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Palaeoart and Archaeological Myths

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This article addresses the question of human symbolic behaviour in the Lower and Middle Palaeolithic. Bednarik argues that the case against such early symbolic behaviour usually relies on untestable propositions about the stages of human cognitive development, and that too little attention has been paid to the full range of evidence for Lower and Middle Palaeolithic symbolism. He urges that the same criteria should be applied in assessing this evidence as in assessing the more widely accepted evidence for Upper Palaeolithic symbolism. In their following reply, Chase & Dibble observe that many quite reasonable hypotheses in the historical behavioural sciences cannot be refuted absolutely, and that where competing hypotheses are presented it may be difficult to decide which is most probable. Finally Davidson, also replying to Bednarik's criticisms, concludes that their different views of the evidence for early hominid symbolic behaviour arise from different objectives and conventions of understanding.

One of the most intractable problems in palaeoart studies, that of art origins, is also one that has traditionally attracted the greatest attention and most heated debate. On close examination this topic is found to be surrounded by such a multitude of speculations, *non sequiturs*, idiosyncrasies, distortions and biases that the enquirer soon despairs. To illustrate the point one needs only to consider the vast discrepancies in the views on, say, the cognitive abilities of the Neanderthals. Not only is there practically no agreement on what the data indicate, there is none on what constitutes the relevant data, what time frame one should refer to, how one would define art or distinguish it from non-art, or any other issue that seems crucial for approaching the topic objectively.

Such a state of affairs is detrimental to the discipline, and it seems useful to examine the reasons for the excessively wide spectrum of views on the earliest cognitive development of humans, and to explore the dynamics determining the direction of research in this area of 'knowledge'. The ability and inclination of some scholars to exploit the inherent weakness of most archaeological interpretations, that they are not refutable (Tangri 1989), has facilitated the emergence of essentially unsupported hypotheses with sometimes quite extreme connotations. These have spawned interesting tabloid headlines in some cases, but they have also left the discipline with the task of hatching these academic cuckoo eggs. That task is not made any easier when, in defending their theories, the protagonists resort to buttressing their propositions by the misapplication of epistemic axioms, reminding us that 'observation statements are fallible' and 'cannot conclusively falsify a hypothesis' (Lewis-Williams & Dowson 1988).

While pluralism as such is to be welcomed in any academic sphere, the cost to our discipline in terms of the resources and effort required to rebut unfounded and whimsical but ardently defended theories should not be overlooked. While in other fields, such as the hard sciences, erroneous or notional conclusions, or the hypotheses deduced from them, tend to be subjected to rigorous testing, this mechanism is not as effective in archaeology. Proponents of a hypothesis in, say, biochemistry or astronomy need to be amenable to refuting evidence, while archaeologists need not necessarily concede weaknesses in their models where these are couched in non-refutable terms. In recent years this has led to a series of prominently publicized hypotheses which seem to have been entertained primarily for their apparent elegance and explicative eloquence, or for an ostensible capability of elucidating not just a few, but a whole class of phenomena or data in one single sweep.

Economy is desirable in explanation, but economizing in the use of data in the process of formulating a hypothesis is quite another matter. Here I will consider the role of exhaustive review of evidence, and how flawed paradigms in archaeology can determine the biases of the researchers championing them. Academic practices may have a tendency to trap researchers in their own theoretical creations; they encourage selective acquisition of confirming 'evidence' and specious defence of favoured models. Thus the heuristic dynamics of archaeology are under review.

In what follows, 'iconicity' refers to the visual quality of a motif which conveys to most contemporary observers, especially Westerners, that a specific object is depicted; it is therefore a subjective, but useful, definitional tool. A non-iconic art is one in which we cannot recognize the objects that may be depicted, or one which does not depict objects.

Symbolism and language origins: two recent hypotheses reviewed

The summary of Middle Palaeolithic evidence of 'symbolism' provided by Chase and Dibble (1987) may seem a reasonable and balanced overview, but its loose epistemology, illustrated for instance by the frequent use of terms such as 'stylistic', 'aesthetic', 'ritual', 'symbolic' or 'intentional', invites a more rigorous review of the evidence. Being unsure about the intended meaning of much of the terminology, I find it difficult to accept the paper's findings, or even follow its reasoning. To give an example: what does the word 'intentional' mean in the context of pre-Upper Palaeolithic art? Is not the deceptive, co-operative or reconciliative behaviour of non-human primates (De Waal 1982; Byrne & Whiten 1988) an example of 'intentional behaviour'? Perhaps it is not, but then I would like to see the differentiation defined before considering speculations about hominid intentionality.

Chase and Dibble

> . . . assume that [art] objects or patterns do have symbolic meaning in the Upper Palaeolithic . . . because they occur in the context of art known by other criteria to be symbolic, criteria which are absent from the Middle Palaeolithic record (Chase & Dibble 1987, 282)

What are these convincing criteria in the Upper Palaeolithic? 'Upper Palaeolithic art is without doubt both aesthetic and representative in nature', we are told. Why must we assume that this art represents, and why could an art consisting of nothing but circles and crosses not represent (Brook 1985)? What is it that renders this corpus of art aesthetic, other than our perception of aesthetics? One would value a definition of the word 'aesthetic' which can be understood by an intelligent organism that lacks any form of anthropocentricity or human conditioning.

Chase and Dibble continue:

> In addition, there are many reasons for believing that [Upper Palaeolithic art] had a symbolic and ritual aspect: the hidden and even hard-to-reach location of much cave art, the non-random distribution of different signs on [images of] different species of animal, the superpositioning of figures, and representations of beings that do not exist.

The almost complete restriction of 'cave art' to caves is certainly not a function of cultural selection, but one of geomorphological selection (Bednarik 1986a). By attributing this distributional bias to the wrong 'crucial common denominator of the phenomenon category' (Bednarik, in press) the authors succumb to the most common error in interpretational archaeology. So-called 'cave art' is not found in caves because it was only produced in caves, but because it survived only in caves - in fact only in certain limestone types and speleoclimatic conditions. The first Pleistocene rock art found in Germany only confirms the problem: cave walls in the Alpine region were frequently reduced to cryoclasts, especially in the final Würm stadial, which is not the case in the Franco-Cantabrian region. The arguments about the geographical distribution of 'cave art' and the comparison with that of portable art are logically wrong and scientifically invalid (Bednarik 1986a), yet they continue to be repeated. They are reminiscent of the frequent archaeological claim that the distribution of the Upper Palaeolithic female figurines indicates the geographical extent of a tradition. What it does indicate, at the very best, is the extent of the area in which they were preserved: with few exceptions the figurines consist of calcium carbonate (ivory or limestone) which has survived only in high-pH, carbonate-rich soils (loesses or cave deposits). Thus the crucial common denominator of this category of phenomenon in respect to distribution is condition of preservation, not the geographical extent of a tradition (Bednarik 1991a).

The perceived 'non-random' distribution of 'signs' is also open to debate. It relies on two subjective assumptions: a) that the relationship expressed is intentional, and b) that certain motifs are signs. Neither assumption has been demonstrated to be true, but neither are they scientifically falsifiable. Nor has it been shown that superimpositioning was intentional, and since most superimpositions in other rock arts

appear to be fortuitous, or refer to two quite different cultures, this argument is also specious. Finally, the identification of certain motifs as non-realistic creatures demonstrates only, in a scientific sense, that there are motifs which Chase and Dibble identify as such creatures on the basis of their own cognitive abilities. Since no-one has demonstrated that modern western neurophysiological processes of matching cortically-established patterns of identifying iconicity are identical to those of Palaeolithic producers or consumers of art, their iconic identifications remain working hypotheses. Objects can of course be depicted non-iconically, and abstract concepts can be graphically expressed in iconic form. It seems to the present writer that the cognitive processes in depicting and perceiving a therianthrope iconically are no more sophisticated than those involved in depicting it non-iconically, say, as a circle.

