A global perspective of Indian palaeoart

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Introduction

In discussing the context of Indian palaeoart, including rock art and portable art of very early periods in human history, I am faced with an initial choice between two alternative approaches to this complex topic. The easy option is to do the obvious: review the published record of Indian rock art, which is predominantly of paintings in sandstone shelters, and then compare the iconographic (figurative) interpretations of a range of students with those of another range of rock art students in the rest of the world. No doubt such a global comparison would bring to light the familiar similarities and contrasts noted previously by various writers. One might once again find that 'x-ray depiction' occurs in India as well as in numerous other regions, from the Arctic to Australia. Perceived stylistic conventions would be found to be repeated in apparently unconnected arts of other countries or continents. For instance, the animated human line figures in an early phase of central Indian rock paintings, often found in greenish pigment (Wakankar 1983), have their parallels in northern Australia, where they are seen as a 'dynamic style', which is also very early in the sequence. Does this correspondence mean anything? Almost certainly not, so why bother with such conundrums? Similar animated styles occur also early in eastern Spain, in the so-called Levantine style, in southern Africa and elsewhere in the world. What could we possibly deduce from such correspondences? What significance is there in the apparent depiction of head trophies in central India and in Peru, or in the apparent chariot depictions, for which we can find parallels from Algeria to northern China? Unless we subscribe to simplistic ideas of migration, diffusion or exotic cultural contacts, there is little value in tracing the distribution of x-ray depiction, or any other perceived stylistic trait or subject, through geographically unconnected regions. Such graphic conventions are more likely related to universals of human cognition than to cultural links.

The second alternative of considering India's place in the world of palaeoart is much more demanding. At the first level of it, one could simply review the status and role of Indian rock art research in the context of the global discipline. There are many facets one might explore: the history of the discipline (Neumayer 1993: 7–11); the current methodology and how Indian specialists can avail themselves of it most profitably (Chakravarty 1991; Bednarik 1991; Chakravarty and Bednarik 1997); or the questions of rock art conservation, protection and site management, and how the Indian experience fits into that of the rest of the specialist community (e.g. Tyagi 1991). These are fascinating subjects, and to explore them would assist both Indian and other scholars. Then there are the topics of deontology (Bednarik 1991/92), the ethics of what researchers do, and of whom exercises control over ancient cultural resources. These questions are intimately linked with those of management and conservation, among others. They have been explored extensively in recent years, which has led to a profound focusing on specific matters in various parts of the world. All of this is comparatively easy to explore and summarise, and it is certainly of great relevance to the Indian specialist.

The task I have set myself here, however, is more ambitious. Instead of recapitulating on recent debates and their outcomes in order to place Indian rock art research into the context of global rock art and other palaeoart research, I propose to develop a contextual frame of global palaeoart knowledge, as it is currently available to us. By placing our knowledge of Indian palaeoart within it we should not only see how it compares, but also more importantly, we should see what avenues of research might most profitably be followed in India, in order to pursue the most profound issues in the entire discipline. For instance, in what way can the Indian specialist contribute most usefully to the universal advancement of palaeoart studies and archaeology? One may argue that each regional school of research has its own preoccupations, and that these do not necessarily coincide with the discipline's aims or tenets elsewhere, or those of science generally. That may be true, but if science is to be universally acceptable, it must exhibit not only extraordinary rigour, but must offer a universality that transcends all cultural, ontological, metaphysical or religion-inspired divisions, as well as those of scholarly preoccupations. These are not just the divisions between individual human concepts of the world, or between the collective models held by specific schools of thought, nations or cultures. A concept can be scientific only if it is valid in any alternative reality, including one held by non-human intelligent beings. Whether they exist in the universe or not is not relevant to this issue; theoretically they could exist, they would be likely to possess very different sensory perceptions from ours, and they would create a reality very different from our anthropocentric model of the world. That in itself demonstrates that alternative paradigms of reality are possible. Real science, in contrast to mechanistic, scientistic or Western science, has to make allowances for all possibilities; it must be valid in any intelligent system of thought, human or other.

This is a very rigorous requirement indeed, but it is essential to separate science from pseudo-science, science fiction and scientific quackery. It is almost impossible for mere human beings to preserve their scientific integrity at all times, we can only hope that someone can point it out to us when we stray from the straight and terribly narrow path of real science. That certainly applies to what follows in my present paper, and I can only hope that what I say is received critically, and is carefully checked by my peers before anyone accepts it as valid. Nevertheless, I can say with good conscience that I do try to follow a sound epistemology, that I keep my propositions falsifiable where possible and that I lack any vested personal interests in the discipline. In contrast to many colleagues I love to be shown to be wrong, or to see my propositions invalidated by good and sound philosophical argument. I have nothing to gain from being right, or from prevailing in academic debate, as I do not partake in academia and the mediocrity it often entails. My ambition is to pursue veracity for its own sake. 'Truth', in science, is so frustratingly elusive; it is much more likely to be 'found' in belief systems. What follows is certainly not the presentation of a true picture, but its worth should be measured by how close it may one day be seen to have come to what then might be considered 'true'. That is all I dare aspire to.

Some reservations

To begin with, we need to discard most of the kinds of knowledge we have been given about the earliest development of human cognition, symbolism or art. The textbook models are practically worthless, and they are based on outdated ideas about human evolution — physical, mental and cognitive. More directly, they relate to a Eurocentric perception of the world, and to paradigms of a discipline that has always been dogged by Eurocentrism: archaeology. Its concept of a 'prehistory' is itself based on the opinion that written records of history are more reliable than oral ones, which is not a falsifiable proposition and hence unscientific and irrelevant. Writing may appear crucial to some societies of intelligent organisms in the universe, but not to others, so it is not an objective criterion to use in dividing history. I would not go as far as to question the human concept of time, but it will be obvious to the reader that some philosophers would find that query quite pertinent. Even some extant human cultures have no concept of time such as mine (e.g. the Hopi), of future and past, and would not see any value in the concept of history as other humans may perceive it. This is simply an 'accidental' result of the anthropocentrising currents in our cognitive evolution.

Similarly, the way we perceive archaeological taxonomies is the result of entirely random historical developments. If archaeological discoveries had been made in a totally different order, by different individuals with different personalities or temperaments, and in different parts of the world or under different world-views, the models developed in archaeology since early in the 19th century would have also been significantly different. They would in effect have resulted in a different archaeology today. To give some examples: the term Neolithic is almost meaningless in most parts of the world, because none of the characteristically Neolithic innovations (stone grinding, pottery, sedentariness, domestication) actually coincide with that period everywhere. We have Pleistocene pottery and stone grinding from various parts of the world (e.g. pottery in Japan, stone grinding in Australia), while the advent of domestication and sedentariness are either uncertain, or did not coincide with these other innovations in most regions. A Bronze Age is not found in most parts of the world. Among the hallmarks of the Mesolithic were microliths and Levantine shelter paintings. Now we think that the latter are Neolithic (Hernandez Pérez et al. 1988), and microliths occur from about 80 000 years onwards (e.g. in the Howieson's Poort industry of southern Africa). Even where the northern and western European cultural sequence is being emulated, such as in India (see below), I have the impression that this is largely due to a polite deference to European models. It would be just as reasonable to model archaeological sequences on, say, Indian standards. In India, there are Lower Palaeolithic assemblages that seem to resemble those of Europe and Africa, but the Middle Palaeolithic, although apparently contemporary with that of Europe, is different, and lacks a Mousterian. The Upper Palaeolithic differs even more from that of Europe, and there is very little that resembles a typical

Danish Neolithic. But one imagines that at the time archaeological research commenced in India, the colonial masters of the day would not have had a great deal of sympathy for anyone voicing a preference for a locally more appropriate archaeological taxonomy. Unfortunately India remains saddled with this colonial model of archaeology. It is interesting that in China, the existence of a Middle Palaeolithic is now rejected by some scholars (Xing Gao and Norton 2002).

To fully appreciate the haphazard progress of archaeology, let us consider a hypothetical scenario: the French had discovered the authenticity and age of their Pleistocene cave art and portable art long before any Palaeolithic tools. They would then not have divided the Palaeolithic period by the perceived styles of stone tool assemblages, as indeed they have done (and this absurd approach has been copied widely in the world), but by the perceived styles of arts. When subsequently discovering the tools that supposedly go with these cultures, the French would have no doubt retained their historical taxonomy based on art. Hence it is obvious that the designations we have inherited are entirely random, they are simply the result of the historical sequence in which certain 'knowledge' was acquired. Moreover, it is obvious that a cultural taxonomy derived from art is vastly superior to one derived from tools. After all, tools do not designate cultures; art does (among other things, which are generally not recoverable from these times).

This will suffice to show that we have no reason to meekly accept every model in archaeology, simply because it has been held for a long time and most practitioners adhere to it. Archaeology is generally based on non-refutable propositions; i.e. it is not scientific. It is largely confirmation derived: models are proposed, confirming evidence is avidly sought, and the models are defended with great tenacity against any attacks. In terms of good science, this is not an appropriate way to practise a discipline. This is not to say that archaeology does not use scientific methods; on the contrary, it does so eagerly. For instance, dating work would be unthinkable without the support of several of the hard sciences. However, archaeologists are quick to misinterpret the data provided to them by scientists, and to misuse them in constructing elaborate interpretations that are often not warranted by the hard data themselves. This has become evident recently with rock art dating results (Bednarik 1994a, 1994b, 1995a, 2002a).

