistic forms (Shay 2001). Bednarik's final conclusion is that the traditional archaeological paradigm ignores these ample data and prefers to stick to the mythology of European origin of art for both ideological and personal reasons. On the basis of the above, Bednarik claims that his two hypotheses concerning styles and Eurocentrism in archaeological research are validated.

Dr Talia Shay
Department of Sociology
The College of Judea and Samaria
Ariel 44837
Israel
E-mail: *talia_shay@yahoo.com*

RAR 20-656

Crusade! Comments on R. G. Bednarik's 'The earliest evidence of palaeoart'

By LAWRENCE GUY STRAUS

How do I craft comments on this both usefully informative *and* highly polemical article without becoming embroiled in controversy or without becoming identified with one camp or the other? Carefully and selectively, but honestly, as I see the issue—which is cloudy and equivocal.

Robert Bednarik has done it again and the profession of prehistoric archaeologists does owe him a debt for once more forcing us to look at those examples of pre-Upper Palaeolithic and early, extra-European 'art-like' manifestations that 'refuse to go away'. Yet some of his assertions (at times bordering on diatribe, no doubt motivated by his perceived 'outsider' status) can be infuriating (e.g. apparent continued rejection of the Upper Palaeolithic age of the Côa valley open-air petroglyphs; insinuation of a Holocene age for *all* the Lascaux Cave paintings). This article—for me at least—is a potpourri of reasonable and usefully provocative material set in the language of scientific positivism, but mixed with bold assertions often bolstered by some pretty problematical evidence (and that evidence indeed frequently couched in the conditional tense), but nonetheless used in the tactic of 'piling-on' to build the case for 'palaeo-art'.

I must admit that before I actually saw the rhythmically engraved Bilzingsleben elephant bone, I was generally sceptical of the existence of pre-Upper Palaeolithic 'art' (i.e. non-utilitarian modifications of natural objects). It was—as Bednarik so rightly points out—so easy to conveniently 'forget about' the La Ferrassie burial slab with pecked cupules on its underside, since it came from an old excavation and was so apparently 'isolated', or to dismiss Dr Henri Martin's worked bones from La Quina again because the excavations happened early in the 20th century. But now we have several (indeed, many) cases of 'palaeo-art'—some utterly spectacular and from apparently good archaeological contexts (e.g. Quneitra, Berekhat Ram, Tan-Tan, Blombos)—along with other indications of ancient conceptual sophistication (see McBrearty and Brooks [2000] for an excellent synthesis of the pre-LSA African evidence).

Bednarik's is a rather convoluted, sometimes disjointed article, and the leap into discussion of the African 'Eve' hypothesis is handled rather abruptly. The reader should be led into his reasoning here more thoroughly. I personally feel that he dilutes his case by including so many problematic and/or poorly dated cases, and I wonder why he includes American cases at all in an article on '*palaeo*'-art, since even a 15 000 or 25 000 BP colonisation of the New World would hardly qualify as being very old—certainly more recent than Chauvet Cave. In his zeal to dethrone Europe as the centre of the early art world, Bednarik is moved to state that 'all of the Pleistocene rock art of Australia should be regarded as essentially Middle Palaeolithic', yet in his substantive discussion of Australia he (probably rightly) rejects 'old' dates both for rock art and for human colonisation of the continent, only giving pitifully slim evidence for some petroglyphs predating 28 000 BP. It is not at all clear how he then decides that it is all 'Middle Palaeolithic'.

All this evidence does (at least in my opinion, as in Bednarik's) call into question the idea that 'culturally superior' modern humans evolved in only one place and swept out of Africa replacing other (presumably 'culturally challenged') hominins in Eurasia. The main point should be that cognitive and cultural evolution had many aspects of *continuity* over long spans of time, albeit perhaps with spurts of accelerated change—not all of which were interregionally simultaneous by any means. Surely the transition to 'modernity' was a much more complex, mosaic process than a punctuation event characterised by total replacement.

Bednarik's other main point is that the famous Upper Palaeolithic art is a 'fluke' of taphonomic survival. The point is well taken, but why then does he insist on continuing to cast doubt on open-air rock art manifestations that occur in precisely one of the areas that saw the least amount of Ice Age temperature and humidity fluctuation in Europe (i.e. southern Iberia)? Upper Palaeolithic open-air rock art may not originally have been an isolated phenomenon, but simply has been preserved in limited, favourable circumstances. Likewise, I find it interesting that Bednarik wants to deny the remnant instances of cave art in central Europe, where the chances of survival are far dicier than in the SW European glacial refugium. (Recently, calcite-sealed parietal engravings of Upper Palaeolithic style also have been reported from Church Hole of Creswell Crags in central England [Bahn et al. 2003].) Is Bednarik trying to have his cake and eat it too concerning taphonomic explanations for presence and absence of rock art?

Other contradictions include Bednarik's rejection of rock varnish chronometric methods, while excoriating J.

Zilhão for his earlier rejection of such 'dating' methods in the case of Côa and for his reasoned defence of traditional stylistic dating and the comparative method. Finally, it is clear that Bednarik wants to be seen as a *scientist*, by actually rejecting several claims for 'palaeo-art', yet many of the cases that he suggests to be very old do not in fact seem to be very well supported, so his use thereof (in the tactic of 'piling on', as mentioned above) seems a bit questionable.

From my point of view, the accumulating number of 'good' cases is sufficient to make Bednarik's main points. Why not leave aside the highly problematic ones? Early humans (and maybe even '*pre-humans*', *if* one chooses to make something of the Makapansgat pebble) were perceiving and expressing things in ways that—although statistically rare—are quite in line with modern human notions of creativity and artistic perception and activity, including Leslie White's 'symbolling'

There are two possible—and not mutually exclu-sive—reasons for the apparent rarity of such behaviour prior to the late Upper Pleistocene: the taphonomic one laid out by Bednarik (with an aura of self-fulfilling prophecy in terms of his argument) and the notion of habitualness. 'Modernity', as observed above, from my perspective, is an on-going process, and part of it (accelerating on a regionally irregular, uneven basis around the beginning of the LSA/UP) involved frequency distribution shifts in the conduct of certain innovative behaviours. The interesting question, of course, is 'why?'—population pressures, genetic mutations, environmental stresses, some combination of the above? At any rate, 'artistic activity' became increasingly routinised with time—but it is still not a universal among all humans. Nor was it so in the Upper Palaeolithic even. Can the isolation of the SW German (in essentially the adjacent Ach and Lone valleys) Aurignacian ivory figurines or of the (generally accepted to be) similarly dated paintings of Chauvet Cave in south-central France be seen as only due to taphonomic accidents? And were they really invented *de novo*, with no regional antecedents? Time may tell as research develops and as new discoveries are made, but it remains possible that some humans at the same or different times and in separate places 'did' art, while others did not. Even in our own society, many may 'appreciate' art, but few produce it. (Note that recent, exhaustive AMS dating work by J. Fortea [2002] at Peña de Candamo throws the same kind of doubt on this method for age determination of charcoal cave drawings as has been suggested by C. Zuechner [1996] in his polemical review of the dating of Chauvet [see also Pettitt and Bahn 2003]. Again, Bednarik is critical of some things, but not of others. Can he too be guilty of something approaching casuistry?)

As an *agent provocateur* and advocate for difficult causes, Bednarik plays a valuable role. The question that the reader needs to sort out is whether and when he has 'gone over the top'... So I am thankful to him for letting me express my opinions, even if they may not always coincide with his. Despite the bombast in this article, Bednarik—like Alexander Marshack before him—has done some serious work that hopefully will push the discipline to look beyond SW Europe and before 32 000 BP, in a more open-minded search for understanding the *process* of and causes for the development of human creative expression and symbolism. In this way, we should hopefully overcome the kinds of almost supernatural explanations that are sometimes offered for the sudden, recent and unprecedented appearance of 'art'. And we should recognise it as a common heritage of hominins on all the different inhabited continents, even if it was (and is) never a universally active attribute of all individuals.

Professor Lawrence Guy Straus
Department of Anthropology
University of New Mexico
Albuquerque, NM 87131
U.S.A.
E-mail: *lstraus@unm.edu*

RAR 20-657

The constraint of minimum necessary competence

By THOMAS WYNN

In general I am sympathetic with Bednarik's criticism of the Eurocentric bias in studies of palaeoart. Most of Palaeo-lithic archaeology still operates within a techno-cultural taxonomy that was initially developed in Europe over a century ago during the heyday of unilineal evolutionism. One of the most insidious assumptions of this traditional system has been the expectation of progressive change, the idea that culture, including art, must become more and more complex and sophisticated over time. Initially at least, more complex meant more like modern Europe. To be sure, there have been serious attempts to break free of the hold this scheme has had on our understanding of the past—Dibble's critique of Middle Palaeolithic stone tool typology comes immediately to mind (Dibble 1987) —but Palaeolithic archaeology has yet to free itself entirely. The field is still in need of serious rethinking.

Bednarik takes on this bias by proposing an alternative interpretation of palaeoart. One of his suggestions is that regular geometric designs are in fact cognitively more complex than two-dimensional depictions, and that Franco-Cantabrian parietal art should therefore not be seen as the acme of Palaeolithic culture. As an advocate of the cognitive approach to the Palaeolithic record, I am most concerned with the cognitive component of Bednarik's argument. What cognitive abilities are in fact required for production of a regular geometric pattern of markings on a two-dimensional surface? On a simple, descriptive level, the engraver needed the appropriate precision in hand-eye co-ordination, and a set of spatial concepts to control the placing of markings. These latter include (based on various Palaeolithic artefacts) the ability to maintain parallels, produce equivalent angles, and reverse patterns to produce

symmetries. At a cognitive level, that is at the level of definable, neural-linked abilities, these spatial concepts can be broken down into a number of more specific abilities. One is the ability to imagine and co-ordinate several visual perspectives at once, something psychologists term 'allocentric perception'. Even something as seemingly simple as a set of parallel lines requires the engraver to break free of his or her ego-centred view point and construct a more artificial perspective (Piaget and Inhelder 1967). A second requirement is some notion of spatial quantity (length, for example). A third is the ability to maintain shape 'under motion', which means to hold shape and size constant as one manipulates the various perspectives. The evolutionary psychologist Irwin Silverman has dubbed this ability 'space constancy', and considers it one of the hallmarks of human spatial thinking (Silverman et al. 2000). Deploying these spatial abilities, a pre-Historic engraver (or painter) could produce the whole range of complex two-dimensional patterns including checkerboards, grids, parallel lines and so forth. Modern humans all possess 'space constancy', though there is a good deal of variability in performance, some of it tied to sex differences. When do these abilities appear in the evolutionary record? Relatively early, as it turns out, and the earliest reliable evidence comes from stone tools. Some time around 500 000 years ago stone knappers produced bifaces with three-dimensional symmetry that required co-ordinated perspective, measured space and conservation of shape under motion (Wynn 2002). The spatial cognitive prerequisites for producing regular geometric markings were in place by the late Acheulian.

This of course leaves unanswered the question of purpose, which may also carry cognitive implications, and which is the crucial piece in Bednarik's argument for complexity. Why would these hominids engrave patterns? If these patterns were arbitrary codes that had explicit referents in a system of meaning, then Bednarik's assertion that they were more sophisticated than two-dimensional depictions has credibility. Iconicity is, from a semiotic perspective, less sophisticated than arbitrary reference. But how can we recognise arbitrary reference without a detailed archaeological context that is beyond the hopes of Palaeolithic archaeologists, a taphonomic fact of which Bednarik is well aware? The answer, unfortunately, is that we rarely can. One of the methodological constraints of cognitive archaeology is the problem of minimum necessary competence (Wynn 1989). We can only assign the minimum abilities required to produce a pattern in question, and hence always risk underestimating ability. If we have no contextual evidence that the marks had a referent, we cannot assume that they did. For most of the objects under consideration by Bednarik, it is simply not necessary to conclude that the markings stand for anything, and if they were not abstract symbols, then this part of Bednarik's argument loses most of its power. Indeed, if we follow Davidson and Noble (Davidson and Noble 1989; Noble and Davidson 1996), we would have to conclude that the use of depiction in parietal art and carved figurines required a cognitive component not required by the geometric marking, that of reference itself. Iconic images do clearly stand for something.

In support of Bednarik, it is also true that some of the marked objects in the Palaeolithic record were clearly referential, but all of these are very late. Marshack (1972, 1985, 1991) and d'Errico (1995, 2001; d'Errico and Cacho 1994) have identified a number of objects, usually bone plaques, that were marked sequentially by different engraving tools, at different times. This internal context indicates that these plaques were almost certainly devices for keeping track of something. Such notational (d'Errico's term) devices require not just spatial concepts, but also the referential ability to consider marks as equivalent to other individual things. In addition, use of devices almost certainly requires enhanced 'working memory' and algorithmic thinking (Wynn and Coolidge 2003), two hallmarks of modern human thinking. These marked objects, then, do require cognitive abilities not required for the production of depictions, and are the most cognitively sophisticated productions of their times. But these times were very late; the earliest such tally device is only about 30 000 years old (Marshack 1985). Earlier objects such as engraved bone from Blombos do not qualify because the marks are not clearly sequential, and not produced by different tools in different episodes. The minimum necessary competence is the same as that attributable to late Acheulian stone knappers, and is therefore not a surprise.