None of this is intended to show that Upper Palaeolithic art had no symbolic function; it merely means that scientific proof is lacking for the postulate as presented. The iconic content of the art is quite prominent, and renders it less likely to be symbolic than the geometric art of, say, the Walbiri (Munn 1973) or the Shipibo-Conibo (DeBoer 1990) - or the zigzag pattern on the Bacho Kiro bone, for that matter.

It is not difficult to see how these authors could have arrived at their conclusions. Notwithstanding their professed 'descriptive rigour' they act as art critics, judging some engravings as quite obviously symbolic, others as 'usually quite crude when judged as art'. They seem to equate simplicity of design with backwardness, in a fashion belonging rather to the ethnography of the nineteenth century. One can only ask how extant societies without art, or with similarly 'crude art', would fare in such a Eurocentric comparison.

It is therefore not surprising that Chase and Dibble make so much of aesthetic sensibilities. While they clearly appreciate that stone tool taxonomies reflect the cognitive categories of archaeologists and not those of their users, they completely fail to see that the same applies to art, and indeed much more so. After all, stone tools do have some utilitarian functions, and not just 'aesthetic' ones.

The evidence is not quite as clear-cut as Chase and Dibble describe it. To begin with, the corpus of rock art they refer to as Upper Palaeolithic is not defined on the basis of proof, but on the basis of what has come to be accepted as belonging to that corpus, largely by perceived style, location and association. Certain parameters are perceived to apply to a tradition, and once established in the mind of archaeologists they become the basis of a confirmationist taxonomy. In fact, very little Upper Palaeolithic rock art is plausibly dated (Bednarik 1992a), and there is only one example of a direct date from any of the 275 sites: from Cougnac (Bahn & Vertut 1988; date in Lorblanchet *et al.* 1990), besides one possible date by association at Tête-du-Lion (Combier 1984). It is self-evident that rock art found concealed by an occupation deposit, as in Pair-non-Pair, La Grèze, Sainte-Eulalie, Gargas, Laussel, Cap Blanc, Roc de Sers, Angles-sur-l'Anglin, Teyjat and Isturitz, is likely to be older than that deposit (but not necessarily so), so at best we might derive minimum ages in this fashion. It should be a warning to consider that similar, subjectively-based chronologies for rock art have recently been rejected in eastern Spain (Hernández Pérez *et al.* 1988) and in the Sahara (Muzzolini 1990), and that the attempted archaeological dating of petroglyphs in Karelia was rejected even though its age estimate turned out to be fairly accurate (Bednarik 1992b).

It may be that all of the rock art Chase and Dibble would consider to be Upper Palaeolithic is indeed so; but surely this requires much clearer resolution than is presently available? There have been many fraudulent claims concerning such art, and there remain controversial sites. Do Chase and Dibble include these or not? Have they taken note of the suggestions by two writers (Bahn 1984; Bednarik 1984a) that there is no clear-cut proof that none of the so-called Upper Palaeolithic art could not predate the period it is hypothetically being attributed to? This is especially pertinent in view of their acceptance of at least one rock art panel (the cupules from La Ferrassie) as Mousterian. Surely greater care is required before we permit these tentative, often highly speculative models to dominate our thinking.

Before designating any rock art as Upper Palaeolithic, archaeologists would need, firstly, to show how they have determined the age of the art in question, and secondly, they would need to clarify at what time the Upper Palaeolithic began, bearing in mind that its temporal overlap with the Neanderthal phase in Europe seems to span many millennia.

Chase and Dibble concede that there are isolated occurrences of symbolic behaviour in the Middle Palaeolithic, but as they attribute a 'rudimentary capacity for symbolic expression' even to chimpanzees one must assume that they postulate very little intellectual change for the entire duration of hominid evolution. Davidson and Noble, in taking a similar approach, are even more explicit: 'All human ancestors without language should be considered as apes, closer to chimpanzees than to humans' (Davidson & Noble

1990). No ambivalence here! After all, these authors perceive no evidence at all for language prior to the Upper Palaeolithic, although, strangely enough, they predict that it will one day be found. And they specify clearly the type of evidence required to demonstrate the faculty of language: iconic depiction.

Davidson and Noble's basic postulate is that mimicry allowed repeated reference to an object, and the 'fixing' of its outline on a surface. 'Memory' was created in this way, and in turn led to language. These authors are even more categorical than Chase and Dibble in rejecting suggestions of pre-Upper-Palaeolithic 'non-utilitarian activities', and Davidson (1990) goes to great lengths in challenging the credibility of several specific finds. Yet at the same time they predict that an iconic graphic art based on mimicry will be discovered which will be older than 32,000 or so years (Davidson & Noble 1989, 136, 151). In rejecting the earlier art finds they make much of what they perceive as the 'uniqueness' of these, claiming that for meaning to be symbolic it must be widely 'shared' (through language). Thus, according to them, symbolic meaning must have been preceded by language, which therefore followed figurative depiction.

Not only is this a highly tenuous model, it is offered without supporting evidence, be it 'unique', ambiguous, weak or controversial. The art tradition which Davidson and Noble predict we will discover one day amounts to no more than a vacuous promise: iconic animal outlines of the period immediately preceding the Upper Palaeolithic are entirely unknown throughout the world. In contrast, evidence of apparently non-utilitarian activities before the Upper Palaeolithic does exist, but most of it is ignored by the authors.

Davidson and Noble reject all Middle Palaeolithic evidence for 'symbolic' behaviour - even though they admit that some of it clearly suggests non-utilitarian behaviour - because it is non-iconic (Noble & Davidson 1991), and because indisputable instances are said to be rare. Presumably we should also reject the engraved chert core from Chandravati, India (Sonavane in Wakankar 1988), the stylized female engraving from Predmost (Jelínek 1988, fig. 21) and thousands of other 'unique' finds, simply because archaeologists have found no suitable pigeonholes for them!

They also describe the first colonization of Australia as the earliest evidence for the existence of language (Davidson & Noble 1990). Since this event probably took place at least 60,000 years ago (Roberts *et al.* 1990) and since there is general agreement that the colonists arrived from Southeast Asia, their hypothesis requires that iconic imagery was being produced there 30,000 years before the Middle Palaeolithic ended in neighbouring India (Agrawal & Kusumgar 1974, 44; Misra 1977), and at a time when 'ape-like' creatures roamed Europe.

This pre-60,000 BP tradition of iconic tracings of gestures which Davidson and Noble's hypothesis demands would presumably have first found expression on soft surfaces. Such markings do indeed exist, and many are thought to be among the earliest markings known. But they are entirely non-iconic (Bednarik 1986a), and the examples cited from Altamira and Pech Merle, both Magdalenian, are entirely irrelevant: much older finger markings exist at sites such as Baume Latrone, Bara Bahau and Croze à Gontran, for instance (Bednarik 1986a).

