

There's no Art – To Find the Mind's Construction – In Offence

Iain Davidson 1992

The editor invited a comment on this 'attack' on Davidson and Noble (1989), but asked me 'not merely to attack points of detail'. Any careful reading of Bednarik's article and comparison with the works it attacks would show that in almost every detail it lays itself open to such attack. Nevertheless I am grateful to the editor for the opportunity to set out some of the argument that Noble and I have been trying to develop, and that so clearly Bednarik has not understood. There are good reasons why it has not been understood: it is clear from every phrase of his article that our objectives are utterly different.

There's no art

Bednarik's article is full of the sorts of 'speculations, *non sequiturs*, idiosyncracies, distortions and biases' of which it accuses others. He points out that there is no agreement on 'how one would define art or distinguish it from non-art' yet offers no definition of art, and clearly ignores some important definitions that are present in the articles it attacks. Davidson & Noble (1989, 136) offered a definition by example:

We use the word 'art' here to include all things generally lumped together as Palaeolithic art (e.g. Ucko & Rosenfeld 1968; Marshack 1972). No assumptions are made about its relation to aesthetics. Any association between aesthetics and symbolism is culturally determined. For prehistoric circumstances we cannot guess at the nature of such associations, much less predicate an argument upon such guesses.

Bednarik might have taken this as a clue that we are not concerned with the beginnings of art (however that may be defined). Rather, we have been developing arguments about the process of evolutionary emergence of language. By the same token, it seems reasonable to suppose that Chase & Dibble (1987) recognize that there is something more fundamental here than the 'origin of art', and that is why they wrote about the quality of claims for Middle Palaeolithic *symbolism*.

Davidson & Noble (1989, 128) stated that they would avoid 'the term "art" at this point because it assumes a present-day perception of what was done that has no warrant.' The only way that 'art' can easily be defined is by a description of the class of objects that are being recognized. There is one, for example, in Davidson (1989a, 441):

Recognize as 'art' a whole series of objects, including cave walls, which have intentional marks or modifications on them. Some of these objects may be functional, and in this case the marks or modifications which constitute the 'art' do not form a necessary part of the functional form of the object. Other objects do not seem to have any function other than as supports for the intentional marks or modifications. Ignore, for the moment, the importance of depiction as a function in itself.

The difficult part of this definition was in recognizing the intentionality of the mark-making. Attribution of intention is a complex issue among humans (see for example Greenfield 1980) and much more so for humans interpreting the behaviour of animals. Whether any animals can attribute states of mind to other animals is unlikely (Cheney & Seyfarth 1990, ch. 8; Povinelli et al. 1991), and certainly a question that requires very precise methods for its investigation (e.g. Savage-Rumbaugh 1986). But the archaeologist (human) is engaged in attributing states of mind to prehistoric animals, that may or may not have been human, on the basis of the material evidence they left behind. Just how difficult this may be, and how meticulously it must be done, is revealed by the various studies in which McGrew has compared the materials which chimpanzees produce with the products of modern or prehistoric people (e.g. McGrew, Tutin & Baldwin 1979; McGrew 1987; 1990; Wynn & McGrew 1989). Some of the striking convergences could be taken to show just how like humans chimpanzees are, or they might be taken to show just how ape-like some hominid tools are (e.g. Wynn & McGrew 1989; Davidson & Noble, in press). But we stop short of the second type of conclusion when the comparison is with modern people. Analysis of one such case suggests that some of the claims for similarity derive primarily from the method used to analyze the tools (Noble & Davidson 1991, 240-2).

Our argument about language emergence concerns the recognition of the meaningfulness of the meaningful utterances in the hominid communication system that became language (e.g. Davidson & Noble 1989; in press; Noble & Davidson 1989; 1991; in press; Davidson 1989b; 1990; 1991a). We suggest that this was achieved through something like mark-making. This was necessary in order to displace the act of uttering from its context and make it apparent, hence permit reflection on otherwise transitory utterances. Once that was achieved, the permanent marks were no longer necessary, and the communication system could continue as a set of transitory utterances. Students committed to the study of 'art' lack a methodology for distinguishing such marks from the necessarily

intentional marks that could reasonably be called art (Noble & Davidson, in press). Bednarik's article is evidence that the problem is much more fundamental than a lack of method. By its lack of the very definition it demands, it suggests a lack of reflection on the concepts involved in recognizing how anything might be called art.