Professor Thomas Wynn
Anthropology Department
P.O. Box 7150
University of Colorado at Colorado Springs
Colorado Springs, CO 80933-7150
U.S.A.
E-mail: *twynn@uccs.edu*

RAR 20-658

REPLY

Crusade for science

By ROBERT G. BEDNARIK

I thank all commentators most cordially for their truly thoughtful and generous consideration of the data and ideas I have put forward. Bearing in mind that my interpretations of these findings remain controversial, it is surprising that there is only limited dissent apparent in this discussion. Comments on my paper have been invited from several scholars long noted for their strong opposition to the 'long-range' model of cognitive evolution, but they have regretfully chosen not to defend their long-held views. It seems that the 'gradualist' model, which during the mid-1990s had been all but snuffed out, is at last having a real impact on the diametrically opposed position. In the present debate only one commentator, Professor Straus, expresses significant opposition to what I have presented, which I appreciate particularly.

The pendulum's motion

Two of the commentators, Chakravarty and Feliks, express mild concern that, in order to arrive at a more balanced view, we need to ensure that the pendulum (cf. Straus 1995) does not swing too far in favour of what currently remains a minority view. I share this concern wholeheartedly, but I believe that the pendulum has yet to approach its 'right place'. The most virulent forms of the 'short-range' model may be all but defeated (for instance, by the Lower Palaeolithic seafaring evidence), but make no mistake, the more moderate replacement dogma of mainstream archaeology still reigns supremely in world archaeology. Until its ascendancy in Pleistocene archaeology is soundly rebutted, this inherently political and religious program needs to be opposed. I recall many examples of revisionist archaeology where a lack of such vigilance allowed the painstaking work of early scholars to be quite unfairly denounced by pretentious but inadequately informed commenters. Examples are the Neanderthal interments at La Ferrassie or the mostly central European evidence of what has been explained as a 'cave bear cult' of the major Würm Interstadial. The reasons for the success of such detractors need to be seen in terms of a nexus with historical currents. For instance, during the 1960s the Neanderthals became Pleistocene flower children, because of some questionable evidence from Shanidar, but during the 'cynical' 1980s their status reverted to that of savages, a trend culminating in Davidson's (Davidson and Noble 1990) pronouncement that they belong to the apes rather than to humans. One may well argue that diversity of opinions is important in a discipline, but this becomes a deceptive if not insidious argument when it is used to justify hypotheses that were proposed purely because their proponents were inadequately informed at the time. I argue that, once their strongly stated views have become effectively untenable, such scholars have an obligation to admit so publicly. I see no evidence that this stage has been reached in the present confrontation, and I believe that any slackening in the resolve of the 'long range camp' would result in an immediate resurgence of the kinds of excesses we have seen in the late 1980s and throughout the 1990s.

One of the many reasons why the orthodox and still dominant model has become an absurdity is well demonstrated by Brumm, who notes that it cannot effectively account for modern behaviour outside Africa before the Big Bang its adherents place at under 50 000 years ago. This red herring, which remains inadequately analysed, came into existence when people confused two unconnected issues. One was the assumed arrival in south-western Europe of 'moderns' from Africa, the other was their perceived emergence in Africa. The two matters are historically separated by at least a hundred millennia, but that did not prevent the development of simplistic notions about their connection.

I am particularly grateful to Bowyer for recognising and emphasising the fundamental importance of taphono-my. Moreover, his formulation of consciousness as 'the mind's way of creating a world from sensations' is truly central to the entire study of palaeoart. Bowyer is most certainly right that a much broader research effort is needed to address this and other fundamental questions, and he seems to concur that such effort needs to be framed very differently from the directions we have so far witnessed. I am surprised that he, as a scientist, suggests that iconic figures are easier to categorise than many of the 'abstract signs'. How would he propose to test (try to refute) these categories? They are constructs of our perception, and no effective process of testing them has been offered. On the other hand, if 'signs' are indeed semiotic, as appears to be the case, they must comply with some form of taxonomy, otherwise they cannot be effective as signs. There is no proof that animal figures necessarily need to be symbolic beyond their iconicity, even though they can of course be so. Some commentators have difficulties with accepting the idea that non-iconic motifs are cognitively more complex than zoomorphs. This may be attributable to cultural conditioning. Certainly, pongids have no great difficulty in recognising graphic imagery (to the point of masturbating while viewing a picture of a nude human), whereas they have not been reported to master the symbolism of non-iconic graphic symbols without extensive tutoring.

Handaxes and palaeoart

Edwards and Harrod raise the old issue of handaxes as palaeoart objects, or as evidence at least of non-utilitarian practice. There has been a great deal of discussion about this over many years, the most frequently cited argument being that many Late Acheulian bifaces are of perfection well beyond mere functionality. While these are persuasive arguments, in a scientific sense they are essentially anecdotic. Edwards comes close to proposing a scientific hypothesis for the positioning of the oft-cited West Tofts handaxe 'around' the fossil cast it contains, when he notes that such a feature 'will hamper thinning the biface'. This would be worth formulating and presenting as a refutable proposition—not because of the West Tofts specimen, but because the greatly protruding fossil echinoid in the Swanscombe example seems to almost negate the functionality of that piece. The most similarly 'misshapen' handaxe I have examined is from Bajanibhat, India, and I must admit that here the stepped lower half does not protrude quite as much as on the Swanscombe specimen (Fig. 1).

The only scientific hypothesis of which I am aware concerning the West Tofts object, or indeed the entire issue, is that presented by Feliks (1998: 114–6) in this very journal. He tested the centrality and symmetry of the West Tofts specimen's *Spondylus spinosus* cast by geometric means that lend themselves to refutation. His finding that the positioning is indeed significant and intentional is based on transparent data open to testing, and until someone presents falsifying data or proposes a more parsimonious hypothesis to account for Feliks' data, his hypotheses stands as the most likely explanation. Those wishing to promote the non-utilitarian aspects of other stone artefacts might profit from examining how Feliks approached the issue —not necessarily to copy his methodology, but to copy his philosophical basis.

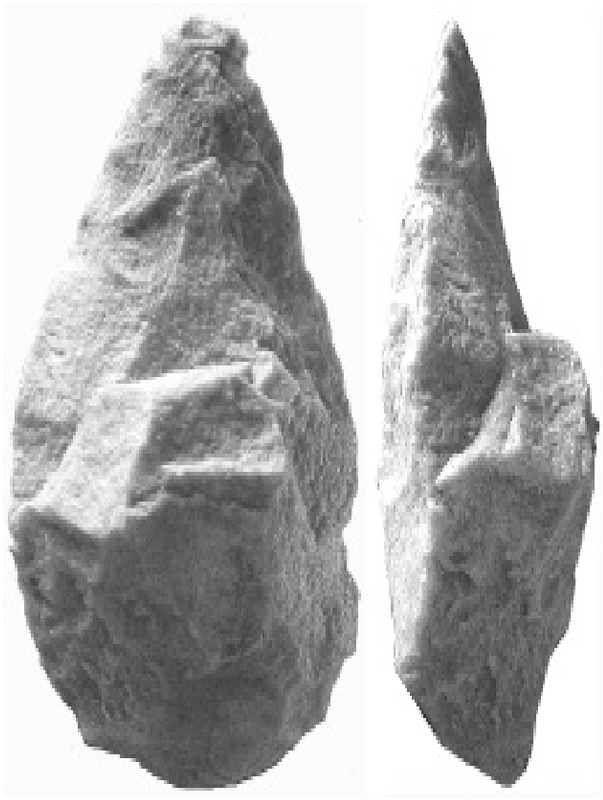

Figure 1. *Acheulian handaxe with greatly protruding aspect on one face, Bajanibhat, Rajasthan.*

This may sound a little over-rigorous, but in view of our predilection for detecting evidence of intentionality it is fully warranted. We see that also in dealing with the 'hundreds of stone sculptures' of early traditions, mentioned by Harrod, some of which have been reported long before Matthes' work (e.g. 1969). Indeed, such reports extend all the way back to Boucher de Perthes (1846) and Dharwent (1902), and recent presenters of such material include U. Benekendorff, J. E. Musch, R. Williams, J. C. van Es and others. Harrod argues that these objects are worthy of scientific examination, and he is undeniably right. The practice of mainstream archaeology, of studiously ignoring issues it finds too hard to deal with, is not conducive to better understanding. On the other hand it is also true that claims concerning this diverse material have not been presented in a scientific format until now. That would involve a process of formulating falsifiable propositions about specific aspects of such finds, and then subjecting them to appropriate testing through refutation. Until recently, all these finds were easy to reject, but the recent developments (prompted by the specimens from Berekhat Ram, Tan-Tan, Erfoud, the re-examination of the Makapansgat cobble, and the wider issues of palaeoart and maritime colonisation in the Early and Middle Pleistocene) render it essential that this matter be investigated scientifically. By themselves, these developments do not 'prove' the relevance of the mostly north-western European corpus. Each and every specimen with a secure archaeological provenience will have to be examined on its own merits, and an archaeology that avoids doing this on principle is of no help in resolving the matter.

The issue of the well-made handaxes boils down to the argument that they indicate 'aesthetic' appreciation. Perhaps they do, but in the way the argument is being presented it is not testable, it could be applied to any utilitarian object that is well made—spears, ceramic pots, motorcars. The issue is addressed in Fiedler's sophisticated discussion of the arbitrary separation of art and non-art. It would have been an act of cultural vandalism to attempt a translation of his magnificent German. However, I need to summarise his position to respond to it here. He begins by demanding that my paper be made required reading for all studying archaeologists, but then concedes that the discipline would take a long time to digest what I say. What I fear more, however, is the watering-down of my demands for scientific culpability of the discipline. Like the Roman Catholic Church, the discipline does know when it needs to bend a little, and in recent years has begun to do so already on these issues. It will, I predict, progressively adopt some diluted version of what I say, but it will not effectively confront the challenge of taphonomic logic. Young and innovative thinkers in the field are the most likely to adopt this kind of approach, but as long as the views of mainstream archaeology are determined democratically, i.e. by consensus, peripheral adjustments and concessions will be of little value. Archaeology has never undergone a paradigmatic shift, and I think if it is to do so, the impetus will have to come from outside the academic discipline, from the sciences. Archaeology lacks the capacity of fundamental or sweeping reform from within.

The principal argument of Fiedler, that archaeology cannot effectively distinguish between art and non-art, or between utilitarian and non-utilitarian artefacts, is of course valid. Indeed, one can easily go one step further and deny the existence of non-utilitarian artefacts altogether. A painting by a Dutch Master has, after all, many perfectly utilitarian roles (for the artist, the investor, the connoisseur, the art dealer, the frame maker). That is precisely why we have forsaken the term 'art' in pre-Historic contexts, and replaced it with the neutral term 'palaeoart'. It implies no more that the object is art than the use of the word 'peanut' implies that one is referring to anything botanically related to either a pea or a nut. It simply defines certain phenomena collectively (see Bednarik et al. 2003). Fiedler arrives at the view that, ultimately, the origins of representation (*Darstellung*) lie in the realm of ethology rather than archaeology or cultural anthropology. Indeed, a 'hominid ethology' would be a more appropriate framework of study than archaeology tainted by dogma. He reminds us that the often-assumed special status of humans is, in the final analysis, highly questionable. I concur wholeheartedly, as apparently does Brumm.

Issues of ideology and epistemology

In response to Edwards' concern about my 'portrayal of the motivations' of those subscribing to the 'short range' model I can only say that I cannot know what their motivations were or are. I have described the *effects* of their preoccupations, which I find more interesting than their reasons for promoting a false model. Edwards errs in his concluding

sentence: 'these people' have not 'examined a considerable amount of evidence', as he argues. For the greater part they have examined no evidence at all, they pre-judged the issue on the basis of inadequate reports by others. Bowyer rightly states that 'it is fair to ask if a subconscious need to award special status' to moderns had contributed to the appeal of these paradigms, and the evidence might point in that direction (see also Fiedler). For a sophisticated discussion of these issues I refer to Shay's sensitive and very original *RAR* Comment. She raises the political dimensions of my paper, reminding us of the 'social and ideological entanglements of archaeological enquiry'. I have attracted the ire of many for my opposition to Eurocentric versions of the human past, and have sometimes been the subject of quite illuminating personal attacks. For instance, one famous Frenchman accused me in print that, by promoting non-European rock art, I was practising inverse racism minimising the importance of European art, while a Spaniard responded to such perceived 'impertinence' and 'insulting writing', as he perceived it, by pointing out that my closest ancestors were European. Who would have thought that tribal loyalties still overruled the canons of scholarship in darkest Europe!

However, Shay is wrong in assuming that my work is, in any sense, based on theories such as those of Bhaba, Hodder or Wylie, or any other writer commenting on alternative ways of thinking about the past. Mine has evolved independently, and I have long become aware that archaeology is a political 'science' more than anything else (cf. Trigger 1989). Therefore my work was based on theoretical notions I developed myself; it reflects the record as I experienced it, the neglect of evidence I perceive, and my epistemological observations of the discipline.