Davidson and Noble's brave hypothesis requires that the meaning of a gesture outlining an animal was communicated without language, and that the gesture thus became a symbol. Ignoring for the moment the fact that this explanation does not in fact explain how the person who has discovered the quality of iconicity conveys it without language to others, we must recognize that sketching the outline of an animal is hardly an effective method of denoting species, age, number, sex, etc. Less ambiguous and more specific gestures or other means of communication are surely possible, and can be more readily understood. Vocalized reference to particular species of animals has been attributed even to vervet monkeys (Seyfarth *et al.* 1980) who seem to have managed to establish this system without iconic depiction. Communication could relate to iconic or non-iconic aspects of the referent without even requiring direct iconic reference (as ethnographic observations would suggest); there is no need for iconicity to create symbolism. In fact the model of Davidson and Noble has so many flaws that to deal with them economically it is best to summarize the major ones in the form of a list:

1. The hypothesis becomes meaningless without Davidson and Noble's Eurocentric concept of iconicity (Tangri 1989). In the context of language origins it is not relevant what Davidson and Noble (or indeed the present writer) consider to be iconic. Alternative realities are possible and can be expressed graphically in forms which these authors would probably not recognize as symbolic.
2. The belief that 'memory' is contingent upon the fixing of gesture which attains 'meaning' (Davidson & Noble 1989, 133) is hardly acceptable to scientists (animal behaviourists, for instance). Extremely complex memory traces

exist in countless animal species, without gesture, depiction or language.

3. The global evidence of non-utilitarian activity is unanimous on one issue: there is a general trend from non-iconic to iconic marks. This has probably nothing to do with an 'upwards evolutionary mobility', but is related to a shift in the dynamics of human reality-building processes: seen more objectively, this shift may mark an aggravation of anthropocentricity rather than 'artistic development' (a self-contradictory concept!).
4. For the authors, culture exists only in the Final Pleistocene, after what they term 'reflective language' began. The scientific definition of culture, which frequently differs from that of archaeologists (e.g. they might consider a series of stone tools to define a specific culture, as if tools were diagnostic of cultures), renders it individual-specific (in that it reflects the individual's life-trajectory: Handwerker 1989; Bednarik 1990a) and differs significantly from that of Davidson and Noble which is entirely anthropocentric. The definition of cultural behaviour in the field of animal (including human) behaviour studies describes it as the passing of information from one generation to the next by non-genetic means. Linguists, philosophers and anthropologists regularly redefine language in order to exclude interloper species, and to defend their threatened ideas against the findings of more scientific disciplines.
5. Their claim that tracking began only after the introduction of language, which their model necessitates, would require that Davidson and Noble's fire-using 'apes' prior to the Upper Palaeolithic survived the climatic and ecological upheavals of the Middle Pleistocene without a faculty that is possessed by non-human predators. While many of the latter rely mostly on the sense of smell, there can be no doubt that the activity of the Laetoli hominid who walked in the footprints of another was visually co-ordinated, and some element of intentionality was probably involved.
6. Noble and Davidson's narrow, anthropocentric interpretation of the term communication (Noble & Davidson 1991) is not generally shared. Contrary to their belief, not all 'communicative systems are collections of gestures, whether vocal, manual or physical'. The communication media used by lower organisms, including chemicals, smells, pheromones, complex visual signals, light signal frequencies, etc., involve no 'gestures'; and some of the systems used simply cannot be detected: not only may the media be beyond our perceptual and technological capacities, we may not even be able to detect the action and response patterns of such communication systems (consider those of plants, for instance).
7. Davidson and Noble subscribe to Gamble's views on the female figurines of the Upper Palaeolithic, which suggests that they have not studied a significant part of this corpus first hand (Bednarik 1990b). There are other passages in their article which lead the present author to think that they have studied hardly any of the evidence they write about. While one does not need to have been to the moon to study its geology, it helps.

Epistemology and palaeoart studies

Davidson and Noble reject pre-Upper Palaeolithic evidence for 'symbolism' on the basis of a sample of 13 cases, implying that these are the only claims for potentially non-utilitarian marks they are aware of (nearly all had been listed by Chase and Dibble). Yet in order for such a hypothesis to be presented effectively it is necessary to deal with every known specimen of relevant evidence (for a list of 60 cases presented in an article before the Davidson and Noble hypothesis was published, see Bednarik 1988a). They list a 'bone fragment with incised lines' from Morín, Spain, but do not clarify which of the several marked bones from that site they mean. Marshack had conclusively refuted the anthropic character of some of these markings (Marshack 1986; 1991). They also list one bear tooth with grooves from Sclayn, Belgium, while Davidson (1990) concludes that three Sclayn teeth inspected by him may have been marked naturally.

This informal approach continues in Davidson (1990), with bias blatantly expressed: 'The [Bilzingsleben] marks do not fit with what we know of the earliest deliberate marking'. This translates readily into: 'The marks do not fit into my hypothesis, according to which they would need to be iconic.' On the other hand, he accepts similar marks on the arm of the Hohlenstein therianthrope as intentional in nature, simply because they occur in an Upper Palaeolithic context. I would argue that this approach of using different yardsticks to reinforce one's bias is fundamentally unscientific.

The Davidson and Noble scheme fails entirely as soon as we consider whether a mark needs to be iconic

in order to convey meaning, or in order to be symbolic. As Australian researchers they would be expected to be familiar with Australian rock art. Pleistocene rock art is much more common in Australia than it is in Europe. Most dates so far obtained for rock art sites in Australia are of the Pleistocene (for dating attempts see Rosenfeld *et al.* 1981; Bednarik 1985; Gallus 1986; Nobbs & Dorn 1988; Morwood 1989; McDonald *et al.* 1990; Loy *et al.* 1990). The total number of rock art sites in the continent is thought to exceed 100,000 and the Pleistocene component is either largely or entirely non-iconic. Iconicity never seems to have reached Tasmania, separated from the mainland towards the end of the Pleistocene.

Davidson and Noble do however put up a valid defence against some of the severe criticism to which their paper has been subjected, challenging Rosenfeld to produce 'a theory about how meaning could come to be attached directly to non-iconic traces' (Davidson & Noble 1989, 340). The problem, however, is not how to attach meaning, but how to communicate it. For conscious communication to be possible, one might argue, a system of communication is required which presupposes an awareness of a common experience. It is true that animals may not be capable of such 'reflective communication'. But I have proposed that externalization of autogeneous patterns of the mammalian visual system (phosphenes), whose form constants cannot be influenced and which remain immutable, would provide such a catalyst (Bednarik 1984b; 1987). Such form constants would not need to be communicated; they existed in all hominids. It was only necessary for their communicative potential to be 'activated'. This would be likely to occur in the course of experiment with random markings - such as those of the Middle Pleistocene which Davidson and Noble are so quick to reject.

Not only does this theory provide a response to their challenge, it is supported by evidence from all continents other than Antarctica, in the form of the hundreds of thousands of motifs which were produced prior to the appearance of iconic marks. In fact every single art motif in the world produced before about 30,000 or 35,000 BP in Eurasia and Africa, and before the final Pleistocene in Australia and the Americas, is non-iconic (Bednarik 1988a). I have furthermore proposed that every one of these seems to derive from a phosphene motif. Nothing would be easier to refute than these two postulates. This approach contrasts with that of many recent writers on the subject who perpetuate the myth that west European Upper Palaeolithic art represents the beginnings of artistic production, favouring a diffusionist model that is as Eurocentric as are its advocates. This ignores the fact that a major rock art tradition existed in Australia (and probably elsewhere) prior to 32,000 BP, and that it was non-iconic. Iconicity may indeed have been introduced independently in different regions and continents, rather than spreading from France to the rest of the world (together with culture and language in some Eurocentric models, evidently perpetuating a neo-colonialist framework). Nearly all of the Franco-Cantabrian cave art belongs to what archaeologists choose to call the Magdalenian, which means that it is nowhere near 30,000 years old, but belongs to the last 8000 years of the Pleistocene. Lindly and Clark, indeed, have convincingly argued in favour of a gradual increase in the incidence of symbolism, beginning in the Middle Palaeolithic (Lindly & Clark 1990).

