Semiotix Course 2008, The epistemology of Pleistocene archaeology

Robert G. Bednarik

Lecture No. 6. **Logic in archaeology**

Introduction

The first five lectures of this series could easily create the impression that its purpose is to discredit archaeology as a discipline. Nothing could be further from the truth. Their purpose was to explain epistemological impairments of various types, in an effort to understand contingent theoretical and procedural deficiencies that are amenable to correction. Having thus clarified specific factors contributing to the epistemic malaise of archaeology, practical as well as theoretical, it is high time to demonstrate a crucial justification for such a comprehensive critique: do I have a better alternative to offer? I will present such an alternative approach in this lecture, and expand on it in the next.

This discussion is primarily about Pleistocene archaeology, because Holocene archaeology, covering the last 10,500 years, is in significantly better shape. Indeed, the closer we come to the present time, the more secure our models seem to be. The archaeology of the medieval and more recent times appears to be little more than a filling in of minor lacunae. To a perhaps lesser degree that can also be said about the Roman, Greek and even earlier Classical periods and societies. However, by the time we explore beyond the introduction of writing, into the early agrarian cultures of the mid- to late-Holocene, the veracity of archaeological explanations and interpretations tends to become increasingly hazy, because it depends more and more on authority as we proceed further back into time. Significant deficiencies appear as we probe into the period claimed to mark the advent of semi-sedentary societies, named the Mesolithic in Europe. Here we find the first suggestions of major misinterpretation of the empirical evidence.

This point is readily demonstrated. The Mesolithic or Middle Stone Age sits rather uneasily between two arbitrarily defined major 'cultural' eras: the farming communities of the Neolithic time, and the presumably nomadic peoples of the Upper Paleolithic periods. Not only is this supposed intermediate stage in human development rather vaguely defined in western Europe, it seems to be largely lacking in southeastern Europe, northern Africa and in the important region of southwestern Asia. Indeed, the term has really little meaning in any other continent. In western Europe it defines societies that may or may not have had semi-permanent settlements, that focused on specific food animals (which simply marks a response to climatic changes), used microliths (as did previous cultures) and interment practices (ditto), and the only distinctive feature is the sudden appearance of coastal adaptation systems, including great shellfish middens. This is an entirely meaningless criterion, because we have not the faintest idea about the coastal cultures of the preceding Upper Paleolithic. Rising sea levels towards the end of the Pleistocene and in the first millennia of the Holocene ensured that all earlier evidence below about 140 m above the former sea level was obliterated (Fig. 1). Indeed, throughout the Pleistocene, the sea rose and fell many times, which practically halves the range of potentially recoverable evidence about the entire period in a very systematic fashion. The low-lying river deltas, the coastal zones and the lower parts of major valleys always presented the richest environments and would have always attracted far greater population densities, as indeed they still do today: over half the world's population resides in regions that would be submerged by the kinds of sea level rises experienced in the Pleistocene. It is also evident that, in contrast to the tribes of the hinterland, of the highlands, steppes and jungles, the coastal tribes would have been far more sedentary. But, alas, we have no knowledge of any kind about the ethnicity, technology, culture or way of life of any Pleistocene people who lived in these favorable en-

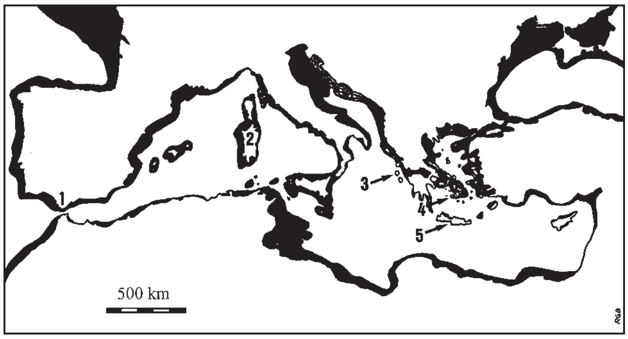

Figure 1. Former Pleistocene sea levels in the Mediterranean, showing the locations of water crossings of the Pleistocene.