I am grateful to Chakravarty for raising the idea of a 'dated uniformitarianism' as it is often applied to archaeology. Uniformitarianism remains a fine universal theory for many disciplines, where it does sterling work, but its adoption in archaeology has been a serious error. Humans and human societies do not necessarily behave in logical and predictable ways, like grains of sand being washed down a slope. Rather, they tend to make mistakes and they behave irrationally more often than rationally. Anyone doubting this really ought to take a long hard look at history, religion or politics. Simplistic deconstruction of faulty deductions, derived from a heavily distorted record in any event, is therefore not the best way to proceed in archaeology.

Among Chakravarty's many profound statements I select just one to illustrate his command of the subject: 'In experiencing life, it was only partially understood by the rock art artist, and, in the process of acquiring its understanding, it is not really being experienced by the rock art researcher'. This addresses a very important point all too fleetingly, and is in fact a rather elegant understatement. More than anything else, palaeoart expresses the awareness of the artist of some form of reality (a reality created, of course, by his own genetic, social and cultural circumstances, precisely as your and my reality is a contingent and *invalid* construct today). To experience the surviving trace of this 'reality awareness manifestation' today and to use our own, similarly contingent reality to 'examine' this totally alien externalisation of an unfathomable ontology is ludicrous. My mind certainly boggles when I think about the incredible chasms to be bridged. Even today, most people seem to exist in a blissful state of self-delusion that the reality of the world they experience is valid. The notion that they can authoritatively explore manifestations of another reality before even having understood that they themselves have no valid construct of reality defies rational discussion.

I can assure Chakravarty that his concern that 'inappropriate claims of priority or sophistication for Asian or Austronesian art' might appear is unfounded. Australia was settled long after palaeoart traditions began elsewhere, and even the position of southern Asia is already in doubt. It could well be that southern Africa can boast similarly early evidence. Moreover, in my model it is of no jingoistic consequence where the 'oldest palaeoart' might eventually be found. Taphonomic logic tells us that the site of art origins will never be established, so there is no Holy Grail of ultimate truth to be found at the end of the palaeoart rainbow. Nevertheless, *contra* Chakravarty, taphonomic logic is not likely to help us with creating better stylistic definition. Metamorphology, which does deal with the broader theoretical issues of interpretation, might do so, provided we leave behind the *traditional* 'stylistic aid, so honed and redefined'. It may have been honed for a long time now, but it remains a pitiful instrument, at least in the way it has been used. Until style is presented as a testable, repeatable tool of analysis, I intend not to waste any effort on considering modes of enquiry based on the undefined and mysterious vibes self-appointed experts have about graphic systems they know nothing about.

Kumar has been personally involved in finding and presenting key evidence of very early rock art. It comes as no surprise that he endorses my quest to replace the superseded model of human cognitive, cultural and technological evolution. Like most other commentators in this debate, he has researched Lower Palaeolithic evidence first hand and he shares their view that this main-period of human evolution has been significantly misinterpreted by orthodox or mainstream archaeology. Kumar has intensively studied the earliest presently known rock art in the world and he is engaged in determining its age. In a debate of the oldest evidence of apparently non-utilitarian hominid behaviour traces his views have precedence over those of scholars who are inadequately familiar with the evidence, but who have unfortunately dominated these debates in the past.

It is important to address the principal concern Feliks raises: that any new paradigm needs to take account of the particular circumstances of cognitive archaeology, lest it might become just another dogma. Not only do I share his concern, I think that the problem he identifies is even more profound than he implies. This discipline is to examine the origins of human cognition with tools that are the results exclusively of the very processes we seek to understand. Epistemologically, this is a foolhardy endeavour: what reason do we have to assume that human cognition, or understanding of reality, is sufficiently advanced *to study*

itself, let alone the transitory if not evanescent evidence of its *origins*? This is indeed, as I have said for decades, the most difficult research problem imaginable for the human animal in its present stage of intellectual evolution. In that sense, Feliks is certainly right: within this specific domain we are in no position to be exclusive—not for a long time yet. Hence the focus of my paper is deliberately on gathering possibly relevant empirical information. If my paper were seen as a scheme that excludes any promising approach it would be misunderstood.

I do not share Feliks' concern that the 'new paradigm' could become simply a modernised repeat of the old. The old paradigm, which has been developed for a century or so, centred on inappropriate and irrelevant evidence and gave no consideration to taphonomic logic. It created a dogmatic view of how art-like production began and then sought to accommodate all new finds into this system. With the exception of a few reasonable basic demands (that taphonomic logic be central to it, that scientific mode of reasoning be employed, and that it be free of geographical and other biases), the model I propose is entirely open-ended. There is currently no blueprint of when, where and how palaeoart began.

Incidental issues

Feliks is mistaken in his criticism of my use of such terms as 'iconic', because I always use such words as they are defined in the *IFRAO Rock Art Glossary*, e.g. 'iconic: providing visual information recognised by most contemporary humans as resembling the form of an object'. Therefore these are the correct terms, they are entirely unrelated to the meaning of the motifs, whereas I reject the names Feliks prefers. 'Geometric' is a subjective term, 'enigmatic' is vacuous because all palaeoart motifs are so to us, and 'unidentified' implies that the meaning of some motifs can be identified (it cannot be identified of any motif, because no such claim is falsifiable). Terms such as 'iconic' or 'non-iconic' are deliberately subjective, in the same way as the term 'anthropomorph' is not an identification, whereas the term 'human figure' certainly is.

The Comment by Straus is the only one in this collection that expresses significant disagreement with my paper—the kind of reaction I had been looking forward to. However, this disagreement turns out to be based on a series of misapprehensions:

1. I do not 'want to *deny* the remnant instances of cave art in central Europe'; there is simply no instance of Pleistocene cave art known from Germany, and none of the region's few other claims have been checked scientifically. Similarly, I would be happy to accept Pleistocene open-air art on schist sites in Iberia if such were presented.
2. I have not rejected 'varnish chronometric methods'; I have rejected cation-ratio analysis (as have almost all other archaeometrists), and I have explained in detail the qualifications to be applied to other methods (Bednarik 2002e).
3. Straus makes a connection between varnish analysis and Côa; he seems to think that there is rock varnish in that valley. The surface deposits analysed at Côa are silica accretions and Straus confused silica skin and rock varnish.
4. I have not 'excoriated' ('torn the skin from') Zilhão for his rejection of scientific attempts to estimate the age of Côa rock art, but I have been severely critical of his role in the wholesale destruction of Portuguese rock art. These are two very different issues.
5. My rejection of the Palaeolithic attribution of the Côa rock art is not 'apparent', as Straus says; I would have thought that it is very *tangible* indeed. It is also very justified, considering that no evidence has ever been presented in favour of this hypothesis, only untestable notions about style. There are no Pleistocene sediments in the lower part of the valley, no credible hearths, no Pleistocene occupation sites, no carbon dates and no OSL dates (see Zilhão 2003 and debate).
6. I have never insinuated that '*all* the paintings' in Lascaux are Holocene. What I did state was that 'the most celebrated paintings of Lascaux' (especially the large bovids and cervids, i.e. the most recent figures) appear to be of the Holocene. I do not know the age of any art in this cave, nor does anyone else. However, Bahn has presented very credible arguments that there is a combination of Holocene and Final Pleistocene art, to which I have referred.
7. It is my impression that Straus misunderstands the term 'palaeoart', mistakenly thinking that it refers to Palaeolithic or pre-Upper Palaeolithic art. This is indicated by several of his comments, including when he questions the inclusion of American material. For the correct definition of the term 'palaeoart' I refer to the *IFRAO Rock Art Glossary*.
8. In the paper I assumed that the reader is familiar with the notion that perhaps 20% of Australian petroglyphs may be of the Pleistocene (e.g. Bednarik 2002f). Straus is apparently not familiar with this, and perhaps also not with the notion of Middle Palaeolithic mode of technology (*sensu* Foley and Lahr 1997).
9. Straus appears to subscribe to an 'archaeological' idea of what science is. In science one always 'piles on' data, as he defines it, that is its very nature. In this case I sought to present a comprehensive list of material that might be relevant to the subject. Straus would rather have me decide which instances I like best and discard the rest. If this is how archaeology works, then I am not surprised that it is often at odds with science (see exhortations of Edwards and Feliks reminding us that one can never know when propositions that may be untenable currently might become significant).
10. Straus seems unaware that I was the first to challenge and reject the carbon isotope analysis of charcoal pigments as secure dating evidence, almost as soon as the first results were published in fact.

Besides these minor points I have more 'philosophical' quandaries with Straus' position as expressed here, disagreeing fundamentally with his epistemological viewpoint. For instance, I ask, how can Straus know that 'artistic activity' became increasingly 'routinised' with time? Perhaps so,

but there is no scientific basis warranting such a platitude. It is part of a mythological construct. Then Straus says that such activity was not even universal in the Upper Palaeolithic. Quite possibly, but how could he know this? To me such pronouncements sound as if we already knew 'what happened', which to my way of thinking we most certainly do not. We have seen so many false models in Pleistocene archaeology that we need to reject all reference to a 'received knowledge', which I perceive is implicit in Straus' comments: 'Time may tell as research develops and as new discoveries are made …'. I believe that research develops with a broadening and changing of attitudes and ideas, and that no discoveries can affect a dogma if they are subjected to the scrutiny of a false epistemological paradigm programmed to uphold the dogma. Traditional archaeological reasoning is simply wrong, and its methods are not suitable to test what is being developed outside its forms of discourse. I posit that we must assume that the overwhelming majority of the propositions archaeology has made about early human history must reasonably be expected to be false, so how can I reconcile this position with the precepts of a discipline whose credibility I question? As Straus observes, I prefer a scientific approach. While I may not be as blunt as Binford (2000/01: 334) who, when discussing 'science bashing' in archaeology, states that the methods of humanists 'are vacuous and their attempts at learning pathetic', I do share his general sentiments about humanistic archaeology which I think operates outside of proper science. Nowhere has this become more evident than in the Côa case, which Straus cites (Straus has seen the Côa rock art; pers. comm. August 2003), and which is a prime example of science bashing by humanist archaeologists as defined by Binford. Straus admits to having been sceptical of the existence of pre-Upper Palaeolithic 'art' until he saw an engraved bone from Bilzingsleben, but when it comes to judging petroglyphs a few centuries old as 'Palaeolithic' he is not sceptical at all. I suggest that we need to be more circumspect in the way we exercise scepticism. Straus was in no position to judge the Bilzingsleben issue, as he was and still is unfamiliar with the subject of art origins, while in Portugal he exercised no scepticism at all. This may sound harsh, but it is true that a Pleistocene archaeologist should not accept a Pleistocene age of rock art without proper and testable evidence—particularly if the rock art in question is only a few centuries old.

Those scintillating phosphenes

Concerning phosphene motifs in rock art, I have before observed that they would be iconic if they did in fact depict the patterns humans (and other animals) experience (Harrod discusses this too). I agree wholeheartedly with Feliks that the phosphene theory cannot explain art origins (as Harrod also observes), let alone 'prove' anything (I set out to disprove, not to prove; scientists cannot 'prove' anything conclusively).

The phosphene hypothesis is, of course, at best peripheral to the gist of my paper, which is about refuting the dominant paradigm of art beginnings by presenting the apparently relevant evidence. Both Hodgson and Feliks promote their own respective ideas, but I see none of the 'areas of disagreement' Hodgson perceives. The vague idea that phosphenes occur in early arts has been around since the 1960s, and two decades ago I noticed that pre-iconic markings seemed to be always of phosphene motifs. I formulated the hypothesis that all anthropic intentional markings made before the advent of graphic, i.e. two-dimensional, iconic palaeoart resemble one of a small number of the phosphene motifs that had earlier been identified. This is a perfectly refutable proposition, hence it is scientific. To this day it has not been falsified, but confirmed by all subsequent evidence (Hodgson 2000a). A key issue in this matter, and the one Lewis-Williams has consistently ignored, is that the same applies to another form of art: that made by infants of a narrow age bracket. The phosphene hypothesis squarely contradicts the crucial part of the shamanistic hypothesis, with which it is incompatible. It does not, however, *explain* the connection between phosphenes and art, it offers no interpretation for the phenomenon it observes. By contrast, Feliks' use of fossils offers a superb bridging argument: fossils appear to be the only external phenomena that happen to be referrer and referent rolled into one. It stands to reason that early humans would have seen that the imprint of a fossil is not the referent, but that it resembles it so closely in every respect that its function as a referrer seems too obvious not to have been realised. This would solve one of the major problems in the development of human cognition, but unfortunately the evidence for curation of fossils is not as common as one would like it to be. Moreover, the hypothesis' falsifiability seems to be weak, although with a bit of tinkering it could perhaps be improved.

