CURRENT ANTHROPOLOGY Volume 36, Number 4, August-October 1995

Concept-mediated Marking in the Lower Palaeolithic

by Robert G. Bednarik

Elucidation of the evolutionary context of the earliest production of humanly made, intentional markings is hampered by insufficient resolution in the relevant archaeological "record," failure to consider the effects of taphonomic selection on this "record," archaeological biases against evidence purported to relate to early cognitive capacities, inadequate familiarity of some protagonists with the available relevant evidence, archaeological conservatism predicated on the durability of unfalsifiable propositions, and a tendency to interpret lack of a particular class of evidence as denoting the historical absence of the phenomenon it is thought to represent. Contending with these and other formidable biases, I review the Lower Palaeolithic evidence that seems to permit glimpses of the cognitive and technological capacities of early hominids. I reject the general concept of abrupt transitions between the major phases of the Palaeolithic, arguing that such models can be accounted for by plateaus derived from taphonomy, methodology, dating techniques, and the archaeological penchant for creating cultural taxonomies. A more gradual evolution of cognitive faculties is favoured, beginning with the Lower Palaeolithic, for which concept-mediated marking is suggested.

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With the slow attrition of the New Archaeology's positivism it has once again become possible to consider the evidence proffered on the cognitive status of early hominids. Several developments have contributed to this academic climate: the reinstatement of the multiregional hypothesis of human origins with the fading of the "African Eve" hypothesis, post-processualism, dissatisfaction with deconstructionism, and taphonomic logic. Only a decade ago it was fashionable for innovative archaeologists to challenge any claim of cognitive sophistication for early humans. Concomitant with the ascendancy of empiricist determinism (interestingly enough, during a phase of waning support for determinist philosophy in other fields) we witnessed the denial of, for instance, hominid fire use, home bases, shelter structures, and social structures and the demotion of pre-Aurignacian hominids to the rank of scavengers rather than hunters. Higher cognitive functions, ritual, symbolism, and language became typical attributes of modern humans: they were seen as having appeared suddenly in western Europe at the time of the demise of the Neanderthals, and no precedents for them were perceived in the "archaeological record." Indeed, the ability to depict objects two-dimensionally was claimed to be a precondition for language, and hominids that lacked language (by virtue of lacking iconic depiction) were said to belong to the apes rather than the humans (Davidson and Noble 1989, 1990).

Inevitably, the New Archaeology had focused on the Middle-to-Upper Palaeolithic transition. Since the occurrence of what seemed to be symbolic evidence became more frequent in western Europe at that time and apparently coincided with many other technological improvements and innovations, as well as with the appearance of modern humans and an altogether more recent-looking tool kit, an explosion-like cultural revolution at the interface between the Middle and Upper Palaeolithic was perceived:

> Clear consensus exists that there was an explosion of art and ornamentation at the beginning of the European Upper Palaeolithic, even if isolated earlier examples have been proposed. [White 1989:366]

> Aurignacian body ornamentation explodes onto the scene in southwest France during the early Aurignacian (i.e. between 35 000 and 33 000 BP). It appears to have been complex conceptually, symbolically, technically and logistically right from the very beginning. . . . This sudden, intrusive, and complex character of the earliest body ornamentation remains one of the greatest explanatory challenges in all of hominid evolution. [White 1989:385; see also, e.g., 1992, 1993*a*]

It is therefore necessary to examine this idea before moving on to more important issues. The Middle-to-Upper Palaeolithic transition is supposed to be marked by the proliferation of prismatic blades, extensive use of bone, antler, and ivory, and the advent of art, ritual, and symboling. Traditionally it was also believed to coincide

with the appearance of modern humans, but this notion has been abandoned since the discovery of pre–Upper Palaeolithic anatomically modern humans outside Europe. This perceived cultural barrier between the Middle and the Upper Palaeolithic has hindered a gradualist view of the cognitive evolution of hominids. Without first breaking through this barrier it will be difficult fairly to consider the evidence for early concept-mediated marking presented below.

Just as the ideas that pottery, domestication, and ground-stone tools are Neolithic innovations and that microliths mark the Mesolithic have been found to be misleading taxonomic pigeonholes, the much earlier "transition" has proved illusory. Stone was ground and clay fired by 25,000–30,000 B.P. in Europe and Australia, and decorated pottery vessels were produced in Pleistocene Japan. Microliths are found in the Howieson's Poort phase of southern Africa, together with engraved ostrich eggshell, in a Middle Palaeolithic industry of probably over 70,000 years ago. A microlithic tradition existed in Matupi, Zaire, by about 40,000 B.P. (Van Noten 1977). The figurative paintings in Apollo 11 Cave, Namibia, date to the Middle Stone Age (Wendt 1974), and modern humans extend at least 100,000 years back in southern and eastern Africa. They occur similarly early in the Levant. The Middle Palaeolithic Aterian of northern Africa is associated with modern humans, not Neanderthals (e.g., at Djebel Irhoud), and the same applies to the Maghreb Mousterian (Hublin 1992) and many other Middle Palaeolithic industries. The Amudian (or Pre-Aurignacian) of North Africa and the Levant is a Neanderthal industry preceding the Mousterian and developed from the Acheulian, but its prismatic blade tools are Upper Palaeolithic in their mode of production (Rust 1950, McBurney 1967). The Levantine *Homo sapiens* of Skhūl and Qafzeh possessed a typical Middle Palaeolithic technology around 100,000 years ago, while the more recent Neanderthals continued to 40,000 B.P. at Amud B and elsewhere. Here the transition from Middle to Upper Palaeolithic technology was gradual, through industries such as those at Ksar Akil and Boker Tachtit (Marks 1983), roughly from 50,000 to 35,000 B.P., to the Ahmarian tradition. The Levantine Aurignacian then runs parallel with the latter industry (Gilead 1991). A similar pattern characterizes the so-called transition in Russia and the Ukraine. The Middle Palaeolithic industry at Starosel'e Cave in the Crimea, with two anatomically modern humans, seems to develop directly into the early Streletsian. The late Streletsian, with its Middle Palaeolithic roots, has provided more beads in just three burials at a single site (well over 13,000) than all of the world's Upper Palaeolithic sites taken together (Bednarik n.d.). By comparison, the Upper Palaeolithic Spitsyn culture, also of Russia, has yielded scarcely 20 beads and pendants. Offering a similar challenge to simplistic evolutionary expectations, ochre use is more frequent in the French Châtelperronian of the Neanderthals than in the contemporary French Early Aurignacian (Harrold 1989). At the upper end of the scale, J-J. Hublin has just reported Neanderthal remains with classic Mousterian tools from Zafarraya Cave in southern Spain, near Málaga, which are said to be less than 30,000 years old (P. Bahn, personal communication).

The diffuse "transition" pattern of Africa, the Levant, and eastern Europe continues in Siberia, China, and India (Bednarik n.d.). Similarly, in central Europe, the Szeletian apparently developed from the Middle Palaeolithic (Allsworth-Jones 1986; cf. 1993), as did other tool traditions of the region. Even in southwestern Europe itself, where this myth first developed, the Neanderthals of the Châtelperronian, who produced objects of ornamentation (which some think they must have scavenged from their "more evolved" cousins [White 1989, 1993*a*]), co-existed with the people producing Early Aurignacian tools. The latter are thought to have been anatomically modern humans, although there is no proof of this because we lack any human skeletal remains from that phase and possibly also lack any from the subsequent Aurignacian 1 (the early burials of Cueva Morín may be an exception). What is certain is that Neanderthals and Early Aurignacians occupied the same general region for at least 10,000 years. Finally, Australia was initially settled over 60,000 years ago by Middle Palaeolithic seafarers (Roberts, Jones, and Smith 1993) who produced rock art more than 45,000 years ago (Bednarik 1994*a*). Their tool tradition was superseded there only comparatively recently and persists in Tasmania, which nevertheless possesses rock art.

Summarizing this broad picture of the presumed transition from typical Middle to typical Upper Palaeolithic tool industries, we see that no part of the world then settled offers clear evidence of a separation. More "evolved" lithic assemblages (microliths, prismatic blades, edge-ground tools) appear early in many regions and are often succeeded by "less developed" ones. Artistic or other apparently non-utilitarian evidence begins before the advent of a fully Upper Palaeolithic mode of production at least in southern Africa, the Levant, India, Australia, Russia, central Europe, and western Europe (Bednarik 1994*a*). Even the original presenters of the notion that language ability must have been preceded by iconic depiction concede today that despite their presumed lack of depictive abilities the Middle Palaeolithic ocean navigators who settled Kefallinía (Kavvadias 1984), Timor, Sahul (Greater Australia [Birdsell 1977]), New Ireland, Buka Island, and several other islands must have possessed language (Noble and Davidson 1993). Any attempt to link the beginning of the Upper Palaeolithic with the appearance of anatomically modern humans is similarly doomed: the idea of ethnic separation based on perceived lithic traditions has always been naive.

Similarly, the idea that the advent of personal ornamentation coincides with that of the Aurignacian is attributable only to insufficient knowledge of the relevant material. Drilled animal teeth and other objects that are several times as old as that "transition" (up to 300,000 years [Bednarik 1992*a*:34]) have been known to exist for many decades. Moreover, the assumption that any human innovation commenced at the time from which we

have the earliest evidence of it is weak—though not as weak as the assumption that personal ornamentation was restricted to beads. Body decoration is widely reported ethnographically, but almost none of it would be archaeologically detectable (Bednarik 1993*a*, 1994*b*).

The purpose of this brief consideration of the strengths of the paradigm dominating most discussions of the Middle-to-Upper Palaeolithic "transition" (for more detailed discussions, see Bednarik 1992*a*, 1994*c*) is to show that a realistic debate about early hominid cognition has long been virtually impossible. Only a few years ago it would have been foolhardy to examine possible evidence of apparently concept-mediated surface markings from the Middle Palaeolithic, much less from the Lower Palaeolithic. In part this polarization was justified: the Pleistocene art record is rife with instances of misidentifications, hoaxes, and frauds, ranging from parietal paintings to figurines carved from genuine mammoth ivory and supposedly Azilian painted cobbles. I have examined some 700 misidentified portable Pleistocene markings from Asia alone (Bednarik 1993*b*). However, the frequency of fallacious claims is no justification for sweeping out with them the few instances we have of very early artlike phenomena. In fact, no forgeries of pre–Upper Palaeolithic art are known, and any researcher reporting such very early art or non-utilitarian evidence is aware of the hostile reception such material is likely to experience.

Rather than beginning with a hypothesis and then testing it, I propose to summarize some of the available evidence suggesting very early symboling and try to establish how much of a pattern in this evidence might emerge without a hypothesis. Theorizing is reserved for the conclusion.

The Bilzingsleben Finds

Processualists subject every claim of early cognitive evidence to severe criticism not in the interest of scientific rigour but in defense of significant biases. Debates about "cognitive evidence" are often marred by protagonists' lack of knowledge of the subject or ignorance of material published in languages other than French and, particularly, English. The finds from the Steinrinne at Bilzingsleben, Germany, for example, were initially published only in German (e.g., Behm-Blancke 1983), and most English-speaking commentators remained unaware of them until they were subjected to international debate in English (Mania and Mania 1988, with comments; cf. Bednarik 1992*b*, Jelínek 1993). One commentator initially accepted the authenticity of the Bilzingsleben markings (Davidson 1988) but questioned their claimed age; after visiting the find site and examining the material he conceded that he "was wrong to give the impression that [he] could attack the stratigraphic dating to the Holstein interglacial complex" (Davidson 1990) but now argued that the markings were incidental cut marks. These are just a few examples of the kind of controversy that early cognitive evidence tends to attract.

Only four marked objects from Bilzingsleben have been reported in English. The Steinrinne is one of the largest excavated Lower Palaeolithic sites in the world. The finds are exceptionally well preserved because of travertine precipitation from a mineral spring (Mania 1991*a*) and include large quantities of vegetal (Mai 1983) and malacological (Mania 1983) remains. Possibly about 350,000 years old (Schwarcz et al. 1988), the excavated 1,000-square-metre occupation floor shows distinctive patterning of activity areas (Mania 1991*b*), including apparent dwelling remains (Mania and Weber 1986, Mania and Vlček 1987). Twenty-five cranial fragments (Bednarik 1993*c*) and seven molars are said to be of a late *H. erectus*, and the well over 100,000 recovered artefacts include polished ivory points, wooden staffs, and a series of incised objects. The animal remains feature a high proportion of large mammals, especially rhinoceros (26.6% of mammalian individuals), with extensive evidence of systematic butchering. The marked objects are bones from large animals (Mania and Mania 1988).

Object 1, made from the flat spall of the tibia of a straight-tusked elephant (*Palaeoloxodon antiquus*), was used as a percussion tool. Its narrow side bevel bears a series of straight lines incised with a stone tool (fig. 1, *a*). The lines are evenly cut and evenly spaced, forming two structured sets occupying most of the bevel surface. It has been argued that they may have been produced incidentally when the artefact served as a cutting board, perhaps for cutting leather thongs. This explanation overlooks the fact that it is the flat (but unmarked) upper surface and not the side bevel that would have been used for this purpose. The markings, neatly spaced and arranged, are intentional, and it seems that they would be accepted as non-utilitarian—that is, as activity traces for which a utilitarian explanation does not seem to suffice—in any Upper Palaeolithic context. Because we do not yet know whether the people concerned had the capacity to create non-utilitarian (or "decorative") markings, their rejection as such is illogical.

Object 2 is the distal part of a flat rib bearing a series of identically angled parallel incisions (fig. 1, *b*). Each of the marks consists of three separate applications of the engraving tool, suggesting a careful, intentional procedure. The object is not suitable for a cutting board, and no convincing utilitarian explanation for the marks or their consistent arrangement has been offered. Object 3, a retouched artefact used for woodworking, is again a piece of elephant bone. It bears a series of long convergent lines of extraordinary straightness (figs. 1, *c*, and 2); they are evenly engraved, and multiple applications of a particular tool point with two minute projections are evident (Bednarik 1988:fig.1). Object 4 is a flat piece of bone with a series of more random cut marks, all of which seem to have been made by the same stone implement (fig. 1, *d*).