Figure 2. The dugout canoe from Pesse, Holland, about 8300 years old, the oldest found watercraft.

vironments, anywhere in the world. But what we do know is this: as the sea level rose to roughly its present level, we see the sudden appearance of evidence of coastal societies we define as Epipaleolithic or Mesolithic. It is far more logical to see in them the coastal counterpart to the hinterland version of late Upper Paleolithic economies. Indeed, it would be strange if coastal adaptations had not been pushed to what is now dry land as the sea rose. But instead of this logical explanation, European archaeology has opted for an implausible alternative, inventing a cultural phase.

The importance of this example is that it shows that there are variables in the archaeology of early phases that can dramatically distort the evidence because of their systematic effects. The sea level changes have affected many other aspects of the surviving record. For instance we know that people of the Pleistocene must have crossed the sea on numerous occasions, because they colonized territories that were never accessible by land, or at least not during the existence of humans. But we cannot reasonably expect to find any remains of the vessels they used in these adventures. Because of the sea level rise around 9000 years ago, the oldest watercraft ever found, in Holland, is precisely that age (Fig. 2).

But there are many other factors that determine the survival of evidence in systematic patterns, so it is fundamentally false to draw one-to-one deductions from the archaeological evidence. Rather there needs to be a program of making allowances for all the systematic distortions likely to affect empirical data. Such a program does exist, it has been proposed in the early 1990s, but remains entirely ignored by mainstream archaeology. It is called *taphonomic logic*.

The role of taphonomy

Distinguished by its frequent absence in dictionaries (even those that normally do contain important terms), the word taphonomy referred initially to the study of the processes to which preserved organic remains had been subjected. Hence the word's original use was in paleontology, but even here the concept has only been seriously considered since the 1960s. Efremov (1940) introduced the term in an effort to seek laws explaining the processes relating to the burial of bones within a single framework. Paleontologists gradually took up this fundamentally scientific approach to the study of fossil remains over the following decades (e.g. Behrensmeyer 1975; 1978; Gifford 1981; Hill 1976, 1979). During the 1980s, archaeologists realized that the underlying principles also applied to their discipline, particularly to the organic aspects of site formation processes (Brain 1981; Binford 1981). After initially restricting their application mostly to faunal remains, some eventually perceived that the concept has broader applications. In Australia, Hiscock realized that the underlying principles were also applicable to stone implements (Hiscock 1985, 1990).

Today the word taphonomy as it applies in archaeology has become somewhat of a misnomer: tapho- is Greek for grave, and -nomy indicates systematization of knowledge. Archaeologically, taphonomy is now taken to refer to the study of the transformation of materials into the 'archaeological record' (Bahn 1992: 489). It has become evident that even this expanded definition may not be adequate, and particularly, that a taphonomy of paleoart, including rock art, places more rigorous demands on practitioners. But to explain this it is useful to first consider the paleontological application of taphonomic concepts, to see what can be learnt from them. That these experiences can be usefully employed in archaeology and rock art research only shows what a powerful epistemological tool taphonomic logic (Bednarik 1990–91, 1992, 1993a, 1994) is.

In paleontology, taphonomy covers all events during the transition of animal and plant remains from the biosphere to the lithosphere, including mode of death, scavenging, ingestion and digestive processes, transport (by animals, wind, water or sediment movement), surface weathering and geological erosion, trampling, differential dissolution of tissues and mineralization or other replacement processes, and even modification of osteal remains as tools by hominins. The organic remains one recovers bear evidence of their preservational history, including degree of completeness, damage patterns, orientation in respect to other debris, surface wear and alteration results. Without a good understanding of how these many processes may have affected statistical indices of the material it would be fairly futile to reconstruct biological models of the species or, indeed, the ecosystem in question.