Much the same can be said about Hodgson's central idea, which on the face of it looks very convincing. Here, too, conditions for refutation need to be laid down, which would render the hypothesis much better supported than appeals to plausibility. Nevertheless, what he says about the neurology of normal visual processes, graphic primitives, de-fleshing marks and the possible derivation of 'geometric' motifs is most persuasive and certainly needs to be examined much more closely. Both Hodgson and Feliks place too much emphasis on perceived differences between their respective positions and the phosphene hypothesis, and I repeat what I have observed before: all three ideas are in fact complementary, and on the road to improved knowledge about these matters they all need to be considered more thoroughly. Hodgson seems to miss a crucial point in his elaboration about neurological mechanisms of perception. The predisposition towards geometric patterns is no doubt ultimately a result of the basic processes of vision, much in the same way as a computer-generated moire pattern is simply a reflection of mathematics and geometry. But what Hodgson seems to overlook is that precisely the same applies to phosphenes: the same simple laws generate them, doubtlessly (even if the details may elude us currently). Indeed, Oster (1970) compared them to the test pattern of a television station. Ultimately, then, Hodgson and I are saying much the same thing, having arrived at our espective views from quite different directions.

Grist on the mill

Wynn's superior command of the subject is no surprise, he has for decades been engaged in researching related topics. He has no difficulty accommodating most of my points and focuses at once on substantive issues rather than trivia. His observations connect almost seamlessly with those of Hodgson, and they need to be included in any serious study of the beginnings of palaeoart. Wynn's argument that we risk underestimating cognitive ability is precisely the driving force behind my series of seafaring experiments, because I realised that by focusing on 'cutting-edge' abilities one would arrive at the most secure approximations of maximum abilities. At least in that sense, Wynn's observations match just as seamlessly with my ideas, and it is here that I sense the beginnings of a broader program of addressing the issues. If we combine the hypotheses of such scholars as Wynn, Hodgson and Feliks with mine, I feel that we have the ingredients for formulating precisely the kind of exploratory paradigm needed for addressing a core issue in cognitive archaeology. Such a paradigm for explaining how hominids acquired symbolling ability seems imminent now and, once formulated, can be stated in such a way that it can be subjected to testing by falsification. We seem to be making excellent progress at last.

This brings me to Wynn's reminder that it needs to be clarified how a symbolic function can be attributed, for instance to markings that may simply represent ludic doodles. He notes that for most of the evidence I offer, it is not necessary that the palaeoart was a symbol. This is the case in some instances, but there are many others that would permit the formulation of falsifiable propositions to test their semiotic status. The vast number of cupules as well as the Lower Palaeolithic use of beads provides in fact more justification for suspecting symbolism than the 'notations' of the Upper Palaeolithic. As I have maintained throughout the announcements by Marshack and d'Errico, neither has actually *demonstrated* notational intent (although quite probably they are right). In my similar replication and microscopic work I have learnt that, while one can identify multiple applications of the same stone tool, one can never demonstrate the use of different tools. The prospects of attributing symbolic function to cupules, pendants or beads are, I would argue, considerably better. Closer attention may even result in convincing cases for such items as engraved portable pieces, such as my comparison of Bilzings-leben 1 and one of the Oldisleben specimens (Fig. 29 in my paper). Could the similarity in the markings strategies evident in these two bone fragments be attributable to pure chance?

A key issue raised by Feliks, and to a lesser extent by other commentators, such as Edwards and Chakravarty, is the concern that I demand a strictly scientific approach, which may exclude less rigorous endeavours. I would like to illustrate how I consider this objection by citing a crucial example: by what mechanisms could symbolling have arisen originally? In non-iconic symbolism, a connection by association or convention, between the signifier (refer-rer) and what it signifies (referent), must be culturally negotiated. It is not known how this was first achieved, but a number of possibilities can be considered. Feliks has provided an elegant solution by suggesting the involvement of fossils, a referrer offering most visual properties of the referent. Another possibility is re-enactment, which presupposes ability in the audience of discriminating between referrer (the actor) and referent (the depicted). One might visualise the successful hunter who, upon returning to the camp, shares his triumph by enacting how he stalked and speared the quarry. Unless there is the appropriate neurological structure in place, his demeanour would simply be registered as strange and inappropriate. The beginnings of such behaviour might be found in the elementary deception chimps are capable of. My favoured explanation is based on my view that iconic resemblance is not self-evident, it is not 'consciously' experienced by pongids in the wild as far as we know, but it is subconsciously experienced by all higher animals, in the form of visual ambiguity (consider, for example, flight response prompted by an outline of a bird of prey). I define the production of iconographic forms as the cultural and intentional creation of features prompting visual responses to a signifier; it induces visual ambiguity intentionally. I therefore call iconic art a *'managed', intentional use of visual ambiguity*. While this does perhaps not explain the precise mechanisms, it does offer a sensible framework within which to hypothe-sise about 'art' beginnings.

In other words, there are various possible explanations for how the crucial link between referrer and referent was established, and in that sense I am in complete agreement with the commentators that all possibilities need to be explored and considered most carefully. My view is that from these 'semi-scientific' raw materials, we must then create or extract testable propositions, which should be possible in various cases. However, this changes nothing about the fundamental axiom that proper science must remain limited to these procedures, however much we let the mind wander into the semi-scientific, exploratory realm. The only condition under which this rule can be relaxed, or indeed abolished, is if we were to find a human being that has access to objective reality. I think we can all agree that such a creature does not yet exist, and as we remain an intellectually deficient species, we should not expect such an animal to appear in the next number of millennia. Since we have nothing more than fragments of reality at our disposal, *refutation is the only avenue open to us* if we crave any betterment of knowledge. Of course, to pose propositions that are refutable, we must first formulate models capable of generating and situating useful testable propositions, as Feliks also observes. In that sense, I agree entirely with the commentators, and I would like to see such issues much more intensively debated (cf. Feliks' 'brainstorming'). It is this very process that will improve our understanding. The processes that I have been critical of, in this and other papers, are those that impede and retard our better understanding.

The outcome of this debate is, I think, symptomatic of a fundamental shift that I first noticed in the mid-1990s: the faddish priorities of archaeological concerns in palaeoart studies (e.g. shamanism, African Eve-ism, catastrophism,

diffusionism, scientism) are gradually giving way to more rigorous discussion. Most of the scholars who globally dominated these issues were always inadequately informed and promoted not paradigms, but fads. The pendulum is certainly swinging back with a vengeance now, having been pushed far too much into one direction. These are exciting times!

Robert G. Bednarik

RAR 20-659

REFERENCES

Abramova, Z. A. 1962. *Paleoliticheskoe iskusstvo na territorii SSSR*. Akademiya Nauk SSSR, Moscow.

Abramova, Z. A. 1990. L'art mobilier paléolithique en Siberie. *Bollettino del Centro Camuno di Studi Preistorici* 25/26: 80–98.

Aikens, C. M. and T. Higuchi 1982. *Prehistory of Japan*. Academic Press, London/New York.

Anati, E. 1986. The rock art of Tanzania and the east African sequence. *Bollettino del Centro Camuno di Studi Preistorici* 23: 15–68.

Aubry, T., X. M. Llach, J. D. Sampaio and F. Sellami 2002. Open-air rock art, territories and modes of exploitation during the Upper Palaeolithic in the Côa Valley (Portugal). *Antiquity* 76: 62–76.

Ayala, F. J. 1996. Response to Templeton. *Science* 272: 1363–4.

Bahn, P. G., 1990. Motes and beams: a further response to White on the Upper Paleolithic. *Current Anthropology* 32: 71–6.

Bahn, P. G. 1991. Pleistocene images outside Europe. *Proceedings of the Prehistoric Society* 57(1): 91–102.

Bahn, P. G. 1994. Lascaux: composition or accumulation? *Zephyrvs* 47: 3–13.

Bahn, P. G. 1995. Cave art without the caves. *Antiquity* 69: 231–7.

Bahn, P., P. Pettitt and S. Ripoll 2003. Discovery of Palaeolithic cave art in Britain. *Antiquity* 77: 227–31. [LGS]

Baldeon, A. 1993. El yacimiento de Lezetxiki (Gipuzkoa, País Vasco). Los niveles musterienses. *Munibe* 45: 3–97.

Barham, L. S. 2000. *The Middle Stone Age of Zambia, south central Africa*. Western Academic and Specialist Press, Bristol.

Barham, L. S. 2002. Systematic pigment use in the Middle Pleistocene of south-central Africa. *Current Anthropology* 43: 181–90.

Barinaga, M. 1992. 'African Eve' backers beat a retreat. *Science* 255: 686–7.

Bar-Yosef, O. and Belfer-Cohen, A. 1988. The early Upper Palaeolithic in Levantine caves. In J. F. Hoffecker and C. A. Wolf (eds), *Early Upper Palaeolithic: evidence from Europe and the Near East*, pp. 23–41. BAR, International Series 437, Oxford.

Basedow, H. 1914. Aboriginal rock carvings of great antiquity in South Australia. *Journal of the Royal Anthropological Institute* 44: 195–211.

Beaumont, P. 1973. The ancient pigment mines of southern Africa. *South African Journal of Science* 69: 140–6.

Beaumont, P. B. 1990. Wonderwerk Cave. In P. Beaumont and D. Morris (eds), *Guide to archaeological sites in the Northern Cape*, pp. 101–134. McGregor Museum, Kimberley.

Beaumont, P. 1992. The time depth of aesthetic and symbolic behaviour in southern Africa. Paper presented to the Biennial Conference of the Southern African Association of Archaeologists, Johannesburg.

Beaumont, P. 1999. *Wonderwerk Cave*. INQUA XV International Conference Field Guide: Northern Cape, pp. 27–31.

Beaumont, P. B. in press. On a search for ancestral petroglyphs in the south-eastern Kalahari. *Rock Art Research*.

Beaumont, P. and A. Boshier 1972. Mining in southern Africa and the emergence of modern man. *Optima*, March 1972 issue.

Beaumont, P., H. de Villiers and J. Vogel 1978. Modern man in sub-Saharan Africa prior to 49 000 B.P.: a review and evaluation with particular reference to Border Cave. *South African Journal of Science* 74: 409–19.

Beck, W., D. J. Donahue, A. J. T. Jull, G. Burr, W. S. Broecker, G. Bonani, I. Hajdas and E. Malotki 1998. Ambiguities in direct dating of rock surfaces using radiocarbon measurements. *Science* 280: 2132–5.

Bednarik, R. G. 1984. On the nature of psychograms. *The Artefact* 8: 27–33.

Bednarik, R. G. 1987a. No pictographs at end of Rochester Creek rainbow. *La Pintura* 15(2–3): 14–18.

Bednarik, R. G. 1987b. Engramme und Phosphene. *Zeitschrift für Ethnologie* 112: 223–35.

Bednarik, R. G. 1988a. Comment on M. F. Nobbs and R. I. Dorn, 'Age determinations for rock varnish formation within petroglyphs: cation-ratio dating of 24 motifs from the Olary region, South Australia'. *Rock Art Research* 5: 124–39.

Bednarik, R. G. 1988b. Archaeological potentials at the Parowan Gap Site, Utah. Paper presented to Symposium B, First AURA Congress, Darwin, Australia.

Bednarik, R. G. 1988c. El arte rupestre Boliviano visto desde el exterior. *SIARB Boletín* 2: 22–8.

Bednarik, R. G. 1989. On the Pleistocene settlement of South America. *Antiquity* 63: 101–11.

Bednarik, R. G. 1990a. An Acheulian haematite pebble with striations. *Rock Art Research* 7: 75.

Bednarik, R. G. 1990b. On neuropsychology and shamanism in rock art. *Current Anthropology* 31: 77–80.

Bednarik, R. G. 1992a. Natural line markings on Palaeolithic objects. *Anthropologie* 30: 233–40.

Bednarik, R. G. 1992b. The Paleolithic art of Asia. In S. Goldsmith, S. Garvie, D. Selin, and J. Smith (eds), *Ancient images, ancient thought: the archaeology of ideology*, pp. 383–390. Proceedings of the 23rd Annual Chacmool Conference, University of Calgary.

Bednarik, R. G. 1992c. Palaeolithic art found in China. *Nature* 356: 116.

Bednarik, R. G. 1993a. About Palaeolithic ostrich eggshell in India. *Indo-Pacific Prehistory Association Bulletin* 13: 34–43.

Bednarik, R. G. 1993b. Palaeolithic art in India. *Man and Environment* 18(2): 33–40.

Bednarik, R. G. 1993c. Pleistocene animal depiction in Asia. *International Newsletter on Rock Art* 6: 2–6.

Bednarik, R. G. 1993d. European Palaeolithic art—typical or exceptional? *Oxford Journal of Archaeology* 12(1): 1–8.

Bednarik, R. G. 1993e. Die Bilzingslebener Gravierungen im Lichte altpaläolithischer Beweise kognitiver Fähigkeit. *Ethnographisch-Archäologische Zeitschrift* 34: 550–4.

Bednarik, R. G. 1993f. About cupules. *Rock Art Research* 10: 138–9.

Bednarik, R. G. 1994a. On the scientific study of palaeoart. *Semiotica* 100(2/4): 141–68.

Bednarik, R. G. 1994b. Art origins. *Anthropos* 89: 169–80.

Bednarik, R. G. 1994c. The Pleistocene art of Asia. *Journal of World Prehistory* 8(4): 351–75.