Another problem with archaeology will bring us to the topic of this essay. Many archaeologists who have written about very early art, evidence of symbolism and various related subjects with great authority, in prestigious scientific journals, have demonstrated a surprisingly poor knowledge of these topics. There are a number of instances of this but I will cite just a few. Chase and Dibble (1987) discussed a handful of finds from the Lower and Middle Palaeolithic periods, and concluded that they were not of an adequate number to think that symbolic traditions existed before the Upper Palaeolithic. Davidson and Noble (1989) followed this pattern, listing some thirteen examples of pre-Upper Palaeolithic 'symbolic evidence', the only ones they were aware of. They proposed that language was contingent upon figurative depiction, and since we lack depictive art prior to the Aurignacian they postulated that this marked the beginning of human language. They went one step further a year later, proposing that humans perceived by them as having been without language, such as the Neanderthals, should be considered as closer to the apes than to humans (Davidson and Noble 1990). In a paper presented in 1988 (Bednarik 1994c), I had listed hundreds of finds that could suggest symboling by pre-Upper Palaeolithic hominids. In 1992 I challenged both Chase and Dibble and Davidson and Noble to an open debate, in the course of which the former conceded various points. But they maintained that their ignorance of most of the evidence did not convince them to change their overall view (Bednarik 1992a, 1992b; Chase and Dibble 1992). Davidson described himself as 'angry' but failed to cite one argument or reason as to why we should take his hypothesis serious (Davidson 1992). Noble and Davidson (1993, 1996), however, conceded that the first settlement of Australia, which occurred at least 30 000 years before the demise or the Neanderthals, does constitute evidence of the use of language and symbolism. Hence language must be at least twice as old as figurative graphic depiction, and cannot be preceded by it, and their hypothesis began to disintegrate. But to make matters worse, seafaring, accepted by Noble and Davidson as solid evidence of language, did not begin 60 000 years ago, as they had thought, but about one million years ago (Sondaar et al. 1994; Bednarik 1995b, 1997a, 1999a; Bednarik and Kuckenburg 1999). Since this has been known for decades (although published only in German until 1994; Maringer and Verhoeven 1970; cf. Koenigswald and Gosh 1973), the ignorance of Davidson and Noble on the subject, and on world archaeology generally, is of concern.

Figure 1. *Flaked bone point and wolf incisor, both perforated. Early or Middle Palaeolithic, Repolust Cave, Austria.*

In a set of authoritative papers on the question of the beginnings of body decoration, White takes the position that such evidence of human self-awareness is first found in perforated small objects that were presumably used as jewellery. He claims quite categorically, on several occasions (White 1992, 1993a, 1993b), that the first such perforated finds are two teeth from the Aurignacian of Bacho Kiro, a cave in Bulgaria. They are over 43 000 years old, clearly beyond the accepted duration of the Aurignacian, and generally regarded as belonging to a Middle Palaeolithic tradition. But this is only an unimportant point: we have in fact quite a number of perforated small objects, presumably used as beads or pendants, from much earlier occupation deposits. The earliest are two items, drilled with stone tools, from the Repolust Cave, Austria (Bednarik 1992a: 34). They are thought to be almost 300 000 years old, ten times as old as the Aurignacian (Figure 1). They are followed by three ostrich eggshell disc beads of the Acheulian, around 200 000 years old and from El Greifa E, Libya (Bednarik 1997b; more have recently been recovered) (Figure 2).

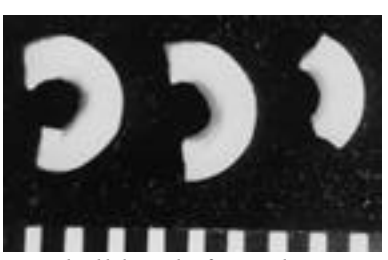


Figure 2. *Three fragments of ostrich eggshell beads from the Late Acheulian of El Greifa E, Libya), similar to beads found at two Upper Palaeolithic sites in India.*

But more importantly still, it is not plausible that the first form of body decoration must have been by beads or pendants, and even if that were so, then these must not necessarily have been made of non-perishable materials. We know from recent hunting societies that they made most of their beads from such perishable items as plant seeds, and we know that even shell, bone or ivory ornaments can only survive in favourable high-pH soil conditions. Moreover, beads and pendants need not necessarily be artificially perforated materials, in many cases naturally perforated objects were used. We also know that most body decoration was by such means as body painting, tattoos, cicatrices, infibulations, headdresses, coiffures, deformation, and by a great variety of materials that never survive in the archaeological record. In short, it is simply unrealistic to expect the archaeological record to reveal the earliest use of body decoration.

But in addition to that, White is not even adequately familiar with the evidence he cites, beads and pendants, omitting in fact all the known early examples. The oldest specimens known, for instance, were first published almost half a century ago (Mottl 1950, 1951; Murban and Mottl 1955), they were on exhibit since then and have been seen by tens of thousands of people — but not by White, who is considered to be a world authority on

early beads and pendants (Figure 1). They were reported in English only much later (Bednarik 1992a), and therein lies one fundamental problem. Most English-speaking archaeologists are mono-lingual, but a great deal of the relevant evidence has been published in other languages, so they often have no knowledge of any finds that have not been disseminated in English. By contrast, most Russian, Chinese, Scandinavian, German, Italian, Spanish, Indian, African and Latin American archaeologists are bilingual or multi-lingual, so collectively these research traditions are likely to be aware of a much greater diversity of data.

The role of taphonomic logic in palaeoart studies

We have seen from these introductory comments that, if we wish to examine the role of Indian palaeoart studies in the global context, we must first determine what that context is. In doing so we would be well advised to be sceptical of any categorical claims in the field of palaeoart studies, and we should be aware that most existing paradigms of early art development are problematic, if not false. By far the greatest single problem of palaeoart studies, and one it shares with general archaeology, is that we have until now failed to fully understand the massive effects of taphonomy, and we have not designed means of addressing these effectively (Bednarik 1994d). All archaeological interpretation is in some way based on qualitative and quantitative information about the types and distribution of material evidence. This information is then used statistically to 'demonstrate' all sorts of things (ranging from technologies to ethnic entities, homogeneous cultures or social systems, even religions and language groups!), and yet, it is a fundamental tenet of taphonomic logic that *all of this information is statistically irrelevant*. It merely describes what still exists, has been found and recognised, and has been reported effectively enough 'to be known'. Empiricist archaeology cannot consider what has not been preserved, what has not been found, what has not been recognised, and what has not been reported, or not been reported in the researcher's own language. It is clear to anyone that the probabilities for archaeological material evidence to survive to the present time vary enormously depending on the nature of that evidence. The necessarily garbled statistics of the surviving remains are unequally distorted by spatially differing conditions of preservation (e.g. due to soil chemistry or climate in different parts of the world), so a phenomenon may survive in one region, but not in another. A further factor is time: so older the remains, so more severe are the effects of the distorting variables (Figure 3).

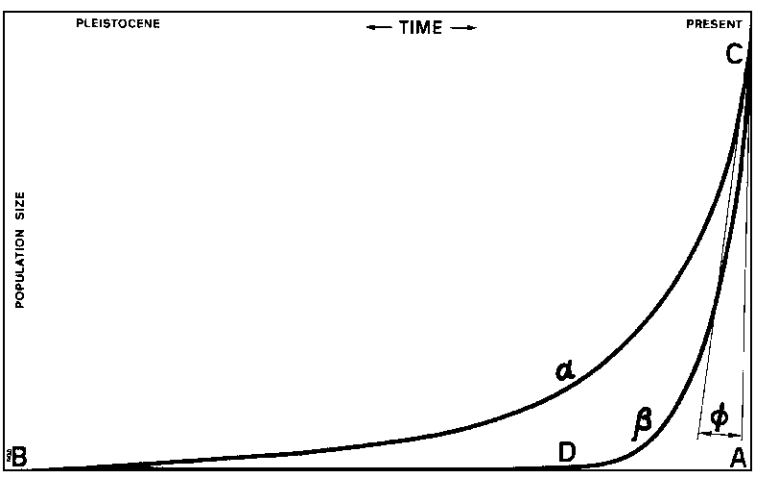


Figure 3. *The principle of taphonomic logic: remains which are subject to a cumulatively increasing proportional loss with time must experience a cut-off point in time beyond which evidence must be extremely scarce. The area below* ***a*** *represents original population of evidence category, the area below* ***b*** *represents surviving population, its decline being a function of angle* ***f****.*

But there are still many other factors which select what is reported, among them modes of deposition (e.g. grave goods are more likely to be found than items deposited randomly); preoccupations and methods of archaeologists, as well as the limitations of their knowledge and research means (these limitations are most severe in all parts of the world); and comprehensiveness of research; selectiveness in what is actually reported, which is itself an extraordinarily complex problem. All of these factors distort already distorted evidence further, and they do so in *systematic* ways, *not in a random fashion*. No archaeological sample can ever be even remotely representative, and it can therefore never be known where it is located within the whole, how it relates to the whole — the whole being what really happened in the past.