This form of taphonomy aims to elucidate how biological information has been altered from the original living systems to a 'fossil record', by biological, physical and chemical degradation or alteration processes. To consider a specific example: the differences in the distribution of individuals in a present environment and one implied from fossil evidence relating to a similar kind can differ most dramatically. The Pleistocene cave bear (*Ursus spelaeus* Rosenmüller and Heinroth) is so named because over 99% of its remains were found in caves. They have been recovered in massive numbers from the sediments of cave lairs, representing many tens of thousands of individuals in some

Figure 3. Massive bone beds of the Drachenhöhle, in Styria, which extends for kilometers and contains the remains of tens of thousands of cave bears.

caves (Bednarik 1993b). For instance some 250 tons of cave bear bones were excavated from the Drachenhöhle in Austria alone (Fig. 3). Therefore the distribution pattern of these remains would suggest that the animal was a habitual cave dweller that normally died inside caves. Yet the species only sought out caves as winter hibernation sites during stadial periods, otherwise it spent no time at all in caves. It was a herbivore whose principal diet consisted of grass. Weakened or old bears often died during hibernation, but there can be no doubt that the vast majority of the cave bears died outside of caves. So why are their remains found almost exclusively in caves?

The answer lies in taphonomy: the probability of the skeletal remains surviving in cave sediments is thousands of times greater than that of surviving in the open. There are essentially two reasons for this: the sedimentary pH, which determines the survivability of osteal remains, is very high inside limestone caves; and they are not subjected to external weathering, but to very stable speleoclimatic conditions. Similar principles apply throughout archaeology, but are widely misunderstood by archaeologists. No data of the distribution of a phenomenon in the Pleistocene can be meaningful without considering such principles. These underlying axioms are not fully exploited by taphonomy itself, or only so for a very specific purpose: to determine what one had to consider to deduce aspects of a living system from the fossil aspects that survive from it. But the underlying principle, like the law of uniformitarianism, is much more profound, and can be defined more broadly, and universally applicable. It can be formulated thus: *the surviving and humanly detectable traces of a phenomenon of the past can define that phenomenon only to the extent that adequate recourse to a specific form of logic has been applied*; it is called 'taphonomic logic' (Bednarik 1990–91, 1994). It applies to any phenomenon or event of the past, be it astronomical, sedimentary, geological, paleontological, palynological or archaeological. This form of logic is quantifiable (at least as an integral function; Bednarik 1994: 73) and is not a hypothesis presented for testing, but a theorem facilitating the assessment of past conditions that cannot be observed directly. This has the most profound effects on our ability to interpret what we regard as archaeological evidence.

It is unfortunately the case that, so far, archaeologists have not understood the relevance of this epistemological tool to their discipline. Contrary to their common perception, taphonomy does not inherently deal with osteal remains (bones and teeth). It could be seen as pure coincidence that the underlying principle was first identified in paleontology. In essence, taphonomy deals with the logic underpinning the idea that the quantified characteristics of a record of past events or systems are not an accurate reflection of what would have been a record of the live system or observed event. It follows that to gain any level of valid understanding of the 'live system' (in the broadest and most inclusive sense) one has to explore the processes that led to the extant traces.

The probably greatest single epistemological encumbrance of archaeology as it has been conducted is the tendency of treating 'empirical evidence' as representing a random sample—as if it amounted to a representative selection of variables defining the entity being explored. The concept of 'random sample' is taken from the practices of the hard sciences, where it is crucial that it is in fact representative. In archaeology, however, it is impossible to secure samples of culture that can be representative of any condition: each site deposit, and each part of each site, is unique. Representativeness is manufactured by the archaeologist, who arranges series of objects arbitrarily and creates a taxonomy from them, then attributing them to 'cultures'. To illustrate how this process can lead to absurd systems, we may consider its application to rock art. Major rock art sites are almost always cumulative assemblages in generally two-dimensional space (ignoring here the possibility of detecting nano-stratigraphies, a technique introduced in the 1970s; Bednarik 1979). The scientific dating of these sequences remains extremely difficult (Bednarik 2002a). So we have single sites or rock panels bearing the artistic precipitate of different periods, perhaps different cultures (we will consider this issue in more detail in the next lecture).

Obviously this can only lead to falsities, and there are countless examples in archaeology where this has taken place. In epistemological parlance, there is a dependency relation called a supervenience: one set of properties (forming a historical event) is supervenient on a second set (represented in the selected sample). The relationship between the two sets cannot therefore be explored by traditional deductive reasoning. However, even if one made allowances for the purely taphonomic issues (the enormous variations in the survival rates of different classes of evidence), the disparities would not be solved (see below). These variations are much greater than most practitioners realize. Of all the events that occurred during the archaeological past, no evidence of any kind survived for more than a second in 99.999% of all cases. Of the still innumerable remaining instances, evidence survives to this day only in a tiny fraction of one-millionth of a percent. Of this remaining 'sample', only an infinitesimal portion can reasonably be assumed to have been recovered, of which an even smaller part has been correctly interpreted. This introduces an even more profound issue: not only do we need to understand the systematic biases of preservation, we also need to consider those of recovery and interpretation.