Bednarik, R. G. 1994d. A taphonomy of palaeoart. *Antiquity* 68: 68–74.

Bednarik, R. G. 1995a. Metamorphology: in lieu of uniformitarianism. *Oxford Journal of Archaeology* 14: 117–22.

Bednarik, R. G. 1995b. Concept-mediated marking in the Lower Palaeolithic. *Current Anthropology* 36: 605–34.

Bednarik, R. G. 1995c. The age of the Côa valley petroglyphs in Portugal. *Rock Art Research* 12: 86–103.

Bednarik, R. G. 1995d. Refutation of stylistic constructs in Palaeolithic rock art. *Comptes Rendus de L'Académie de Sciences Paris* 321(série IIa, No. 9): 817–21.

Bednarik, R. G. 1995e. Traces of cultural continuity in Middle and Upper Palaeolithic material evidence. *Origini* 18: 47–67.

Bednarik, R. G. 1996a. Only time will tell: a review of the methodology of direct rock art dating. *Archaeometry* 38: 1–13.

Bednarik, R. G. 1996b. Crisis in Palaeolithic art studies. *Anthro-pologie* 34: 123–30.

Bednarik, R. G. 1997a. The role of Pleistocene beads in documenting hominid cognition. *Rock Art Research* 14: 27–43.

Bednarik, R. G. 1997b. Pleistocene stone pendant from Western Australia. *Australian Archaeology* 45: 32–4.

Bednarik, R. G. 1997c. The earliest evidence of ocean navigation. *International Journal of Nautical Archaeology* 26: 183–91.

Bednarik, R. G. 1998a. Microscopic analysis of 'engraved plaques' and other objects from Devil's Lair. *Journal of the Royal Society of Western Australia* 81: 165–75.

Bednarik, R. G. 1998b. The 'australopithecine' cobble from Makapansgat, South Africa. *South African Archaeological Bulletin* 53: 4–8.

Bednarik, R. G. 1999. The speleothem medium of finger flutings and its isotopic geochemistry. *The Artefact* 22: 49–64.

Bednarik, R. G. 2000. Age estimates for the petroglyph sequence of Inca Huasi, Mizque, Bolivia. *Andean Past* 6: 277–87.

Bednarik, R. G. 2000. New evidence from the Côa valley, Portugal. Paper presented to symposium 'Dating rock art', Third AURA Congress, Alice Springs (in press).

Bednarik, R. G. 2001a. Pilbara petroglyphs dated. *Rock Art Research* 18: 55–7.

Bednarik, R. G. 2001b. An Acheulian figurine from Morocco. *Rock Art Research* 115–6.

Bednarik, R. G. 2001c. The taphonomy of beeswax figures. *Rock Art Research* 18: 91-5.

Bednarik, R. G. 2001d. The early Indian Petroglyphs Project (EIP): rationale for an international commission. *Rock Art Research* 18: 72. [GK]

Bednarik, R. G. 2001e. Progress report of the EIP Project – November 2001. *Rock Art Research* 18: 138–40. [GK]

Bednarik, R. G. 2002a. The Saudi Arabian rock art mission of November 2001. *Atlal* (in press).

Bednarik, R. G. 2002b. About the age of Pilbara rock art. *Anthropos* 97: 1–15.

Bednarik, R. G. 2002c. An outline of Middle Pleistocene palaeoart. *Purakala* 13: 39–44.

Bednarik, R. G. 2002d. Paläolithische Felskunst in Deutschland? *Archäologische Informationen* 25(1–2): 107–17.

Bednarik, R. G. 2002e. The dating of rock art: a critique. *Journal of Archaeological Science* 29(11): 1213–33. [RGB]

Bednarik, R. G. 2002f. About the age of Pilbara rock art. *Anthropos* 97(1): 201–15. [RGB]

Bednarik, R. G. 2003a. The human ascent: a critical review. *Anthropologie* 41(2): 101–5.

Bednarik, R. G. 2003b. Seafaring in the Pleistocene. *Cambridge Archaeological Journal* 13: 41–66.

Bednarik, R. G. 2003c. A figurine from the African Acheulian. *Current Anthropology* 44(3): 405–13. [WB] [JF]

Bednarik, R. G. in prep. The Middle Palaeolithic engravings from Oldisleben, Germany.

Bednarik, R. G., M. Consens, A. Muzzolini, D. Seglie and Y. A. Sher 2003. *Rock art glossary: a multilingual dictionary*. Brepols, Turnhout. [RGB]

Bednarik, R. G. and K. Devlet 1993. Problemy konservatsiï pamyatnikov naskal'nogo iskusstva verkhneï Leny. *Sovre-mennye problemy isucheniya petroglifov*, pp. 37–48. Spornik Nauchnykh Trudov, Kemerovskiï Rocudarstvennyï Universi-tet, Kemerovo.

Bednarik, R. G., G. Kumar and G. S. Tyagi 1991. Petroglyphs from central India. *Rock Art Research* 8: 33–5.

Bednarik, R. G. and You Yuzhu 1991. Palaeolithic art from China. *Rock Art Research* 8: 119–23.

Behm-Blancke, G. 1983. Altpaläolithische Gravuren von Bilzingsleben, Kr. Artern. *Ethnographisch-Archäologische Zeitschrift* 24: 304–20.

Belfer-Cohen, A. and O. Bar-Yosef 1981. The Aurignacian at Hayonim Cave. *Paléorient* 7(2): 19–42.

Belfer-Cohen, A., and N. Goren-Inbar 1994. Cognition and communication in the Levantine Lower Paleolithic. *World Archaeology* 26(2): 144–57. [SWE]

Beltrán, A. 1992. Crisis in traditional ideas about European rock art: the question of diffusion and convergence. In M. Lorblanchet (ed.), *Rock art of the Old World*, pp. 401–413. IGNCA Rock Art Series 1, Indira Gandhi National Centre for the Arts, New Delhi.

Benekendorff, U. 1990. From the information recorder stone to a picture book of the Stone Age. *Archaeologische Berichten* 20: 14–28, 43–65. [JBH]

Bhaba, H. 1994. *The location of culture*. Routledge, London. [TS]

Bierman, P. R. and A. R. Gillespie 1991. Accuracy of rock-varnish chemical analyses: implications for cation-ratio dating. *Geology* 19: 196–9.

Bierman, P. R., A. R. Gillespie and S. Kuehner 1991. Precision of rock-varnish chemical analyses and cation-ratio ages. *Geology* 19: 135–8.

Binford, L. R. 1981. *Bones: ancient men and modern myths*. Academic Press, New York. [SWE]

Binford, L. R. 2000/01. On science bashing: a bashful archaeologist speaks out. *Bulletin of the Deccan College Post-Graduate Research Institute* 60/61: 329–35. [RGB]

Binneman, J. and P. Beaumont 1992. Use-wear analysis of two Acheulean handaxes from Wonderwerk Cave, Northern Cape. *South African Field Archaeology* 1: 92–97.

Birkner, F. 1938. Die erste altsteinzeitliche Felszeichnung in Deutschland. *Bayerisches Vorgeschichtsblatt* 15: 59–64.

Black, D., T. de Chardin, C. C. Young and W. C. Pei 1933. The Choukoutien cave deposits. *Geological Memoirs, Geological Society of China* A(2). [SWE]

Bohmers, A. 1939. Die Felszeichnung in der Kastlhänghöhle. *Germania* 1939: 39–40.

Boriskovski, P. I. (ed.) 1984. *Paleolit SSSR*. Izdatel'stvo Nauka, Moscow.

Bosinski, G. 1982. *Die Kunst der Eiszeit in Deutschland und in der Schweiz*. Kataloge Vor- und Frühgeschichtlicher Alter-tümer 20, Habelt, Bonn.

Boucher de Perthes, J. 1846. *Antiquités celtiques et antédiluvien-nes*. Abbeville. [RGB]

Bouvier, J.-M. 1987. Bases objectives de la chronologie de l'art mobilier en Périgord, Charente et Poitou. In *Pré-actes, Colloque International d'Art Mobilier Paléolithique*, pp. 13–20, Foix.

Bouvier, J.-M. 1993. Généralités. In GRAPP (ed.), *L'Art pariétal paléolithique: techniques et méthodes d'étude*, pp. 7–19. Ministère de L'Enseignement Supérieur et de la Recherche, Paris.

Brookfield, J. F. Y. 1997. Importance of ancestral DNA ages. *Nature* 388: 134.

Capitan, L. and D. Peyrony 1921. Les origines de l'art à l'Aurignacien moyen: La Ferrassie. *Revue Archéologique* 31: 92–112.

Carbonell, E., M. Mosquera, A. Ollé, X. Rodríguez, R. Sala, J. Vergès, J. Arsuaga and J. Bermúdez de Castro 2003. Les premiers comportements funéraires auraient-ils pris place à Atapuerca, il y a 350 000 ans? *L'Anthropologie* 107: 1–14. [JBH]

Chakravarty, K. K. and R. G. Bednarik 1997. *Indian rock art and its global context*. Motilal Banarsidass, Delhi.

Chaffee, S. D., M. Hyman and M. Rowe 1993. AMS ^{14}C dating of rock paintings. In J. Steinbring, A. Watchman, P. Faulstich, and P. S. C. Taçon (eds.), *Time and space: dating and spatial considerations in rock art research*, pp. 67–73. Occasional AURA Publication 8, Australian Rock Art Research Association, Melbourne.

Chen Zhao Fu 1991. *History of the discovery of Chinese rock art*. Shanghai People's Publishing House, Shanghai.

Clark, J. D. 1987. Transitions: *Homo erectus* and the Acheulian: the Ethiopian sites of Gadeb and the middle Awash. *Journal of Human Evolution* 16: 809–26. [SWE]

Clark, J. D. 1988. The Middle Stone Age of East Africa and the beginnings of regional identity. *Journal of World Archaeology* 2: 235–305.

Clark, J. D. 2001. *Kalambo Falls prehistoric site III, the earlier cultures: middle and earlier Stone Age*. Cambridge University Press, Cambridge. [SWE]

Clark, J. D., Y. Beyenne, G. W. Gabriel, W. K. Hart, P. R. Renne, H. Gilbert, A. Defleur, G. Suwa, S. Katoh, K. R. Ludwig, J.-R. Boisserie, B. Asfaw and T. D. White 2003. Stratigraphic, chronological, and behavioral contexts of Pleistocene *Homo sapiens* from Middle Awash, Ethiopia. *Nature* 423: 747–51. [AB] [SWE]

Clottes, J., J.-M. Chauvet, E. Brunel-Deschamps, C. Hillaire, J.-P. Daugas, M. Arnold, H. Cachier, J. Evin, P. Fortin, C. Oberlin, N. Tisnerat and H. Valladas 1995. Les peintures paléolithiques de la Grotte Chauvet-Pont d'Arc, à Vallon-Pont-d'Arc (Ardèche, France): datations directes et indirectes par la méthode du radiocarbone. *Comptes Rendus de l'Académie des Sciences Paris* 320: 1133–40.

Collins, M. B. 2002. The Gault Site, Texas, and Clovis research. *Athena Review* 3(2): 31–42, 100–1.

Collins, M. B., T. R. Hester and P. J. Headrick 1992. Engraved cobbles from the Gault Site, central Texas. *Current Research in the Pleistocene* 9: 3–4.

Collins, M. B., T. R. Hester, D. Olmstead and P. J. Headrick 1991. Engraved cobbles from early archaeological contexts in central Texas. *Current Research in the Pleistocene* 8: 13–15.

Conard, N. J. and H.-P. Uerpmann 2000. New evidence for Paleolithic rock painting in central Europe. *Current Anthropology* 41: 853–6.

Cooke, C. K. 1963. Report on excavations at Pomongwe and Tshangula Caves, Matopos Hills, southern Rhodesia. *South African Archaeological Bulletin* 18: 73–151.

Coolidge, F. L. and T. Wynn 2001. Executive functions of the frontal lobes and the evolutionary ascendancy of *Homo sapiens*. *Cambridge Archaeological Journal* 11: 255–60. [AB]

Copeland, L. and F. Hours 1977. Engraved and plain bone tools from Jiita (Lebanon) and their early Kebaran context. *Proceedings of the Prehistoric Society* 43: 295–301.

Crémades, M., H. Laville, N. Sirakov and J. K. Kozlowski 1995. Une pierre gravée de 50 000 ans B.P. dans les Balkans. *Paleo* 7: 201–9.

Crick, F. and C. Koch 1995. Are we aware of neural activity in primary visual cortex? *Nature* 375: 121–3. [DH]

Crivelli Montero, E. A. and M. M. Fernández 1996. Palaeo-indian bedrock petroglyphs at Epullán Grande Cave, northern Patagonia, Argentina. *Rock Art Research* 13: 112–7.

Cuddhy, J. M. 1987. *The ordeal of civility*. Beacon Press, Boston [TS]

Dart, R. A. 1974. The waterworn australopithecine pebble of many faces from Makapansgat. *South African Journal of Science* 70: 167–9.