Language and art are related. There are vernacular definitions of art: 'The application of skill to subjects of taste, as poetry, music, etc.' (*Shorter Oxford English Dictionary*); 'the production or expression of what is beautiful (esp. visually), appealing, or of more than ordinary significance' (*Concise Macquarie Dictionary*); or the 'application of skill to production of beauty (esp. visual beauty) and works of creative imagination, as in the fine arts' (*Chambers Twentieth Century Dictionary*). All of these definitions involve concepts that involve values or evaluations that result from the use of language: skill, taste, beauty, appeal, significance, creativity, imagination. The issue for prehistorians, of course, is whether the creations that they study can be said by any relevant criteria to fit any of these definitions. Without such criteria, and without any understanding of the issues involved in the identification of the evolutionary emergence of language, there can be no advance in understanding the 'origin of art'.

To find the mind's construction

Our scenario for the first appearance(s) of language involves a communication system that included mimetic gestures, and the accidental or incidental 'freezing' of such gestures in a more permanent medium. Mimesis may involve some measure of iconicity, but we do not, cannot, and would not define iconicity by the canons of our own conventional perceptions. Yet Bednarik attacks us, and presumably others, for using ethnocentric concepts of iconicity. The basis for this attack results only from the sole definition he gives, which is an astringently Eurocentric definition of iconicity. Because he is expressing ethnocentric views, he can only assume that others are.

There is a similar claim about communication. We are very well aware of the many means by which communication takes place among organisms, and have acknowledged this in our articles (e.g. Noble & Davidson 1991, 225). Davidson (1991a, 44) stated that 'The animate world is replete with communication. Almost all of the senses of one creature or another guide them, their predators or their prey to determine their friendly and unfriendly interactions.' Noble (1987, 141) once included 'cries, sweating, flatus, tics, changes

of bodily colour', and we have consistently followed Mead (1934) in referring to such actions as gestures when they convey meanings (Davidson & Noble 1989, 130-1). We have argued at length that only among humans with language is there any analysis of the communicative acts so as to recognize not only the meaning being communicated but also the fact that it is communication that is going on. In this we follow, amongst others, Harré & Secord (1972, 84-5) who note that 'people, alone among animals, possess the power to monitor their monitorings.' Our view of communication and its subtleties is, deliberately and appropriately, anthropocentric for these reasons. While that view may not be 'generally shared', we seem to be in good company, and hope that it may become more widely shared if people will take the trouble to monitor our monitorings!

There are, of course, no such things as communication 'systems ... [that] simply cannot be detected.' If they cannot be detected, how can there be any communication going on, and if all that is meant is that humans cannot detect them, how can humans recognize that any communication is going on? This is mumbo-jumbo.

Bednarik brandishes the label 'science' (and its derivatives), without definition, in a way that can only be interpreted as 'Science is something I find in the writings that I like but not in those I do not.' Our complex argument that memory was *in a sense* created by the mimetic mark-making is presented in a context of conclusions by Vygotsky (1930), Leroi-Gourhan (1964-65) and Gibson (1966). This is said to be 'hardly acceptable to scientists'. Is this because we or they are not scientists in the author's view? No 'acceptable' scientists are cited in support of the criticism, so it is difficult to judge. The conventions of scholarship within which the author's argument is to be judged would normally require such citation.

In offence

It should not be necessary to remind readers, each time culture is discussed, that Kroeber & Kluckhohn (1954) collected more than 150 definitions of culture (there are presumably many more by now). How many of these 'scientific definitions of culture' would Bednarik accept, and would the, by no means eccentric, definition by Geertz (1964) quoted by Noble & Davidson (1991) be acceptable? How does Bednarik's statement about what culture is, relate to 'the imposition ... of symbolic meaning on reality'? And what is the distinction being made that makes culture incapable of being 'anthropocentric'? Almost all of the definitions collected by Kroeber & Kluckhohn start from the

assumption that culture is initially defined as a property of humans. Even the most generous students of animal behaviour recognize that, in any attempt to show a similarity between animal behaviour and cultural behaviour, something needs to be demonstrated by evidence and argument (e.g. McGrew & Tutin 1978). What it seems is being suggested in such animal studies is that some part of the phenomenon understood to be cultural can be found in creatures not generally considered cultural. It is egregious to suppose, on the grounds that some animals are more cultural than we expected, that the concept of culture is not to be used anthropocentrically.