But the most serious limitation of this 'archaeological record' has not been mentioned yet: with few exceptions, it is generally interpreted with subjective taxonomies, artificial pigeonholes of styles, types, mental templates, classes and categories of information. All material evidence is taxonomised on the basis of modern perceptions, by people who have little idea of what it was like to have lived in the time in question. These subjective taxonomies of tools, rock art motifs and so forth are presented as the 'archaeological record' — as if they were objective data. They cannot reasonably be expected to be so. In fact it would be very naive to even expect them to be objective. They merely reflect the limited comprehension of reality possessed by the hapless 'analyst'. These 'records' are much better suited to study the world the archaeologist exists in, than the world of the pre-Historic people he or she seeks to illuminate.

Lower and Middle Palaeolithic symbolic evidence

In the absence of systematic application of taphonomic logic to archaeological data and hypotheses we need to consider the available record of the earliest evidence of non-utilitarian activities as a very minimal information source. The earliest evidence we have of non-utilitarian behaviour in hominids is of two types: in the form of manuports of novel or exotic objects, such as crystals, fossils, and unusually coloured or shaped pebbles. Secondly, the occurrence of colouring matter at occupation sites in geological contexts indicating human transport to the find sites. India provides some of the earliest such evidence in the world. Six complete quartz crystal prisms were found at the base of the Lower Acheulian site Singi Talav, Rajasthan (d'Errico et al. 1989). They are clearly too small to have been considered as a source of stone tools, being as small as 7 mm, and only one shows any evidence of fracture (Figure 4). Most importantly, mineralogical differences in the crystals indicate that they were obtained from different crystal flowers, which could imply that they were progressively collected, possibly from different localities. Colouring matter, in the shape of haematite pebbles or other iron oxides or hydroxides, are found in Indian Acheulian deposits, including quite a number from Hunsgi, a site in Karnataka (Paddayya 1982). One of these pebbles bears a wear facet with well-preserved striations, indicating that it was used crayon-like, to colour a rock surface (Bednarik 1990).

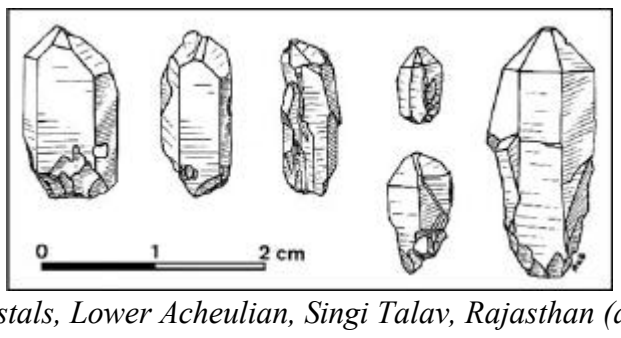


Figure 4. *Six small quartz crystals, Lower Acheulian, Singi Talav, Rajasthan (after d'Errico et al. 1989).*

These finds agree exceptionally well with those in other parts of the world. Some of the oldest red ochre manuports we know of are from the south African Early Acheulian site Wonderwork Cave, where they are estimated to be up to 800 000 or 900 000 years old. They co-occur with quartz crystals and imported coloured river pebbles (one of which is engraved) in that huge cave, and they are found in all of its Acheulian levels (Bednarik 1994c). Elsewhere in southern and eastern Africa, the use and mining of iron pigments has been described widely from the later Middle Stone Age (Leakey 1958; Oakley 1981; Klein 1978; Beaumont and Boshier 1972; Beaumont et al. 1978; Singer and Wymer 1982; Wendt 1974; Inskeep 1962; Clark 1988; Walker 1987; McBrearty 2001; Barham 2002). In Europe, Acheulian use of ochre is also well documented, from Ambrona in Spain (Howell 1966) and Terra Amata in France (Lumley 1966), for instance. At the latter site many of the seventy-five pieces were fire treated to alter their colour, and several are faceted from use. At the Czech Acheulian site Beçov, a striated piece of haematite was found on an ancient floor entirely covered in haematite powder (Marshack 1981). Modified clear rock crystals were found in the Acheulian occupation layer

of the Gudenus Cave in Austria (Bednarik 1992a: Fig. 2), and about twenty more at the classical *Home erectus* site Zhoukoudian I, China (Pei 1931: 120). Rock crystals found in the Acheulian deposit of Gesher Benet Ya'aqov, Israel, are so small that it is not even certain that humans brought them to the site (Goren-Inbar et al. 1991). Pyrite crystals have been found at two French Neanderthal sites (Bednarik 1995c), and various types of fossil cast manuports occur at many occupation sites, from the Acheulian through to the Châtelperronian (Oakley 1981; Marshack 1991).

Figure 5. *The Makapansgat manuport, a natural jasperite cobble, is the oldest known palaeoart object in the world.*

The oldest reported apparent manuport, and possible oldest palaeoart object in the world is again from South Africa. The Makapansgat cobble, bearing two or three faces, is from an australopithecine-bearing fossiliferous cave breccia of the late Pliocene, between 2.5 and 3 million years old (Figure 5). It was collected many kilometres from the cave it was found in, and carried back to the homebase of either *Australopithecus africanus* or a very early hominid, presumably because of its startling natural markings and its red colour (Bednarik 1998, 1999b). Other very early palaeoart specimens are the human proto-figurines of Berekhat Ram (Goren-Inbar 1986) and Tan-Tan (Bednarik 2001a), and the Erfoud manuport (Bednarik 2002b).

Figure 6. *Acheulian petroglyphs in Auditorium Cave, Bhimbetka, the oldest known rock art in the world.*

The oldest form of known rock art, anywhere in the world, consists mostly of cupules, and the oldest known site is in India. The petroglyphs in Auditorium Cave (Bhimbetka III F-24) are thought to be of Acheulian age (Bednarik 1993a). They include ten cupules (or cup marks) and a long, meandering line. Nine of the cupules occur in a most conspicuous location above present floor level (Bednarik 1996), the much larger tenth cupule together with the line marking were uncovered in the course of an archaeological excavation. All nine motifs are extremely corroded, particularly those found below ground. The latter figures occur on a massive boulder below a substantial Middle Palaeolithic occupation layer that is solidly cemented by carbonate precipitation, which excludes the possibility of stratigraphic disturbance. They occur contiguous with two Acheulian strata, with bifaces, cleavers and scrapers, which overlie a pebble tool industry of choppers and scrapers (Wakankar 1975; V. N. Misra 1977). These Acheulian petroglyphs are the oldest rock art currently known in the world (Figure 6). There is no dating available from Auditorium Cave itself, but the Indian Acheulian is generally thought to be of the same antiquity as that of Africa and Europe. Most attempts of thorium-uranium dating have shown this tradition in India to be beyond the limit of the method, 350 000 years (S. Mishra 1992). Exceptions are two dates of about 290 000 BP from animal molars (at Teggihalli and Sadab).

Recently, Kumar (1996) has reported the discovery of 498 cupules in the cave of Daraki-Chattan, in the Chambal valley region. He has suggested that this rock art dates from the Acheulian or Middle Palaeolithic periods, essentially because the stone tools of these periods occur on the floor deposit within the cave. In view of the petroglyphs in Auditorium Cave these claims need to be carefully examined, and the International Federation of Rock Art Organisations has assembled a commission to investigate the extraordinary evidence from Bhimbetka, Daraki-Chattan and more such sites in India (Bednarik 2001b; Kumar et al. 2002).

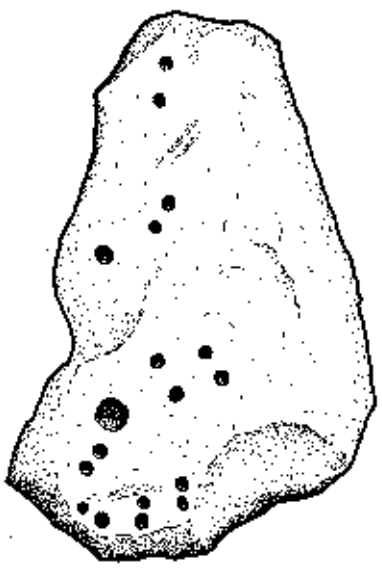


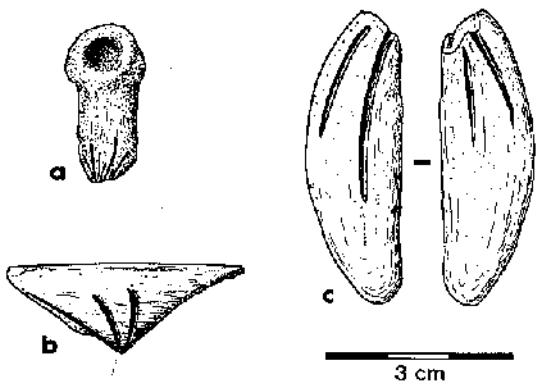
Figure 7. *Large limestone slab over Neanderthal child's grave, La Ferrassie, France, bearing eighteen cupules on its underside.*

A set of eighteen similar cupules has also been found on a large rock slab placed intentionally over the grave of a Neanderthal child in La Ferrassie, France (Figure 7), apparently belonging to the Mousterian (Peyrony 1934). Elsewhere, too, cupules are thought to be among the oldest forms of rock art, especially in North America, where they are widely regarded to be earliest (Parkman 1992), and in Australia (Bednarik 1993b) where they may be up to 55 000 old. However, before we draw from this the hasty conclusion that this means that cupules were indeed the first rock art, we would be well advised to remember that this is taphonomically unlikely. Petroglyphs or rock carvings are generally of greater longevity than rock paintings, and among petroglyphs, cupules probably last longest. If the oldest form of rock art in a region happens to be the one best able to survive, logic tells us that this is probably not so because no other art was produced at the time, but because only the most resistant art managed to survive. In fact, the extremely early haematite pebbles with wear facets and striations show us that rock surfaces were coloured with them, and whatever was drawn on such surfaces, it had no chance of surviving to the present time.