In addition to these four artefacts, a number of other marked objects have been excavated at Bilzingsleben. One of these is a large polished ivory point made from a split elephant tusk (fig. 3, *a*). The site has yielded numerous examples of large bone and ivory artefacts ex-

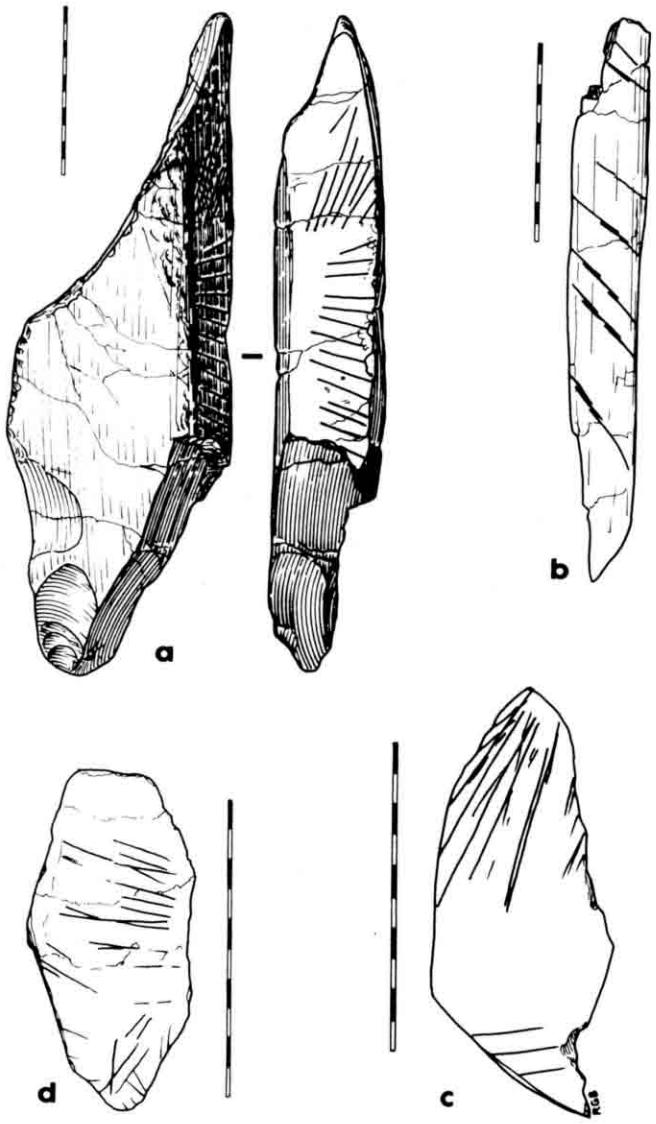

FIG. 1. *Engravings on bone objects from the Steinrinne, Bilzingsleben, associated with skeletal remains possibly of* Homo erectus. a–d, *objects 1–4. Scales 10 cm.*

FIG. 2. *Internal analysis of Bilzingsleben object 3.*

pertly split with wedges. The 65-cm-long ivory point is incompletely preserved and was perhaps used as a thrusting or piercing weapon or as a lance point on which a charging large animal would impale itself. Its size suggests that it may have been lashed onto a wooden staff. It bears two parallel arcs about 30 mm in diameter engraved into its polished surface. An apparently non-utilitarian intentional marking occurs also on a 45-cm-long quartzite slab (fig. 3, *b*). It is a well-executed elongated D-shape 35 mm long and 25 mm wide. The marking was engraved with a number of strokes, and it seems that the artisan experienced problems in shaping the curved part of the figure symmetrically, correcting the line several times in the process. To my mind this clearly indicates intentionality. Moreover, the nature of the material renders implausible the assumption that the slab was a cutting board: no Palaeolithic person would have damaged a stone tool by using a quartzite support. Finally, on the concave surface of a complete metatarsal bone, again of *P. antiquus*, a very complex arrangement has been incised (fig. 3, *c*). Neither the structure of the marking nor its relationship to its support suggests a utilitarian origin. The bone is hardly suitable as a cutting board, and no alternative explanation has been offered for the marks. Any random arrangement of superimposed cut marks will of course result in some sort of geometric pattern, but in the case of this arrangement it seems far-fetched to explain its geometry as fortuitous. There is an apparent relationship between the spatial distribution of the incisions and the borders of the available area which random markings would not be expected to reflect, and the marking strategies apparent seem to indicate a sophisticated level of intentionality. If, as seems likely, this arrangement was indeed made intentionally, then it represents significantly more advanced concept-mediated marking than has been attributed to these hominids in the past.

By themselves, the Bilzingsleben markings may not

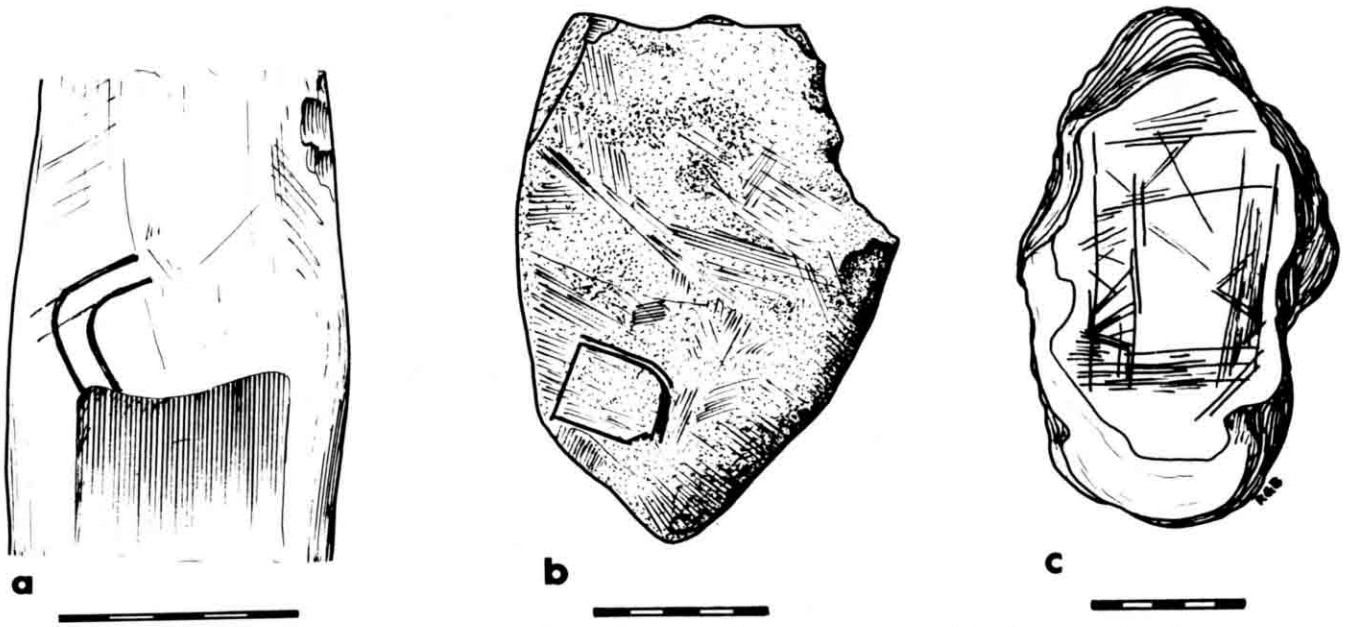

FIG. 3. *Engravings on further objects from Bilzingsleben.* a, *double arc on a polished ivory point;* b, *arcuate design on a quartzite slab;* c, *geometric arrangement on a metatarsal bone. Scales 5 cm.*

be sufficiently distinctive to present a strong case for concept-mediated marking, but they would be of interest in this respect if more conclusive evidence from the period were available elsewhere.

Other European Engravings of the Lower Palaeolithic

One more incised elephant bone has been recovered together with supposed *H. erectus* remains at Stránská skála, Czech Republic (Valoch 1987), a Central European site about 500 km from Bilzingsleben. It is an incomplete juvenile vertebra bearing a variety of markings, including seven radially arranged narrow incisions forming a fanlike design reminiscent of that on Bilzingsleben object 1 and four broad, shallow markings made with a different stone tool (fig. 4). In some of the latter, very fine parallel striations can still be discerned, and all the markings are clearly ancient (Valoch 1987:141). The seven radial lines are all wider at their upper ends, which together with their similar cross-sections suggests that they were made in a single sequence.

The Stránská skála lithic industry, like that of Bilzingsleben, contains virtually no bifaces. Several Acheulian assemblages have, however, produced incised markings that some researchers regard as non-utilitarian. An early example is the marked aurochs rib from Pech de l'Azé, France, first described by Bordes (1969) and later examined by Marshack (1977). It bears a surprisingly complex arrangement of curved lines, including what appear to be separately cut deep subparallel marks and a series of shallow V-shaped marks. The object's markings have attracted some controversy, primarily because for many years they were the only allegedly intentional engravings of their age. Now that other evidence of cognitive abilities at 300,000 years ago has begun to emerge, this object deserves reassessment. The markings on it are obviously incomplete because of fracture, but the inclusion of conjunctive elements and the joining of subparallel elements might be seen as suggesting intentionality in their production.

The pre–Upper Palaeolithic markings reported from Italy include those on a limestone cobble from Grotta dell'Alto (Leonardi 1988:fig. 1), which could be either Acheulian or Mousterian. However, they may well be incidental or taphonomic marks. Some of the Mousterian specimens Leonardi presents are considerably more convincing. Among them are the densely and deeply in-

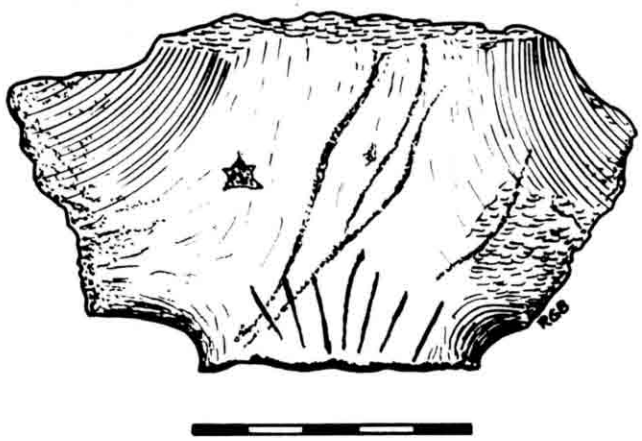

FIG. 4. *Engravings on a juvenile elephant vertebra associated with* H. erectus, *Stránská skála. Scale 5 cm.*

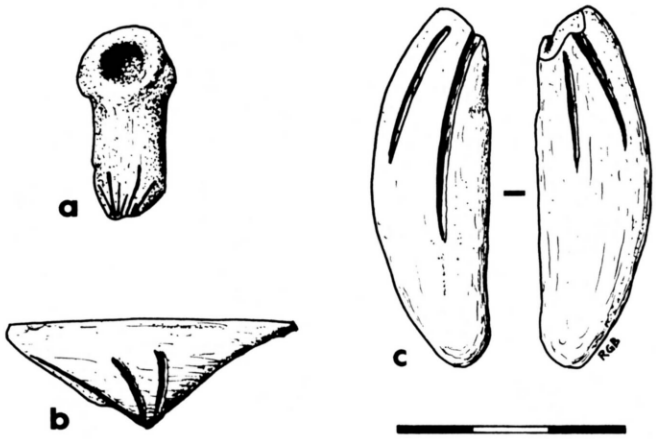

FIG. 5. *Engravings from the Micoquian of Prolom 2.* a, *a modified phalanx;* b, *a bone splinter;* c, *a horse canine. Scale 3 cm.*

cised bone fragment from Valle Radice (1988:fig. 2*b*), three flint flakes with possibly engraved lines from the Tagliente shelter (1988:figs. 3, 8), and especially a limestone cobble from the same site with intentional engravings (1988:fig. 4). A curved double line engraved on a Mousterian bone fragment, again from the same site, resembles the markings on the Pech de l'Azé rib mentioned above (1988:fig. 7).

There is no shortage of Middle Palaeolithic engravings; they are of particular interest in the present context when they closely resemble those of the Lower Palaeolithic. In particular, I draw attention to the recent Micoquian finds from Prolom 2, on the Crimean peninsula (Stepanchuk 1993). The four occupation layers contained not only a total of 111 perforated animal phalanges but also 4 engraved objects: a first phalanx of *Saiga tatarica* bearing a set of seven convergent radial lines (fig. 5, *a*), two bone splinters with engravings, one of which again bears a distinctive set of convergent lines (fig. 5, *b*), and a horse canine with five deeply engraved lines, four of which form two convergent pairs (fig. 5, *c*). Other types of engraved arrangements from the European Middle Palaeolithic are the crosses found on both sides of the silicified nummulite from Tata, Hungary (Vértes 1964), the deeply carved zigzag patterns on a bone fragment from Bacho Kiro, Bulgaria (Marshack 1976), and several bone fragments with parallel engraved lines or paired markings from sites in France, Belgium, and Spain (Bednarik 1992*a*:36–37).

Some Non-European Evidence

Among the Acheulian finds from the Levant for which some non-utilitarian function has been proposed is the scoria pebble from Berekhat Ram (Goren-Inbar 1986). Found in an occupation deposit sandwiched between two roughly dated basalt flows, the 35-mm-long object is apparently older than 233,000 years and younger than 800,000 years. It bears several distinct grooves which are patinated like the rest of the surface but considered to have been man-made. They emphasize the pebble's natural shape, resembling that of a woman's head, arms, and upper torso, thus reinforcing the idea of their intentionality. The status of this find remains controversial because of its surface condition (see Pelcin 1994), but Marshack has examined it and found clear proof that it was carved (personal communication, August 1994).

Until recently, rock art was entirely lacking from the Lower Palaeolithic period, and the oldest known example of rock art remained the 18 cupules from La Ferrassie, France (Peyrony 1934). They were found on the underside of a large limestone slab placed over the grave of a Neanderthal child and presumably belong to the Mousterian (fig. 6). Australian cupules at several sites are also thought to be of an antiquity corresponding to the Middle Palaeolithic (Bednarik 1993*d*).

In 1990, petroglyphs were discovered in an Indian sandstone cave called Auditorium Cave at Bhimbetka, near Bhopal (Bednarik 1992*c*, 1993*b*). They include ten cupules and a long, meandering line. Nine of the cupules occur in a most conspicuous location above present floor level; the larger tenth cupule and the line marking were uncovered in the course of an archaeological excavation. All the figures, particularly those found below ground, are extremely corroded. The latter occur on a massive boulder well below a substantial Middle Palaeolithic occupation layer that is solidly cemented by carbonate precipitate, excluding the possibility of stratigraphic distur-

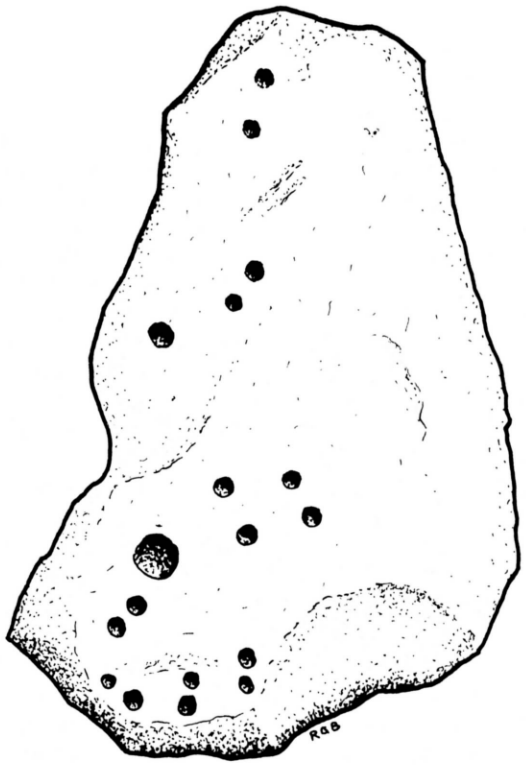

FIG. 6. *The 18 cupules on the limestone slab covering Neanderthal burial 6, La Ferrassie, 16 of which are arranged in pairs.*

bance. They are covered by the upper portion of substantial Acheulian strata, with bifaces, cleavers, and scrapers, overlying a pebble tool industry of choppers and scrapers. These Acheulian petroglyphs are by far the oldest rock art currently known. There is no dating available from Auditorium Cave itself, but the Indian Acheulian is generally of the same antiquity as that of Africa and Europe. Most attempts at thorium-uranium dating have shown this tradition in India to be beyond the limits of the method, 350,000 years (Mishra 1992). Exceptions are two dates of about 290,000 B.P. from animal molars (at Teggihalli and Sadab). Bhimbetka is close to the find site of the Narmada cranial fragment, which I have examined and regard as intermediate between erectoid and sapienoid finds but closer to the latter.

Rock art of the Middle Palaeolithic (or Middle Stone Age) occurs in many regions (Bednarik 1994*a*), although it seems to be abundant in only one, Australia. One Middle Stone Age layer, in Apollo 11 Cave in Namibia, has even yielded several figurative pictures (Wendt 1974), and there is ample evidence of portable engravings, decorations, and ochre markings of that period from various parts of Africa (see Bednarik 1994*a*). There are numerous portable markings from the Middle Palaeolithic of Eurasia. In Australia, a large proportion of the thousands of sites featuring archaic linear petroglyphs and cupules belongs to an essentially Middle Palaeolithic tool tradition introduced from Asia probably well over 60,000 years ago.