If it is really necessary to have seen 'a significant part of the corpus first hand' why publish anything? In the case of the European female figurines older than 20,000 years, Davidson & Noble (1989, 136) draw attention to the lack of faces and feet on figures that are not male. The general lack of faces, which would be sufficient to establish that there was a convention, is confirmed by Gvozdover (1989b, 42). There are, of course, a few exceptions, such as the faces on a figure from Dolni Vestonice (Marshack 1972, 298) or on Avdevo 77-1 (Gvozdover 1989a, 23), but not many. We have acknowledged throughout our writings on language emergence that there is a necessary feature of any evolving system, whether biological or cultural. This is the occurrence by accident (in any system) or design (only in cultural systems) of minor variation which may, or may not, become selected naturally or culturally as the system evolves. Our choice of stylistic features differs from that of Gamble (1982). Gvozdover (1989a; b) has shown that the stylistic conventions of some of the figures are susceptible to much more detailed analysis. There are certainly minor exceptions that emerge in each of these analyses but they do not contradict the recognition of general conventions.

Finally, what purpose is served by a list of 'specimens and phenomena' that is deliberately incomplete (why?) and which deliberately includes some 'specious' examples (though we are not told which)? How many of these has the author of the article seen? Does that matter? His attempt to ridicule my inspection of the fox canine from La Quina suggests he has not himself seen it: part of this object is only available for inspection through glass because the original excavator attached it to a glass slide. No one has since been able to examine this specimen any more closely! Bednarik also questions the listing of a bone fragment from Morin (Davidson & Noble 1989, table 2), without reference to the publication cited, where the piece refuted by Marshack does not figure. But what are we to make of this claim, since the published figures in Bednarik's article include, among other

rejected pieces (Figure 5: Davidson 1990; Figure 8: D'Errico 1991), a drawing (Figure 7) of the renowned Pech de l'Azé rib, refuted, on the same taphonomic grounds, by the same author (Marshack, pers. comm.; see Davidson 1990).

This list, spread over several hundred thousand years and several continents, is the most eloquent support for my statement that 'non-depictive marks on objects earlier than the Upper Palaeolithic will not be found in repeated patterns, restricted in time and distribution, from which we might infer a convention or code through which meaning could be recognised' (Davidson 1990). This is a hypothesis that could be refuted. Lindly & Clark (1990), cited in different terms by Bednarik, find 'no archaeologically discernible symbolic component' for 'pre-Upper Palaeolithic morphologically modern humans.' Similarly, Chase (1991) concluded that 'So far, no one has been able to demonstrate the presence of adjunct style before the Upper Palaeolithic.' (Again we seem to be in good company.) In using the term Upper Palaeolithic, I would restrict this hypothesis to the area of distribution of the Upper Palaeolithic: Europe, the Mediterranean and western Asia. But we have always acknowledged that the first colonization of Australia represents the earliest known evidence for the emergence of language and the abilities it allows (Davidson & Noble 1989, 137; see Davidson & Noble 1992). There is, inevitably, a logical entailment that the place of emergence must be outside Australia, but, we contend, there is no known evidence that represents it.

There is much more in Bednarik's article that is inaccurate, incorrect or deliberate distortion. I am sure that an unbiased reading of the various articles in which Noble and I have attempted to work out this argument would convince most people that we have tried to avoid speculations wherever possible; are no more prone to *non sequiturs* than any other scholars; are idiosyncratic deliberately and precisely because we are not satisfied with the complacent acceptance of 'normal science' approaches to cognition or to the interpretation of the archaeological record (Kuhn 1962; Davidson 1991b); have not knowingly distorted our presentation of the published material or our first-hand observations of objects; and are aware of the difference between bias and adherence to a paradigm, between prejudice and adopting a particular set of principles that allow fresh insights in the interpretation of the archaeological record.

