

## The Stuff Legends in Archaeology are Made of: A Reply to Critics

Robert G. Bednarik

Chase and Dibble, replying to my article in the last issue of the *Cambridge Archaeological Journal*, deserve full marks for scanning it so carefully for any hint that I might have included a non-refutable claim (Chase & Dibble 1992). They did question the refutability of my suggestion that some of the parietal art attributed to the Upper Palaeolithic may actually predate it, but the original statement (Bednarik 1986) reads: 'there is no proof for this or any reliable earlier dating, and no evidence to exclude the possibility of assigning the early *montmilch* markings to the Eem interglacial'; a carefully worded and perfectly refutable claim. Let me at least express my appreciation for a highly constructive commentary, especially where Chase and Dibble discuss taphonomy, refutation, and perforated and grooved artefacts. These highlights render the debate most worthwhile.

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Chase and Dibble's separation of archaeology into scientific and humanistic branches may well explain the 'profound gulf' Davidson notes in his reply to my article (Davidson 1992), and his observation that the 'convention of understanding the past' which is my starting point is fundamentally opposed to his own. In considering language (or rather, what he defines as 'reflective language', a term criticized by Black 1989) as central to human development, Davidson ignores Dibble's (1989) thoughtful arguments against perceiving 'language' as the ultimate 'prime mover' in the origin of hominids. In addition, Graves (1989) has observed that most of the radical changes in hominid morphology took place before 100,000 years BP.

Davidson embraces a humanistic approach which presupposes that communication only takes place when humans manage to detect it. It is, however, as unscientific to apply such self-referential and entirely '*sapiens*-centric' definitions to Neanderthals as it would be to apply similarly conceived concepts of what is communication to other animals, be they primates or insects. Davidson claims that much of my article is inaccurate, incorrect and distorted, and I understand only too well why he thinks so: I am trying to ease his humanist theory into a refutationist frame, with predictable consequences.

Chase & Dibble, in questioning the validity of a taphonomic explanation of the paucity of available evidence for symbolism prior to the Upper Palaeolithic,