The most interesting observation from an Indian perspective is that not only does the earliest Indian evidence of non-utilitarian behaviour match that from elsewhere, some of the earliest such finds are actually from India. This simple fact does not seem to have been much appreciated so far, which may be because the focus in art origins has entirely been on western European Upper Palaeolithic parietal art and mobiliary art (Bednarik 1994c, 1995c). Even older portable markings from Europe have been largely ignored, although it is generally agreed that

several are indisputably non-utilitarian. The Tata (Hungary) nummulite has engraved lines on each face, forming a perfect cross with a natural fracture line (Bednarik 1992a: Fig. 4), and like the polished and ochred plaque made from a mammoth molar from the same site belongs to the Mousterian (Vértes 1964; Marshack 1976). So does the bone fragment from Bacho Kiro (Bulgaria), with its several engraved zigzag patterns, or the bone with numerous parallel lines found in another Neanderthal grave in La Ferrassie (Capitan and Peyrony 1921). The engravings on three bone fragments and a horse tooth from Prolom II on the Crimea belong to the Micoquian (Figure 8), and are indisputably intentional and non-utilitarian (Stepanchuk 1993).

Figure 8. *Engravings from the Micoquian of Prolom II, Ukraine. A modified phalange (a), a bone splinter (b), and a horse canine (c). Scale 3 cm.*



Older still are the seven engraved objects from Bilzingsleben, a German site that is perhaps 300 000 to 350 000 years old (Mania and Mania 1988; Bednarik 1995c). Also of the Lower Palaeolithic, in fact of the Acheulian, is the engraved bone from Sainte Anne I, at Polignac, Haute-Loire, France (Crémades 1996). There are many further notched or engraved objects (Bednarik 1992a; Bednarik and Kuckenburg 1999), some of which are doubtful, and there are a couple of hundred objects with holes from Lower and Middle Palaeolithic sites. Some are clearly natural holes, but the possibility that such objects were used as beads should not be simply ignored. The majority of these finds is not naturally perforated, but may have been punched or drilled with stone tools. Certainly this applies to teeth with neat holes drilled or reamed through their proximal ends, and to ostrich eggshell beads.

No finds of the many types listed here would attract scepticism as to their non-utilitarian status if they were found in an Upper Palaeolithic deposit, but when they occur in older strata they are almost inevitably rejected by some practitioners. Clearly, archaeologists apply different standards to the data according to its age, and while they are ostensibly engaged in determining what the cognitive abilities of hominids might have been, they seem to be entirely confident that they already know the answer to that question. There is no semblance of scientific rigour in these skewed endeavours with their pre-emptive ideology, because archaeologists certainly have no idea of what the faculties of hominids were; it is therefore grossly negligent to treat the data as if they did.

Upper Palaeolithic symbolic evidence

While the Indian evidence from these earliest periods matches that from elsewhere so well, by the time we reach the Upper Palaeolithic we note a significant divergence. There is nothing to correspond with the western European 'cave art' in India, which is not an important point because that art is itself obviously a taphonomic remnant (Bednarik 1986, 1994d). We could thus easily explain its absence in India by a lack of limestone caves with suitable speleoclimates and geology. The absence of an industry of portable art is more conspicuous, although even here, taphonomic arguments could be valid. However, I believe that the real explanation lies primarily in the pattern of artistic comparisons within Eurasia. It is often perceived by European archaeologists that aspects of the Franco-Cantabrian art can be recognised as far east as the Irkutsk area in Siberia, which begs the question why they are not apparent in regions such as India. However, the proposition is itself misleading. The western European rock art traditions with their frequently figurative content are entirely absent east of the Rhine and Italy. The only possible exceptions are two caves in the Ural mountains (Kapova and Ignatiev; the Pleistocene age of their art still needs to be demonstrated and is questionable, cf. Steelman et al. 2002) and two claimed Palaeolithic sites in the Balkan region (Badanj and Cuciulat, also unconfirmed). The only Pleistocene rock art reported in central Europe is non-iconic (Hahn 1990) and has been rejected (Bednarik 2002c), and even the portable art (other than sculptures) to the east is almost without exception non-iconic (meaning that we cannot recognise any depicted objects). There are only two or three graphic (two-dimensional) depictions in Russia, and two in Siberia. Apart from that, all Pleistocene rock art and portable art in about 97% of Eurasia, where such art occurs, is totally non-iconic (Bednarik 1993c).

The question why the rock art of 3% of the area of Eurasia should have ever served as a universal model in this discipline can only be plausibly answered in a political sense. If the Franco-Cantabrian art had been discovered in South-east Asia, it would have attracted very little interest, if indeed any. Even more importantly, the same pattern, generally speaking, seems to apply to the rest of the world then occupied by humans, most particularly in Australia, but probably also in the Americas. Only in southern Africa do we have iconic depiction from the Pleistocene, but only from one single site (Apollo 11 Cave, Namibia). Therefore it is reasonable to regard the western European rock art, particularly of the Solutrean and Magdalenian (i.e. the late Upper Palaeolithic) as unusual and exceptional, and certainly not as typical of the Final Pleistocene.

It would be quite wrong to expect this kind of pattern in India, or in any other region isolated from western Europe. Moreover, it would be far more correct, and scientifically balanced, to generalise about Upper Palaeolithic rock art primarily from that found in the rest of the world. The portable graphic Pleistocene art we have from eastern Europe, Siberia, China, Israel, Afghanistan and India is essentially 'geometric', consisting of sometimes exceptionally complex patterns which are conceptually more sophisticated than the 'cave art' of south-western Europe (Bednarik 1993c). They appear to be derived from geometric graphic traditions of the Middle Palaeolithic or Middle Stone Age of Europe and Africa. Precisely the same applies in Australia, where geometric petroglyphs are in excess of 30 000 years old (Bednarik 2002d) and belong to a Middle Palaeolithic mode of technology. But whereas we have tens of thousands of art motifs in Australia, which are thought to be of the Pleistocene, the total number in Asia remains minute. For instance, both China and India have each only yielded one item of genuine Upper Palaeolithic art at the present time, and very few other 'symbolic' finds (Figure 9).