Davidson, I. and W. Noble 1989. The archaeology of perception: traces of depiction and language. *Current Anthropology* 30: 125–55. [TW]

Davidson, I. and W. Noble 1990. Tools, humans and evolution—the relevance of the Upper Palaeolithic. *Tools, language and intelligence: evolutionary implications*. Wenner-Gren Foundation, Cascais, Portugal, pp. 1–21. [RGB]

Davidson, I. and W. Noble 1992. Why the first colonisation of the Australian region is the earliest evidence of modern human behaviour. *Archaeology in Oceania* 27: 113–19. [AB]

Davis, S. 1974. Incised bones from the Mousterian of Kebara Cave (Mount Carmel) and the Aurignacian of Ha-Yonim Cave (Western Galilee), Israel. *Paléorient* 2: 181–2.

Dharvent, I. 1902. *Premiers essays de sculpture de l'homme préhistorique*. Rouen. [RGB]

Débenath, A. and L. Duport 1971. *Os travaillés et os utilisés de quelques gisements préhistoriques charentais*. Mémoires, Société Archéologique et Historique de la Charente, pp. 189–202.

Delluc, B. and G. Delluc 1997. Dix observations graphiques sur la grotte ornée de Pair-non-Pair (Prignac-et-Marcamps, Gironde). *Bulletin de la Société Préhistorique Française* 94(1): 41–50. [WB]

Dennett, D. C. 1990. Memes and the exploitation of imagination. *Journal of Aesthetics and Art Criticism* 48: 127–35. [WB]

d'Errico, F. 1995. A new model and its implications for the origin of writing: the La Marche antler revisited. *Cambridge Archaeological Journal* 5: 163–206. [TW]

d'Errico, F. 2001. Memories out of mind: the archaeology of the oldest memory systems. In A. Nowell (ed.), *In the mind's eye: multidisciplinary approaches to the evolution of human cognition*, pp. 33–49. International Monographs in Prehistory, Ann Arbor. [TW]

d'Errico, F. and M. Allard 1997. The Mousterian engraved bone of the Grotte de Peyrere 1 (Hautes-Pyrenées, France. Abstract in M. Strecker (ed.), *Congreso Internacional de Arte Rupestre, Cochabamba, Bolivia, 1–6 de Abril de 1997*, p. 41. SIARB, La Paz.

d'Errico, F. and C. Cacho 1994. Notation versus decoration in the Upper Palaeolithic: a case study from Tossal de la Roca, Alicante, Spain. *Journal of Archaeological Science* 21: 185–200. [TW]

d'Errico, F., C. Gaillard and V. N. Misra 1989. Collection of non-utilitarian objects by *Homo erectus* in India. *Hominidae. Proceedings of the 2nd International Congress of Human Paleontology*, pp. 237–39. Editoriale Jaca Book, Milan.

d'Errico, F., C. Henshilwood and P. Nilssen 2001. An engraved bone fragment from c. 70 000-year-old Middle Stone Age levels at Blombos Cave, South Africa: implications for the origin of symbolism and language. *Antiquity* 75: 309–18.

d'Errico, F. and A. Nowell 2000. A new look at the Berekhat Ram figurine: implications for the origins of symbolism. *Cambridge Archaeological Journal* 10: 123–67.

d'Errico, F. and P. Villa 1997. Holes and grooves: the contribution of microscopy and taphonomy to the problem of art origins. *Journal of Human Evolution* 33: 1–31.

d'Errico, F., J. Zilhão, M. Julien, D. Baffer and J. Pelegrin 1998. Neanderthal acculturation in western Europe? A critical review of the evidence and its interpretation. *Current Anthropology* 39: 1–44 (supplement). [DH]

Dibble, H. 1987. Reduction sequences in the manufacture of Mousterian implements in France. In O. Soffer (ed.), *The Pleistocene Old World*. Plenum, New York. [TW]

Dissanayake, E. 1988. *What is art for?* University of Washington Press, Seattle and London. [JF]

Dorn, R. I. 1996a. A change of perception. *La Pintura* 23(2): 10–11.

Dorn, R. I. 1996b. Uncertainties in ^{14}C ages for petroglyphs from

the Olary province, South Australia. *Archaeology in Oceania* 31: 214–5.

Dorn, R. I. 1997. Constraining the age of the Côa valley (Portugal) engravings with radiocarbon dating. *Antiquity* 71: 105–15.

Dorn, R. I. and D. S. Whitley 1984. Chronometric and relative age determination of petroglyphs in the western United States. *Annals of the Association of American Geographers* 74: 308–22.

Dorn, R. I., P. B. Clarkson, M. F. Nobbs, L. L. Loendorf and D. S. Whitley 1992. New approach to the radiocarbon dating of rock varnish, with examples from drylands. *Annals of the Association of American Geographers* 82(1): 136–51.

Dortch, C. E. 1976. Two engraved stone plaques of late Pleistocene age from Devil's Lair, Western Australia. *Archaeology and Physical Anthropology in Oceania* 11: 32–44.

Dortch, C. E. 1984. *Devil's Lair, a study in prehistory*. Western Australian Museum, Perth.

Edwards, P. 1991. Wadi Hammeh 27: an Early Natufian site at Pella, Jordan. In O. Bar-Yosef and F. Valla (eds), *The Natufian culture in the Levant*, pp. 123–148. International Monographs in Prehistory, Ann Arbor, Michigan.

Edwards, S. W. 2000. Flaked-stone basalt technology in the northern Sierra Nevada of California. *Journal of California and Great Basin Anthropology* 22 (2): 361–74. [SWE]

Edwards, S. W. 2001. A modern knapper's assessment of the technical skills of the late Acheulean biface workers at Kalambo Falls. In J. D. Clark (ed.), *Kalambo Falls prehistoric site III*, pp. 605–611. Cambridge University Press. Cambridge. [SWE]

Evans, U. 1994. Hollow Rock Shelter, a Middle Stone Age site in the Cederberg. *Southern African Field Archaeology* 3: 63–73.

Feathers, J. K. 2002. Luminescence dating in less than ideal conditions: case studies from Klasies River main site and Duinefontein, South Africa. *Journal of Archaeological Science* 29: 177–94.

Feliks, J. 1995, 1997a, 1997b. The impact of fossils. Preliminary drafts submitted for publication and widely circulated. [JF]

Feliks, J. 1998. The impact of fossils on the development of visual representation. *Rock Art Research* 15: 109–34.

Feliks, J. 2000. Iconic interface between the worlds. Comment on D. Hodgson, 'Art, perception, and information processing: an evolutionary perspective'. *Rock Art Research* 17: 23–5. [JF]

Feraud, G., D. York, C. M. Hall, N. Goren-Inbar and H. P. Schwarcz 1983. $^{40}Ar/^{39}Ar$ age limit for an Acheulian site in Israel. *Nature* 304: 263–5.

Ffythche, D. H. and R. J. Howard 1999. The perceptual consequences of visual loss: 'positive' pathologies of vision. *Brain* 122: 1247–60. [DH]

Fiedler, L. 2002: Form, Funktion und Tradition; die symbolische Repräsentanz steinzeitlicher Geräte. *Germania* 80(2): 405–20. [LF]

Foley, R. and M. M. Lahr 1997. Mode 3 technologies and the evolution of modern humans. *Cambridge Archaeological Journal* 7: 3–36. [RGB]

Fortea, F. J. 2002. Trente-neuf dates C14-SMA pour l'art parietal paleolithique des Asturies. *Bulletin de la Société Préhisto-rique Ariège-Pyrenées* 57: 7–28. [LGS]

Freeman, L. G. 1994. The many faces of Altamira. *Complutum* 5: 331–42.

Freeman, L. G. and J. González Echegaray 1983. Tally-marked bone from Mousterian levels at Cueva Morín (Santander, Spain). In *Homenaje al Prof. M. Almagro Basch*, Vol. I, 143–147. Ministerio de Cultura, Madrid.

Freund, G. 1957. L'art aurignacien en Europe centrale. *Bulletin de Société Préhistorique de Ariège* 12: 55–78.

Fullagar, R. L. K., D. M. Price and L. M. Head 1996. Early human occupation of northern Australia: archaeology and thermoluminescence dating of Jinmium rock-shelter, Northern Territory. *Antiquity* 70: 751–73.

Furmanski, C. S. and S. A. Engel 2000. An oblique effect in human primary visual cortex. *Nature Neuroscience* 3(6): 535–6. [DH]

Gai Shanlin 1986. *Petroglyphs in the Yinshan Mountains*. Cultural Relics Publishing House, Beijing.

Garrod, D. A. E. and D. M. A. Bates 1937. *The Stone Age of Mount Carmel*, Vol. I. Claredon Press, Oxford.

Gibbons, A. 1997. Doubts over spectacular dates. *Science* 278: 220–2.

Gibson, J. J. 1979. *The ecological approach to visual perception*. Houghton Mifflin, Boston. [DH]

Gleitman, H., A. Fridlund and D. Reisberg 1999. *Psychology.* W. W. Norton and Company, New York. [DH]

Goren-Inbar, N. 1986. A figurine from the Acheulian site of Berekhat Ram. *Mi'Tekufat Ha'Even* 19: 7–12.

Goren-Inbar, N. 1990. *Quneitra—a Mousterian site on the Golan Heights.* Quedem 31, Institute of Archaeology, Jerusalem.

Goren-Inbar, N., Z. Lewy and M. E. Kislev 1991. Bead-like fossils from an Acheulian occupation site, Israel. *Rock Art Research* 8: 133–6.

Goren-Inbar, N. and S. Peltz 1995. Additional remarks on the Berekhat Ram figurine. *Rock Art Research* 12: 353–4.

Goring-Morris, A. N. 1998. Mobiliary art from the late Epipalaeolithic of the Negev, Israel. *Rock Art Research* 15: 81–8.

Grün, R. and P. Beaumont 2001. Border Cave revisited: a revised ESR chronology. *Journal of Human Evolution* 40: 467–82.

Guidon, N. and G. Delibrias 1986. Carbon-14 dates point to man in the Americas 32 000 years ago. *Nature* 321: 769–71.

Hahn, J. 1991. Höhlenkunst aus dem Hohlen Fels bei Schelklin-gen, Alb-Donau-Kreis. *Archäologische Ausgrabungen in Baden-Württemberg* 10: 19–21.

Harrod, J. 1987a. European Upper Paleolithic semiotic systems: context, classification, and a semiotic analysis of a Franco-Cantabrian protolanguage. *Valcamonica Symposium l987: Prehistoric and primitive art*. Centro Camuno di Studi Preistorici, Valcamonica (Brescia). [JBH]

Harrod, J. 1987b. European Upper Paleolithic thematic semantics and the double goddess of Grimaldi. *Valcamonica Symposium l987: Prehistoric and Primitive Art*. Centro Camuno di Studi Preistorici, Valcamonica (Brescia). [JBH]

Harrod, J. B. 1992. Two million years ago: the origins of art and symbol. *Continuum* 2(1): 4–29. [JBH]

Harrod, J. 2002a. Notes on Early Acheulian stone tools: constitutive operations and analogies of the soul. *http://www.OriginsNet.org*. [JBH]

Harrod, J. 2002b. Notes on Middle Acheulian spirituality: stone tool logic structures and analogies of the soul. *http://www.OriginsNet.org*. [JBH]

Harrod, J. 2003. Five Later Acheulian symbolic traditions: interpretation and decoding of palaeoart and palaeoreligion (500 000 to 100 000 BP). *http://www.OriginsNet.org*. [JBH]

He, S. and D. I. A. Macleod 2001. Orientation-selective adaptation and tilt after-effect from invisible patterns. *Nature* 411: 473–6. [DH]

Hélèna, P. 1963. L'art figuré du paléolithique ancien dans la région narbonnaise. In Bosch-Gimpéra (ed.), *A Pedro Bosch-Gimpéra, en el septuagésimo aniversario de su nacimiento* pp. 189–193. Mexico City. [JBH]

Henshilwood, C., F. d'Errico, R. Yates, Z. Jacobs, C. Tribolo, G. A. T. Duller, N. Mercier, J. C. Sealy, H. Valladas, I. Watts and A. G. Wintle 2002. Emergence of modern human behavior: Middle Stone age engravings from South Africa. *Science* 295: 1278–80. [DH]

Hodder, I. (ed) 2002. *Archaeology theory today*. Polity Press, Cambridge. [TS]

Hodgson, D. 2000a. Art, perception and information processing: an evolutionary perspective. *Rock Art Research* 17: 3–34.

Hodgson, D. 2000b. Shamanism, phosphenes, and early art: an alternative synthesis. *Current Anthropology*. 41: 866–73. [DH]

Hovers, E. 1990. Art in the Levantine Epi-Palaeolithic: an engraved pebble from a Kebaran site in the lower Jordan valley. *Current Anthropology* 31: 317–22.

Hovers, E., B. Vandermeersch and O. Bar-Yosef 1997. A Middle Palaeolithic engraved artefact from Qafzeh Cave, Israel. *Rock Art Research* 14: 79–87.

Howell, F. C. 1966. Observations of the earlier phases of the European Lower Palaeolithic. *American Anthropologist* 68(2): 88–201.

Huyge, D. 1990. Mousterian skiffle? Note on a Middle Palaeoli-thic engraved bone from Schulen, Belgium. *Rock Art Research* 7: 125–32.