Should we be surprised by the very early appearance of petroglyphs? Lower and Middle Palaeolithic hominids used colouring matter, notably iron minerals, to mark surfaces of rock (and presumably other materials). Although, as might have been expected, none of their pigment markings have survived, in a few instances their crayons have been recovered and recognized (Marshack 1981, Bednarik 1990*a*). (More such "ochre" crayons are known from the pre–Upper Palaeolithic of four continents [see Bednarik 1994*a* for an incomplete list].) We have no idea why Acheulian hominids marked rocks or what these markings may have looked like. It is likely that they were capable of such activity, both physically and mentally: the production of the stone and wooden implements of the Acheulian demands considerable manual dexterity, and evidence of various types (spatial conceptualization, permanent home bases, capability of killing megafauna, evidence of non-utilitarian behaviour) suggests that the ability to mark rock surfaces would have been well within their cognitive capabilities. In any case, the evidence of striated wear facets on some haematite pebbles from Acheulian sites suggests that such activities took place even in the Early Acheulian.

It should be emphasized that Late Acheulian people were probably archaic *H. sapiens*, not *H. erectus*; there is little evidence of a connection between erectoid hominids and developed Lower Palaeolithic biface assemblages. Nevertheless, even late *H. erectus* made use of bone, antler, wood, and ivory artefacts, hunted and butchered the largest animals in their environment, split long bones and ivory tusks with wedges to make implements, divided semipermanent camps into activity areas, erected shelters, and used fire at several of the sites we know of. They also manufactured such sophisticated tools as the large polished ivory lance point from Bilzingsleben and probably the bone harpoon from Ngandong, and they made wooden implements. The Acheulians (as well as the Zhoukoudian 1 hominids) collected quartz crystals (Pei 1931; d'Errico, Gaillard, and Misra 1989; Bednarik 1992*a*:34; 1993*e*) and probably fossils (Oakley 1981), neither of them for utilitarian purposes (the crystals were often much too small for tool use). Among their manuports are also various colourful or oddly shaped pebbles, in some cases modified. Many hundreds of haematite or ochre manuports occur in the Acheulian sites of Africa, Europe, and Asia, and those from South Africa are thought to be up to 800,000–900,000 years old (P. B. Beaumont, personal communication; Bednarik 1994*a*). These hominids distinguished between ordinary and unusual or exotic object types: they had begun to classify the object world (the significance of this capacity is discussed in Bednarik 1992*d*), and they were undeniably using red pigment. We have no reason to assume that their level of encephalization was significantly less than that of more recent archaic *H. sapiens*; in fact it had begun to approach that of anatomically modern humans (Bradshaw and Rogers 1993:338–49). Since they are thought to have produced coloured rock markings, presumably non-utilitarian, and collected "aesthetic" objects, the only surprising thing about the archaic petroglyphs from Auditorium Cave is that we should have managed to locate them; the odds against this are considerable.

Some Taphonomic Considerations

The petroglyphs in Auditorium Cave are the only rock art we have found so far from the Lower Palaeolithic, and empiricists might perceive them as evidence of the earliest rock art. However, taphonomic reasoning renders this most unlikely: if the earliest available evidence of a phenomenon is the most deterioration-resistant manifestation of it (e.g., deeply carved cupules, in the case of rock art), it most likely reflects taphonomic truncation of the record rather than historical reality (Bednarik 1994*b*). In contrast to the Middle Palaeolithic/Middle Stone Age period, with its abundant evidence of non-utilitarian or symbolic behaviour (Bednarik 1992*a*, 1994*a*), there is a notable paucity of such traces from the Lower Palaeolithic. While Middle Palaeolithic markings and perforations have been reported throughout this century, no corresponding phenomena from the Lower Palaeolithic were known until 25 years ago. It is difficult to say whether this is attributable to taphonomy (Bednarik 1994*b*), a significantly lower incidence of symboling, or a combination of the two; most likely the last-mentioned is closest to the truth. Taphonomic selection can safely be assumed to be a realistic model for much of what the Pleistocene record provides us with in terms

of quantity, type, and distribution of evidence. The assumption of less symboling the farther back one goes in time is pure speculation, based not on logic or evidence but on a theoretical evolutionary premise reinforced by a century of confirmationist investigation. It is no doubt true that human cognition has evolved, but it is a fundamental error to assume that we know within what time frame that evolution has taken place. We do not know this, and there is no justification for assigning cognitive or intellectual levels to various hominids on the basis of inadequate or biased evidence.

The considerations pertaining to the taphonomy of palaeoart are very similar to those for other largely perishable evidence. For instance, strings, ropes, and thongs were no doubt used for much of the Palaeolithic, and the use of such materials suggests the use of knots. We have no direct evidence of knots from the Pleistocene and almost no remains of cordage (but see Leroi-Gourhan 1982, Nadel et al. 1994, Warner and Bednarik n.d.). Yet it is evident from Upper Palaeolithic figurines from Pavlov and Kostenki 1 that plaited or twined strings were worn and may have even possessed some symbolic significance (Marshack 1991). We have ample evidence from the Middle Palaeolithic for the hafting of stone tools (e.g., Mania and Toepfer 1973; Shea 1988, 1990), which probably involved strings and knots, and hundreds of Middle Palaeolithic perforated objects, which could have involved the use of cordage. Again, all of the available material evidence of the use of watercraft dates to no earlier than the beginning of the Holocene (with the finds of Holmgaard, Star Carr, and Pesse), as does most evidence of ocean fishing, whaling, sealing, and sea-birding. Yet even essentially Middle Palaeolithic people routinely navigated the high seas, eventually traversing up to 180 km of ocean (New Ireland to Buka Island), and shorter distances in nine earlier cases that we know of. The total lack of surviving watercraft (or associated material evidence) from the entire Pleistocene and the lack of any comprehensive evidence for a marine-based economy are not attributable simply to preservation biases but primarily the result of eustatic fluctuations which have left any evidence on the sea floor. This massive taphonomic effect has led to a severely distorted picture of *all* Pleistocene economies, restricting it to inland economies because all of the remains of former coastal ones are under water. In the case of sea-level rises, such effects are self-evident, but many other factors have similarly profound effects on the material evidence. Among them is the nature of find-site types in terms of their soils, deposition processes, and alteration and preservation processes superimposed on the nature of the material evidence itself. This results in an enormous bias in favour of certain types of sites (e.g., limestone caves, loess strata) combined with certain types of remains (e.g., bone, ivory). The primary information gleaned from this taphonomically distorted sample is valid as such, but it is not suitable for constructing sound ecological models of the societies in question without extensive recourse to taphonomic logic.

It follows that the composition, mode of occurrence, spatial distribution, and statistics of archaeological materials are not representative of the material culture of the society in question. They relate much more to complex taphonomic coefficients than to the cultural, technological, or social characteristics of the societies concerned. Among these coefficients (which determine the forms of evidence we use in model building) are manner of use and deposition, chemical, physical, and biological conditions at the site of deposition, preservation characteristics, geomorphological processes, archaeological methods of data recovery, scholarly biases and competence, the language in which the evidence is published, and a multitude of other factors. The severity of taphonomic distortion of archaeological evidence increases with its age. By the time we reach the Lower Palaeolithic, we can expect only very limited resolution in the record in terms of yielding valid models of interpretation. It would therefore be rash to make definite statements about the capabilities of these hominids. The record is far too coarse to justify such appraisal, and we do not have a valid picture of the cognitive, intellectual, even technological state of hominid society at that stage. Therefore the familiar response to new evidence—that we "know that these people were not capable of such-and-such, so there must be something wrong with the contrary data"—only betrays ignorance of what the record's rigorous interpretation can realistically be expected to yield. I will attempt a more holistic interpretation of the limited evidence we have from this period, by far the longest in human cultural evolution.

Early Concept-mediated Marking

In developing a model of the earliest hominid marking we first need to express serious reservations concerning not just the quality of the evidence offered but also the effects of the taphonomic distortion that we must expect applies to it. The only way in which we might attempt to compensate for this is by subjecting the available evidence to metamorphological projection. For this, we first need to establish what constitutes acceptable evidence, which in turn means that criteria for concept-mediated marking need to be stated.

I begin by considering a more recent find, the Tata nummulite (fig. 7). (Mineralized fossils were "curated" from the Acheulian onwards.) One of the lines on this partly translucent silicified fossil is a fracture visible on both sides and dividing it in half. The object must have attracted the attention of a Mousterian person, which suggests an appreciation of perfect geometric form or unusual material character. In adding an engraved line on each face at right angles to the natural line and so dividing the circle into four equal quarter-segments, the "artist" was clearly reacting to the natural line as well as to the circular outline: these are not randomly positioned marks. Not satisfied with the mere possession of an object with "aesthetic" qualities, the artist improved them, commented upon them. This is not to suggest

FIG. 7. *Silicified nummulite with natural and engraved lines, Mousterian, Tata.*

that "geometric comprehension" is the key to this issue or that there was "conscious" understanding of geometric principles. Nor are we concerned about Wittgensteinian ideas of internal representations or their grammars (Cooper 1982). Rather, we should be content with recognizing that an established tradition of "marking strategies" (Bednarik 1994*a*)—graphic solutions to issues of space that were not "invented" whenever the opportunity arose but already in place, both cognitively and intellectually, and not restricted to one individual—seems a probable presupposition. One is tempted to doubt that the artist merely turned the object round and round, thinking, "What could I possibly do with this thing?" until, by some stroke of genius, the solution appeared like a thunderbolt: "I would get a cross by adding a perpendicular second line in the middle of the first!" The artist *already* had a clear concept of "cross," having made or seen such marks before, and on seeing the dissected circular shape perceived the possibility of obtaining a cross with a single line. The level of conceptualization and intentionality is further underscored by the addition of a second line, duplicating the first, on the other face of the object.

The mere capacity for such a response suggests that it may have been the outcome of a long tradition of "geometric" conceptualization and marking and that there may be earlier evidence of less sophisticated "decoration" (though of course these marking strategies were not mere decoration). This does not in itself prove that such abilities existed in the Lower Palaeolithic (or that their products must have survived), but, as we have seen, further evidence for them is available. Moreover, that evidence is precisely what we would expect to find if we assumed a very slow and gradual evolution of those abilities. The Lower Palaeolithic markings we do have are considerably less sophisticated than those on the Tata nummulite. If they played a role in cognitive evolution, they should provide some evidence of being concept-mediated. The effect of a mental state is that it relates the organism to its environment through the notion of content. Insofar as the content links environment and behaviour, it is adaptive. There is a graduated differentiation between concept-mediated behaviour and mere stimulus generalization (D'Amato and van Sant 1988), the latter being common in non-human animals. Concepts, in this sense, enable organisms to generalize over variable stimuli, perceiving underlying common features. I have called this discrimination of entities in the object world *taxonomization* (Bednarik 1990–91, 1992*d*): the creation of essentially anthropocentric taxonomies to conceptualize physical reality as humans perceive it. The Tata hominids seem to have possessed a developed frame of reference that could not have been significantly less evolved than that of modern humans. Moreover, at the level of taxonomization we can reasonably attribute to them, "naming" and consequently some form of "reflective" communication, verbal or other, would have been available to them (I am not claiming that this involved spoken language, although that seems possible).

The question of intentionality (Grice 1957) is a rather more difficult one, but it is necessary to consider it if we are to illuminate the status of the Lower Palaeolithic markings. First, we need to recognize that some level of taxonomization was available to Acheulian hominids: they collected tiny crystals and apparently fossils and carried them to their home bases, presumably perceiving in them "aesthetic" or novel characteristics. Other manuports include unusual pebbles, some of which were engraved, and haematite or ochre pieces. Thousands of such "exotic" specimens occur; essentially non-utilitarian, they show that these hominids discriminated between unusual, aesthetic or non-utilitarian objects and common ones. We have seen that they used haematite to mark rock surfaces and produced the first known petroglyphs.

The engraved portable objects described above may add another dimension to the faculties of their makers, but to demonstrate intentionality we need to detect consistent patterns of graphic organization in these markings. This is not a question of complexity; we have seen that an extremely simple marking (two lines on the Tata specimen) can imply considerable cognitive complexity while a very complex pattern of incidental cut marks on a bone may signify no intentionality at all.

To facilitate a comparison, I have extracted the markings from some of the engraved specimens mentioned above (fig. 8). Not only is it difficult not to notice the distinctive marking strategies in this small series but there is even a hint of "graphic evolution" from the rather general "concept" of convergence of many lines in the older specimens to the more succinct expression of this principle, forming a true "motif." It could be argued that this apparent development from sets of unconnected lines to groups of lines intentionally joined represents a distinctive conceptual evolution and a refinement of concept-mediated marking. If this were a reflection of the cognitive capacities of the populations in question, it could tell us a great deal about their cognitive statuses and would lead to a number of deductions relating these to a variety of technological and other conditions. I am not convinced that such far-reaching deductions are warranted by so few specimens, but we should not lose sight of the possibility that they might be correct. What we can say with certainty is that

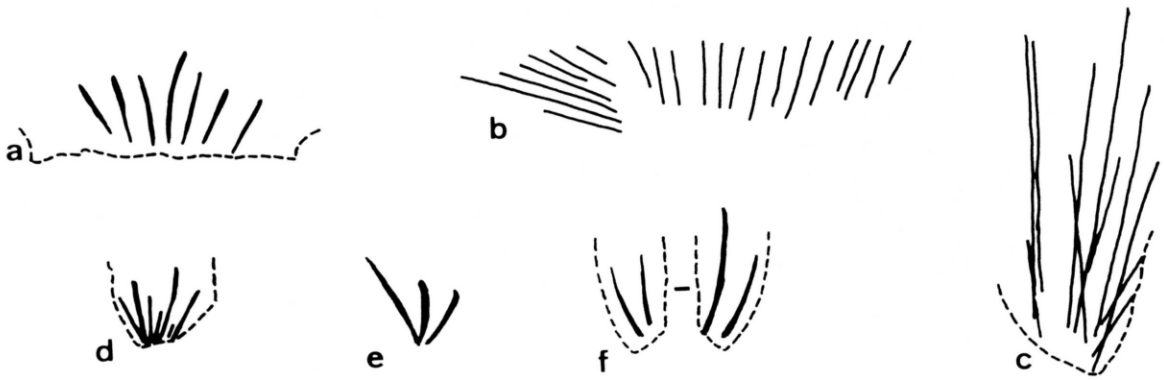

FIG. 8. *Engraved markings from Lower* (a–c) *and Middle* (d–f) *Palaeolithic contexts, showing consistencies in marking arrangements.*

this possibility seems to accord with what little we know about the societies in question. At the same time, we need to concede that the traditional opposition to early cognitive evidence is not particularly conducive to increasing our very small data base in this area.

The more recent markings of the late Mousterian, notably the Tata crosses and the Bacho Kiro zigzags, represent a distinctive increment in conceptual complexity that would be consistent with evolutionary expectations. We can also see continuity and evolution in the petroglyphs produced, those of the Acheulian of Auditorium Cave and of the Mousterian of La Ferrassie: the former cupule arrangement is unstructured, as far as we can tell, while in the latter the cupules are clearly arranged in pairs. Pairedness of markings is a conceptual hallmark of the late Mousterian (Bednarik 1992*a*:37) and early Aurignacian (White 1989), occurring on many portable objects (Bednarik 1994*a*) and eventually in rock paintings. It may even occur first in the Micoquian of Prolom 2: one of the engraved bone splinters bears two parallel marks, and four of the marks on the horse canine seem to indicate pairs.