The full complexity of the issue is perhaps best illustrated by example. We could consider the many factors that contribute to the relative over-representation of, say, gold objects in the archaeological record. Apart from the obvious advantage in preservation of a noble metal, gold objects are far more likely to be collected, noticed, salvaged, recorded or found with detectors than other remains. Moreover, they are more likely to occur in select places—tumuli, shipwrecks, pyramids or hoards—especially likely to attract the interest of archaeologists, who may well prefer not to dig in places without promise. Even the preoccupations of archaeologists become issues resembling taphonomic factors, and are decisive in determining what we innocently call the 'archaeological record'. Once found, a gold object is more likely to be mentioned in a publication than, say, a bone object. Thus the observation that there are x times more bone objects than gold objects in the 'archaeological record', without further qualification, is meaningless. Even a snowman made by a Neanderthal could theoretically have survived, while many gold objects have been destroyed. Therefore probability of survival can never be nil, nor can it be 100%. Or, to use the language of taphonomic logic, gold objects have an extremely short *taphonomic lag time* (effectively the time span between a phenomenon's introduction and its first common appearance on the available 'archaeological record'), snowmen have an extremely long one. The point in time separating the taphonomic lag from the period from which specimens occur in good numbers is called the taphonomic threshold, which must lie somewhere between the phenomenon's first appearance and the present, but can never coincide with either. The importance of this is that, for the vast majority of phenomenon categories, such as objects of leather, cordage, bark and so forth, the lag time tends to account for over 99% of the duration of their existence in the past. On the other hand, for most phenomenon categories it is perfectly possible for highly isolated instances to occur beyond the threshold. Most archaeological misinterpretation of the past is relatable to a lack of appreciation of these factors, which inevitably leads to minimalist assumptions and endemic under-rating of the societies concerned: perceived absence of evidence is interpreted as evidence of absence, and isolated specimens from the taphonomic lag time are sometimes explained away as "running ahead of time" (Vishnyatski 1999), but are more commonly rejected as flukes, as the result of faulty stratigraphy and so forth. Therein lies the explanation of the huge gap of credibility between dominant paradigms of Pleistocene archaeology

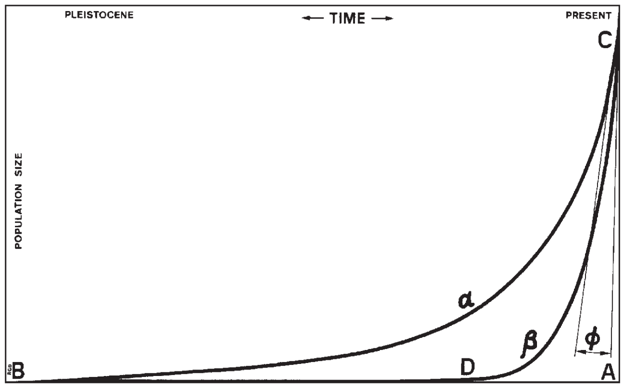

Figure 4. Principles of the relationship of total production of an archaeological phenomenon s_α *to its surviving instances* s_β *as a function of angle* φ. *These principles are the basis of taphonomic logic.*

and the models demanded by other disciplines, by rational thought and by plain common sense. Typically, the discipline under-interprets significantly the technological, cognitive, intellectual and cultural levels of all hominins of the Pleistocene, and this is the reason for it.

Metamorphology: an alternative

The observation that a 'taphonomic logic'-style of discourse needs to be applied to various factors other than taphonomy proper expands the scope of this discussion considerably. The solution is the introduction of metamorphology as the scientific version of archaeology. It is a logic-based, refutable system of reviewing archaeological information that determines whether archaeological propositions could have scientific legitimacy. It is developed especially from taphonomic logic, which as we have seen hinges on the concept of cumulative data loss as a function of time (the principle is depicted graphically in Fig. 4). It replaces inductive uniformitarianism, hitherto the de-facto basis (Cameron 1993), as a unified theory, of archaeology.