Huyge, D. 1998. Possible representations of Palaeolithic fish-traps in Upper Egyptian rock art. *Rock Art Research* 13: 3–11.

Hyman, M., K. Sutherland, M. W. Rowe, R. A. Armitage and J. R. Southon 1999. Radiocarbon analyses of rock paintings: Hueco Tanks, Texas. *Rock Art Research* 16: 75–88.

Iakovleva, L. and G. Pincon 1998. The Upper Palaeolithic sculptured ibex of Angles-sur-l'Anglin, France. *Oxford Journal of Archaeology* 17(3): 257–68. [WB]

Imbrie, J., J. D. Hays, D. G. Martinson, A. McIntyre, A. C. Mix, J. J. Marley, N. G. Pisias, W. L. Prell and N. J. Shackleton 1984. The orbital theory of Pleistocene climate: support from a revised chronology of the marine $\delta^{18}0$ record. In A. L. Berger, J. Imbrie, J. Hays, G. Kukla and B. Saltzman (eds), *Milanko-vitch and climate, Part 1*, pp. 269–305. Reidel, Dordrecht.

Ingold, T. 2002. Culture and human nature: an obituary notice. Paper presented at the Ninth International Conference on Hunting and Gathering Societies, Heriott-Watt University, Edinburgh, Scotland. Also available at *http://www.abdn.ac.uk/chags9/1Ingold.htm* [AB]

Inskeep, R. 1962. The age of the Kondoa rock paintings in the light of recent excavations at Kisese II rock shelter. *Actes du IV Congrès Panafricain de Préhistoire et de l'Étude du Quaternaire*, pp. 249–256.

Jasiewicz, Z. and A. Rozwadowski 2001. Rock paintings—wall paintings: new light on art tradition in central Asia. *Rock Art Research* 18: 3–14.

Jones, N. 1940. Bambata Cave: a reorientation. *Occasional Papers of the National Museum of Southern Africa* 1(12): 11–28.

Jones, P. 1981. Experimental implement manufacture and use; a case study from Olduvai Gorge, Tanzania. *Philosophical Transactions Royal Society of London* B(292): 189–95. [SWE]

Jones, R. (ed.) 1985. *Archaeological research in Kakadu National Park.* Australian National Parks and Wildlife Service, Canber-ra.

Klein, K. 1978. Preliminary analysis of the mammalian fauna from the Redcliff Stone Age cave site, Rhodesia. *Occasional Papers, National Museum of Southern Rhodesia* A4(2): 74–80.

Klein, R. G. 2000. Archeology and the evolution of human behaviour. *Evolutionary Anthropology* 9: 17–36. [AB]

Klein, R. and B. Edgar 2002. *The dawn of human culture*. John Wiley & Sons, New York. [AB]

Knight, C. C. Power and I. Watts 1995. The human symboling revolution: a Darwinian account. *Cambridge Archaeological Journal* 5: 75–114.

Kozlowski, J. K. 1992. *L'Art de la préhistoire en Europe orienta-le*. CNRS, Paris.

Kohl, P. L. and C. Fawcett (eds) 1995. *Nationalism, politics, and the practice of archaeology*. Cambridge University Press, Cambridge.

Ksica, M. 1973. Felsbilder in der Sovietunion IV. *Anthropologie* 11: 145–87.

Ksica, M. 1984. *Vypravy za pravekym umenim*. Obzor, Bratislava.

Kubarev, V. D. 1997. O petroglifakh Kalguty. *Naskal'noe iskusstvo azii*, pp. 88–97. Kyzbassvuzizdat, Kemerovo.

Kubler, G. 1962. *The shape of time: remarks on the history of things* (13th edn). Yale University Press, New Haven and London. [KKC]

Kuckenburg, M. 1997. *Lag Eden im Neandertal? Auf der Suche nach dem frühen Menschen*. Econ Verlag, Düsseldorf and München.

Kuckenburg, M. 2001. *Als der Mensch zum Schöpfer wurde. An den Wurzeln der Kultur*. Klett-Cotta, Stuttgart.

Kuhn, T. S. 1962. *The structure of scientific revolutions*. University of Chicago Press, Chicago. [JF]

Kumar, G. 1996. Daraki-Chattan: a Palaeolithic cupule site in India. *Rock Art Research* 13: 38–46.

Kumar, G. 1998. Morajhari: a unique cupule site in Ajmer district, Rajasthan. *Purakala* 61–4. [GK]

Kumar, G. 2000/01. Chronology of Indian rock art: a fresh attempt. *Purakala* 11/12: 5–36. [GK]

Kumar, G. 2002. EIP Project Report-I: archaeological excavation and exploration at Daraki-Chattan: a preliminary report. *Purakala* 13: 5–20. [GK]

Kumar, G. and R. G. Bednarik 2002. The quartz cupules of Ajmer, Rajasthan. *Purakala* 13: 45–50. [GK]

Kumar, G., R. G. Bednarik, A. Watchman, R. G. Roberts, E. Lawson and C. Patterson 2003. 2002 progress report of the EIP Project. *Rock Art Research* 20: 70–1.

Kumar, G. and M. Sharma 1995. Petroglyph sites in Kalapahad and Ganesh Hill: documentation and observations. *Purakala* 6: 56–9.

Kumar, G., G. Narvare and R. Pancholi 1988. Engraved ostrich eggshell objects: new evidence of Upper Palaeolithic art in India. *Rock Art Research* 5: 43–53.

Lahr, M. M. and R. A. Foley 1994. Multiple dispersals and modern human origins. *Evolutionary Anthropology* 3(2): 48–60. [AB]

Lahr, M. M. and R. A. Foley 1998. Towards a theory of modern human origins: geography, demography and diversity in recent human evolution. *Yearbook of Physical Anthropology* 41: 137–76. [AB]

Laidler, P. W. 1933. Dating evidence concerning the Middle Stone Ages and a Capsio-Wilton culture, in the South-East Cape. *South African Journal of Science* 30: 530–42.

Leakey, L. S. B. 1958. Recent discoveries at Olduvai Gorge, Tanganyika. *Nature* 19: 1099–1103.

Leakey, M. D. 1971. *Olduvai Gorge. Vol. 3: excavations in Beds I and II, 1960–63*. Cambridge University Press, Cambridge.

Leonardi, P. 1988. Art Paléolithique mobilier et pariétal en Italie. *L'Anthropologie* 92: 139–202.

Lewis-Williams, J. D. and T. A. Dowson 1988. The signs of all times. Entopic phenomena in Upper Palaeolithic art. *Current Anthropology* 29: 201–45. [DH] [JF]

Li Fushun 1992. A survey on the methods of dating rock art in China. *International Newsletter on Rock Art* 2: 15–20.

Liu Yiqing 1991. New discovery and historical division of the petroglyphs in Helan Mountains. Paper presented to the International Conference of Rock Art Study, Recording and Conservation, Yinchuan, China.

Loendorf, L. 1986. A radiocarbon date at the Rochester Creek site, Utah. *La Pintura* 12(3): 8–9, 17–18.

Lorblanchet, M. 1989. From man to animal and sign in Palaeo-lithic art. In H. Morphy (ed.), *Animals into art*, pp. 109–143. Unwin Hyman, London. [WB]

Lumley, H. de 1966. Les fouilles de Terra Amata à Nice. Premiers résultats. *Bulletin du Musée d'Anthropologie Préhistori-que*

de Monaco 13: 29–51.

McBrearty, S. 2001. The Middle Pleistocene of East Africa. In L. Barham and K. Robson-Brown (eds), *Human roots: Africa and Asia in the Middle Pleistocene*, pp. 81–92. Western Academic and Specialist Press, Bristol.

McBrearty, S. and A. S. Brooks 2000. The revolution that wasn't: a new interpretation of the origin of modern human behavior. *Journal of Human Evolution* 39(5): 453–563. [AB]

Macintosh, N. W. G. 1977. Beswick Creek Cave two decades later: a reappraisal. In P. J. Ucko (ed.), *Form in indigenous art*, pp. 191–197. Australian Institute of Aboriginal Studies, Canberra.

Mania, D. and U. Mania 1988. Deliberate engravings on bone artefacts of *Homo erectus*. *Rock Art Research* 5: 91–107.

Marr, D. 1981. *Vision*. Freeman and Sons, San Francisco. [DH]

Marshack, A. 1972. Cognitive aspects of Upper Paleolithic engraving. *Current Anthropology* 13: 445–77. [TW]

Marshack, A. 1976. Some implications of the Paleolithic symbolic evidence for the origin of language. *Current Anthropology* 17: 274–82.

Marshack, A. 1979. Upper Paleolithic symbol systems of the Russian Plain: cognitive and comparative analysis. *Current Anthropology* 20: 271–311.

Marshack, A. 1981. On Paleolithic ochre and the early uses of color and symbol. *Current Anthropology* 22: 188–91.

Marshack, A. 1985. *Hierarchical evolution of the human capacity: the Paleolithic evidence*. American Museum of Natural History, New York. [TW]

Marshack, A. 1989. Methodology in the analysis and interpretation of Upper Palaeolithic image: theory versus contextual analysis. *Rock Art Research* 6: 17–38.

Marshack, A. 1991. The Tai plaque and calendrical notation in the Upper Palaeolithic. *Cambridge Archaeological Journal* 1: 25–61. [TW]

Marshack, A. 1996. A Middle Palaeolithic symbolic composition from the Golan Heights: the earliest known depictive image. *Current Anthropology* 37: 357–65.

Marshack, A. 1997. The Berekhat Ram figurine: a late Acheulian carving from the Middle East. *Antiquity* 71: 327–37.

Martin, H. 1907–10. *Récherches sur l'évolution du Moustérien dans le gisement de la Quina (Charente). Industrie osseuse*, Vol. 1. Schleicher Frères, Paris.

Mason, R. 1988. *Cave of Hearths, Makapansgat, Transvaal*. Occasional Paper 21, Archaeological Research Unit, University of the Witwatersrand.

Matthes, W. 1964/65. Die Entdeckung der Kunst des älteren und mittleren Paläolithikums in Norddeutschland. *Jahrbuch für Prähistorische und Ethnographische Kunst (JPEK)* 21: 1–18. [JBH]

Matthes, W. 1969. *Eiszeitkunst im Nordseeraum*. Otterndorf. [RGB]

Mellars, P. and J. Tixier 1989. Radiocarbon-accelerator dating of Ksar 'Akil (Lebanon) and the chronology of the Upper Palaeolithic sequence in the Middle East. *Antiquity* 63: 761–8.

Miller, G. H., P. B. Beaumont, A. S. Brooks, H. J. Deacon, P. E. Hare and A. J. T. Jull 1999. Earliest modern humans in South Africa dated by isoleucine epimerization in ostrich eggshell. *Quaternary Science Reviews* 18: 1537–48.

Misra, V. D. 1977. *Some aspects of Indian archaeology*. Prabhat Prakashan, Allahabad.

Molodin, V. I. and D. V. Cheremisin 1993. *Drevneïshie petroglify Altaya*. Obozrenie, Novosibirsk.

Molodin, V. I. and D. V. Cheremisin 1994. Rock art of the Ukok Plateau. *International Newsletter on Rock Art* 8: 10–12.

Moog, F. 1939. Paläolithische Freilandstation im Älteren Löß von Wyhlen (Amt Lörrach). *Badische Fundberichte* 15: 36–52.

Morse, K. 1993. Shell beads from Mandu Mandu Creek rock-shelter, Cape Range Peninsula, Western Australia, dated before 30 000 b.p. *Antiquity* 67: 877–83.

Mountain, J. L. and L. L. Cavalli-Sforza 1997. Multilocus genotypes, a tree of individuals, and human evolutionary history. *American Journal of Human Genetics* 61: 705–18. [AB]

Muheisen, M. 1988. The Epipaleolithic phases of Kharaneh IV. In A. N. Garrard and G. H. Gebel (eds), *The prehistory of Jordan*, pp. 353–367. British Archaeological Reports International Series 396, Oxford.

Musch, J. E. 1987. Beestachtig en Beregoed (deel 1). *Archaeologi-sche Berichten* 18: 108–129. [JBH]

Muzzolini, A. 1990. The sheep in Saharan rock art. *Rock Art Research* 7: 93–109.

Neumayer, E. 1983. *Prehistoric Indian rock paintings*. Oxford University Press, Oxford.

Neumayer, E. 1993. *Lines on stone. The prehistoric rock art of India*. Manohar, Delhi.

Noble, W. and I. Davidson 1996. *Human evolution, language and mind: a psychological and archaeological inquiry*. Cambridge University Press, Cambridge. [TW]

Novgorodova, A. E. 1983. *Arshan-Khad — drevneïshiï pamyatnik izobrazitel'nogo iskusstva Vostochnoï Mongolii*. Istoriya I kul'tura Tsentral'noï Azii.