There are still other consistencies in these early marking strategies. Most seem to be reactions to aspects of the form or shape of the surface decorated in their extent, orientation, and "focus." For instance, the sets of seven convergent lines on the Stránská skála vertebra and on the Prolom 2 phalanx both radiate from the object's end, as do the lines on the Prolom 2 tooth; the bundles of lines on Bilzingsleben objects 1 and 3 reflect the geometry of the support area, although not in the conceptually sophisticated way that the marks on the Tata nummulite do this. As I have noted before (e.g., Bednarik 1994*a*), all of the markings of the Lower and Middle Palaeolithic resemble modern doodling, which is spontaneous and subconscious. Contemporary doodling, the scientific value of which remains almost entirely ignored, could well have its neuropsychological roots in our early cognitive history.

Another consistency in early marking is that all the arrangements are recognizable as phosphene motifs: the convergent-lines motif so widespread in rock arts the world over and the structurally related radial motif (which includes the cross) are among the 15 phosphene motifs of modern humans (Kellogg, Knoll, and Kugler 1965); so are the sets of parallel lines, the dots (cupules), and the zigzags. Together with the phosphene types arc (which occurs at Bilzingsleben), circle, and spiral, these motif types account for practically all the motifs found in the Middle Palaeolithic petroglyphs of Australia (the archaic linear tradition), which number in the hundreds of thousands. This earliest large assemblage of rock art that we know of consists entirely of phosphene motifs, which has given rise to the phosphene theory (Bednarik 1984, 1987, 1990–91, 1994*a*). That theory remains unrefuted (despite being "eminently testable" [Chase and Dibble 1992:50]) and widely ignored, in contrast to the competing entoptic/shamanistic theory, which is still popular despite having been refuted. The arts engendering the most persistent use of phosphene motifs are certainly not those of shamans; they are those which are ontogenetically and phylogenetically the earliest (Bednarik 1990*b*). It seems likely that phosphene motifs are "signs of all times" (Lewis-Williams and Dowson 1988) and not those of shamans.

Summary

Although the availability of organic materials in the archaeological record can only be a reflection of taphonomy, materialists have used indices such as the incidence of bone and ivory artefacts to designate what they call "cultures." Wooden remains are in fact more numerous from the Middle Palaeolithic/Middle Stone Age than from the subsequent 20,000-year period and still more numerous from the Lower Palaeolithic (well over 200 finds). Nowhere in the world is there a sharp separation at the much-debated Middle-to-Upper Palaeolithic transition, and Neanderthals were contemporaries of other physical types for enormous periods of time in many regions. Traditions firmly rooted in the Middle Palaeolithic yielded far more beads and pendants (see White 1993) than typical Upper Palaeolithic traditions, and the Neanderthals of the Châtelperronian seem to have used more ochre than their Aurignacian contempo-

raries in France. Should we deduce from this that they were more artistically inclined than the presumed *H. sapiens sapiens* populations? If we adopted the logic of naive empiricism, this is what we would do. We would go on to deduce that most Upper Palaeolithic people produced no wooden artefacts and that they were incapable of crossing the sea (except to Melos).

Stale empiricism leads us nowhere in the case of the early cognitive and technological development of humans. No scientific constructs of this development can be formed without rigorous recourse to taphonomic logic, which decrees that the earliest abundance of any form of archaeological evidence (and particularly of the more perishable classes of evidence) must not be interpreted as the earliest occurrence of the phenomenon it seems to represent (Bednarik 1992*b*, 1994*b*). This applies, for instance, to the technology of strings, ropes, and knots, watercraft and marine economies, clothing (of which we also know almost nothing from the Pleistocene), dwelling construction, mining technology (Bednarik 1992*e*), body ornamentation (Bednarik 1994*d*, n.d.), and, of course, symbolic production, which for the greater part is among the most ephemeral phenomena of human culture. Common logic tempts us to think that there must be some correlation between the amount of evidence we find of a practice and the frequency of that practice at the time in question, but taphonomic processes are extremely complex. They may truncate the record entirely, and even where they do not the cumulativeness of sample reduction processes must result in a cut-off point of evidence in numbers relatable to the historical frequency beyond which the remnant numbers are almost entirely unrelated to such frequency, merely hovering just above the zero point (Bednarik 1994*b*:fig. 2).

A combination of taphonomic logic and evolutionary deduction would have been able to predict the type of earliest "artistic" evidence we should have expected to find. It is fortunate, then, that such logic has not so far been systematically applied in our search, because this could have prevented the largely random search we have managed to conduct over the past century. If the type of evidence that our random search has delivered proved to be the type that taphonomic logic would have predicted, the interpretation of that evidence would be considerably strengthened. Indeed, it would be hard to challenge, because such agreement could not plausibly be attributed to chance. Therefore, in evaluating the authenticity of the evidence proffered for very early symbolism, taphonomic logic is of immense value, and it helps us greatly to ask, totally independent of the record, what types of evidence we *should* have expected.

The answer is simple. The oldest forms of evidence must be among the most deterioration-resistant possible for their time. If any rock art were found it would be almost a miracle, unless it consisted of deeply carved motifs in sheltered locations, well protected from weathering. Bone could be expected only in permanently alkaline sediments, and even engravings on stone would rarely have survived in most locations. Therefore we must expect a severely truncated, almost imperceptible record.

What should these very early markings consist of? If we take the non-figurative final Pleistocene art that we know of and develop a trajectory of "decreasing cognitive complexity" through the range of known Middle Palaeolithic markings (bunched or convergent lines, parallel lines, crosses, zigzags, arcuate marks), we could speculate that we should expect less structured sets of such linear marks with still greater age—precisely what one finds on the objects from Bilzingsleben. A similarly developed trajectory into the past for sculpture might lead to the notion of "proto-sculpture": the curation and eventual modification of natural forms. We should then expect to find pebbles resembling objects which were modified to increase their suggestiveness, and before that—that is, before the discovery of iconicity (Davis 1986)—objects lacking iconographic potential such as crystal prisms, silicified fossils, and perhaps coloured stones.

Instead of beginning with these postulates, I have chosen to survey the empirical data first, in precisely the same way as historically we acquired the cumulative record. Metamorphological theory was developed only after accumulating the empirical data, but there is no reason it could not have been posited much earlier. Retrospectively, it is apparent that the record consists of precisely the kind of material we would have expected had we had such a theory. Not only does this strongly reinforce the validity of the work of the field archaeologists who have gathered the accumulated data but also it shows that interpretations of these data not informed by sound theory must inevitably lead to false paradigms, particularly when the data are themselves substantially incomplete (e.g., Byers 1994).

I have tried here to examine the data on their own merits, avoiding overinterpretation. Even the most tentative construct of Lower Palaeolithic phenomena may need to be completely revised in response to just one new discovery. A great deficiency of traditional models of such remote times is their reliance on the absence of types of evidence. Any such model is susceptible to radical revision. It is presumptuous to base a hypothesis on the expectation that an absence of evidence will persist forever, and we know that it leads to ossification in archaeology. This is because of the discipline's limited susceptibility to refutation (Tangri 1989) and the tendency of some practitioners to rely on this limitation (Bednarik 1992*a*:27). It is clearly more desirable to keep an open mind and not to commit ourselves to any model prematurely. If we do, we should be prepared to review new evidence on its own merits rather than forcing it into our preconceived frames of reference. It would be fundamentally unscientific to base archaeological scenarios on the apparent absence of evidence.

I have tried to illustrate the enormous limitations of the evidence available to us and would not dare to offer any deductive conclusion or hypothesis. We have a few instances of markings, and these are not even generally accepted as non-utilitarian. We have some good indica-

tion that early hominids had begun to create taxonomies of the physical world, and we have a few instances of surprising technological complexity from widely separated regions. This is not sufficient to create any level of certainty, but it is sufficient to render the model of a comparatively gradual development much more acceptable (Clark and Lindly 1991, Lindly and Clark 1990). Taphonomy clearly creates artificial plateaus of evidence classes, as do archaeological practices themselves (radiocarbon dating, for instance), and when we allow for these factors there remains no unambiguous evidence of sudden developments, sharp divisions, cultural or other quantum jumps. Of the two competing models of Palaeolithic development (Duff, Clark, and Chadderdon 1992), the "cumulative model" is far more likely to be correct. The same seems to apply to cognition and symboling. The existence of symbolic traditions in the Middle Palaeolithic is now hardly open to dispute, and these traditions certainly included artlike productions. Any model which ignores this evidence is entirely out of step with current archaeological knowledge (cf. Byers 1994). Middle Palaeolithic symboling was perhaps preceded by more rudimentary traditions, and these may have extended into the Lower Palaeolithic. This provides no real indication of the full repertoire of symbolic behavior at that time, but it does squarely contradict one of the greatest myths in Palaeolithic studies.

Comments

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There is little that I can say about this paper, since I agree so completely with its sentiments and its evidence. Bednarik is to be congratulated for drawing out this subject so clearly and in particular for his role in bringing the important Bilzingsleben material to a wider audience, first in his journal *Rock Art Research* in 1988 and now here. It is also a breath of fresh air to have these problems set out in global terms instead of referring simply to Europe or, worse, to the Dordogne—a region which the late David Clarke, with tongue in cheek, sometimes described in lectures as an insignificant cultural cul-de-sac! It is now eight years since I put together a survey of Pleistocene "art" (Bahn and Vertut 1988), and yet—despite the subsequent appearance of several major volumes on the same topic in France, Italy, and elsewhere—it is still, as far as I am aware, the only such book to include, or even mention, the existence of Pleistocene art outside Europe. Marshack's forthcoming publications of his analyses of little-known or unpublished Palaeolithic objects from Anatolia and the Near East will undoubtedly have the most profound effect on our views of the "origins of art" or the supposedly abrupt transition from Middle to Upper Palaeolithic. In the meantime, we have Bednarik's carefully argued and persuasive case that taphonomy and bias have played an enormous role in developing the currently orthodox view.

Where the supposed "transition" is concerned, the archaeological data are so fragmentary and ambiguous that one can find support for a wide range of views: for example, Klein (1995) has recently pointed out that Stiner's (1995) data on Neanderthal and "modern" exploitation of animals in Palaeolithic Italy provide support for both advocates and opponents of continuity between Neanderthals and modern Europeans, and within the French Pyrenees (Bahn 1984) there are sites where one can see a clear break between Middle and Upper Paleolithic—in terms of subsistence, tool technology, and raw materials—and others where one cannot clearly separate the two. Extrapolated to a European or even an Old World scale, this situation underlines the fact that arguments about the degree of replacement are still far from being resolved. As Hayden (1993) has argued convincingly in a well-documented paper, an objective perusal of the evidence in Europe reveals that the biological, behavioural, and cognitive differences between Middle and Upper Paleolithic populations were actually very slight.

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Bednarik follows the recent and somewhat unpopular tradition of Marshack, White, and Hayden (the last of whom he does not cite, though Hayden's [1993] coverage and conclusions are similar) of emphasising an essential continuity between the art of archaic and anatomically modern humans. His case is convincing, and so, unfortunately, is his account of the reasons previously given for denying a capacity for art, symbolism, language, culture, and so on, in more archaic populations. We do of course tend to categorise and see discontinuities (e.g., an Upper Palaeolithic explosion/replacement) where there is really a continuum or at best a blurred boundary. The phenomenon of Mach bands (subjective enhancement of brightness discontinuities), categorical perception of consonant sounds (where differences in the acoustic waveform are continuous), and sharp tuning curves in neuronal responsivities to ranges of acoustic frequencies all testify to a tendency to see black and white instead of shades of grey.

Bednarik views a continuum in art, culture, and cognition across an Upper Palaeolithic boundary as incompatible with Out-of-Africa replacement by anatomically modern peoples of preexisting more archaic populations and in conformity with (continuous) multiregional evolution. While the two concepts are undoubtedly related, one does not necessarily presuppose the other. There is nowadays renewed enthusiasm for ascribing cognition, problem solving, and maybe even (self-) consciousness to other species on the basis of behavioural and neurological commonalities. So, too, may earlier hominids not on our own direct evolutionary trajectory have pos-

sessed linguistic, artistic, technological, and cultural capacities differing only quantitatively from our own. Whether or not there is a cultural discontinuity at the start of the Upper Palaeolithic and whether or not this reflects a change in population, it would be foolish to deny that Neanderthals (Houghton 1993, Schepartz 1993) or even *H. erectus* or *H. habilis* (Bradshaw and Rogers 1993, Deacon 1990) possessed some language capacity, *whether or not* they were our direct ancestors (Wood 1992).

Bednarik perhaps far too readily rejects the Out-of-Africa hypothesis; while it relies on molecular genetics (particularly mitochondrial DNA) rather than on fossil evidence and certain of its statistical assumptions have recently been questioned, Africans do show the most divergence, indicating greatest population antiquity. As the molecular geneticists observe, living genes have indisputable ancestors, while dead fossils may have left no descendants. In any case Lahr (1994) finds that the regional features claimed to characterise East Asian and Australian evolutionary lines were not exclusive to these regions either spatially or temporally and that many of the traits were functional and therefore subject to selective pressure.

For an anatomical correlate of art and culture in the hominid brain we must turn to the prefrontal structures (Bradshaw and Mattingley 1995), a region involved in foresight, planning, social responsivity, attention, and response flexibility and one which is twice that predicted by extrapolation from anthropoid trends (Deacon 1990). It includes, of course, Broca's motor speech area. Hominid brain evolution, continuous or otherwise, has involved less the development of new structures than reorganisation and change in existing capacity. It would be strange if our ancestors or extinct cousins did not think and behave somewhat like ourselves.

We would predict that the *development* of art (ontogenetic or archaeological) would progress from simple lines to line clusters, with organisation according to the perceptual principles of gestalt closure, and finally to representationality. And this is what is found. Halverson (1992), moreover, notes the role of (out)line surrogacy (for full three-dimensional form) in ancient and modern art, again reflecting the operation of fundamental perceptual properties.

While art implies intentionality (and so does the aesthetic symmetry of an Acheulian biface), it need not be restricted to the nonutilitarian. Bednarik is to be congratulated on a fine synthesis which will promote a long-overdue paradigm shift in our thinking on ancient culture.

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The main points of Bednarik's essay are (1) that evidence for "concept-mediated marking" (artlike phenomena) not qualitatively different from that of the European Upper Paleolithic is found in many parts of the world and (2) that some of it, at least, is demonstrably much older than the Middle–Upper Paleolithic transition in Europe, conventionally dated at ca. 40,000 years B.P. The evidence occurs in well-dated contexts that extend back to the Middle and Lower Paleolithic. The appearance of significant quantities of "art" in Europe at the Middle–Upper Paleolithic boundary is attributed to a kind of time-factored, universal taphonomic threshold that destroyed most earlier examples. Early Upper Paleolithic art, therefore, probably just represents the deepest point in time from which we have a relatively large corpus of surviving examples. Although I have not compiled a data base like Bednarik's, I do monitor this literature, and my construal of pattern in the evidence for symbolic behaviour is in broad agreement with his. I think that CA readers need to know that the dominant paradigm for the appearance of evidence for symbolism (what I call "the standard model"—Duff, Clark, and Chadderdon 1992), based on decades of European research, can in fact be called into question on empirical grounds.