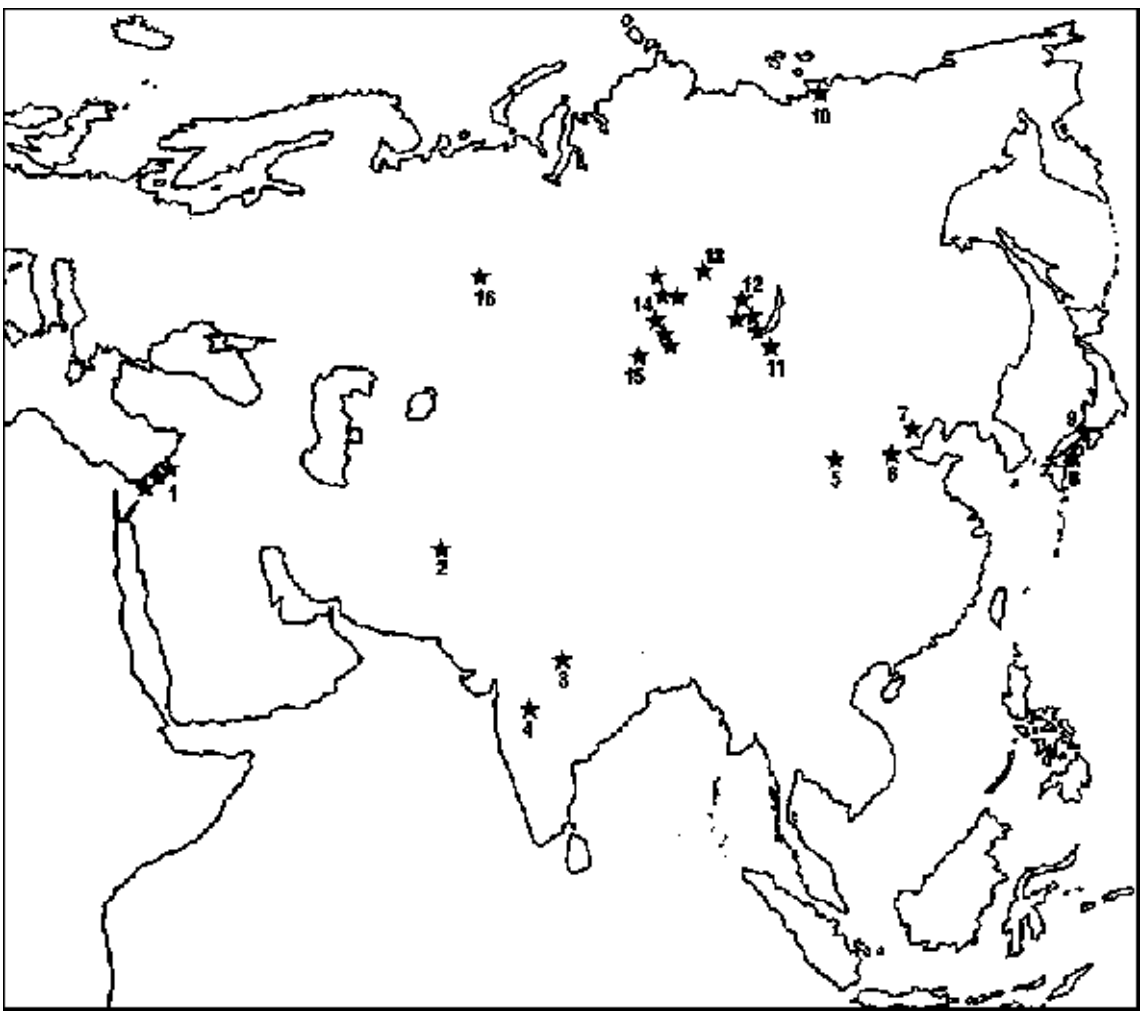


Figure 9. *The distribution of all currently known finds of Pleistocene 'symbolic' material in Asia.*

The situation is not significantly better elsewhere in Asia and Africa, and it is clear that we need to explain this pronounced paucity of evidence. This is particularly so as the combined surviving art production of the Middle Stone Age of Africa, and the Middle Palaeolithic cultures (or those based on their technological traditions) of Europe, Asia and Australia is vastly greater than the combined surviving art production of typically Upper Palaeolithic and Late Stone Age traditions.

There are essentially three possibilities to explain the paucity of Upper Palaeolithic evidence of art traditions, almost anywhere in the world then settled:

(a) That art was not produced: this is unlikely, particularly if some art is in hand from a region, as is the case in India;
(b) That it has not survived in sufficient numbers;
(c) Or that it has not been found yet, either because of inadequate research or taphonomy, or because research has been guided by incorrect notions of the types of such art one would expect to find.

The last-mentioned possibility does sound particularly plausible to me, and in most regions so affected I suspect that the true explanation is a variable combination of this factor with inadequate research and poor preservation. Indian researchers have tried to locate material resembling that of European Upper Palaeolithic types, for example by interpreting the bone harpoon from Lohanda Nala, Uttar Pradesh (Figure 10), as a female figurine ('mother goddess'; V. D. Misra 1977).

Figure 10. *Damaged Upper Palaeolithic bone harpoon from Lohanda Nala, which has mistakenly been interpreted as a female figurine.*

Some have considered the early dynamic anthropomorphous rock paintings in central India as belonging to the Upper Palaeolithic (Wakankar 1983). It was subsequently shown by superimposition that these animated figures were preceded by a style called 'intricate patterns' (Tyagi 1988), which are entirely geometric and non-iconic (Figure 11).

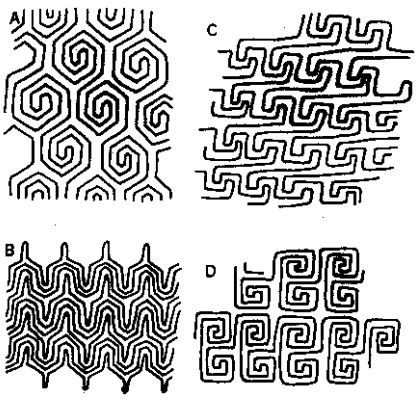


Figure 11. *Rock paintings of 'intricate patterns', central Indian rockshelters (after Tyagi 1988).*

These repetitive patterns are vaguely reminiscent of those on the engraved antler fragment from the Upper Palaeolithic in Longgu Cave, China (Bednarik and You 1991). The engraved ostrich eggshell fragment from Patne (Sali 1980), the only Upper Palaeolithic art object from India so far, also bears geometric decoration (Figure 12).

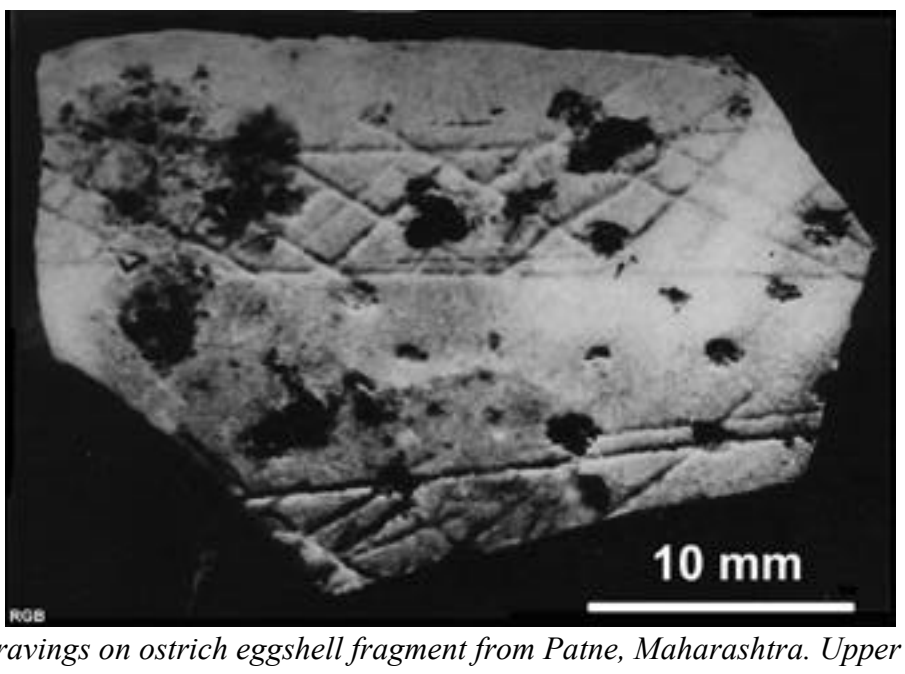
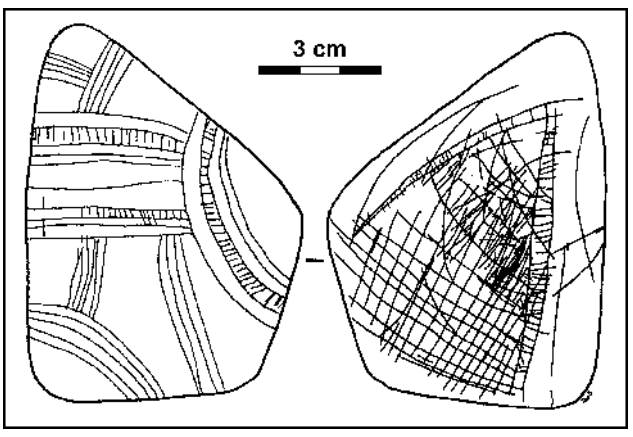


Figure 12. *Engravings on ostrich eggshell fragment from Patne, Maharashtra. Upper Palaeolithic.*

Conversely, on the basis of my microscopic examinations I reject Neumayer's (1993: 33) notion that the

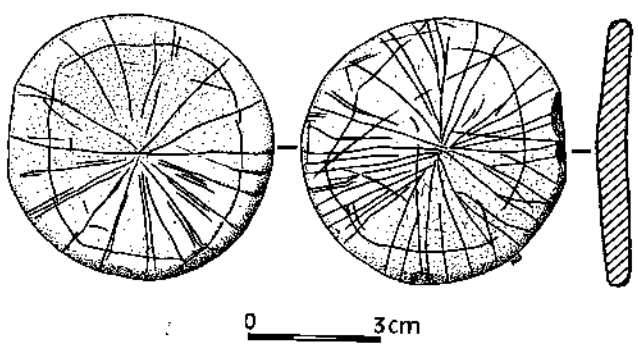
Indian ostrich eggshell objects were made from already fossilised material. The Patne specimen has been radiocarbon dated to about 25 000 years BP and I have described its markings in detail (Bednarik 1993e). I have rejected the anthropic origins of markings on forty-five other ostrich eggshell fragments from several Indian sites as a natural process caused these marks. The general structure of the engraving on the Patne fragment resembles that of an Upper Palaeolithic find in Israel, which is perhaps pure coincidence but is nevertheless worth mentioning. The engraved limestone cobble from Urkan e-Rub is between c. 19 000 and 14 500 years old (Figure 13). The borderlines evident on the Patne specimen occur also on the Chinese find mentioned above, and



in other early palaeoart.