I suggest it is time to give more attention to epistemology and rigour in these debates. More specifically, attempts to disprove the existence of non-utilitarian activities prior to the Upper Palaeolithic should be admitted only if their exponents have considered all relevant data, are familiar with a reasonable portion of it, and offer a reasonably objective approach.

As a first step in this direction, the rest of this article will present a list of some of the specimens and phenomena which should be considered in this context. The list is by no means complete - on the contrary, it is incomplete by intention. Nor is it implied that all the listed examples should be accepted as proof for anything - on the contrary, I have intentionally included several items which I regard as specious. I am merely offering a list of some of the items to be considered which have been mentioned in the literature.

This evidence may first be divided into objective classes, determined by material type and formal attributes. This does not suggest that the objects grouped together necessarily refer to similar practices: they are simply groups of 'similar' finds which have been reported by researchers with widely differing biases and preconceptions, who have been unable to explain the phenomena in terms of utilitarian function. The evidence would be considered as non-utilitarian in any post-Middle Palaeolithic context, and if we are to assess it objectively we need to ignore the bias of researchers who reject them because they believe they understand the capacities of early hominids. We possess no such insight, nor can we hope ever to gain any if we accept judgments based on preconceived models. The age of these finds is, I would argue, irrelevant to their potential symbolic role. The need for such an objective approach is illustrated by my recent study (Bednarik 1991b) of 46 'engraved' ostrich egg shell fragments from the Upper Palaeolithic of India,

which resulted in the clear rejection of 45 of them: confirmationists have been far too quick in rejecting or accepting non-utilitarian evidence on the basis of its age.

Some claims for evidence of Lower and Middle Palaeolithic symbolic behaviour

Use of ochre or haematite

Iron oxides and hydrous iron oxides are the most commonly reported pigments from prehistoric societies throughout the world. They were used from the Acheulian onwards. Ground ochre may have served for the production of utilitarian body paint (for protection against sunburn or, as in Tasmania, with fat as protection against the cold) or to colour objects or clothing. The distinction is not very significant, since any behaviour involving the use of ochre can be assumed to relate to comparatively complex cultural practices. To obtain ground powder one would grind the pebbles, whereas pebbles with striations on wear facets were probably hand-held and rubbed against a harder surface. Several of the very early haematite pebbles I have examined are very hard, and would need to be rubbed against quartzitic rock to acquire the marks they bear. They were almost certainly used as crayons. The following cases may be considered:

1. Haematite with striations and pigment powder on Acheulian floor at Beçov, Czechoslovakia (Marshack 1981). The piece of ochre is striated on two faces and measures 33 mm. At the base of the rock shelter, near where it was found, a wide area of the floor was covered with ochre powder.
2. Haematite pebbles, one with striated facet, from Acheulian site of Hunsgi, India (Bednarik 1990c). The striated specimen is only about 20 mm long and the striations, although clearly ancient, are so well preserved that the type of rock that made them could be determined by replication.
3. Several faceted fragments among 75 pieces of red, brown and yellow fire-treated limonite from Acheulian site of Terra Amata, France (de Lumley 1966).
4. Slab of ochre, apparently shaped, from Acheulian site of Ambrona, Spain (Howell 1966, 129).
5. Haematite or ochre, including many striated specimens, from most levels of the Lindner Site, Australia (Jones 1985). Although not firmly dated, recent research suggests that the oldest of these crayons may be in the order of 50,000 years old (R. Jones, pers. comm.). The haematite is unusually hard and a large number of pieces bear distinct striations, though there are no abraded facets.
6. Two lumps of ochre from the Developed Oldowan levels at Olduvai BK II, Tanzania (Leakey 1958).
7. Ochre from Charama levels (c. 125,000 BP) at Bambata Cave, Zimbabwe (Klein 1978).
8. Two granite slabs with ochre patches from Middle Stone Age (MSA) levels at Pomongwe Cave, Zimbabwe (Walker 1987). Both measure under 10 cm and show well-defined patches of pigment (Fig. 1).

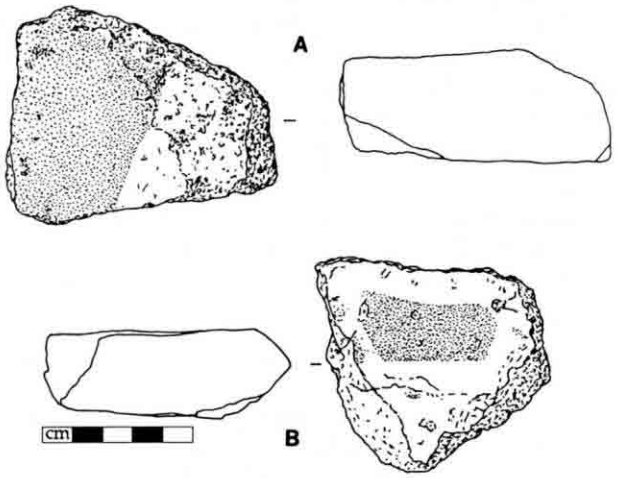

Figure 1. *Two stone slabs with ochre patches, Middle Stone Age. Pomongwe Cave, Matopos, Zimbabwe. (After Walker 1987)*

9. Three stone slabs with ochre, MSA, >40,000 BP, from Nswatugi, Zimbabwe (Walker 1987). One of the slabs has definite and two have probable faint paint stains.
10. Evidence of extensive MSA ochre mining, *c.* 43,000 BP, at Lion Cavern, Swaziland (Beaumont & Boshier 1972). The evidence includes stone mining tools and tens of thousands of Middle Stone Age implements.
11. Haematite with wear facets and striations, MSA, Border Cave, Swaziland (Beaumont *et al.* 1978).
12. Several pieces of ochre with facets and striations from various MSA levels at Klasies River Mouth, South Africa (Singer & Wymer 1982).
13. Ochre crayons and other pigments from various MSA levels at Apollo 11 Cave, Namibia (Wendt 1974).
14. Ochre crayons with facets from MSA deposit at Kisese II, Tanzania (Inskeep 1962).
15. Haematite with wear facets from MSA levels at Porc Epic, Ethiopia (Clark 1988).
16. Ochre at centre of oval arrangement of mammoth bones, Mousterian, Molodova, Ukraine (Klein 1973).
17. Ochre present throughout Mousterian deposits at Qafzeh, Israel, including two pieces with burial 8 (Vandermeersch 1981).
18. Human incisors with red ochre in Mousterian context at Pinar, France (Jullien 1965).
19. Ochre on dentine plaque from Mousterian context at Tata, Hungary (Vértes 1964; Marshack 1976). The surviving traces are faint, but confirm Neanderthal use of ochre to colour portable objects. There is general agreement that the object is artificially shaped (Davidson 1990).
20. Remains of a fallow deer burial with ochre, Mousterian context at Nahr Ibrahim, Lebanon (Solecki 1975).

21. Dense concentrations of powdered red ochre in remains of two circular Châtelperronian dwellings, Grotte du Renne, France (Leroi-Gourhan 1961).