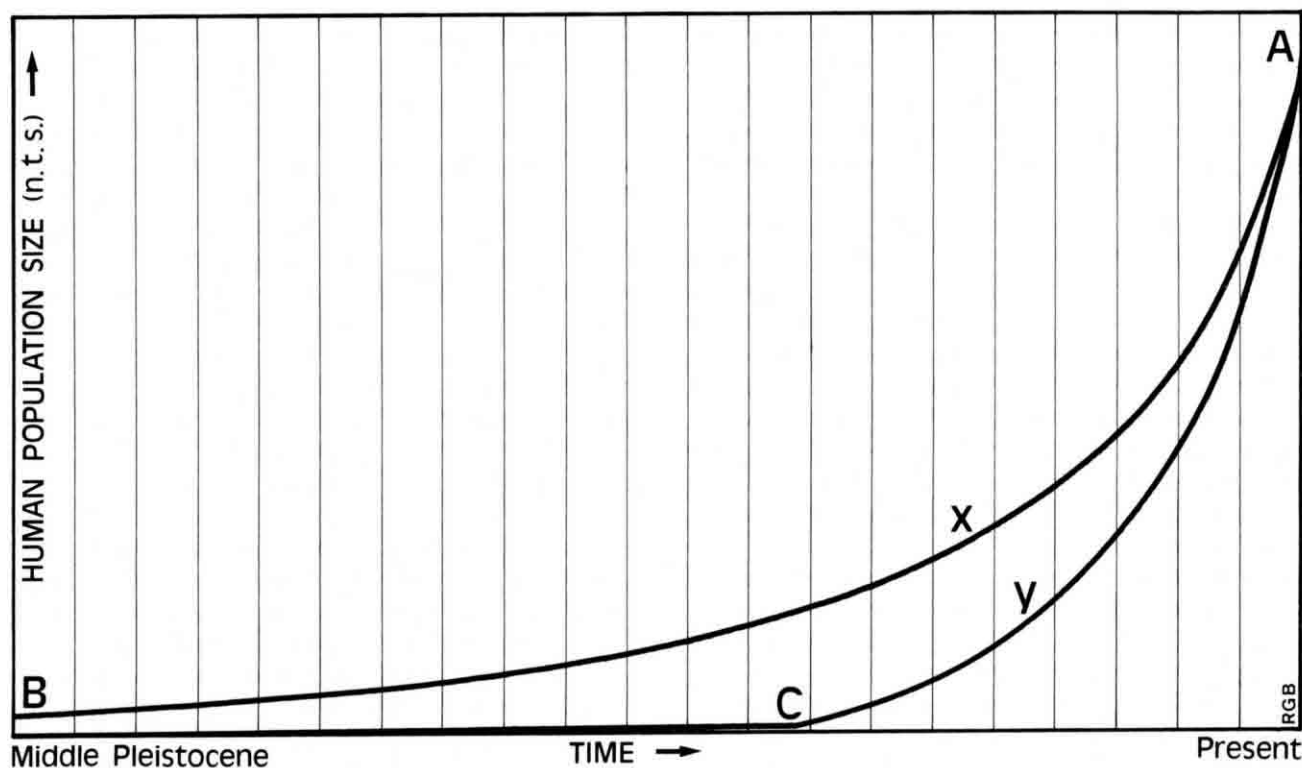
rely entirely on the argument of *scale*. Not only is this invalid logically (some relevant considerations are presented by the authors themselves), it also shows that they are willing to consider scale as being relevant in one context, but not in another. The argument that most evidence remains below ground level, while probably true, provides absolutely no excuse for ignoring most of the evidence that has been presented by previous writers. I have approached these questions from a different direction, and have arrived at views that are often at variance with existing dogma. For instance, I would ask how much evidence would we actually have of symbolism from the western European Upper Palaeolithic if artists had not transferred some mark-producing activities into limestone caves? We would have no rock art at all, because the very few open-air sites we have found recently (Bahn 1985) would never have been accepted. How much evidence would we have of portable art if there had not been a technological shift in the favoured artefact material towards ivory, bone and antler at about the Göttweig interstadial? Very little, if indeed any, in view of the attitudes of some archaeologists to perceived 'uniqueness'. How many of the thousands of Pleistocene petroglyphs in the Olary region of Australia would have survived on the dolomitic siltstone pavements had they not been protected by a veneer of rock varnish? Not a single one. How many of the Upper Palaeolithic portable art objects of Eurasia would have survived if they had all been deposited in low-pH soils? Only the few made of non-carbonate stone or of clay — and their authenticity would almost certainly have been rejected by conservative scholars; we only need to consider the response of the French archaeological establishment a century ago to the claims of cave art. Thus in the debate over evidence for early human symbolic behaviour, history is repeating itself.

This kind of reasoning shows that, whenever Pleistocene art has survived, almost miraculous combinations of circumstances are responsible. In most world regions then settled these conditions may not have existed. It is also clear that the proportion of surviving evidence would have dwindled with increasing age, and that there will be a point in time when it should be expected to become exceedingly rare. To show that even the comments by Chase and Dibble — though far more rigorous than their earlier article (Chase & Dibble 1987) — still contain many *non-sequiturs* I shall select just one such claim for analysis, and it is one that is crucial to maintaining the position of the commentators. My alternative will even meet their criteria of a 'simple' explanation.

In arguing that the Lower and Middle Palaeolithic lasted much longer than the Upper Palaeolithic, the commentators present a seemingly rational argument. But let us assume, for the sake of the argument, that every human and hominid of the Quaternary produced  $n$  number symbolic artefacts. If the sum of the human population numbers is  $p$ , the total production of symbolic artefacts  $s$  would be  $s = np$ . There would be a percentage of natural loss per time unit, which would increase cumulatively with age in a linear fashion. Whatever this percentage might be, there must be a point in time at which the loss approaches artefact population, meaning that theoretically no artefact should exist beyond that age. This concept may be depicted graphically (Fig. 1).