Metamorphology (Bednarik 1995, 2006) is the science of how forms of evidence of events in the past become the forms as which they are perceived or understood by the individual researcher today. In accounting for the considerable gap that exists between the reality of what actually happened at some point of time in the distant past, and the abstraction of it as it is perceived by, for example, an archaeologist, it is crucial to focus on the individual. The discipline is not a quasi-democratic reflection of the view of all, its paradigm is based on the authority of a political hegemony. Metamorphology obviously has to take into consideration myriad factors and it cannot be expected to provide precise interpretations; but it somehow needs to determine how the individual interpreter of the past arrives at his or her pronouncements. Knowledge in archaeology is not some mysterious collective unconscious to which practitioners are somehow connected; it is individual knowledge of individual practitioners, limited by many factors. Like refutation in general, metamorphology provides us with models of what is unlikely to be valid, and so strengthens archaeology by weakening its dogmas. It also rejects the concept of an 'archaeological record', or a collective knowledge of the discipline.

The most obvious of these factors accounting for metamorphology, and for the gap between archaeologists' constructs and what really happened in the past, is taphonomy. It distorts archaeological evidence systematically, and it does so in forms that have often not been appreciated adequately. Indeed, after the paleontological concept of taphonomy was introduced into archaeology over forty years after its 1940 inception, it was soon misunderstood and effectively became actuopaleontology—which ironically taphonomy was originally intended to replace (see Soloman 1990 for a superb discussion). Hence the potential of taphonomy itself has remained significantly under-utilized in archaeology in more ways than one. But if the inherent principles are extended to the methods of recovery of evidence; those of its interpretation; those of its reporting and selective dissemination; those of its statistical treatment; or to the individual researcher's own biases and limitations, such as limitations of knowledge or language; and to a variety of other factors, it becomes apparent that these also tend to be systematic. These factors may include the priorities of research traditions, of individual leaders in the discipline, of specific institutions, of funding agencies or of society as a whole. In the previous lectures we have already met some of these factors of bias. There can be no doubt that there is a very considerable gap between the reality of what happened in the distant past, and the abstraction of it as perceived by the individual archaeologist interpreting a specific, subjectively selected and non-random sample of the remaining evidence. This is already obvious from the observation that there is no universal world archaeology; there are countless archaeologies (see Lecture 3). To account for this gap of incommensurability, to decide what the distorting factors are and what their respective effects and interplay might be, a separate sub-discipline is required that addresses the epistemological basis of archaeology. Taphonomy itself is not the whole answer, because it accounts for only *some* of these truncating and modifying factors.

For metamorphology to be scientific, its propositions must be refutable. It is logic based and draws heavily on knowledge of taphonomic processes, and on a variety of other falsifiable observations. A unified theory of metamorphology has been formulated and published, at least in embryonic form (Bednarik 1995, 2006). It has been shown that metamorphological quantification, although extremely difficult, should be possible, at least in general or abstract forms (e.g. as integral functions). It extends the underlying principle of taphonomic logic (that scientific access to the human past is contingent on the coherent identification of *that part of the extant characteristics of the evidence that is not the result of taphonomic processes*; Bednarik 1990–91) to all aspects of archaeological interpretation. These include the way data are *collected, stored, interpreted and disseminated*. They include the biases of the individual researcher (cognitive, religious, ontological, academic, intellectual), of specific schools or the discipline as a whole, and many other external factors that have a bearing on how the so-called evidence is individually perceived, reported and interpreted. For instance, the researcher's own limitations are a powerful factor in how evidence may be reported. These may be limitations of knowledge or of language. Ignorance of researchers concerning existing data, language barriers, and biases through preconceived models has not only severely influenced hypotheses and their defense, it has also stifled the flow of information in palaeoart studies and archaeology (e.g. Bednarik 1992, 1995b, 1995c, 1999). It is certainly a quantifiable factor. The academic system itself, which is so crucial to the dissemination of knowledge, can also stifle that very process and act as a filter in quite a number of ways. All of this can cumulatively add up to such distortions in dominant models that these bear little resemblance to what historically happened in the past. This is because many of the distortions are not random; *they are systematic*.