Crystal prisms and fossils

That hominids possessed taxonomizing systems of perception in the Lower Palaeolithic is not only shown by early colour discrimination and preference, but also by their differentiation of common and unusual objects (Bednarik 1988b). For instance, they collected crystals and fossil casts. Examples are:

1. Some 20 quartz crystals, one preserved with all facets intact, the others partly preserved, with *Homo erectus* remains at Zhoukoudian, China (Pei 1931, 120).
2. Fragment of large clear quartz crystal with several crystal facets in an Acheulian level at Gudenushöhle, Austria (Bednarik 1988b). Smaller crystal fragments occur in the same level (Fig. 2).

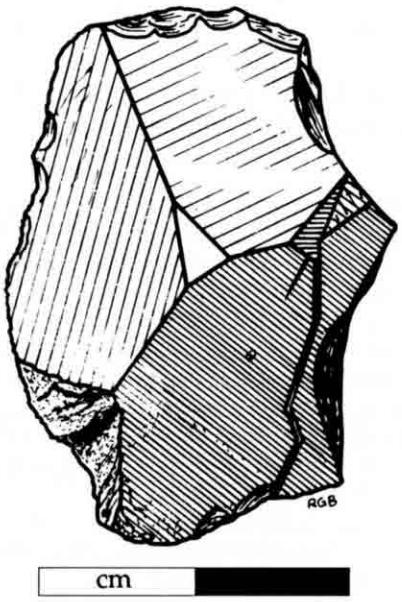

Figure 2. *Fragment of large clear quartz crystal with several prism facets intact. Occupation layer I, Acheulian, Gudenushöhle, Lower Austria.*

3. Six complete quartz crystal prisms, 7-25 mm, with one exception entirely unmodified, from Lower Acheulian at Singi Talav, India (D'Errico *et al.* 1989). Like many of the finds listed here this has not been prominently reported. Mineralogical differences between the six crystals suggest that they originate from different crystal flowers, and were probably brought to the site independently. All are too small to be tools.
4. Several angular quartz crystals from Acheulian deposits at Gesher Benot Ya'aqov, Israel (Goren-Inbar *et al.* 1991).
5. Stone artefacts worked around fossil casts, in British Lower Palaeolithic contexts (Oakley 1981). It has long been argued that non-utilitarian factors were involved in the production of these stone tools.
6. Iron pyrite from Mousterian at Arcy-sur-Cure, France (Leroi-Gourhan 1967, 39).
7. Fossil shark tooth, apparently modified, from Mousterian level at Darra-i-kur, Afghanistan (Dupree 1972).

Perforated portable objects

It has been claimed, incorrectly, that Upper Palaeolithic beads indicate the beginnings of both 'self awareness' and personal adornment (White 1989). The thousands of ivory and other beads in the double burial of Sungir, for example, provide no proof for self awareness, but only adornment. Small perforated objects were almost certainly used in adornment and occur widely in early cultures. There are too many such artificially perforated specimens from pre-Upper Palaeolithic deposits to warrant the view that this is an innovation of the Aurignacian:

1. Artificially perforated wolf vertebra from Micoquian context (c. 110,000 BP) at Bocksteinschmiede, Germany (Narr 1951; Marshack 1991). Marshack has recently provided satisfactory evidence that the perforation is anthropic.
2. Artificially perforated wolf metapodium, Micoquian, Bocksteinschmiede (Marshack 1991). Following Davidson's (1990) claim that the hole is the product of gnawing, Marshack has provided convincing evidence that the perforation is humanly made.
3. Artificially perforated wolf incisor from Middle Palaeolithic level of Repolusthöhle, Austria (Mottl 1951). The 36-mm long tooth is very well preserved and expertly drilled through the base (Fig. 3). Very recently, Rabeder has proposed that the occupation deposits are not around 100,000 years old, but in fact nearly 300,000 years old, on the basis of the phylogeny of the bear remains.
4. Perforated flaked bone point, Middle Palaeolithic, Repolusthöhle (Fig. 3; Mottl 1951). The finds from the Repolusthöhle are from two occupation strata found well below an Aurignacian horizon, and separated from it by substantial stadial deposits. The lithic industry of 1996 items is not Mousterian, but has been variously described as Levalloisian, Tayacian and Clactonian. It is best regarded as an undefined Middle Palaeolithic assemblage. If Rabeder's assessment of the fauna were correct, the two objects from the Repolusthöhle would be Lower Palaeolithic, and by far the earliest perforated objects in the world.
5. Two perforated canines from Proto-Aurignacian (Middle Palaeolithic) layers at Bacho Kiro, Bulgaria (Marshack 1991). Both are incomplete, and the area of drilling was in one case prepared by repeated surface incisions. That both perforations were humanly made

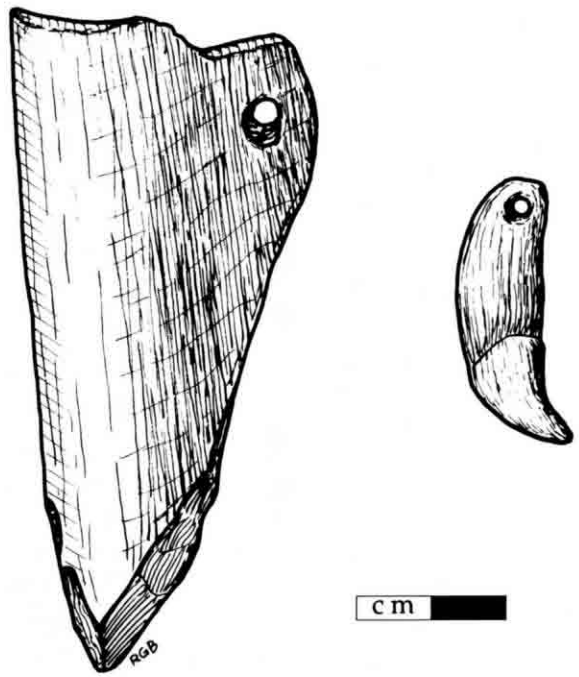

Figure 3. *Two perforated objects (flaked bone point and wolf incisor) from Middle Palaeolithic layers of the Repolusthöhle, Styria.*

has not been disputed.

6. Thirteen artificially perforated objects from the lower occupation layer at Kostenki 17, Russia, which is below a volcanic deposit thought to be about 38,000 years old. These objects include three polar fox canines, three gastropod shells, four fossil objects of amber colour and three stone objects, including one large, elongated and well-made pendant. All holes are distinctly conical or bi-conical in section, and the rotation marks are clearly visible, often to the naked eye. In the hole of the large pendant even the wear of the suspending string is clearly discernible.
7. Partly-perforated fox canine from Mousterian levels at La Quina, France (Martin 1907-10; Marshack 1991). A superficial examination (through glass) led Davidson (1990) to suggest that the partial perforation is the result of animal chewing, though he admits that 'why an animal would chew a tooth is less obvious!' Moreover, the positioning of the hole centrally in the root is fully consistent with Palaeolithic perforation of animal canines and incisors.
8. Perforated bone fragment from Mousterian level at Pech de l'Azé, France (Bordes 1969).
9. Perforated reindeer phalange from Mousterian levels at La Quina, France (Martin 1907-10; Marshack 1976).
10. Perforated marine shell from MSA 2b infant burial, Border Cave, Swaziland (Beaumont *et al.* 1978).

Artificially perforated objects seem to be lacking from the Lower Palaeolithic period. Microwear indicating the use of stone tools as borers or reamers has however been found on Acheulian implements (Keeley 1977).