The Bednarik essay has implications for the modern-human-origins debate, since it implies a gradual evolution of cognitive faculties that long predates the appearance of "moderns" (however defined). This lends support to the multiregional-continuity position of Brace (1995), Wolpoff (1989; Wolpoff et al. 1994), Frayer (Frayer et al. 1993), and Clark (Clark and Lindly 1989 *a*, *b*; Clark 1992). While symbolism manifest as "art" is a widely invoked behavioural criterion of "modernness," it is important to realize that it is only one of many evidential domains that combine to make replacement scenarios utterly implausible. These domains include evidence of (1) specialized, structured use of space and (2) the construction of shelters dated to the Acheulean at Terra Amata and Lazaret (and perhaps even to the Basal Pleistocene at DK 1, Olduvai Gorge); (3) sophisticated Levallois technologies in Lower Paleolithic contexts (e.g., at Cagny la Garenne) that are, if anything, more cognitively complex than the single-platform blade technologies that supposedly typify the European Aurignacian; (4) blade technologies extending back far into the Levantine Lower Paleolithic (e.g., at Jabrud in Syria, at the Haua Fteah in Libya); (5) economically rational behaviour analogous to that of modern foragers; (6) displacement (the ability to take into account events removed in space and time) in the Italian Middle Paleolithic (Kuhn 1992, Stiner and Kuhn 1992); and (7) ritual and intentional burial in at least some European Neandertal sites (e.g., La Ferrassie). In sum, it is clear and definite that these widely invoked criteria for "modernness" show *no correlation whatsoever* with the appearance of morphological moderns anywhere. They originated long before the appearance of Neandertals, became elaborated in Neandertal contexts, and became still more marked features of the human condition during the Upper Paleolithic (see, e.g., Hayden 1993). To argue that change is temporally vectored, however, does not necessarily mean that it was regular (Clark and Lindly 1989*a*, Barton, Clark, and Cohen 1994).

Of the various factors summarized in Bednarik's essay that impede understanding of the evolutionary significance of artlike phenomena, the most important ones are clearly epistemological (Clark and Lindly 1991). There are currently about a dozen partly incommensurable theories that purport to explain artlike phenomena in deep time (Duff, Clark, and Chadderdon 1992). Because they are based upon different paradigms, these theories proceed from different sets of biases, preconceptions, and assumptions about the nature of art. Art cannot be considered in isolation, however; as noted, it is only one evidential domain relevant to the question of our evolving cognitive faculties. The science philosopher Richard Bernstein (1983) has argued that when researchers grapple with incommensurable theories, they cannot proceed by a linear movement from premises to conclusions or—as here—from individual facts to generalizations but instead must exploit multiple strands of diverse kinds of evidence to assess the overall credibility of a hypothesis or theory. Bednarik's essay will doubtless provoke denunciations from the strict empiricists among us and from those inclined to take issue with his definition of concept-mediated marking. The important point, however, is that, when there is no common ground for assessing the credibility of an argument, the cumulative weight of multiple skeins of evidence can be rationally decisive (Bernstein 1983:69, 74). The cumulative weight of the evidence clearly supports multiregional continuity so far as modern human origins are concerned.

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This provocative paper invites us to keep an open mind on a still open question. The "symbolic-explosion" explanatory model for the Middle–Upper Paleolithic transition, criticized by Bednarik, has the merit of emphasizing the entirely modern character of the Aurignacian behaviour; however, it does so at the cost of oversimplifying and often neglecting possibly relevant European and non-European data for earlier periods. Moreover, its usefulness is undermined by a lack of interest in the evolution of cognitive abilities for the preceding time span, a period on which we need to focus our attention. In contrast to what Bednarik seems to believe, however, processual archaeology has not systematically denied or challenged the possibility of investigating the first "nonutilitarian" behaviours as long as goals are clear and models and methods are made explicit (Renfrew 1993). To translate this design into practical reality we need to develop analytical criteria, preferably experimental ones, for a convincing internal analysis of the archaeological record. Only by developing such criteria will we be able to answer a number of pertinent questions. Were the Bilzingsleben marks produced with a point or with a cutting edge? Was each marking sequence made with the same tool or with several tools? Can the directionality of the marks be established? In what order were the lines engraved? Do the marks present the same wear as the adjacent surface? A close reading of the evidence is a prerequisite for proving the human origin of the marks and for attributing concept-mediated purposes to the identified behaviour.

It seems to me that this challenging research requires the support of more accurate renderings of marks, such as photos or computerized images. For instance, on object 2, the presence of three closely juxtaposed lines is interpreted as the result of three separate applications of the engraving tool, suggesting a careful intentional procedure. However, numerous experiments have shown that a single movement of a cutting edge (Shipman and Rose 1983) or of a point (d'Errico 1989, 1994) can produce multiple offset lines similar to those engraved on object 2. The close similarity in the disposition of the three juxtaposed lines suggests that this is likely to be the case. In the absence of photographs or scanning-electron-microscope micrographs it is impossible to choose between the two hypotheses.

Actualist studies of elephant-bone accumulations near dry-season water holes (Conybeare and Haynes 1983, Haynes 1988) have shown that flaked and split fragments of long bones and tusks and ivory pseudopoints are common, the latter being the products of fights for access to water and other resources or to mates (Conybeare and Haynes 1984). Tusk fragments virtually identical to those recorded by Haynes are reported as tools by Howell and Freeman (1983) and by Radmilli (1984) from the sites of Torralba and Ambrona (Spain) and Castel di Guido (Italy) respectively. Clearly, prehistorians have much to learn from African elephants. Field observations by G. Giacobini and me (unpublished) during excavations at Castel di Guido, one of four Italian sites which have yielded handaxes made from *Palaeoloxodon antiquus* long bone fragments (Villa 1991), have shown that spiral tusk fractures resulting in pointlike pseudotools can be produced by the pressure of sediments in the presence of microfaults. According to Haynes (1988:150) a variety of tusk fragment morphologies can be produced by nonhuman means. Elephants smooth the fractured surfaces of broken tusks in the course of feeding or debarking trees; refracturing of worn tusks will produce fragments with one broken end that is scratched and smoothed and another that is sharp and unmodified. Bone and ivory fragments near water holes are often subjected to heavy trampling and manipulation by elephants. This results in deep V-shaped pseudo–cut marks that may be difficult to distinguish from marks produced by lithic tools. Parallel and converging sequences of such marks are reported from these sites. No systematic study of nonhuman marks in elephant-bone assemblages has yet been performed. Considering that elephant carcasses are particularly frequent near water holes, Haynes's observations may apply to a travertine site such as Bilzingsleben.

The grooves interpreted as intentional marks on the

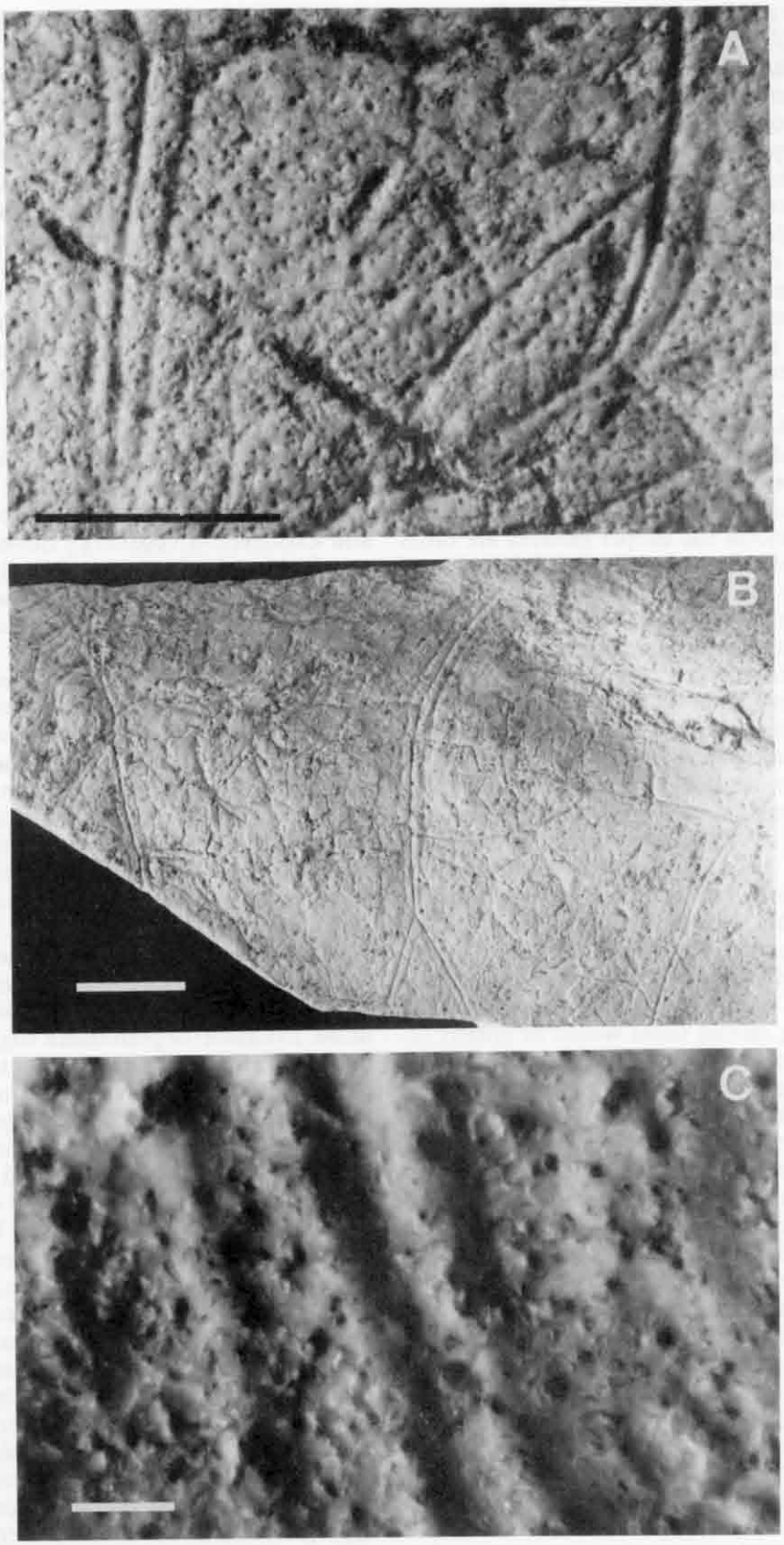

FIG. 1. *Long bone fragments from the Epipaleolithic levels of La Borie del Rey shelter* (a, c) *and from Magdalenian levels at Roc de Marcamp* (b) *showing a pattern similar to that interpreted as intentional marking by Bordes (1969) and Marshack (1975) on a Pech de l'Azé rib and by González Echegaray and Freeman (1971:159) on a bone fragment from Cueva Morín;* c, *close-up of* a *illustrating the U-shaped section of the grooves, the roundness of their edges, and the absence of internal striations. Scale,* a *and* b = 1 cm; c, = 1 mm.

Pech de l'Azé rib and similar patterns on bone fragments from Cueva Morín (González Echegaray and Freeman 1971:159) and from the Tagliente shelter (Leonardi 1988) should not be regarded as man-made. Virtually identical patterns can be seen on bone pseudotools from northern Spain (d'Errico 1993:306, fig. 11), on two bone fragments from the Epipaleolithic levels of La Borie del Rey (fig. 1, *a* and *c*), published as engravings by Coulonges (1963), and on another bone fragment (fig. 1, *b*) from the Magdalenian levels of Roc de Marcamps (M. Lenoir, personal communication). These fragments show the same "complex arrangement of curved lines, including what appear to be separately cut deep subparallel marks," cited by Bednarik as characteristic of Pech de l'Azé rib. Under the microscope these grooves, like those of the Pech de l'Azé bone (Marshack 1975:290, pl. 4*b*; 291, pl. 5), show U-shaped sections and rounded edges; they display none of the characteristics of experimental marks produced by a lithic point, such as internal striations and sharp edges. In all these objects the deeper grooves are often bordered on both sides by more superficial U-shaped grooves between 500 μm and 1 mm apart. Histological causes for this parallel arrangement have yet to be investigated; however, the microscopic morphology of the grooves, which merges perfectly into the bone structure, and their spatial arrangement strongly suggest that they are blood-vessel impressions produced by a surficial remodelling of the bone tissue.

These observations do not refute all of Bednarik's identifications of concept-mediated marking in the Lower Palaeolithic or his hypotheses about human modification and use of bone and ivory at Bilzingsleben. Some of the items described in this paper are undoubtedly pertinent to the debate on Lower and Middle Palaeolithic nonutilitarian behaviours. However, before accepting some of Bednarik's conclusions or those of the researchers he cites, alternative hypotheses should be considered. Choosing among them would be greatly facilitated by the provision of analytical data about the objects and their markings and by the development of new analytical methods appropriate to their study. Many of these objects are cited in the archaeological literature, but very few of them have been submitted to internal analysis that takes into account the depositional and taphonomic history of the site. This should not prevent us from elaborating exploratory models, but, as Bednarik judiciously reminds us, we should be flexible enough to reassess the cognitive value of new (and old) evidence.

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Bednarik's article contributes to the ongoing modern-human-origins debate on three levels. First, Bednarik presents descriptions of several artifacts that document expression of early cognitive capacities in a forum that encourages timely reaction. The coherent formulation of the relationship between taphonomic processes and evaluation of evidence for early symbolic expression is another major contribution. Finally, Bednarik has articulated disciplinary biases that hinder the objective evaluation of limited yet significant instances that *may* represent "concept-mediated" behavior prior to the Upper Paleolithic. I limit my comment to the latter two points.

Bednarik has explicitly characterized the manner in which taphonomic factors select against preservation of all materials from Lower and Middle Paleolithic contexts. It is not that people do not understand taphonomic factors themselves but that they fail to appreciate the linkage of these factors with evaluation of premodern symbolic (nonutilitarian) expression, tending to maintain that geographically and temporally repeated patterning constitutes acceptable evidence for symbolic behavior. As has been suggested elsewhere (Duff, Clark, and Chadderdon 1992:214), these conditions cannot be satisfied for the Lower and Middle Paleolithic. Consensus as to what constitutes satisfactory evidence for premodern symbolic expression is unlikely to emerge in the near future.

Bednarik calls for objective consideration of each case on its merits. He clearly finds the gradual evolution of symbolic capacity and expression consistent with archaeological evidence and taphonomic expectations and supports his contention. I imagine that other commentators will present plausible utilitarian explanations for the materials he discusses. Until there are mutually agreed-upon standards of evaluation, evidence will continue to be filtered through paradigmatic biases (Clark and Lindly 1991).

Bednarik's attempt to model early "concept-mediated" behavior provides a framework within which alternative interpretations can be outlined and scrutinized. Evidence for premodern cognitive abilities continues to mount, as he notes, and it is increasingly untenable that it *could not* have existed in the early Paleolithic. However, I think that Bednarik prematurely buries the "standard model" for the appearance of symbolic behavior; what he has accomplished here and elsewhere (Bednarik 1992*a*) is a compilation of instances that must be satisfactorily accounted for by advocates of that model. The idea that there is no evidence for early symbolic expression, now in question, is further jeopardized by the inconsistencies in the archaeological record of the Middle–Upper Paleolithic transition aptly summarized by Bednarik. Marked disjunctures, if any, appear unrelated to the taxonomic distinctions created by archaeologists and paleoanthropologists. The archaeological record should be accorded primacy; the hominid taxa with which it is associated (a similarly complex subject on which agreement regarding acceptable evidence and appropriate distinctions has not emerged [see, e.g., Lieberman 1995]) should not structure its interpretation. In the end, this is what Bednarik is asking, and it is a reasonable request.

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What does it mean for a behavior to be "concept-mediated"? Bednarik refers to a mental state linking behavior and environment through the "notion of content" (presumably a concept). In the case of representational art, it is not difficult to see what this means: between the perception of a horse and the depicting of a horse lies the concept "horse," a mediating mental state. But in the case of markings it is less easy to see what there might be in the environment that could be conceptually linked to the marking behavior. What did the hominids have a concept *of* as they made their marks? The most obvious possibility is that they had a concept of mark making, but this seems to lead nowhere.