To correct this we need to be able to understand the nature and effects of these distortions, be they taphonomic or related to other epistemic encumbrances. This would provide the kind of framework we require to account for the gap between what happened in the distant past, and the abstraction or reified construct of it as it is perceived by the individual researcher interpreting a specific 'sample' of the remaining evidence of this event or connected events. For instance, we need to understand the effects of false hypotheses and of their ardent defense if we are to obtain a valid reflection of metamorphology. There is surely no reason why the dynamics of knowledge acquisition or academic power politics in the discipline should be immune from scholarly analysis. Archaeology, like anthropology, often does not hesitate to study the taboos of the societies it investigates, be they extant or extinct groups; so the study of itself should not be taboo. These are realities, they have significant effects on the discipline, and these dynamics need to be understood like any other process contributing to our knowledge. Therefore this aspect should be studied as carefully as any other that contributes to metamorphology. The discipline would be in a sorry state if such research would be discouraged because the 'reputation' or sensitivities of individuals are considered to have precedence over its integrity or veracity.

Why the dominant paradigm is wrong

All these considerations are, however, theoretical, and to probe their practical applications and implications we return to the specific example we began this subject with: the effects of Pleistocene sea-level changes. The self-evident corollary would be that, to secure a balanced picture of the cultures, technologies and genetics of Pleistocene humans, we would need to take into consideration that part of humanity that lived in the most fertile environments, i.e. those of low elevations. We can reasonably assume that it made up well over half of humanity, perhaps even far more than half, and that it was almost certainly more developed and more sedentary. But with the exception of a few unique glimpses, we know nothing about these people. Pleistocene archaeologists appear uniformly incapable of appreciating the effects of having studied nothing more than the remains of mobile inland tribes that followed the herds of the steppes, upper valleys and highlands. To

Figure 5. Artist's impression of a Homo erectus *man constructing a bamboo raft.*

imply that this is the complete story of Pleistocene people is severely misleading, and demonstrably false. It is precisely the reason why most Pleistocene archaeologists find it hard to explain how Middle Paleolithic seafarers could have been capable of settling Australia, traveling many days across the open sea to do so (Bednarik and Kuckenburg 1999). Much earlier, almost a million years ago, their Lower Paleolithic ancestors, of *Homo erectus* stock (Fig. 5), reached the islands of Wallacea across the sea (Bednarik 1999, 2003a).

Most orthodox archaeologists have great difficulty accepting this, which indicates how severely their thinking has been conditioned by false models. To successfully colonize any landmass for the long term, a founding population of adequate genetic variability to result in a viable breeding group is absolutely essential (we have seen with the Flores 'Hobbit' and other insular populations what happens when a gene pool is too small). This demands that at least dozens, preferably hundreds of people, comprising a good number of fertile females, had to travel, implying considerable organizational and language ability (Fig. 6). We lack any knowledge of the coastal tribes of these hominins, but the only logical explanation for their many successful colonizations in Indonesia and the Mediterranean is that their technology was more advanced than that of the inland tribes. Orthodox archaeology assumes that permanent settlements were introduced with the 'Neolithic revolution', unaware that the earliest-known villages of stone huts date from the Acheulian, at least 200,000 years earlier (Ziegert 2007). But of course they could not be found in the coastal zones along seashores. They were located on a former giant inland lake in the Libyan Sahara, Lake Fezzan. That lake has since disappeared together with its aquifer, which is precisely why the remains could survive in these unusual circumstances. They allow a rare glimpse into the sophistication of a littoral people of the Lower Paleolithic, showing that complex villages of stone-walled huts, which arrived in inland regions only with the Holocene, have been in use for hundreds of millennia where permanent food sources permitted sedentary settlements. There is even evidence of islands in Fezzan Lake having been reached in the Acheulian, probably by reed rafts (Werry and Kazenwadel 1999). This evidence can explain many aspects: the early appearance of paleoart, of beads and pendants, of Lower Paleolithic

Figure 6. Replication experiment of Lower Paleolithic seafaring off the coast of Flores, Indonesia, conducted by the author on 13 April 2008.

seafaring, the ability of hominins to colonize regions of cold climates. It would, however, also imply that the ecology of the Paleolithic periods has been completely misinterpreted; that the orthodox Pleistocene paradigm is just a monumental distortion of history.