Figure 13. *Epipalaeolithic engravings on both faces of the limestone cobble from Urkan e-Rub, Israel.*

There are a few other Palaeolithic finds from India which may have non-utilitarian significance, such as the circular stone discs reported by Wakankar from the Acheulian of Bhimbetka III F-2·1 and by J. M. Pal from Maihar. Murty has excavated bovid teeth in the Upper Palaeolithic occupation deposit of the uppermost of the Kurnool Caves (Andra Pradesh) which bear grooves that facilitated the attachment of strings (Murty and Reddy 1975). Such finds of various types of pendants (teeth, bones, ivory, fossil shell) that were not perforated, but incised around one end, occur widely in various Upper Palaeolithic traditions from Spain to Siberia, including even the Neanderthals' Châtelperronian of France (White 1993b). Once again the evidence is consistent, but the small number of Upper Palaeolithic beads from India (perforated or grooved), occurring at only three sites in total (including perforated ostrich eggshell beads at Patne and Bhimbetka III A-28), indicates once again that we are dealing with a savagely truncated record. Quite clearly, there is far too much that we do not know, or do not yet know, to permit ourselves any definitive statement concerning any aspect of the palaeoart of the Indian



Palaeolithic periods.

Figure 14. *Both sides and section of Upper Palaeolithic engraved ivory disc from Afontova Gora III, western-central Siberia.*

In Siberia, supposed Upper Palaeolithic portable art has been reported from eighteen sites, several of which have also yielded engraved items (Figure 14). However, portable sculptures are more prominent, as they certainly are in Russia and the Ukraine. Claims for Middle Palaeolithic portable art and Upper Palaeolithic rock art in South Korea lack any authentication, and the only non-utilitarian objects from the Japanese Palaeolithic are a perforated stone disc, a polished stone object and some engraved pebbles (for all Asian references, see Bednarik 1995d).

The scarcity of demonstrably Upper Palaeolithic rock art in India is not unique; it applies to all countries of Asia. Although it is quite likely that such art does exist in China, none has been shown to be of such age so far, but this is not surprising. Chinese rock art, like that of India, remains largely undated (Bednarik and Li 1991). In Siberia, a few motifs at the sites Shishkino and Tal'ma have been attributed to the Upper Palaeolithic, but this has been rejected (Bednarik and Devlet 1992). The only possible exceptions in all of Asia are some linear petroglyphs in caves of Mt Carmel, Israel, which have been suggested to possibly be of Palaeolithic age (Ronen and Barton 1981). Bearing in mind the enormous size of Asia, it is evident that there has to be a valid explanation for this profound paucity of finds.

However, there are now a number of petroglyph sites known from central India, which appear to be of very great age. Whether they could be of the Palaeolithic has not been ascertained so far, except in one case (Auditorium Cave), but I should not fail to mention them here. The few petroglyphs in one of the many painted shelters at Raisen (near Bhopal) are concealed by a silica skin that prevents exfoliation or weathering; they could therefore be of extreme antiquity. G. Kumar has discovered extensive cupule patterns and pounded grooves at two sites near Kota (Kanyadeh II and IIA; Bednarik et al. 1991) which are deeply patinated and covered by a mineral deposit. He has also found complex cupule patterns at Chattaneshwar in the nearby Chambal region, which precede a sequence of exfoliation events and are also deeply patinated. It must be cautioned that petroglyphs, including cupules, occur widely in the north and south of India as well, but that these traditions seem to be very significantly younger. The petroglyphs are often unpatinated or only partly patinated. There does not appear to be a connection with the older tradition in central India.

Holocene evidence of Indian art

It is only with the advent of the glorious surviving rock painting traditions, especially in the central regions of India (the Bhopal region, Chambal valley, Mahadeo Hills, northern Vindhya Hills and southern Deccan are the major concentrations), that Indian rock art comes into its own (Chakravarty 1984; Brooks and Wakankar 1976; Gupta 1980; Mathpal 1985; Neumayer 1983, 1993; Sankalia 1978; Tewari 1990; Chakravarty and Bednarik 1997). By that time, however, quite distinctive styles had been developed, and the more recent the art is, the more it becomes irrelevant to attempt simplistic comparisons with art traditions elsewhere. The time of commencement of the traditions that produced this vast corpus of surviving rock paintings remains unknown. As already mentioned, none of this art is dated, and it seems to commence with non-iconic, intricate patterns. One is tempted to suggest that these might be of the final Upper Palaeolithic, but this is pure speculation. The geometric patterns are followed by the dynamic and animated figures, often painted in green copper minerals (notably in the impressive series of sandstone shelters at Jaora, south of Bhopal). Again, this may be an Epipalaeolithic tradition, or it may be younger. Most Indian rock art specialists regard the subsequent major tradition of figures with geometric infill patterns as Mesolithic, but even here it must be cautioned that this is based on inductive reasoning of various types, not on established dating methods. Moreover, modern rock art research rejects the reliance on stylistic reasoning, i.e. the idea that researchers can reliably identify styles, or that stylistic similarity inevitably proves cultural unity. It must be appreciated that many of these models and practices are the legacy of a period when rock art studies were entirely tied to archaeology, and when such studies were conducted with essentially Eurocentric ideas of past societies and their cultural dynamics. Many of these ideas have been questioned and even rejected, so it would be inappropriate to continue using the guidelines and philosophies developed by these approaches. Modern rock art studies emphasise the importance of epistemologically sound reasoning, of scientific methods of enquiry, and of the practical application of refutationist or post-processualist thinking (Bednarik 2001c). For instance, it is not science's role to prove or disprove theories conclusively. In most cases, science is incapable of doing this.

More specifically, it is not the role of scientists to determine or pronounce what is depicted in the art. Controlled experiments (which have only been possible in Australia) have shown that scholars are incapable of identifying rock art motifs correctly in most cases (e.g. Macintosh 1977). Where they maintain their postulated identifications this is only possible because these are incapable of being refuted. I cannot emphasise enough that it

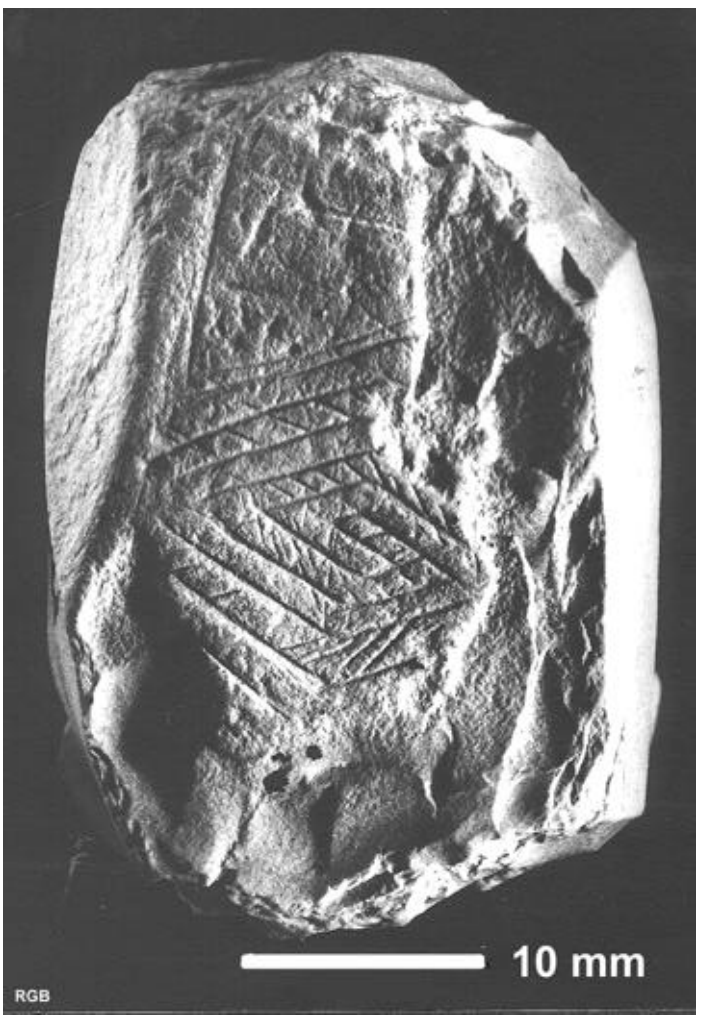
is in most circumstances irrelevant what archaeologists think is depicted in an ancient rock art. The only exception would be if one wanted to study the cognitive perception of archaeologists. Other than that, real scientists are not interested in anyone's pronouncements about the iconographic content of very early rock art (i.e. what it depicts). This does not mean that I am asking Indian rock art connoisseurs to discontinue the practice of telling themselves or anyone else what they think is depicted in rock art, if this is what they feel inclined to do. If they cannot resist this urge, perhaps this is how they should engage their creative talents. The only request I have is that they do not posit their intuitions as scientific propositions; an academic role does not entitle one to mislead the public. The simple fact of the matter is that we, all of us, *do not know* what is depicted in rock art *with the degree of certainty we demand of science*, unless the artist or a traditional custodian of the art has provided an authoritative interpretation of it. I imagine that there is no rock art anywhere in the world beyond an age of just a few thousand years for which the initial cultural interpretation could have possibly survived intact. Consequently L. Wittgenstein's dictum applies: 'Whereof one cannot speak thereof one must be silent'.