Is intentionality the key? Supposing the Bilzingsleben markings were intentional, does an intention imply a concept? Perhaps not. Doodling, which Bednarik considers briefly, would seem to be intentional in some minimal sense, but it is doubtful that it is concept-mediated. Usually doodling is done while the conceptualizing mind is focused on something else; it is in fact "absent-minded." Between the view that early Paleolithic markings are nothing more than the accidental residue of some other purposeful, conceptualized behavior and the view that they are consciously intentional there is the possibility of semiconscious, nonutilitarian but equally nonpurposive and nonconceptualized behavior. Thus even if we were sure that the markings were intentional we would still not know if they were "concept-mediated."

Assuming that they were, however, would they indicate "sophisticated cognition" or "considerable cognitive complexity"? As a matter of fact, all the markings illustrated here could have been made by chimpanzees or three-year-old modern humans. All of the characteristic patterns of line marking that Bednarik cites—dots, parallel lines, crossed lines, radial lines, space constraints—are well attested among drawing chimpanzees (Morris 1962) and small children (Kellogg 1959). Bednarik suggests that radial markings might imply "a distinctive cognitive evolution and a refinement of concept-mediated behavior." Yet such fan shapes were the chimp Congo's favorite motif and were even executed by a capuchin monkey (Morris 1962:32–34). Kellogg lists fan-shaped configurations among the "basic scribbles" of two-to-four-year-olds (Kellogg and O'Dell 1967:21). The Tata specimen, on which a prominent natural diametrical fissure was apparently embellished with a line crossing it at right angles, suggests to Bednarik "considerable cognitive complexity." But again, P. Schiller's chimp Alpha showed "a strong inclination to cross boldly-drawn lines or bars at right angles and this tendency was also shown by the Russian chimpanzee Joni" (Morris 1962:87). According to Kellogg, very young children demonstrate a comparable tendency to make crosses and, indeed, crossed circles (exactly like the Tata object). For both chimps and children the borders of the surfaces being marked influence their marking strategies. Morris believes that chimpanzees have "a basic feeling for symmetry" (p. 70).

It would appear, then, that the level of cognition inferable from the Lower and Middle Paleolithic markings is about the same as that of modern adult chimpanzees or three-year-old human children. Bednarik's use of words like "sophistication" and "complexity" suggests that he would not be satisfied with such an estimate. Yet markings per se seem not to indicate any advance beyond a "pongid status" for the early hominids. What *does* demonstrate such an advance is, above all, complex stone knapping, which is clearly beyond any pongid capabilities. But there is no reason that "marking strategies" should have kept up with tool-making advances. The latter are genuinely adaptive and practical, the former are not. The latter imply conceptualization, the former do not. What we probably have here is an example of "modular evolution" (Brown 1993), the cognitive equivalent of biological "mosaic evolution." Pre-Aurignacian hominids had their own kinds of intelligence and resourcefulness, which need not have had any reflection in "markings" at all. Indeed, there seems to be no a priori reason to suppose that these early markings can tell us much of anything about intelligence. Their repetitive and rhythmic character points to sensorimotor schema formation via circular reactions, which occurs at the extreme beginnings of thought, well before genuine concept formation. Such markings may be made equally by a doodling genius or a retarded person; they do not seem to be diagnostic of cognitive achievement in any way.

Nor, I think, do they carry any implications about the tempo of cognitive and cultural evolution. "Gradual" does not necessarily mean "slow"; it means "step by step." From the potato washing of Japanese macaques to the invention of printing, cultural innovations often spread very rapidly while the step-by-step processes of dissemination are nevertheless quite clear. Such changes may also have relatively sudden cognitive consequences: ways of thinking can change almost literally overnight. Bednarik's cautions against hasty generalizations are sound, but the Lower and Middle Paleolithic markings do not seem to affect the issue of abrupt transitions.

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Bednarik's review of symbol production and the occurrence of "nonutilitarian" artifacts during the Paleolithic period offers researchers an excellent opportunity to discuss an issue which is fundamental to our understanding of the evolution of human behavior. I will largely con-

fine my comments to the theoretical framework within which this paper is written. I wish to focus on three points—scientific bias, the role of taphonomy in archaeological interpretation, and the significance of the origin of specific hominid behaviors.

For Bednarik the positivism and empiricism of processual archaeology have impeded the study of hominid cognition. For example, he writes, "Processualists subject every claim of early cognitive evidence to severe criticism not in the interest of scientific rigour but in defense of significant biases." Unfortunately, he does not recognize his own bias. He offers many examples of early symboling without adequately acknowledging the debate which surrounds many of these items, among them the scoria pebble from Berekhat Ram (see Pelcin 1994) and some of the punctured and perforated objects. In the same vein, Bednarik draws conclusions from some of the items which cannot be substantiated. Because ocher crayons have been recovered in the archaeological record for the Acheulian, he states that these hominids marked the surfaces of rocks, even though no such petroglyphs dating to this period have been found and the ocher may have been used for other purposes, such as to tan hides (Keeley 1978, 1980). He also infers from the scratch on the Tata nummulite that "the Tata hominids seem to have possessed a developed frame of reference that could not have been significantly less evolved than that of modern humans." It is necessary and fruitful to posit scenarios in order to generate testable hypotheses, but it is equally important not to present these scenarios as facts. In addition, Bednarik writes, "it would be fundamentally unscientific to base archaeological scenarios on the apparent absence of evidence." Is it not equally unscientific to create scenarios with little evidence? It is not that archaeologists use more stringent criteria to evaluate early claims for symboling but simply that when an uncertain item is found in the Upper Paleolithic, where many examples of symbolic production exist, one is more inclined to grant it validity; for earlier periods, when so little is known about the cognitive capacity of hominids, we must be more judicious.

Bednarik rightly emphasizes the role of taphonomy in shaping and filtering our knowledge of the past. Ironically, far from hindering the study of symbolic production, taphonomy can be instrumental in testing the validity of many claims for early symboling and other aspects of hominid behavior. Very often, taphonomic explanations can be found for what was once thought to be behavioral. Many such studies of the effects of natural and cultural processes upon the archaeological record have been conducted, including Binford and Ho's (1981) investigation of fire usage at Zhoukoudian, Shipman and Philips-Conroy's (1977) and Brain's study of Dart's (1957) osteodontokeratic culture, Jéquier's (1975) look at the cult of the cave bear, White and Toth's (1991) work at Monte Circeo, and Brain's (1981) study of site formation processes in South African caves. Taphonomy is an important area of research which can have profound implications for the reconstruction of hominid behavior, and much work remains to be done.

Taphonomic processes bias the archaeological record against the preservation of organic materials except under certain conditions. However, this does not give an archaeologist license to state what would have been found if it were not for these processes. Granted, archaeologists do attempt to reconstruct the diet of early hominids when for the most part all that remains in the archaeological record are lithic and faunal materials. But there is a fundamental difference between looking to biology, primatology, and ethnology and hypothesizing that plant material represented an important subsistence source for early hominids and maintaining that Acheulian hominids possessed sophisticated cognitive functions on the basis of the presence of ocher and grooved "nonutilitarian items." These markings may well have nonutilitarian functions, but are they symbolic (see Chase 1991, Chase and Dibble 1992)? Furthermore, the issue is not even whether the objects Bednarik describes could have been symbolic—almost every archaeologist would agree that the possibility exists—but whether their symbolic nature can be proven.

Finally, Bednarik states that the "assumption that any human innovation commenced at the time from which we have the earliest evidence of it is weak." I agree with him in that we may never discover the very first instance of fire use, of big-game hunting, of artistic endeavor, or of many other behaviors because these activities may have evolved gradually and their earliest traces may be invisible or unrecognizable. Nonetheless, one cannot assume what one is trying to prove (see Chase and Dibble 1987). Some hominid behaviors may indeed have appeared suddenly. Furthermore, is it more important to uncover this early experimentation or to document when these activities became incorporated into a group's behavioral repertoire (Chase and Dibble 1987)?

Symboling and hominid cognition are topics which promise to challenge researchers for a long time to come. Healthy skepticism combined with willingness to test new scenarios will lead to more fruitful explorations of these topics and to a greater understanding of the evolution of hominid capabilities.

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Despite prehistory's love affair with a tripartite and absolutist subdivision of the Paleolithic, the old walls are tumbling. Ironically, this is happening against the backdrop of emphatic proclamations of explosive, abrupt changes, most notably between the Middle and Upper Paleolithic. Reality, like truth, is often in the eye of the beholder. Bednarik's provocative article is just the latest in a series of works challenging many of the "gospel-truth" absolute distinctions between the Upper Paleolithic and all the rest of hominid culture that preceded it. In the realm of lithic technology, for example, it is clear that not even prismatic blades (sometimes made by the crested-blade technique) are restricted to the Upper

Paleolithic throughout the Old World (see Conard 1990, Lamdan and Ronen 1989, Révillion and Tuffreau 1994).

Bednarik, with his great Australian experience, is right to chide a still Eurocentric discipline (indeed, a still often Périgord-centric one!) with the facts of Middle Paleolithic–age navigation to Sahul and rock art in Australia. Despite the salubrious and necessary critiques of specific instances of *false* evidence for Mousterian art, ornamentation, and even burial during the past couple of decades, there are nagging instances of ritual/symbolic behavior that refuse to go away—and new ones constantly coming to light. I have always been impressed by the large deliberately and regularly pocked slab atop one of the La Ferrassie burials—known by the profession since its discovery in 1921 by Capitan and Peyrony. Acceptance of Bednarik's general premise would by no means absolve the discipline of a scientific obligation to attempt to eliminate natural explanations before accepting any rare, unusual shape as a human artifact—especially an ornamental/symbolic one. It is better to deal dispassionately with bear teeth grooved by the bears themselves, etc. (e.g., Gautier 1986, Chase and Dibble 1987) along the taphonomic lines so well traced long ago by Pei (1938) than to have to backtrack later. Careful analyses of stratigraphic provenience, possible site disturbance processes, natural or nonartistic modification factors, etc., are essential. When all is said and done, however, there remains a residue of surprising objects—limited in number for no doubt *both* behavioral *and* preservational reasons. I have recently been struck, for example, by the discovery of a "notationally" engraved bone and a perforated (fox?) canine apparently in a late Mousterian level at Cova Beneito (Alicante, Spain) (Iturbe et al. 1993) and by the spiral-engraved flint nodule from a tuft-sealed Mousterian horizon at Quneitra on the Golan Heights (Marshack n.d.). Bednarik takes us through a litany of cases that "won't go away" and provides a reasonable discussion of their possible significance for human evolution.

As with so many aspects of cultural (and biological) evolution, the development of "marking" occurred as a long-term frequency shift, and even when in some world regions it did become common it remained relatively rare (or absent, at least in nonperishable materials) in others for long periods of time. The world is not the Périgord or Württemburg—or Bhopal. Culture change is a mosaic in time and space. This has recently been brought home even for Western Europe by the growing evidence of *very* late-surviving (≤30,000 years ago) Mousterian technology and of Neandertals in southern Iberia—separated from the supposed homeland of a supposedly adaptively advantaged African Eve by the 10-km Straits of Gibraltar (Straus, Bischoff, and Carbonell 1993, Straus 1994, both with references).

While looking with favor on Bednarik's general argument (and without wishing to forego critical, objective analyses of some of the cases he cites, since accidental or practical explanations might exist for some of them), I consider his remarks concerning the New Archeology's supposed dismissal of any and all traces of modernity before 40,000 B.P. gratuitous. A critical approach to these kinds of evidence is necessary. My belief in the scientific method (hence in positivism?) has not been completely eroded. If, after all, we had been fully modern 100,000 or 50,000 years ago, why would we have continued to evolve?

Finally, if we are ever to escape the restrictions of such culture-stratigraphic units as Lower, Middle, and Upper Paleolithic, should we not also question the absolute separation of Bednarik's "erectoid hominids" from *Homo sapiens*? Have we met the ancestors and found that they are a version of us? The pendulum in interpreting the "humanness" of Neandertal and *H. erectus* seems to be swinging again.

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In these days of "the archaeological record as text," I have come to appreciate arguments, however much I disagree with them, that can be evaluated by reference to some material reality. In my comments on this provocative paper I will emphasize substantive evidence (much of which I have examined personally) and issues not because I am an empiricist but because vaporous arguments of plausibility take us nowhere. My familiarity with particular materials enables me to evaluate quantitative claims such as the existence of "thousands" of exotic objects prior to the Upper Paleolithic. I shall leave to others an evaluation of Bednarik's characterization of the non-European record.

My own position is to remain open to the possibility that Middle Paleolithic/Middle Stone Age peoples, whoever they were biologically, were neurologically capable of symbolic representation. I have argued (White 1992) that what was missing was a social matrix that made enduring, material forms of representation useful and desirable. Bednarik's paper lacks any clear sense of the relationship between symbolic representation and emerging social formations, a relationship that I have sought to explore through my analysis of Early Aurignacian personal ornaments (White 1994). While it is certainly possible (although not particularly plausible) that pre–Upper Paleolithic humans used exclusively perishable materials to construct personal and social identities, the close relationship between long-distance procurement and personal adornment at the beginning of the Upper Paleolithic speaks to new, socially mediated conceptions of social and material value.

I have not (as Bednarik claims) explained the presence of pierced teeth and marked objects in the Chatelperronian as resulting from their having been scavenged by Neandertals from Aurignacian sites. Rather, that was one of a series of working hypotheses (White 1993*a*) that included the possibility that they were manufactured by Neandertals. Curiously, the pattern in France (most notably at Quinçay) is that there are no such objects in the Chatelperronian until about 33,000 B.P., thousands

of years after the first Aurignacian sites appeared. Indeed, to my knowledge there are but three late Chatelperronian sites that have yielded such objects, while essentially all Aurignacian sites contain them. Marked objects and pierced/circuminscribed teeth from the Chatelperronian are indistinguishable from those in the Aurignacian with respect to both the materials chosen and the techniques employed. By the way, I suspect that there is more red ochre from two French Aurignacian sites (25 km from Abri Castanet and Abri Blanchard) than exists in collections from all known Middle Paleolithic sites combined.

I do not agree with Bednarik's characterization of the Russian early Upper Paleolithic sites. My own firsthand analysis of the collections from Sungir yielded no evidence for its having roots in the Middle Paleolithic. As Grigoriev and Leroi-Gourhan before me have observed, the lithic and bone/antler/ivory assemblage from Sungir resembles the Aurignacian typologically and technologically. The Streletskaya culture, the origin of which precedes Sungir by several thousand years, shows Mousterian elements at Kostienki 6 and Kostienki 12 (Praslov and Rogachev 1982). In contrast, a few hundred meters away at Kostienki 17 (level 2), the contemporaneous Spitsinskaya culture is typologically and technologically Upper Paleolithic but not Aurignacian. It has recently been dated at ca. 37,000 B.P. by thermoluminescence on overlying volcanic ash. An excavated area in level 2 of only 66 m^2 (in contrast to the 4,500 m^2 excavated at Sungir) yielded an array of pierced teeth, belemnite beads, pierced coral polyps, and repetitively marked bone artifacts, totaling ca. 20 objects (White 1993*b*).

Processual archaeology maintains a critical posture not to defend its inherent biases but to evaluate unjustified and unsupported claims such as many of those presented by Bednarik. Bednarik states, for example, that if the marked objects from the Lower and Middle Paleolithic that he presents were found in an Upper Paleolithic context they would be accepted without question as purposeful, concept-mediated, symbolic representations. However, it is simply not enough to say that they are intentional because they are "neatly spaced and arranged." In fact, having studied virtually all the known Aurignacian marked objects and personal ornaments from across Europe, I would not accept any of the objects presented by Bednarik as purposeful if I found them in a drawer of Aurignacian material. Indeed, such marking, which I take to be an incidental by-product of a variety of tasks and activities, continues throughout the Upper Paleolithic alongside structured, conventionalized, repetitively marked objects and graphic representations (figs. 1 and 2).