Circular and discoid objects

Various types of spherical and circular objects are known from Lower and Middle Palaeolithic sites. Here are some for which no utilitarian interpretations could be found:

1. Artificially grooved and pecked phonolite cobble, Olduvai FLK, Tanzania (Leakey 1971, 269).
2. Soft sandstone disc with centripetal flaking around periphery, from Acheulian context at Maihar, India (Bednarik 1991b).
3. Shaped circular limestone disc from Mousterian levels at La Quina, France (Henri-Martin 1947).
4. Fossil nummulite with natural and engraved lines forming a cross from Mousterian context at Tata, Hungary (Vértes 1964). Marshack has shown that the cross visible on both sides of the partially translucent fossil is a combination of a natural fracture and engraved lines on each face at right angles to the fracture (Fig. 4).

Figure 4. *Silicified nummulite with natural and engraved lines forming a cross. Mousterian, Tata, Hungary.*

5. Elongated plaque, finely polished and bevelled, made from a lamella of mammoth molar, from Mousterian context (*c.* 50,000 BP) at Tata, Hungary (Vértes 1964; Marshack 1976).
6. Circular sandstone pebble with central groove and two cupules from Mousterian deposit at Axlor, Spain (Barandiarán 1980). More information is required about this possibly important object and its find context.
7. Cairn of spheres found in the Mousterian of El Guettar, Tunisia (Gruet 1955; 1959).
8. Fragment of a circular disc of ostrich eggshell with smoothed margin from Nagda, India. Although this

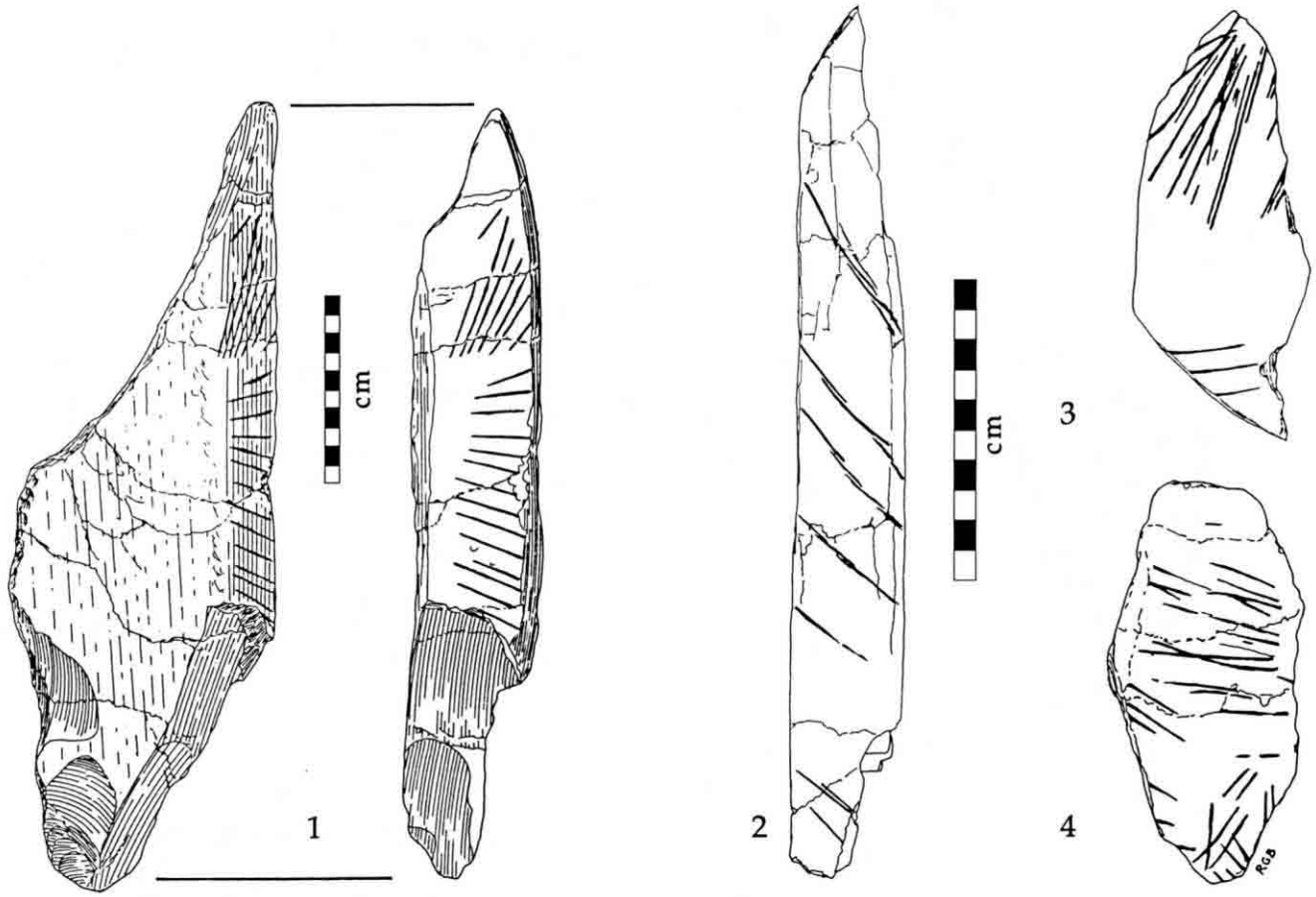

Figure 5. *Four bone fragments from major Holsteinian occupation floor at Bilzingsleben, Germany. 1 is a flat slab, engraved on the bevel only; 2, a thin rib fragment, bears a series of marks, each produced by the repeated application of a stone tool; 3 shows marks of the repeated application of a stone tool. The same occupation floor also yielded several cranial fragments of Homo erectus.*

has been suggested to be early Upper Palaeolithic, it has a minimum age of 31,000 years and may thus be Middle Palaeolithic, since that period persisted to about 30,000 BP in India (Kumar *et al.* 1988).

Engraved or notched bone fragments

1. Four engraved bone fragments from *Homo erectus* occupation floor of Holstein interglacial age (>250,000 BP) at Bilzingsleben, Germany (Mania & Mania 1988) (Fig. 5).
2. Engraved elephant vertebra from *Homo erectus* site of Stránská skála, Czechoslovakia (Valoch 1987) (Fig. 6).
3. Engraved fragment of an ox rib from Acheulian levels at Pech de l'Azé, France (Bordes 1969) (Fig. 7).
4. Serrated and incised fragment of mammoth bone from Mousterian context at Schulen, Belgium (Huyge 1990) (Fig. 8). Huyge has shown that the serrations were made with stone tools, and that the transverse incision at the top may have facilitated snapping the bone along a predetermined line.
5. Deeply engraved bone fragment from Mousterian context at Bacho Kiro, Bulgaria (Marshack 1976). As far as the writer is aware, the zigzag pattern on this object is generally accepted as having been intentionally carved.
6. Bovid shoulder blade with long parallel lines, Mousterian, La Quina, France (Martin 1907-10; Marshack 1991).