It may seem inconceivable that, after 150 years of research, such a state should still be possible, but when the many errors in the history of this discipline (some of which were listed in Lectures 1 and 4) are considered, the petulant question does arise: have these patterns of the past been left behind? On closer examination, evidence to the contrary does emerge. Consider, for instance, the intricate mythology archaeology has created around the Ice Age cave art of southwestern Europe: almost every key interpretive aspect of it appears to be false. Symboling did *not*, as claimed almost universally, commence with the advent of the Upper Paleolithic in Europe, but at least twenty times as long ago. Even the traditional Darwinist sequence of emerging symbolic capabilities needs to be discarded. Franco-Cantabrian cave art is *not* a form of rock art endemic to caves; its exclusive occurrence in deep limestone caves is almost certainly a taphonomic phenomenon. It was *not* created by shamans or great artists; much or most of it appears to be the work of children and teenagers (Bednarik 1986, 2002b, 2008; Guthrie 2005). Ethnographic evidence suggests that figurative art may by some societies be regarded as a 'juvenile' art form (Sreenathan et al. 2008). Contrary to a widely held view, figurative imagery is cognitively less developed than non-figurative. Whereas in figurative symbolism, the connection between referent and referrer is purely via iconicity—a relatively simple cognitive factor building on visual ambiguity and accessible even to animals other then humans—the symbolism of non-iconic art is only navigable by possessing the relevant cultural 'software'. Figurative art results from a deliberate creation of visual ambiguity (Bednarik 2003b: 408, 412) and is therefore based on lower levels of perception and neural disambiguation than non-figurative art.

If one adds to these considerations the orthodox misconceptions that European Paleolithic cave art consists mainly of zoomorphs (it does not; most of it is nonfigurative); or that zoomorphs mark Pleistocene art (less than 1% of the world's surviving Ice Age palaeoart is even figurative); or that all Pleistocene rock art is of the Upper Paleolithic (in fact its lion share is Middle Paleolithic rock art); or that the art of the early Upper Paleolithic (such as that of the Châtelperronian and Aurignacian) is the work of anatomically fully modern humans (it appears to be made by Neanderthaloid people), one begins to appreciate the depth of the issue. Practically all widely held beliefs about Pleistocene palaeoart are either false, or are very probably false.

Having shown how many challenges a single taphonomic factor, sea-level fluctuation, provides to the mainstream, orthodox paradigm of Pleistocene human history, I need to emphasize that these challenges can be multiplied hundreds of times, through hundreds of similar factors. This was presented as just one of many examples illustrating the effects of metamorphology: taphonomic logic, combined with a rigorous review of the practices of interpretation and dissemination, can and must be applied to all archaeological claims about Pleistocene humanity. This, put very simply, is the basis of a sound epistemology of Pleistocene archaeology.

REFERENCES

Bahn, P. (ed.) 1992. *Collins dictionary of archaeology*. HarperCollins Publishers, Glasgow.

Bednarik, R. G. 1979. The potential of rock patination analysis in Australian archaeology — part 1. *The Artefact* 4: 14–38.

Bednarik, R. G. 1986. Parietal finger markings in Europe and Australia. *Rock Art Research* 3: 30–61, 159–170.

Bednarik, R. G. 1990–91. Epistemology in palaeoart studies. *Origini* 15: 57–78.

Bednarik, R. G. 1992. The stuff legends in archaeology are made of: a reply to critics. *Cambridge Archaeological Journal* 2(2): 262–265.

Bednarik, R. G. 1993a. Refutability and taphonomy: touchstones of palaeoart studies. *Rock Art Research* 10: 11–13.

Bednarik, R. G. 1993b. Wall markings of the cave bear. *Studies in Speleology* 9: 51–70.

Bednarik, R. G. 1994. A taphonomy of palaeoart. *Antiquity* 68(258): 68–74. See also *http://mc2.vicnet.net.au/home/epistem/shared_files/antiquity94.pdf*

Bednarik, R. G. 1995. Metamorphology: in lieu of uniformitarianism. *Oxford Journal of Archaeology* 14(2): 117–122.