I find it problematical that Bednarik cites 111 perforated animal phalanges from the Micoquian of Prolom II without noting the excavator's cautious interpretation (Stepanchuk 1993:33):

> The origin and purpose of these holes is not quite clear. The study of phalanges with holes has already been going on for more than 150 years, and various explanations have been proposed: the obtaining of marrow; use as whistles; and the result of biting through by a carnivore while the animal was alive. Other hypotheses seem to be fantastic, for example, that they were vessels for poison. It is possible that some of the phalanges with holes were really used as whistles. R. Wetzel wrote that phalanges with roughly pierced holes from Bocksteinschmiede H which he had recognized as "hunters'" pipes were shown by experiment to utter quite strong, shrill sounds. . . . One cannot completely exclude the hypothesis about marrow procuring, although in many ways it does not withstand criticism. New evidence about natural causes has recently been adduced (Chase 1990) In any case, the abundance of phalanges with holes at Prolom II cannot be comprehensively explained by any one of the causes mentioned above. Maybe in future investigations of these artefacts at Crimean sites . . . will make clear their enigmatic origins.

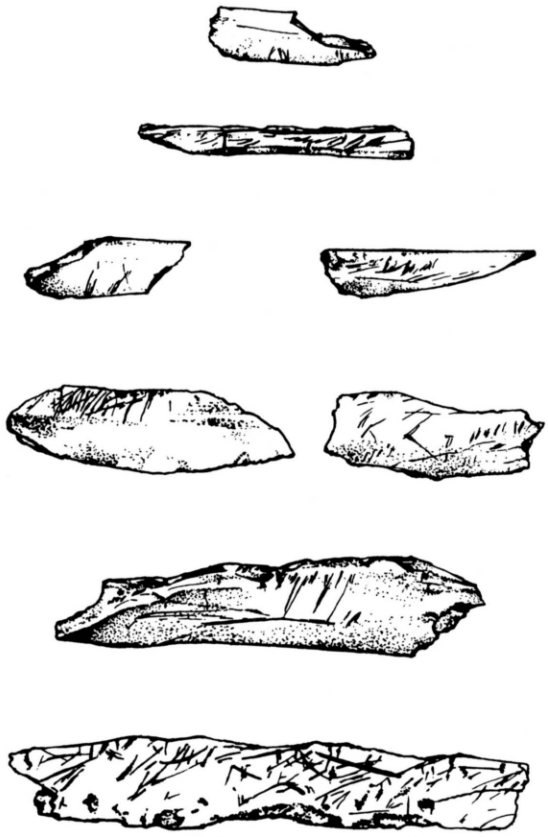

FIG. 1. *Aurignacian I bone fragments from Abri Cellier, France, showing patterned markings not considered to be purposeful.*

As far as I can tell from Stepanchuk's photographs, these phalanges are not perforated all the way through in such a fashion as to permit suspension. No photomicrographs are presented that might allow a reasoned taphonomic evaluation of the processes that created these objects or

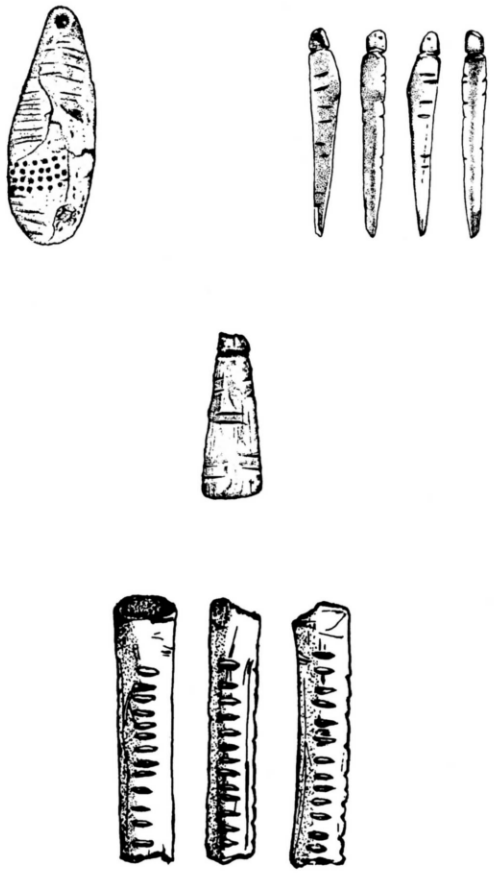

FIG. 2. *Some Aurignacian I bone and ivory objects from France considered to be purposely marked (top-right and bottom objects from Abri Cellier).*

the four other bone/tooth fragments with "subparallel incisions" from the same site.

Such perforated phalanges in Mousterian and Aurignacian contexts have been considered natural phenomena since the pioneering work of Henri Martin in the early part of this century and the more recent observations of Chase (1990). If found in an Aurignacian context, they would not be accepted as artifactual, let alone as evidence for some kind of symbolic activity. Moreover, I remain wholly unconvinced that any of the markings presented by Bednarik (unfortunately in the form of line drawings rather than photographs) show any more patterning than is exhibited by my kitchen cutting board. The problem, of course, is that claimants for early symbolic representation do not use experimental replication and testing to evaluate interpretations and hypotheses that emerge from close observation. For this reason, analyses like those by Marshack on the so-called zigzag from Bacho Kiro, the Tata nummulite, or the Pech de l'Azé rib remain controversial if not downright unconvincing. And the proposition that the naturally marked, hinge-fractured tusk tip presented by Bednarik is a "point" fails to consider natural damage marks and tip fractures almost always present on adult elephant tusks.

Finally, with respect to the Middle–Upper Paleolithic transition, the picture is certainly more complicated than when I wrote on the subject in 1982. There does appear to be a long period of archaic/modern contemporaneity in some regions. However, the idea that there is no skeletal evidence to suggest that the Aurignacian was the work of anatomically modern humans is overdrawn. Gambier (1989) summarizes the relevant skeletal material in France, referring all of it to modern humans. Bednarik seems to have forgotten the modern human crania from Aurignacian sites like Vogelherd, Cro-Magnon, and Mladeč.

In sum, we do not have hundreds of perforated objects and "concept-mediated" marks from the Lower and Middle Paleolithic, nor do we have "thousands" of "exotic" specimens from pre–Upper Paleolithic sites. What we have is a rag-bag of incidental marks that result from patterned human technological behaviors that have little to do with concept-mediation. As Bednarik notes, we need well-founded criteria for distinguishing intentional marks that were intended to be viewed from somewhat patterned arrangements of marks created incidental to other tasks. However, he does not provide such criteria; rather, he presents a "taphonomic hypothesis" and some very dubious examples in support of the notion that marked objects were more frequent early-on than has been hitherto recognized.

However—and this point should not be lost—even if a handful of these specimens really are purposely constructed visual forms, the abundance of material representation at the beginning of the Upper Paleolithic across Europe and at roughly the same time as far afield as Australia and Africa becomes ever more interesting: If the predecessors of the Aurignacians were capable of graphic representation of a symbolic nature in durable materials, why did such behavior occur at such a low frequency? Given the hundreds of Mousterian sites excavated to date, many of them with excellent organic preservation, I do not accept a taphonomic explanation.

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I wish that Bednarik had devoted more space to his cognitive interpretations. As it stands, there is very little about cognition in this article, despite his apparent advocacy of a cognitive approach. Cognitive scientists have developed powerful theories about human and nonhuman cognition that could have provided him with well-established concepts applicable to his evidence. He introduces the notion of "concept-mediation" more as an afterthought than as a centerpiece of interpretation. He certainly does not define it adequately, nor does he develop it as a concept that has particular relevance to the data he discusses. There are two places in his interpreta-

tion where he could have made much more effective use of the cognitive-science literature.

The first is his discussion of intentionality. Given his emphasis on levels of intentionality, it is remarkable that Bednarik does not refer to the very extensive literature on this concept. Even if he did not apply the more philosophical considerations of Dennett (1987), he could certainly have made use of some of the primatological applications, such as those of Cheney and Seyfarth (1990), Whiten and Byrne (1992), and Tomasello, Kruger, and Ratnet (1993). The question of intentionality has even been applied in Palaeolithic archaeology (Wynn 1993). Had Bednarik consulted some of this literature, his discussion of intentionality and levels of intentionality would have been much richer and perhaps more convincing.

The second is his discussion of "geometric conceptualization." In recent years archaeology has made solid contributions in the area of cognitive evolution by focusing on well-defined spatial abilities. Robson Brown (1993), for example, has documented "mental rotation" in the spatial repertoire of late *Homo erectus* at Zhoukoudian. Mental rotation is a cognitive ability that has been extensively documented in the psychological literature. The resulting understanding of how it develops and how it is tied to other abilities (Halpern 1992) provides the theoretical milieu within which interpretations of prehistoric behavior can be made and evolutionary developments can be understood.

Recently, many scholars have appeared willing to describe themselves as cognitive archaeologists. Unfortunately, much of the work produced so far has been poorly grounded in cognition. Rejecting an evolutionary or materialist explanation for one's archaeological evidence does not automatically make one a cognitive archaeologist. It is also necessary to use concepts developed to describe and explain cognition, to understand the theories, and to be able to modify the methods to fit the limitations of archaeological data. Cognitive archaeology need not be a poorly defined attempt to deconstruct archaeological "texts." Cognitive theories and concepts are well established and often extensively corroborated by experimental and comparative evidence. Why not make use of them?

Reply

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I thank the commentators for having been so generous with their patience, time, and knowledge. The wide spectrum of views reflected in their comments suggests that my essay, however "provocative" to some, still seems to occupy the middle ground in this debate. While Halverson and Wynn advocate even more "radical" positions than I, the conservative majority in the discipline is represented only by White and Nowell. White's gallant defense of his besieged model is therefore of particular interest here. He has undoubtedly written more than anyone about the earliest beads and the origins of body adornment. His frequent claims that the perforated teeth from Bacho Kiro are the earliest credible personal ornaments in Europe (White 1992:546, repeated verbatim in 1993*a*:279 and 1993*b*:333) would lead one to assume that he had studied this kind of evidence closely. Not only, however, are they epistemologically false (surely what he means to say is that these teeth are the earliest *known* such objects) but also they are grave errors of fact. Instead of responding to my crucial claim that drilled animal teeth and other objects may be up to six or seven times as old as the Bacho Kiro teeth, he retreats to citing Stepanchuk's reflections on much younger material. He has seen neither that nor the earliest known drilled objects, those from the Repolusthöhle, first published over 40 years ago (though not in English until 1992), viewed by tens of thousands of people, and examined by me 30 years ago (Mottl 1951; Bednarik 1992*a*:fig. 3), nor has he considered most other relevant material predating the Aurignacian or from outside Europe. He emphasizes that he has examined much of the relevant evidence personally, but what does this really mean? That he has "studied virtually all the known Aurignacian marked objects and personal ornaments from across Europe" but not those of earlier periods—presumably because he does not consider them relevant to a study of Aurignacian material. That may be so, but does his neglect of pre-Aurignacian material entitle him to reject it?

In another notional proposition, White "suspects" that there is more ochre from two French Aurignacian sites than exists in collections from all known Middle Palaeolithic sites (globally, presumably) combined. Is he not aware than an estimated 100 tons of iron ore was mined at just one Middle Stone Age site, Ngwenya, carbon-dated to about 43,200 B.P. (Dart and Beaumont 1971)? It is not likely all to be held "in collections," but in any case this would not justify ignoring the widespread use of ochre or haematite by Lower Palaeolithic hominids in three and Middle Palaeolithic ones in four continents. Conversely, petroglyphs are not, as Nowell thinks, the result of ochre application; they result from a reductive process, and the ochre crayons I referred to bear microscopically identified rock striations, not traces of hide working. What property of ochre other than its color (which would still suggest a nonutilitarian reason) could possibly invite its use as a tanning agent?

White thinks he disagrees with me concerning the Russian sites but then goes on largely to confirm what I said: the Strelets culture (with its distinctive Middle Palaeolithic elements and diagnostic triangular bifacial points, still abundant at Sungir) yielded in just three burials (occupying only a few square metres) an incredible number of beads, while the typically Upper Palaeolithic Spitsyn culture produced only about 20 "decorative" objects. Where does White see the disagreement?

We have examined similar evidence, as guests of the Moscow Academy of Sciences, even in the same year, and we have drawn fairly similar conclusions from the experience: that the industry with Mousterian roots so well reflected in the lithic typology at Sungir [cf. Bader 1978:figs. 87–89, 91] has yielded about 700 times (!) as many beads as the roughly contemporary industry which seems free of Mousterian traits. If it were White's intention to demonstrate that beads were introduced by invading moderns, then he would need to show that they were common in imported traditions, not that they are most common in apparently locally evolved ones! The only substantive disagreement I perceive here concerns the dating of the Spitsyn culture at Kostenki 17: White says it has been dated to ca. 37,000 B.P., whereas I have said it is older (Bednarik 1992*a*). I let the readers decide whether an occupation deposit is dated by an *overlying* sediment or only minimum-dated by it.

Again, White chides me for claiming that there is no skeletal evidence that the Aurignacian was the work of anatomically modern humans. Not only did I clearly state that such evidence is lacking from the *Early* Aurignacian (not from the Aurignacian generally) but I added that this may also apply to the subsequent Aurignacian 1. Surely he knows that the French Aurignacian is divided into a series of phases and that the Cro-Magnon remains are probably from the Aurignacian 2 (Movius 1969). The human remains from Cro-Magnon, Vogelherd, and Mladeč are irrelevant to the question of the physical appearance of the people of the Early Aurignacian (or "Aurignacion 0") vis-à-vis those of the Châtelperronian (absent in Central Europe). In fact, from White's own perspective Mladeč is a rather unfortunate reference because that series of hominid remains includes distinctive Neanderthal traits, for example, occipital buns (Frayer 1986, Smith 1985, Trinkaus and Le May 1982; see also Předmostí 3), rendering the straight replacement hypothesis particularly vulnerable.