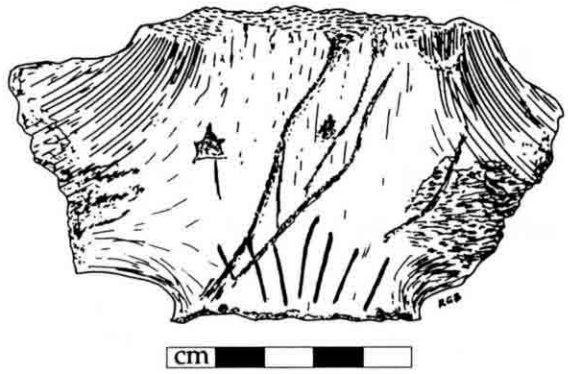

Figure 6. *Engraved elephant vertebra from occupation deposit which also produced Homo erectus remains, Stránská skála, Czechoslovakia.*

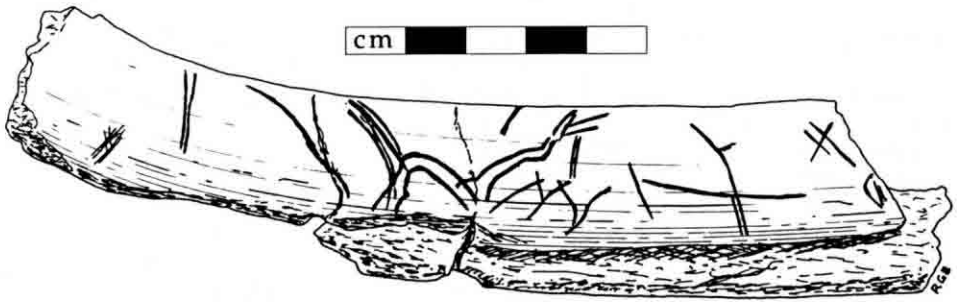

Figure 7. *Rib fragment with a series of marks thought to have been made by humans or hominids. Acheulian, Pech de l'Azé, France.*

7. Small bone with numerous parallel lines, from a Neanderthal grave at La Ferrassie, France (Capitan & Peyrony 1921; Marshack 1976).
8. Small bone with several transverse notches, Mousterian, La Ferrassie, France (Capitan & Peyrony 1912).
9. Five engraved bone fragments from Mousterian context at the Tagliente rockshelter, Italy (Leonardi 1988).
10. Bone retoucher with numerous incised lines, Mousterian, Tagliente shelter, Italy (Leonardi 1988).
11. Utilized bone fragment with a series of five barb-like incised marks, from Mousterian levels at Cueva Morín, Spain (Freeman & Gonzalez Echegaray 1983). The repetitive pattern of these marks seems to exclude the possibility that they are accidental, but the writer remains sceptical.
12. Rib fragment with paired line markings from Mousterian levels at Cueva Morín, Spain (Freeman & Gonzalez Echegaray 1983). It should be noted that the quality of 'paired-ness' is found in many very early markings.
13. Mammoth shoulder blade with various patterns from Mousterian occupation at Molodova I, Ukraine (Frolov 1981). In the writer's opinion the markings are natural, and attributable to drying cracks and other alteration phenomena.
14. Incised bones from Mousterian levels at Kebara Cave, Israel (Davis 1974).
15. Three rib fragments with serrated edges, MSA, Klasies River Mouth, South Africa (Singer & Wymer 1982).
16. Small bone fragment with four parallel grooves from MSA at Klasies River Mouth, South Africa (Singer & Wymer 1982).
17. Notched rib fragment from MSA 3 context at Border Cave, Swaziland (Beaumont *et al.* 1978).
18. Two notched bone fragments from MSA 2b context at Apollo 11 Cave, Namibia (Wendt 1974).

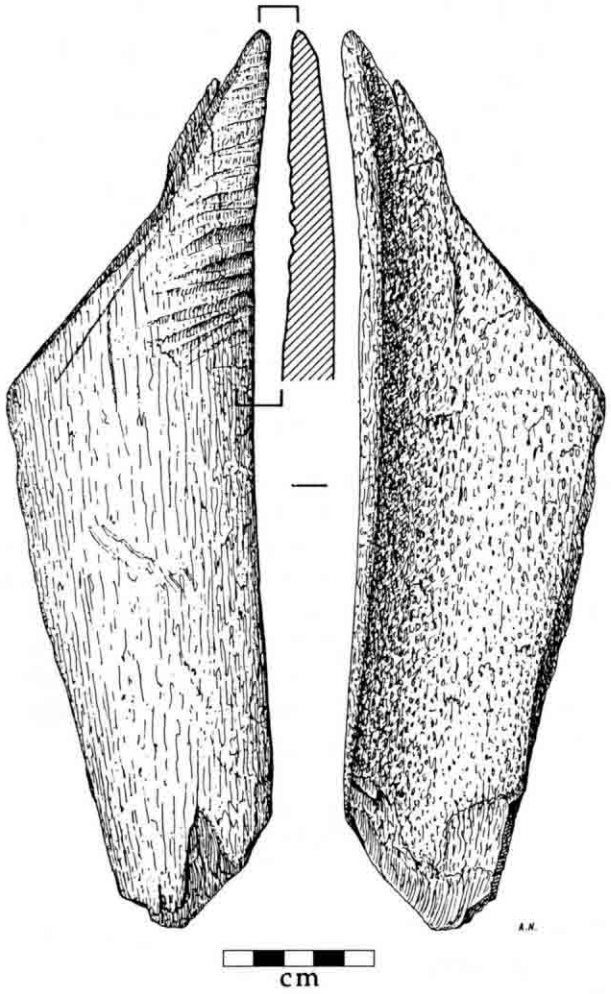

Figure 8. *Two views and partial section of the serrated and incised mammoth bone fragment from Schulen, Belgium. Both types of marks are thought to have been made with stone tools.(After Huyge 1990; drawing by A. Nijs)*

Other engraved objects

1. Scoria pebble with several deep, apparently anthropic grooves that underline the object's shape as a female figure, from Acheulian (>230,000 BP) at Berekhat Ram, Israel (Goren-Inbar 1986). The overall form is natural, but the grooves seem to be artificial and intentionally placed. Davidson (1990) has suggested that the stone may have been at the occupation site 'because hominids were carrying stones around', for which 'there is plenty of evidence from the time of the earliest evidence for stone tool-making'. This tautology does not address, however, why this particular stone may have been 'carried around' (it is too soft to be used as a tool) and ignores the possibility that the people concerned may have 'carried stones'

only when they saw a reason for doing so; perhaps the pebble's odd shape had something to do with it. This shape and the several long grooves which are said to be artificial should not be brushed aside because they do not fit in with one's theories.

2. Quartzite slab with arcuate engraving, from Holstein interglacial context at Bilzingsleben, Germany (Mania & Mania 1988). Details of this specimen are still to be published but D. Mania (pers. comm.) has been discouraged by the hostile response to his earlier report from those opposing the existence of non-utilitarian marks before the Upper Palaeolithic.
3. Limestone pebble with markings, Acheulian or Mousterian date, from Grotta dell'Alto, Italy (Leonardi 1988). These may be accidental marks.
4. Incised pebbles from Mousterian contexts in Hungary (Vértes 1965).
5. Several grooved bear teeth from Mousterian deposit at Sclayn, Belgium (Gautier 1986). The marks may have been caused by silica-rich plant foods, such as grasses, which the dentation was not adequately suited for.
6. Antler fragment with approximately eight transverse, tool-cut notches, from Mousterian levels at the Grotte Vaufrey, Dordogne (Vincent 1988). The notches are of different sizes and unequally spaced.
7. Three flint flakes with possibly engraved lines from Mousterian context at the Tagliente shelter (Leonardi 1988).
8. Limestone cobble with intentional engravings, Mousterian, Tagliente shelter (Leonardi 1988). Of the several examples Leonardi lists, this seems to be the best specimen: the markings are anthropic, with clear arrangements that closely resemble those of early Upper Palaeolithic markings recently found on the walls of the Hohlen Fels, Germany (Hahn 1990).
9. Flint flake with possible engravings from Mousterian levels in the Solinas shelter, Italy (Leonardi 1988).
10. Possibly engraved chert artefact from Quneitra, Israel (Goren-Inbar 1990).