The present chronology of Indian rock art may well be correct, in a broad sense, but we should not rely on this too much. We know that the Saharan and eastern Spanish Levantine rock art traditions (and others) were badly misjudged on the basis of similar arguments about style, content and superimposition, and on simplistic preconceptions of what determines arts of hunters and arts of agriculturalists. These naive assumptions are based on inaccurate reasoning. We know that many Neolithic rock arts are dominated by apparent hunting motifs (e.g. Scandinavia, Karelia, Sahara), while the arts of true hunting and foraging peoples may lack any depictions of hunting scenes, animals, weapons and so forth (e.g the Bradshaw traditions of the Kimberley, Australia; see Figure 15). We need to be careful not to become trapped in our own chronological (and interpretative) constructs, so until scientific dating of Indian rock art has commenced we would be well advised to reserve our judgment. And once scientific data do become available, let us not misinterpret them in the naive fashion of some Western researchers (Bednarik 1994b, 2002a), but remember that these results always have serious limitations. They are inevitably subject to a variety of logical, statistical and technical qualifications.

Figure 15. *Typical Bradshaw rock paintings from Kimberley region, Australia. This art was produced by*

hunting peoples but is free of hunting motifs and weapons, its primary concern being the elaborate headdresses and other apparently ceremonial paraphernalia on anthropomorphs (after Welch 1993). These paintings are thought to be 4000 to 10 000 years old, but remain inadequately dated.

Portable art of the Indian Mesolithic would help us considerably, but as in the preceding Upper Palaeolithic, there is almost no evidence of it available. The only instances I know of are the classical Chandravati chalcedony core with its delicate geometric engraving (Sonawane 1991), an engraved human tooth (Bednarik 1993a) and a few engraved bone objects described by Wakankar (1975) from Bhimbetka III A-28. In view of the enormous number of rock paintings credited to the Mesolithic, the almost complete lack of evidence of Mesolithic portable art is conspicuous. In fact, the Chandravati core (Figure 16), which is a key exhibit in the current chronological model, is not even archaeologically dated. It is merely assumed to be Mesolithic because of its context and



artefact typology.

Figure 16. *Probably Mesolithic bipolar micro-core of chalcedony, with engraved geometric pattern, from Chandravati (after Sonawane 1991).*

Subsequent to the presumed Mesolithic art traditions, there is a great deal of proto-Historic and Historic rock art in India. The chronological resolution then becomes progressively sharper with time, especially with the introduction of Brahmi script. Much the same applies in China, where epigraphic styles become reliable chronological markers with the advent of writing. In India, 'proto-Historic' usually refers to the period preceding the Mauryan empire; i.e. to more than 4000 years ago. In parts of the country it is synonymous with the Harappan civilisation, whose script remains undeciphered, or with the Chalcolithic period. The latter term refers to the Eneolithic, and there is no distinctive Neolithic period in India.

A common difficulty is that, at many rock art sites that were used in Historic periods, there are often preceding pre-Historic traditions present. They are frequently ignored, being poorly preserved, patinated, simpler, less photogenic, less 'interesting' and often concealed by the more recent art, even modified by it at times. This does not help greatly in finding and recognising earlier art motifs, most particularly at densely decorated panels. There should be no doubt that the greatest unrealised potential in the exploration of Indian rock arts relates to the almost total neglect, up to now, of the 'less picturesque' and the less conspicuous component of these arts. One of the greatest assets of Indian rock art, its immense wealth of figurative, colourful and visually exciting pictorial art, is perhaps also its greatest disadvantage. It has completely eclipsed the scientifically more important components and aspects of these arts, and among them are no doubt its earliest components.

Rock art research in India

The study of Indian rock art began in the 1860s, with Archibald Carllyle's explorations in the northern Vindhya Ranges, in the Mirzapur district of Uttar Pradesh (Carllyle 1885), followed by the research there of Cockburn in the late 1800s (Cockburn 1883, 1888, 1894). Actually, the earliest reference we seem to have relates to petroglyphs at Almora and dates from 1856 (Chakravarty and Bednarik 1997). Much earlier, the Chinese geographer Li Daoyuan (fifth century A.D.) published detailed descriptions of Chinese rock art in his book *Shui Jing Zhu* (which means *Notes on the systems of rivers*), mentioning also the existence of rock art in regions that are now part of India and Pakistan. Fawcett (1901) examined the sites Koppagallu and Edakal Cave in the 1890s; Francke (1902, 1903, 1925) worked at a series of petroglyph sites in Ladakh. Silberrand (1907) in the Banda District, Anderson (1918) studied the rock paintings at Singhanpur, Brown (1923) those at Raigarh, but the first monograph on Indian rock art did not appear until well into the 20th century (Ghosh 1932). Ghosh had studied the Mirzapur region rock art complex as well as several sites in Madhya Pradesh, at Raigarh and Hoshangabad, in the early 1920s. His work was followed by that of Gordon (1935, 1951; Gordon and Gordon 1939, 1941; Gordon and Allchin 1955).

Excavations at rock art sites began with those at Piklihal (Allchin 1960) and Tekkalkota (Nagaraja Rao and Malhotra 1965). While A. Sundara continued the detailed reconnaissance in southern Deccan (Sundara 1974, 1975, 1984), Wakankar (1955, 1975) focused on the hills of Madhya Pradesh. In 1971 and 1972, excavations in the cave were begun by Sagar University (S. K. Pandey) and Vikram University (led by Wakankar) respectively. While Pandey abandoned his dig after a week, Wakankar continued excavation at Bhimbetka until 1977, having been joined by V. N. Misra of Poona University in 1973. In 1978, H. D. Sankalia produced his seminal *Prehistoric art in India*. Just two years earlier, Wakankar had published the book *Stone Age paintings in India*, together with R. R. R. Brooks. Many other competent rock art specialists began to emerge in India during the 1970s, including from among the researchers who worked with Wakankar. Some of them remained in the discipline for many years, particularly the artist Yashodhar Mathpal, the first to conduct major rock art recording work at the Bhimbetka site complex (Mathpal 1978). He subsequently went on to publish two more volumes on rock art, one on Bhimbetka (Mathpal 1984), the second on the petroglyphs of the Himalayan foothills (Mathpal 1995). Another of Wakankar's co-workers who went on to produce valuable work was Erwin Neumayer, an Austrian scholar who worked closely with local researchers and produced several reference works on Indian rock paintings (Neumayer 1983, 1988a, 1988b, 1993) as well as a series of articles.

During the early and mid-1980s, regional surveys were conducted in most major rock art regions of the country. Various major syntheses appeared now, in particular the major volume edited by K. K Chakravarty (1984), containing a collection of the works of many Indian researchers, conveys the new-found confidence of the discipline, and in many ways anticipates developments that were to follow (e.g. Tewari 1990).

The First AURA Congress, held 1988 in Darwin, Australia, has been described as the birth of a new discipline (Odak 1993a). It was attended by 343 leading researchers from all parts of the world, representing the 35 countries most active in this field, and among them were eight Indian scholars. Their attendance of the congress led directly to the establishment in 1988 of the Rock Art Society of India (RASI) by Giriraj Kumar, another of Wakankar's pupils, who also founded the journal *Purakala*. Kumar has allied both RASI and *Purakala* with the international research community, and there can be no doubt that this will influence the future and the direction of Indian rock art studies. A number of other factors in the course of the 1990s have added to these recent developments, such as the involvement of the Indira Gandhi National Centre for the Arts in New Delhi in matters to do with rock art (Lorblanchet 1993a). In 1993 the Centre staged an international rock art congress in New Delhi.

The appointment of Chakravarty as the Director of the National Museum of Man, Bhopal, has also had a significant effect. As already noted, he had edited an influential rock art book in 1984, and had continued his support for rock art studies over the years. Ten years later he staged an impressive rock art exhibition at the World Archaeological Congress in New Delhi, and subsequently published a major book on the place of Indian rock art in world rock art (Chakravarty and Bednarik 1997). He has also facilitated the creation of colour re-constitution in rock art recording through the IFRAO Standard Scale (Bednarik and Seshadri 1995). Thus India has become an important contributor to international rock art research, a process currently culminating in the EIP Project of RASI, AURA and IFRAO (Kumar et al. 2002).

Discussion

In any search for Palaeolithic art in India we should be guided by logic. I am confident that such art does exist there, but that it has generally not been recognised so far. The type of such art that would have survived and is likely to be detected is fairly easy to determine by logic. Petroglyphs generally last longer than rock paintings, except in deep caves or where a silica skin covered paintings. Both conditions apply rarely in India. Among various types of petroglyphs, those that are deepest have by far the greatest potential to survive, and cupules and simple geometric figures are usually the most deeply pounded. In looking for particularly ancient rock art, these should be the essential guidelines. Of course there are some additional considerations: a petroglyph's life may be very significantly prolonged if it occurs in a sheltered location or on highly weathering-resistant rock.