This model (which is of course susceptible of refinement at various levels) would predict that the scarcity of evidence would increase rapidly as one considers evidence of increasing age until, just before a cut-off point  $C$ , one would predict that only very little evidence would be found, and that only in most exceptional environmental circumstances. This, needless to say, is precisely what we do find in the record, and it would appear that this cut-off point might be around 30,000 years BP. If this were the case it would be almost futile to look for older evidence, and it would be pointless to invoke an argument which might appeal to common sense but which has in fact no logical basis: that there should be more evidence from the preceding periods because they were very much longer. Their duration would be entirely irrelevant. In practice, however, it would still be possible that, through some extraordinary and rare conditions or flukes, an occasional specimen might survive from earlier periods. Again, this is precisely what the record has produced, and it would then be entirely wrong to read anything into the paucity of evidence.

This model, which would require that most interpretations of palaeoart be rewritten, should be preferred by those who favour 'simple' over 'rich' explanations. It is significantly simpler than the rich alternative favoured by Chase and Dibble and most colleagues, involving cultural, cognitive or evolutionary interpretations of distributional, compositional or statistical indices. It also explains a number of phenomena which cannot be explained by the old model, for instance, why the oldest surviving figurines (Hahn 1971; Marshack 1985; Bednarik 1989) are so incredibly sophisticated. It would also explain why art extends so much further into the past in a few, often well-defined but apparently unconnected or poorly-connected world regions.



**Figure 1.** Schematic depiction of the principle of taphonomic reduction of sample size in a continually increasing population of symbolic artefacts. A = population of recent artefacts; B = artefacts produced at some point in the Middle Pleistocene; C = cut-off point at which all older artefacts should have been lost; area below curve X = total production of symbolic artefacts; area below curve Y = total surviving artefacts.

This model shows that it is perfectly possible to create a rational explanation of the existing data that differs most fundamentally from the gospel according to Eurocentric scholars — a model that renders the same entirely worthless, is more plausible and convincing, and is eminently refutable. In the same way, my article was offered to show that many other ideas about palaeoart, nursed and ‘confirmed’ for decades (Bednarik 1992a), are in fact entirely without real substance, and that this soon becomes apparent when they are subjected to rigorous and logical testing. My paper is in fact only one of a series, all of which attack establishment consensus on palaeoart (e.g. Bednarik 1991, 1992b, 1993), or more specifically, the Eurocentric version of the origins of palaeoart, language, cognition and human concepts of reality. For a full century, Eurocentric scholars have presented a largely unsubstantiated model which greatly overemphasizes the role of southwestern Europe in bringing art, language, religion and culture to the world. Enough is enough! These scholars can no longer continue to ignore the fact that most Pleistocene palaeoart is found outside Europe and that it predates the Upper Palaeolithic in various continents. The

Australian petroglyph which has recently been shown to be significantly older than 36,000 years (Dorn *et al.* 1992) is just one of tens of thousands of similar designs, a good many of which are likely to be of a similar age, or older. Three further minimum dates have just been obtained for petroglyphs in the same region (M. Nobbs, pers. comm., Second AURA Congress) and they range from about 43,000 to 45,000 years BP. Two of them are based on AMS radiocarbon determinations, not on the controversial cation-ratio method, and their proximity to the limit of the former method is evident.

Australian palaeoecologists no longer accept the ‘archaeological’ first colonization at around 60,000 BP, but think in terms of 120–140,000 years. Irrespective of the validity of this spectacular claim, we agree that, whenever these people arrived, they already had a form of language, planned their actions well beforehand, probably possessed some form of art, and had sea-going watercraft. It is impossible to reconcile this with the idea that the Neanderthals, who disappear from the record a mere 30,000 years ago, did not use language. While such a view may have derived some support from the ‘African Eve’ theory, which argued for a significant genetic gap between Eve’s progeny

and the hapless Neanderthals, that support has been lost now that the 'Eve' hypothesis is being brought into doubt. It remains only for more sensible forms of archaeological inquiry to finally dispose of the idea that language and other forms of early human symbolic behaviour did not exist until a mere 30,000 years ago.

Robert G. Bednarik  
International Federation of Rock Art Organizations  
P.O.Box 216  
Caulfield South  
Victoria 3162  
Australia

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