Bednarik, R. G. 1999. Maritime navigation in the Lower and Middle Palaeolithic. *Comptes Rendus de l'Académie des Sciences Paris, Earth and Planetary Sciences* 328: 559–563.

Bednarik, R. G. 2002a. The dating of rock art: a critique. *Journal of Archaeological Science* 29(11): 1213–1233.

Bednarik, R. G. 2002b. Paläolithische Felskunst in Deutschland? *Archäologische Informationen* 25(1–2): 107–117.

Bednarik, R. G. 2003a. Seafaring in the Pleistocene. *Cambridge Archaeological Journal* 13(1): 41–66.

Bednarik, R. G. 2003b. A figurine from the African Acheulian. *Current Anthropology* 44(3): 405–413.

Bednarik, R. G. 2006. A unified theory for palaeoart studies. *Rock Art Research* 23: 85–88.

Bednarik, R. G. 2008b. Children as Pleistocene artists. *Rock Art Research* 25 (in press, November issue).

Bednarik, R. G. and M. Kuckenburg 1999. *Nale Tasih: Eine Floßfahrt in die Steinzeit*. Thorbecke, Stuttgart.

Behrensmeyer, A. K. 1975. The taphonomy and paleoecology of Plio-Pleistocene vertebrate assemblages east of Lake Rudolf, Kenya. *Harvard University Museum and Comparative Zoology Bulletin* 146: 473–578.

Behrensmeyer, A. K. 1978. Taphonomic and ecologic information from bone weathering. *Paleobiology* 4: 150–162.

Binford, L. R. 1981. *Bones: ancient men and modern myths*. New York.

Brain, C. K. 1981. *The hunters or the hunted? An introduction to African cave taphonomy*. Chicago.

Cameron, D. W. 1993. The archaeology of Upper Palaeolithic art: aspects of uniformitarianism. *Rock Art Research* 10: 3–17.

Efremov, J. A. 1940. Taphonomy: a new branch of paleontology. *Pan American Geologist* 74(2): 81–93.

Gifford, D. P. 1981. Taphonomy and paleoecology: a critical review of archaeology's sister disciplines. In M. A. Schiffer (ed.), *Advances in archaeological method and theory*, Vol. 4, pp. 365–438. Academic Press, New York.

Guthrie, R. 2005. *The nature of Paleolithic art*. The University of Chicago Press, Chicago/London.

Hill, A. 1976. On carnivore and weathering damage to bone. *Current Anthropology* 17(2): 335–336.

Hill, A. 1979. Butchery and natural disarticulation: an investigatory technique. *American Antiquity* 44: 739–744.

Hiscock, P. 1985. The need for a taphonomic perspective in stone artefact analysis. *Queensland Archaeological Research* 2: 82–97.

Hiscock, P. 1990. A study in scarlet: taphonomy of inorganic artefacts. In S. Solomon, I. Davidson and D. Watson (eds), *Problem solving in taphonomy: archaeological and palaeontological studies from Europe, Africa and Oceania*, pp. 34–49. Tempus, Archaeology and Material Culture Studies in Anthropology, Vol. 2, University of Queensland, St. Lucia.

Solomon, S. 1990. What is this thing called taphonomy? In S. Solomon, I. Davidson and D. Watson (eds), *Problem solving in taphonomy: archaeological and palaeontological studies from Europe, Africa and Oceania*, pp. 25–33. Tempus, Archaeology and Material Culture Studies in Anthropology, Vol. 2, University of Queensland, St. Lucia

Sreenathan, M., V. R. Rao and R. G. Bednarik 2008. Palaeolithic cognitive inheritance in aesthetic behavior of the Jarawas of the Andaman Islands. *Anthropos* 103 (in press).

Vishnyatsky, L. B. 1994. 'Running ahead of time' in the development of Palaeolithic industries. *Antiquity* 68: 134–140.

Werry, E. and B. Kazenwadel 1999. Garten Eden in der Sahara. *Bild der Wissenschaft* 4/1999: 18–23.

Ziegert, H. 2007. A new dawn for humanity: Lower Palaeolithic village life in Libya and Ethiopia. *Minerva* 18(4): 8–9.