Early bone tools

There is a misapprehension among some archaeologists that bone points, and the skilled use of bone and antler generally, do not appear before the Aurignacian. There are in fact numerous earlier examples on record. The Micoquian site of Salzgitter-Lebenstedt alone provides ten bone points, mostly on mammoth ribs, besides the delicate and complex 'winged point' and an antler implement (Tode 1953). The well-made bone harpoon from the Ngandong deposit on the Solo River, Java (Narr 1966, 123) may be even older, but is widely ignored because of its age by archaeologists who believe that the harpoon is an invention of the European upper Magdalenian. They are unaware of the excellent bone harpoon from Lohanda Nala, India (Bednarik 1991b). The Salzgitter-Lebenstedt 'winged point' is in effect a harpoon-like artefact, and may have been detachable. It is no less complex than Upper Palaeolithic bone points.

Wooden objects

These have rarely survived and yet the number of Lower Palaeolithic specimens known is far greater than that from the Upper Palaeolithic: consider the Kalambo Falls Acheulian implements and the spears from Lehringen and Clacton-on-Sea. The following objects may be non-utilitarian and are of particular interest here, being technologically comparable to recent Australian artefacts:

1. Willow plank, shaped and bearing anthropic polish, from Middle Pleistocene context (>240,000 BP) at Gesher Benot Ya'aqov, Israel (Belitzky *et al.* 1991). The fragment is 25 cm long and broken at both ends. Only one side bears polish, which is quite flat except for a slight convexity along one edge. This surface transects the grain of the wood at a low angle.
2. Thin, worked plank of mulberry wood, which is from a species now extinct in Japan. The piece, from the site of Nishiyagi, has been shaped by stone tools. Dating is imprecise, but an age of 50-70,000 years is the most probable (Bahn 1987).
3. Curved wooden implement with parallel markings on the end from MSA context at Florisbad, Orange Free State (Volman 1984).

Rock art

A strong bias against finding pre-Upper Palaeolithic rock art can be assumed: it is likely to be simple and non-iconic, and where it occurs among more recent art it will inevitably be considered part of that younger corpus. Moreover, an iconic mark is far more likely to be noticed and recorded than a non-iconic mark, and the recently discovered non-iconic Aurignacian or Gravettian rock art of southern Germany only demonstrates this point. Archaic non-iconic rock art has been studied in detail only in Australian caves, and the past biases in the study of palaeoart have severely retarded the discipline. In contemplating the few examples that follow one should note that they refer to cases with fairly sound dating evidence, be it absolute or relative: the number of 'convincingly' dated Upper Palaeolithic rock art motifs is in fact smaller!

1. Pattern of 18 cupules, mostly arranged in pairs, on a large limestone slab over Mousterian burial 6 at La Ferrassie, France (Peyrony 1934).
2. Large cupule and meandering groove on boulder covered by Palaeolithic deposits, with nearby group of seven cupules found above ground, Bhimbetka, India (Bednarik 1991b).

3. Stone plaques with red, black and white paint, including iconic motifs, from MSA deposits at Apollo 11 Cave, Namibia (Wendt 1974).

Many Australian non-iconic petroglyphs are thought to be over 32,000 years old. Nobbs and Dorn (1988) provide minimum cation-ratio ages of up to 31,700 years for petroglyphs on panels 16 and 25 at Karolta I, South Australia. At Sandy Creek, Queensland, the occupation sequence begins well before 31,900 years BP (Morwood 1989); the occupation deposit concealed a number of petroglyphs, but erosion prevents a clear determination of their lowest limit.

Underground mining

Evidence of chert and ochre mining in various continents indicates an understanding of geological formations, and an ability to predict the viability of initially unproductive mining operations. It thus provides more relevant information about cognitive abilities than speculative phenomena such as those cited by Chase and Dibble (Bednarik 1990d). There is extensive evidence of ochre mining in Africa and Australia, in the former continent extending well back into the Middle Stone Age. Very early underground chert mining has been reported in the following regions:

1. Southern Australia: in nine deep limestone caves (Bednarik 1986b), extending up to 300 m from entrance, and, in one cave, at least beyond 31,000 BP (Gallus 1971). Five basic mining methods are recognized (Bednarik 1990d), one of which involved driving long wooden stakes into solid limestone to gain access to concealed chert seams.
2. Upper Egypt: two sites on the Nile, *c.* 33,000 BP and possibly also Middle Palaeolithic, with mining shafts and tunnels in an alluvial deposit covered by unproductive overburden (Vermeersch *et al.* 1989).
3. France: chert nodules fractured by impact prior to cave bear claw marks at Bara Bahau (Bednarik 1986b).
4. Hungary: Mousterian chert mining with 'pickaxes' in cave near Budapest (Gábori-Csánk 1988).

Conclusion

The academic ethos encouraging scholars to seek to confirm rather than refute their speculative hypotheses once they have been proposed is being questioned. While innovative hypotheses are certainly to be welcomed, the processes involved in formulating them could profitably be reviewed. I have shown with two recent examples of hypotheses addressing symbolism and language origins that the implementation of simple strategies could save the discipline much publication space and unproductive debate, by discouraging the publication of extemporaneous hypotheses.

Two simple rules would assist future discourse on controversial archaeological topics such as the beginnings of symbolism, discrimination between natural and artificial objects and markings, the first settlement of the Americas, or the significance of female figurines. Firstly, protagonists in such areas can be expected to have considered all of the relevant evidence which has been published on the subject, not just a small sample of it, and preferably will have studied a good part of it first hand.

Secondly, our understanding of these issues and the level of the debate would profit from a shift in argument from the confirmationist strategy towards a refutationist dialectic. While it might be naive to hope that one day scholars would outrival one another in trying to refute their own hypotheses, any improvement in epistemic standards would be commendable.

In this paper I have avoided taking sides in the debate on cognitive development. Instead, I have simply summarized some of the data which may be relevant. One of my concerns in this debate is the tendency to continually 'shift the goal posts', as it were. It seems to me that criteria for the assessment and acceptance of evidence should be established, and that the age of this evidence, or the belief of some researchers that they know what it should show, should have no influence on our findings.

On the one hand, those who are too ready to interpret phenomena as indicative of symbolism need to be more aware of taphonomic processes and of the extremely complex process of discriminating between natural and anthropic markings (cf. Bednarik 1991a; 1991c). On the other, those who reject the evidence of pre-Upper Palaeolithic symbolism should provide scientific criteria for accepting such evidence in the Upper Palaeolithic or more recent periods. Precisely the same criteria should apply to the earlier periods. The argument that an object seems unique is unacceptable: the first instances of all new classes of evidence were of necessity unique, and some unique objects are likely to remain so (for example, the Statue of Liberty), but this should not impair their symbolic quality.

The notion of knowing what the cognitive or intellectual capacities of hominids were is clearly suspect: we do not know what these capacities were. Nor are we likely to find out if we use a mode of enquiry that leads us to accept the ability of a hominid of 3.6 million years ago to step 'intentionally' into the footprints made by another (at Laetoli), while claiming at the same time that the humans of the Middle Palaeolithic, 3.5 million years later, lacked the ability

to track, because they produced no iconic pictures and hence lacked language.

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