Most major hypotheses about art beginnings have dealt exclusively with the subject of western European Upper Palaeolithic art, and earlier evidence from other parts of the world was persistently ignored, or systematically rejected as being unacceptable. The reason for its rejection had less to do with its nature, recovery or authenticity, than with the fact that much of this material did not come from Europe. It obviously challenged the idea that art and culture are European innovations. That Eurocentric notion has been maintained for a century, but having at long last come under sustained scrutiny it is beginning to crumble. The bitter exchanges between European traditionalists (Lorblanchet 1993b) and their antagonists (Bednarik 1993f; Odak 1993b) are attributable to the understandable reluctance of the former to relinquish their pre-eminent position in the discipline. This coincides with the reluctance of numerous academic luminaries who see their espoused models of the past severely threatened. The current development is obviously a major threat not only to neo-colonialist ways of thinking, but also to traditional academic structures in the Western world. Resolving the resulting tensions is going to be painful for the discipline, but we must confront these issues squarely if we are to claim scientific credibility.

For Indian researchers, these issues are considerably less disruptive or threatening than for those who have fostered the extreme polarisation of certain underlying issues (particularly some western European and North American palaeoart specialists). For instance, in terms of the earliest art forms practised in India, the research focus merely needs to be shifted from a preoccupation with European developments to an appreciation of the global pattern (most especially, the Asian pattern) and complexity in the development from non-utilitarian behaviour, to graphic marking traditions, to iconicity and to historical art forms. I have tried to relate such a model here. It would seem that all hominid populations began with essentially the same initial cognitive building blocks, developing these into the graphic marking traditions that reigned universally during the long period of the Middle Palaeolithic. With few exceptions, their forms seem to express a unified concept of reality which is perhaps best preserved in the vast numbers of motifs in the archaic linear petroglyph traditions of Australia (Bednarik 1987). It would seem from the available evidence that, at that stage, the cultural unity of humanity was probably universal (Bednarik 1995c).

It is particularly important to note that the objective record of palaeoart and related phenomena provides no justification at all for distinct cognitive differentiation between human 'subspecies' we perceive in the Pleistocene. This applies to the division between *Homo erectus* and archaic *Homo sapiens*, just as it applies to the much-discussed separation between Neanderthals and their late contemporaries in Europe. For instance, the Neanderthals of the Châtelperronian used ornamentation (ivory rings, perforated and incised pendants, ochre, fossils and crystals) that is so similar to that of the contemporary Early Aurignacians that it prompted White (1993b) to propose that they must have scavenged these articles from the abandoned camps of the presumably pre-Cro-Magnon people. This is merely a desperate bid to save his doomed theory of an 'explosion-like' appearance of ornamentation. Firstly, White cannot know what sort of people the Early Aurignacians were (we have no skeletal remains of them). Secondly, how can he know that it was not the other way round, and that the Aurignacians took their ornaments from the Châtelperronians? This is tautological science fiction, not science, and once again I stress that we do not know what the intellectual or cognitive capabilities of hominid groups were. To label one as more primitive than another without substantial cognitive and mentalistic evidence is no different from racist assumptions about ethnographic peoples. But most absurd is White's effective proposition that the Châtelperronian Neanderthals were incapable of producing 'symbolic' artefacts, yet that they collected such artefacts and presumably used them. For what? It is my view that a hominid that has no concept of symbolism at

all will not perceive a non-utilitarian object as being symbolic or in any other way useful, and worthy of carrying around. The implication that such hominids adopted symbolism by 'learning' it in this way is particularly incongruous. It ignores the concept-mediating role of symbolism, and that symbolism can only be acquired through the identification of intangible conditions or perceived truths by artistic understanding. Symbolism, art origins and the advent of human conceptualisations of reality are closely correlated, which seems to escape White. His criticism of what he calls the sterility of the question of the 'origins of art' (White 1992: 549) implies that he lacks an appreciation of these relationships, and that he assumes reality to be as we perceive it. It is clear, on this basis alone, that he and I have practically no common ground in an epistemological sense, because I would most vigorously reject the positivistic notion that some Western model of anthropocentric reality, concocted by confirmationist scientism such as that predicated on the empiricist determinism of some shamans of 'science', resembles real reality (Bednarik 1990-91, 1992d).

The taxonomic separation of the distant human past into designated phases is quite arbitrary. These phases are anti-scientific because they encourage the collection, interpretation and reporting of all evidence to be conducted in such a fashion that the results always confirm such divisions. Artificial divisions are created between periods and peoples, and then confirmed ad infinitum as if these conceptual artefacts were real divisions. Ultimately they lead to grotesque explanations such as those I have mentioned here. There are numerous other forms of conceptual plateaus introduced into the archaeological mythologies about the past, through methods of dating (e.g. radiocarbon dating has created and reinforced artificial plateaus) and data recovery, and through a variety of taphonomic processes (eustatic oscillations, geomorphological processes or events etc.). The epistemologically naive interpretation of the kind of 'archaeological record' we can reasonably expect from this potpourri of 'evidence' is what dominates most archaeological interpretation. This is not science, it is a mythology, and it does no justice to the peoples of the past.

Seen in the global context of a more sophisticated approach to the past — one that is predicated on a comprehensive knowledge of the data and lacks dogmatic adherence to superseded models — Indian palaeoart is of outstanding importance. Even the extremely patchy information currently at our disposal is adequate to show us that Indian finds are among the oldest evidence we have of nonutilitarian hominid behaviour. It is also clear that all of the early record is profoundly incomplete: we have almost no confirmed art of the Upper Palaeolithic as well as of the Mesolithic, even though we reasonably believe that a large proportion of Indian rock art is of the latter period. But what I find most important about the Indian record of palaeoart, and what it clearly has in common with the rest of the world, is a distinctive patterning in the form of the art-like evidence. So older the art, so more consistent is the occurrence of those types of evidence that are most likely to survive the longest.

This reflects the global pattern precisely, and it seems to demonstrate the opposite of what naive empiricists would say about it. To assume, as they do, that the earliest art was the most resistant seems fundamentally illogical. Surely it is much more logical that this is the oldest surviving art precisely because it had the best prospects of survival. Or in other words, all the other arts of those very early times were of types that had no chance of survival. Taphonomic logic demands that this is by far the most likely explanation of the evidence patterns we perceive, in fact it is almost certainly valid. It means that we have no direct access to the art forms that have been lost, and that it would be wrong to base any propositions of cognitive or intellectual status on this severely truncated record. In other words: the evidence can at best inform us about the minimum level of the hominid faculties in question, but is certainly not 'typical' evidence or an accurate basis of evaluation. In the same way as we have almost no direct evidence whatsoever of any strings or ropes from the Pleistocene but have indirect evidence, such as their depiction on female figurines from Pavlov and Kostenki 1, we have no evidence of pigmented rock markings of the Acheulian, but we know that Acheulian hominids made marks on rock surfaces. The fact that we will probably never find these marks does not negate the fact that they were made.

In the final analysis, palaeoart students are divided into two camps: those who would say that the reconstruction of cultures is to be based on their limitations as implied by current lack of relevant evidence; and those who would say that so older a culture is, so larger the proportion of evidence of it that has become unavailable, hence so larger the gap between the surviving evidence and the culture's true reflection. It is clear that the former approach, to be consistent, would then apply right across the entire spectrum of archaeology. We should then also assume that early hominids lacked internal organs because we have never found any soft tissue with their remains, or that Pleistocene humans lacked boats because we have never found any physical evidence of them. Here, however, the naive empiricists concede that there must have been soft tissue on the skeletal remains, or boats and well developed ocean navigation, simply because they cannot think of another way to explain the presence of hominids on the many islands they did colonise, not to mention Australia some 60 000 years ago. If it is reasonable to extrapolate in such cases it is equally reasonable to extrapolate in similar circumstances, including in the introduction of symbolism. It must have occurred very much earlier than empiricists are willing to concede, and we even have limited evidence for it — including in India. But perhaps most importantly, taphonomic logic decrees irrefutably that the first frequent occurrence of evidence of an ephemeral phenomenon, in archaeological remains that are subject to a cumulatively increasing proportional loss with time, cannot possibly mark the time of that phenomenon's first occurrence. It can only mark a very much later point in time (Bednarik 1994d).

Archaeology cannot be scientifically comprehended without appreciating this principle, therefore the most splendid contribution Indian palaeoart studies are capable of making might well be the determination of the effects of cumulative taphonomic reduction on early samples of symbolic evidence. This can be best studied in those regions of the world where the continuous use of symboling seems to reach back furthest. In my view, and despite the immense lacunae in our present knowledge, India provides us with such a region — and a unique opportunity to probe the most difficult subject in science: the origins of the human mind. Seen in an all-embracing global perspective, this could be India's finest contribution to the study of palaeoart.

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