Oakley, K. 1972. Skill as a human possession. In S. L. Washburn and P. Dolhinow (eds), *Perspectives on human evolution*, pp. 14–50. Holt, Rinehart, and Winston. New York. [SWE]

Oakley, K. P. 1973. Fossil shell observed by Acheulian man. *Antiquity* 47: 59–60. [JF]

Oakley, K. P. 1981. Emergence of higher thought, 3.0–0.2 Ma B.P. *Philosophical Transactions of the Royal Society of London* B 292: 205–11.

Okladnikov, A. P. 1959. *Shishkinsie pisanitsi*. Irkutsk.

Okladnikov, A. P. 1977. *Petroglify verkhnei Leny*. Izdatel'stvo 'Nauka', Leningrad.

Okladnikov, A. P. and W. D. Saporoshskaya 1959. *Lenskije pisanitsi*. Izdatel'stvo Academii Nauk SSSR, Moscow/Leningrad.

Oliva, M. 1996. Le Paléolithique supérieur de la république Tchèque (1991–1995). In M. Otte (ed.), *Le Paléolithique supérieur européen: bilan quinquennial 1991–1996*, pp. 115–129. UISPP 1996, Kommission 8, Band 76, Liège.

Oster, G. 1970. Phosphenes. *Scientific American* 222(2): 83–7. [RGB]

Parenti, F. 1993. Le gisement préhistorique du pléistocène supérieur de Pedra Furada (Piauí, Brésil). Considérations chronostratigraphiques et implications paléoanthropologi-ques. *Documents Laboratoire Géologique Lyon* 125: 303–13.

Parkman, E. B. 1992. Toward a Proto-Hokan Ideology. In S. Goldsmith, S. Garvie, D. Selin, and J. Smith (eds), *Ancient images, ancient thought: the archaeology of ideology*, pp. 365–370. Proceedings of the 23rd Annual Chacmool Conference, University of Calgary.

Pei, W. C. 1931. Notice of the discovery of quartz and other stone artifacts in the lower Pleistocene hominid-bearing sediments of the Choukoutien cave deposit. *Bulletin of the Geological Society of China* 11(2): 109–46. [SWE]

Pennisi, E. 1999. Genetic study shakes up Out of Africa Theory. *Science* 283: 1828.

Pettitt, P. and P. Bahn 2003. Current problems in dating Palaeoli-thic cave art: Candamo and Chauvet. *Antiquity* 77: 134–41. [LGS]

Peyrony, D. 1934. La Ferrassie. *Préhistoire* 3: 1–92.

Piaget, J. and B. Inhelder 1967. *The child's conception of space* (transl. F. Langlon and J. Lunzer). Norton, New York. [TW]

Pinker, S. 1998. *How the mind works*. William Morrow, New York. [DH]

Rabinovich, R. and D. Nadel 1994. Bone tools from Ohalo II—a

morphological and functional study. *Journal of the Israel Prehistoric Society* 26: 32–63.

Ramachandran, V. S. and W. Hirstein 1999. The science of art: a neurological theory of aesthetic experience. *Journal of Consciousness Studies.* 6(6–7): 15–51. [DH]

Raynal, J.-P. and R. Séguy 1986. Os incisé acheuléen de Sainte-Anne 1 (Polignac, Haute-Loire). *RACF* 25: 79–80.

Ricks, M. F. and W. J. Cannon 1985. The Lake County, Oregon rock art inventory: a data base for rock art research. Paper presented to the American Rock Art Research Association Symposium, Santa Barbara.

Roberts, R. G., R. Jones and M. A. Smith 1990. Thermoluminescence dating of a 50 000-year-old human occupation site in northern Australia. *Nature* 345: 153–6.

Roberts, R. G., R. Jones and M. A. Smith 1993. Optical dating at Deaf Adder Gorge, Northern Territory, indicates human occupation between 53 000 and 60 000 years ago. *Australian Archaeology* 37: 58–9.

Roberts, R. G., M. Bird, J. Olley, R. Galbraith, E. Lawson, G. Laslett, H. Yoshida, R. Jones, R. Fullagar, G. Jacobsen and Q. Hua 1998. Optical and radiocarbon dating at Jinmium rock shelter in northern Australia. *Nature* 393: 358–62.

Robertson, T. 1999. Symbolic and notational expressions of central Texas Paleo-Indian culture: engraved artifacts of the Gault Site. *McNair Research Journal* 3: 123–36.

Roginsky, Y. Y., M. M. Gerasimov, S. N. Zamyatnin and A. A. Formozov 1954. Zaklyuchenie po nakhodke iskopaemogo cheloveka v peshchernoi stoyanke Starosel'e bliz g. Bakhchi-sary. *Sovetskaya Etnografiya* 1954: 39–41.

Ronen, A. and G. M. Barton 1981. Rock engravings on western Mount Carmel, Israel. *Quartär* 31/32: 121–137.

Russ, J., M. Hyman, H. J. Shafer and M. W. Rowe 1990. Radiocarbon dating of prehistoric rock paintings by selective oxidation of organic carbon. *Nature* 348: 710–11.

Sacks, O. W. 1999. *Migraine.* Revised and expanded edition. First Vintage Books Edition, Random House, New York. [JF]

Sauvet, G. and A. Wlodarczyk 1995. Éléments d'une grammaire formelle de l'art pariétal paléolithique. *L'Anthropologie* 99: 193–211. [WB]

Schick, K. D. and N. Toth 1993. *Making silent stones speak*. Simon and Schuster, New York. [SWE]

Shay, T. 2001. From real to abstract or vice versa—material culture and cultural beliefs. *Rock Art Research* 18: 101–5. [TS]

Singer, R. and J. Wymer 1982. The Middle Stone Age at Klasies River Mouth in South Africa. University of Chicago Press, Chicago.

Silverman, I., J. Choi, A. Mackewn, M. Fisher, J. Moro and E. Olshansky 2000. Evolved mechanisms underlying wayfin-ding: further studies on the hunter-gatherer theory of spatial sex differences. *Evolution and Human Behavior* 21: 201–13. [TW]

Sohn Pow-Key 1981. Inception of art mobilier in the Middle Palaeolithic period at Chommal Cave, Korea. In *Resumenes de Comunicaciones, Paleolítico Medio*, pp. 31–32. Xth UISPP Congress, Mexico City.

Stapleton, P. and J. Hewitt 1928. Stone implements from Howieson's Poort near Grahamstown. *South African Journal of Science* 25: 399–409.

Steelman, K. L., M. W. Rowe, V. N. Shirokov and J. R. Southon 2002. Radiocarbon dates for pictographs in Ignatievskaya cave, Russia: Holocene age for supposed Pleistocene fauna. *Antiquity* 76: 341–8.

Steguweit, J. 1999. Intentionelle Schnittmarken auf Tierknochen von Bilzingsleben — Neue lasermikroskopische Untersu-chungen. *Praehistoria Thuringica* 3: 64–79.

Steinbring, J., E. Danziger and R. Callaghan 1987. Middle Archaic petroglyphs in northern North America. *Rock Art Research* 4: 3–16, 150–61.

Stepanchuk, V. N. 1993. Prolom II, a Middle Palaeolithic cave site in the eastern Crimea with non-utilitarian bone artefacts. *Proceedings of the Prehistoric Society* 59: 17–37.

Straus, L. G. 1995. Comment on R. G. Bednarik, 'Concept-mediated marking in the Lower Palaeolithic'. *Current Anthropology* 36: 622–3. [RGB]

Strauss, E. 1999. Can mitochondrial clocks keep time? *Science* 283: 1435–8.

Tang Huisheng 1993. Theory and methods in Chinese rock art studies. *Rock Art Research* 10: 83–90.

Templeton, A. R. 1993. The 'Eve' hypothesis: a genetic critique and re-analysis. *American Anthropologist* 95: 51–72.

Templeton, A. R. 1994. 'Eve': hypothesis compatibility versus hypothesis testing. *American Anthropologist* 96: 141–7.

Templeton, A. R. 1996. Gene lineages and human evolution. *Science* 272: 1363.

Thorne, A., R. Grün, G. Mortimer, A. A. Spooner, J. J. Simpson, M. McCulloch, L. Taylor and D. Curnoe 1999. Australia's oldest human remains: age of the Lake Mungo 3 skeleton. *Journal of Human Evolution* 36: 591–612.

Tixier, J. 1974. Poinçon decore du Paléolithique supérieur a Ksar Akil (Liban). *Paléorient* 2: 187–93.

Tratebas, A. 1994. Paleo-Indian and Archaic petroglyphs on the Northern Plains. Paper presented to the International Rock Art Congress, University of Northern Arizona, Flagstaff.

Treisman, A. 1986. Features and objects in visual processing. *Scientific American* 255: 106–15. [DH]

Trigger, B. G. 1989. *A history of archaeological thought*. Cambridge University Press, Cambridge. [RGB]

Turner, W. and R. Reynolds 1974. The age dating of the Salton Sea petroglyphs. Paper presented to the International Congress of Americanists, Mexico City.

Turville-Petre, F. 1932. Excavations in the Mugharet el-Kebarah. *Journal of the Royal Anthropological Institute* 62: 271–6.

Tyagi, G. S. 1988. Comment on G. Kumar et al., 'Engraved ostrich eggshell objects: new evidence of Upper Palaeolithic art in India'. *Rock Art Research* 5: 49–50.

Valoch, K. 1987. The early Palaeolithic site Stránská skála I near Brno (Czechoslovakia). *Anthropologie* 25: 125–42.

Vértes, L. 1964. *Tata: eine mittelpaläolithische Travertin Siedlung in Ungarn*. Akadémiai Kiadó, Budapest.

Vértes, L. 1965. *Az öskökor és az tmeneti kökor emlékei Magyar-orsz gon*. Akadémiai Kiadó, Budapest.

Vishnyatsky, L. B. 1994. 'Running ahead of time' in the development of Palaeolithic industries. *Antiquity* 68: 134–40.

Volman, T. 1984. Early prehistory of southern Africa. In R. Klein (ed.), *Southern African prehistory and palaeoenvironments*, pp. 169–220. Balkema, Rotterdam.

Wang Ningsheng 1984. An introduction to rock paintings in Yunnan Province (People's Republic of China). *Rock Art Research* 1: 75–90.

Wakankar, V. S. 1975. Bhimbetka—the prehistoric paradise. *Prachya Pratibha* 3(2): 7–29.

Wakankar, V. S. 1983. The oldest works of art? *Science Today* 20: 43–8.

Walker, N. J. 1987. The dating of Zimbabwean rock art. *Rock Art Research* 4: 137–49.

Watchman, A. 1989. Comment on Nobbs and Dorn. *Rock Art Research* 6: 65–6.

Watchman, A. 1992. Investigating the cation-ratio calibration curve: evidence from South Australia. *Rock Art Research* 9: 106–10.

Weidenreich, F. 1939. Did Peking man practice cannibalism? *Bulletin of the Geological Society of China* 19: 49–63.

Weidenreich, F. 1943. The skull of *Sinanthropus pekinensis*. *Palaeontologia Sinica* n.s. D(10): 1–292. [SWE]

Weinstein-Evron, M. and A. Belfer-Cohen 1993. Natufian figurines from the new excavations of the el-Wad Cave, Mt Carmel, Israel. *Rock Art Research* 10: 102–6.

Wendt, W. E. 1974. Art mobilier aus der Apollo 11 Grotte in Südwest-Afrika. *Acta Praehistorica et Archaeologica* 5: 1–42.

White, T. D. 1986. Cutmarks on the Bodo cranium: a case of prehistoric defleshing. *American Journal of Physical Anthropology* 69: 503–9. [SWE]

White, T. D. 2001. Once were cannibals. *Scientific American* August 2001: 50–5. [SWE]

Wreschner, E. E. 1980. Red ochre and human evolution: a case for discussion. *Current Anthropology* 21: 631–44.

Wylie, A. 2002. *Thinking from things*. University of California Press, Berkeley. [TS]

Wynn, T. 1979. The intelligence of later Acheulian hominids. *Man* 14: 371–492. [SWE]

Wynn, T. 1989. *The evolution of spatial competence*. University of Illinois Press, Urbana. [TW]

Wynn, T. 2002. Archaeology and cognitive evolution. *Behavioral and Brain Sciences* 25(3): 389–438. [TW]

Wynn, T. and F. Coolidge 2003. The role of working memory in the evolution of managed foraging. *Before Farming*. [TW]

Yakimov, V. P. 1980. New materials of skeletal remains of ancient peoples in the territory of the Soviet Union. In *Current argument on early man: report from a Nobel Symposium*, pp. 152–169. Pergamon Press, Oxford.

You Youzhu 1984. Preliminary study of a Palaeolithic bone engraving. *Kexue Tongbao* 29(1): 80–2.

Zeki, S. 1999. *Inner vision*. Oxford University Press. Oxford. [DH]

Ziegert, H. 1995. Das neue Bild des Urmenschen. *Uni hh forschung* 30: 9–15.

Zilhão, J. 1995. The age of the Côa valley (Portugal) rock-art: validation of archaeological dating to the Palaeolithic and refutation of 'scientific' dating to historic and proto-historic times. *Antiquity* 69: 883–901.

Zuechner, C. 1996. The Chauvet Cave: radiocarbon versus archaeology. *International Newsletter of Rock Art* 13: 25–7. [ΛΓΣ]