While I perceive no value in White's figure 1 (I have studied vast numbers of taphonomic markings on bone and various other materials, as have many other researchers), I do welcome his figure 2, even as line drawings. Two of the objects he depicts as among those from the Aurignacian bearing double incisions or punctations on which White has himself commented (1989:377). As I noted in my paper, a convention of making such double marks occurs also in the Mousterian (fig. 9). White has not noticed this in the field or in my paper, so in another slip he offers evidence that supports my model rather than his. I have depicted similarly marked objects from both periods side by side (Bednarik 1994*c*) and described "pairedness" as frequent in early markings (Bednarik 1992*a*:37). I include the eight pairs of cupules from La Ferrassie (fig. 6) with this evidence. If such a distinctive cultural tradition appears in both these periods, does this mean that the Aurignacians copied it from the Mousterians? This is certainly a different scenario from White's suggestion that the Neanderthals scavenged decorative objects from the Cro-Magnons. There are several possible scenarios for the cultural (and racial) articu-

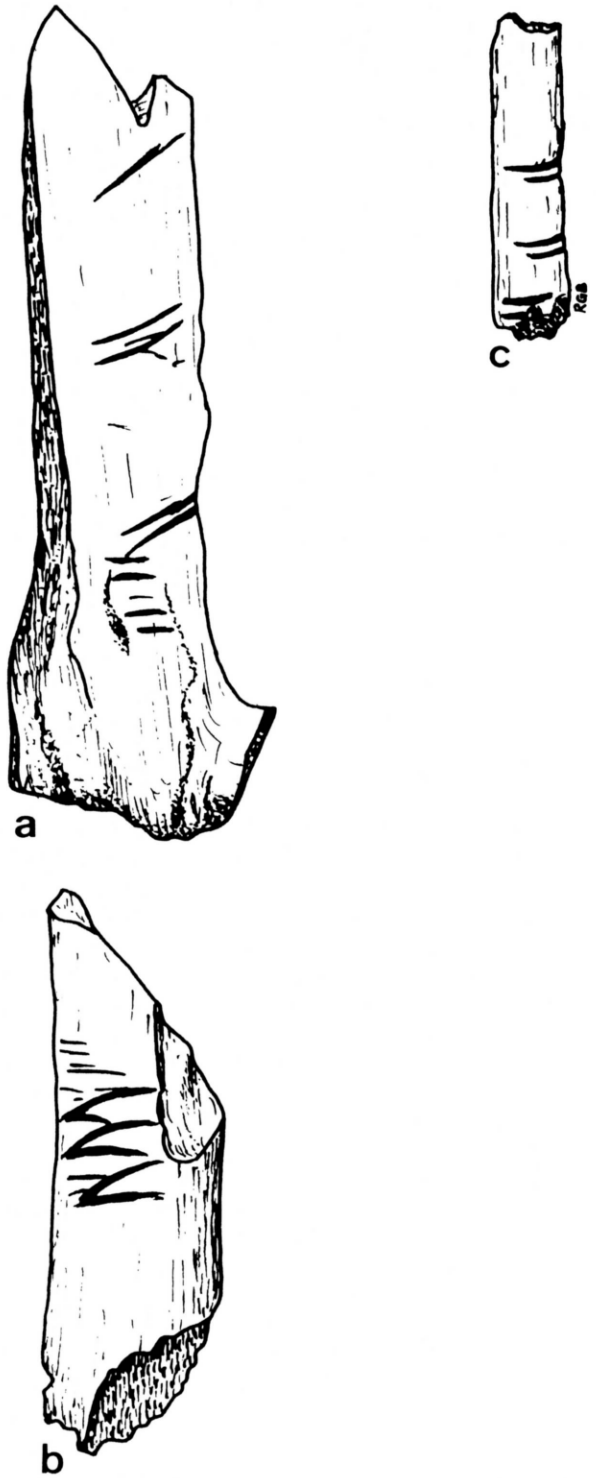

FIG. 9. *Paired incisions on Mousterian artefacts from Abri Lartet* (a, b) *and Cueva Morín* (c).

lation of the Mousterian, Châtelperronian, and Aurignacian, and perhaps White could consider not just those that would preserve his hypothesis but also the more plausible ones among them. True, he does suggest two alternatives to the Neanderthals' scavenging of ornaments, but they are both just as absurd. There are many traces of cultural continuity connecting Middle and Upper Palaeolithic material evidence: paired marks, stone walls, cupules, mammoth bone dwellings (consider Molodova 1 and 5), underground mining, seafaring capability, ochre use, and beads and pendants, to name but a few.

This is not to say that White's comments are without merit in the present debate. Their illustration of epistemological dynamics in the discipline is most helpful, showing us how dominant paradigms can be predicted on the limitations of their most ardent protagonists. White's comments are best seen as those of an expert on just French and Russian beads of just one material, ivory, and just one period, the early Upper Palaeolithic. It should be self-evident that the main theatres of hominid history were not in Europe, and yet Pleistocene prehistory is clearly a field in which the tail continues to wag the dog. Bahn and Straus are among the commentators who find this parochial approach stifling and "Périgord-centric," while White seems to make a virtue of Eurocentrism. He expressly declines to debate the non-European record, "leaving it to others" to evaluate my characterization. White's Aurignacian beads *might* be relevant in the context of a debate on global early symbolism if they were in fact the earliest "fully iconic warrants" (Byers 1994), but since we have significantly earlier beads (even in Europe) they are not relevant at all—even before we consider the very doubtful relevance of beads in this context in any case (see Duff, Clark, and Chadderdon 1992). His theory of body decoration has no basis in either fact or logic: beads occur in the Middle Palaeolithic technologies of four continents, and they are unlikely to have been the first form of personal adornment.

The dominant model has "art" first appearing in the Aurignacian and initially restricted to the Upper Palaeolithic of Europe. On the other side of the globe, in Australia, a Pleistocene art body greater than that of Europe relates to technologically Middle Palaeolithic traditions. No archaeological acumen is needed to see that Asia is crucial in reconciling the data; it is to cognitive evolution what Africa is to the physical evolution of hominids, and yet Asian evidence of Pleistocene symbolism has been badly neglected. Because of the discipline's preoccupation with Europe, only one researcher has ever considered Asian Pleistocene art on a pan-continental basis (Bednarik 1995*a*). Does this constitute a balanced, scientific approach to the subject? Would such a partisan approach be acceptable in any other discipline, say, in plate tectonics?

While White's retrogressive views are of value only in a heuristic sense, I regard certain criticisms by Wynn and Halverson as constructive and valid. For instance, I readily concede that I made inadequate use of cognitive theory in this particular paper. Of course my theme has implications for cognitive archaeology, just as it is relevant to the topic of modern human origins. But I did not set out to address either of these or any of the other topics that are related to my subject. The purpose of my paper was posited adequately and has been appreciated by most commentators and aptly spelled out by Duff and Clark. I should mention that I find all cognitive pronouncements about early lithic technology tenuous: it seems too easy to impose the analyst's concepts on such evidence. For instance, I remain sceptical that "mental rotation" of tools has been demonstrated, even though it could quite plausibly be attributed to *H. erectus*. The complex Levallois technology cited by Clark seems more cognitively revealing than our ideas about "concepts" of tools, for much the same reason as the mining of concealed minerals provides sound information of this kind (Bednarik 1995*b*). I believe, however, that non-utilitarian activity traces are still more likely to reveal that most elusive of archaeological variables, intentionality. The inertia of a discipline steeped in a fundamentalist belief that all modern human characteristics appeared with a big bang in France on the first day of the Aurignacian has to be overcome before Wynn's and Halverson's valid arguments are likely to receive adequate consideration. I appreciate Halverson's references to the "marking strategies" of infants and nonhuman primates, and I alluded to these in my paper when I referred to the ontogenetically and phylogenetically earliest arts (as does Bradshaw), but they are of little interest to a discipline in which human ethologists are massively outnumbered by students preoccupied with the "roots of Western civilization."

Responding to Wynn's demand for a discussion of intentionality and concept-mediation, I have to say that it was precisely my wariness of the slippery concept of intentionality that prompted me to opt for the second term. A concept is merely a mental formulation, especially one derived from specific instances and imposed on an orderless reality. Humanly perceptible externalizations of the imposition of human concepts onto physical reality (especially art) render human reality-building processes possible, because the neural structures supporting such concepts become available for processing natural sensory stimuli in a taxonomizing format (Bednarik 1994*e*:149–50). Human consciousness derives from this. The manufacture of the Acheulian cupules (Bednarik 1995a:fig. 5), for instance, demanded a sustained effort (hundreds of tool applications) involving a clear idea (a *concept*) of the intended outcome. Without that it would have been impossible to achieve such abstract, purposeless products. Apes produce no cupules (and in the wild no markings that we know of) or any other kind of mark involving repeated or "structured" application of a tool to create single marks. Nor have I heard of pongids' burying their young in a cave and placing a rock slab on the grave after first having hammered cupules into the slab's underside. I am surprised that neither Wynn nor Halverson cites the pebble from Makapansgat, supposedly a manuport indicating iconic rec-

ognition by australopithecines (Dart 1974). Conversely, simple stone knapping is not beyond pongid capabilities, and even in the wild there can be surprisingly complex "tool kits": the chimpanzees of Tai Forest have been reported to use 19 different tool types. Nevertheless, there is a qualitative difference between their tools and those of the Lower Palaeolithic, and precisely the same applies to markings.

As a neuropsychologist with a deep interest in hominid evolution, Bradshaw agrees that an instant explosion of modern cognitive faculties some 35,000 years ago is biologically improbable. For centuries, humanist disciplines have preserved the religious distinction between humans and other animals, but we know today that humans possess not one faculty that is not shared by other species. Communication, symboling, intentionality, deception, tool making, and so forth, can all be found elsewhere in the animal kingdom. However, Bradshaw's characterization of White's position in this debate is mistaken, and Hayden's (1993) coverage, which I have cited elsewhere, cannot be similar to mine of the Lower Palaeolithic by virtue of its dealing with Neanderthals. Concerning the notion that I reject the African origins of anatomically modern humans, I find it much more important to appreciate that there is no correlation between the anatomical, the cognitive, and the technological evolution of hominids than to partake of that overrated argument. It is amply evident from my paper and the comments by Bahn, Bradshaw, Clark, Duff, and Straus that none of the major advances in hominid development are related to stages in physical evolution as we perceive them. The sub-Saharan Middle Stone Age continues through the first two-thirds of Europe's Upper Palaeolithic, and there is nothing to imply that the moderns were, for most of their known history, outstanding innovative. Archaic *H. sapiens*, however, clearly did produce objects we tend to think were non-utilitarian. It is of no relevance to the topic of the earliest concept-mediated markings whether morphological moderns evolved exclusively in Africa. Most commentators accept and appreciate this point; some view it as supporting the multiregional model of recent hominid evolution.

No one could justifiably quarrel with d'Errico's proposition that Marshack-style "internal analysis" and replicative experimentation would be useful in assessing early engravings. But it still needs to be appreciated that such methods refer to forms of uniformitarian analogy; they provide no absolute proof. D'Errico's technological approach guarantees no immunity from errors of judgement (e.g., d'Errico 1991, Huyge 1991). He seems unaware that I have questioned markings from Cueva Morín (Bednarik 1992*a*) and rejected thousands of other markings which archaeologists had identified as portable or rock art (e.g., Bednarik 1994*f*). The controversial status of the Pech de l'Azé object and some of Leonardi's material was also acknowledged in my paper. In relation to the Bilzingsleben objects, traces of multiple application of tools on object 2 are clearly discernible in the superb published photographic enlargements (Mania and Mania 1988; see the distinct splaying in the alignment of individual sets on object 2), of which d'Errico does not seem to be aware.

D'Errico's observations concerning taphonomic effects related to elephants are much appreciated. The possibilities he suggests need to be considered objectively rather than in the context of the still-dominant paradigm, which is biased against pre-Aurignacian evidence of symbolism. The status of Lower Palaeolithic engraved markings on bone is not even central to the topic of my paper, and I emphasized that by themselves most do not present a strong enough case. But the engraving on the Bilzingsleben stone slab is not a gnaw mark, blood-vessel impression, or trampling mark, and d'Errico's favored methods are unsuitable for testing interpretations of cupules, pigment use, and the collection of crystals or other exotic manuports (nor, I am sure, are there any cupules on White's oft-mentioned kitchen cutting board). The Quneitra engravings that Straus mentions and Bahn refers to are highly relevant here. Their engraver struggled with precisely the same problem as the artisan who marked the Bilzingsleben slab: how to engrave a curved line on a particularly hard surface. In both cases there were repeated attempts to accomplish this, visible as short tool strikes veering off course. I am satisfied that Marshack has convincingly demonstrated the repeated application of tools on the Quneitra stone object.

D'Errico's "entirely modern character of the Aurignacian behavior" should be seen in the light of Clark's eloquent rejection of such simplifications: what we know about the human behavior of any Pleistocene period with any semblance of scientific rigor is so inadequate as to make any definitive construct based on it premature. This applies equally to White's construct of "emerging social formations." Increased sophistication of research techniques as advocated by d'Errico could certainly improve the situation, but it would be of limited value if we continued without subjecting our work to the rigor of a valid unified theory.

A principal difficulty with my argument for a few commentators is that they interpret it as a "taphonomic hypothesis." Taphonomic logic is not a hypothesis but a logical framework within which to test whether archaeological propositions warrant our perseverance. The fundamental question is not whether any or all the examples I list in my paper are evidence of non-utilitarian activity but rather how to explain the perceived *frequent* appearance of "symbolism" towards the final Pleistocene (around 32,000 years ago) in terms of the model demanded by cumulative taphonomic reduction. My review of the available evidence suggests that it is entirely consistent with what the theory would predict. If we were to view this as a confirmation of taphonomic logic we would only return to the epistemologically debilitating confirmationism I am trying to avoid. Similarly, I could cite the pongid markings that Halverson mentions to show that they confirm some trajectory of graphic evolution through time. Perhaps they do, but again I am not willing to resort to confirmation for the sake of

bolstering my hypothesis. The reason for this—and it has not been appreciated by some of the commentators—is that I favor a scientific approach over a traditional archaeological one.

Taphonomic logic is an irrefutable axiom of logic; it is not here to be tested. To question it one would have to show that a cumulatively increasing body of material evidence that is subjected to continuous and uniform reduction processes would not reach a point when almost all residue over a certain age had been eliminated from its composition and that this point is not later than the time when the evidence commenced accumulating (see Bednarik 1994*b*:fig. 2). This is logically impossible and empirically unsustainable: are we to assume that Pleistocene hominids had no soft tissue because we have not found any? Or that *H. erectus* hitched rides on trained dolphins to cross the ocean to reach Flores perhaps 700,000 years ago (Sondaar et al. 1994) because we have no material evidence of Pleistocene boats? Why do we have such vast numbers of rock paintings in all continents beginning with the end of the Pleistocene but none from the preceding periods (except in limestone caves, with their totally different preservation conditions, or under silica skins)? Science prefers simple explanations. Why would one construct some elaborate hypothesis for this "sudden global flowering" of rock painting 10,000 years ago when taphonomic logic not only offers an extremely simple solution but in fact demands it? *Every form* of archaeological evidence has a universal taphonomic threshold, as noted by Clark, ranging in time from infinitely little to almost 100% of the phenomenon's historical duration, depending on the type of evidence and countless other factors. No evidence of any kind survived for more than a second of over 99.99% of all the things that ever happened in the archaeological past. Of the still innumerable remaining instances, evidence survives for only a tiny fraction of one-millionth of a percent. Of this, again only an infinitesimal portion has been recovered by archaeology, of which an even smaller part has been correctly construed. The dynamics of preservation, recovery, and interpretation all entail massive and *systematic* biases, and they are the subject of metamorphology (Bednarik 1995*c*), the scientific version of archaeology. To take the unfiltered "record" and "interpret" it within the framework of an unfalsifiable discipline on the basis of random and subjective parameters and without any underlying theory of how this "record" might relate to historical reality of the past does involve more than optimism; it requires blind faith. Lacking the missionary zeal of those who try to tell us that they already know what happened in the past, I can only regard the success of archaeology in producing *some* plausible and consistent models without the benefit of metamorphology and taphonomic logic as a credit to the discipline's ingenuity. Nevertheless, there is a scientific framework available to us now, and to misinterpret an application of it, such as my paper, as some optional "taphonomic hypothesis" which we can indulge in if we like that sort of thing does not do justice to the present debate—or to science.

This debate's outcome is that Bahn, Bradshaw, Clark, Duff, and Straus support my sentiments or findings either completely or at least substantially. D'Errico, Halverson, and Wynn seem to agree wtih my main premises in principle, although they would like to see certain alternative avenues better explored. Nowell thinks that "we must be more judicious," apparently advocating adherence to the orthodox model as if it had any redeeming features. White is clearly unaware that his "rag-bag" would need to hold not only hundreds of tons of ochre and over 40 tons of boulders with cupules but also vast numbers of Middle Palaeolithic petroglyphs from Australia. The latter alone probably outnumber the combined rock art motifs and portable art objects of the Upper Palaeolithic. The notion of an "explosion-like" appearance of certain cognitive faculties seems no longer sustainable: such an event cannot plausibly be attributed to a specific time or place; the various forms of evidence cited for it seem to be spread over a period of perhaps 300 millennia and cannot consistently be related to any one region in the world. The conclusion, then, should be the same as in response to a previous fundamentalist model, which concerned the separation of humans from other animals. Just as that anthropocentric differentiation was designed to preserve contingent ideological prejudices of Western-based academic culture and withered away under scientific scrutiny, this last ideologically motivated humanist stain on the past will similarly evaporate under sustained critical attention.

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