Chasing the signs of language

The work of Davidson and Noble has been summarized elsewhere (Davidson 1991a), but in the light of this

empiricist attack on individual claims in a couple of our articles it is worth clarifying what our exploration has been about. We began, from our separate perspectives - psychology of perception (Noble) and prehistoric archaeology (Davidson) - to explore the theoretical issues involved in the emergence of language (Davidson & Noble 1989; Noble & Davidson 1989). This emergence must have occurred during the course of hominid and human evolution. This is why it is necessary to be sceptical of all the early evidence: we cannot assume that the hominids responsible for it had, or exercised, the same abilities as fully modern humans. There is general agreement that the material from the Upper Palaeolithic and later is consistent with modern humans who had language, but scepticism is warranted for all earlier claims. Wynn & McGrew (1989) produced a persuasive argument that the tools of the Oldowan could have been produced with the abilities documented for chimpanzee tools. Thus all claims for abilities that can only be found among present-day humans need to be demonstrated and cannot be assumed. This scepticism can lead to the sort of clash of objectives demonstrated by Bednarik's article.

Language, by any definition, is a system of symbolic communication capable of referring to things in the absence of those referents. There is no necessary relationship between the symbol (word) and the referent - this is the notion of arbitrariness. But there is a convention about the relationship between sign and signified, so that a community of language users all understand the same thing (or nearly the same thing) by the sign. Arbitrariness defined by convention allows the possibility, and is allowed by the possibility, of recognizing that meaning is a property of utterances.

The use of language generally among members of the human species, together with the related perceptual abilities of modern humans, appears to distinguish humans from other modern higher primates (Davidson & Noble 1989). If it is correct to suppose that we share a common ancestor with chimpanzees, bonobos and gorillas (and we do not see any reason to doubt that), then such abilities must have emerged during that evolutionary process, and their products might be observable in the archaeological record.

Two problems immediately present themselves. The first is to define the effects of the emergence of language on human consciousness and perception; the second is to consider the manner in which inferences about the materials from the past can be interpreted in the light of our knowledge of the present. Language probably transformed human perception and awareness as thoroughly as literacy later did in the

argument of Olson (1986) and others. Human perception is mediated by language (Noble 1987; in press), and this permits reflectivity on past and future events. What, we asked, would be the sign that this transformation had taken place in the evolution of hominids? The first sign is the presence of objects that necessarily required of their makers an anticipation of unperceived events. Such a sign is given by the first evidence for the use of sea-going boats implied by the first colonization of Australia.

The only other sign is the appearance of material items indicative of the use of symbols; and symbols must be defined in terms of the use of convention in the manufacture of items where the resulting shape could have been arbitrarily chosen. Mellars (1989) refers to artefacts with imposed form. Chase (1991) has dealt with such issues in detail, arguing, in a manner showing astonishing similarity to much of the ground that we have covered, that style and standardization are two attributes of artefacts in which symbols *might* be recognized. He differs from us on one important point. We do not think it is necessary that artefacts themselves were symbols for the makers to have had a use of symbols. He defines the possibility that standardized types could be produced as a result of constraints of physical function, of technology, or of raw material (Davidson 1991a; Davidson & Noble, in press). They could also, in Chase's opinion, result from concepts of ideal types for physical function or technology. But, we argue, such *conceptualization* cannot be shown for any other animal than a language-using one, and, we propose, conceptualization results from the use of symbolic utterance. Thus, in our treatment of the early stone industries (Davidson & Noble, in press), we recognize the same points about the physical, technological and raw material constraints, but do not admit the conceptual ones. If we could show the existence of such ideal concepts we would have to admit the existence of language.

We have also addressed the problem of the mechanisms involved in the emergence of language (Noble & Davidson 1991). Some notable recent discussions pay no attention to such mechanisms (Cavalli-Sforza *et al.* 1988; Renfrew 1991), but we have found that, when explanatory mechanisms have been proposed, the majority have fatal flaws. There are two reasons for this. First, they have not paid sufficient attention to the interpretation of the archaeological record, and secondly, they have not developed appropriate theory (cf. Chase 1991). We have attempted to improve matters in both regards. Like all other attempts, ours is not immune to criticism, but Bednarik's article does not show any understanding of the issues with which we have attempted to deal.

Because of this, its scatter of criticisms does not increase our understanding of anything.

Conclusion

This exchange illustrates one of the most important features that emerged with language: the clash of understandings that arises from the use of communicative means in different conventions. It is clear that the convention of understanding of the past that is the starting point for Bednarik is fundamentally opposed to the intent and practice of the exploration that Noble and I have been engaged upon. Under these circumstances there is no surprise that our views of the past are different; but that is no excuse for inaccurate reporting and misrepresentation.

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