b. A single reindeer bone does not necessarily entitle us to infer that reindeer were present in the general location at a particular time. Many finds have been displaced, by a great variety of post-depositional processes, not all of which are likely to be apparent to most excavators.
c. Zigzags and dots are not the only patterns Marshack identifies as depicting water in Palaeolithic imagery. He has suggested that long, subparallel lines indicate water (Marshack 1989: Figs 14, 20), as do meandering lines, festoon lines, stippling, or even just vacant space. If stippling occurs in an animal outline, he says it depicts fur, and if a zigzag line occurs there, he does not define it as water. Marshack's notion that certain marks depict water is in part attributable to his belief that he can identify iconic outlines of water creatures, such as the three animals he perceives on the Kirillovskaya tusk. There is in fact no kingfisher on that tusk, nor a fish, nor a turtle. While some of Marshack's iconic interpretations may well be correct, within an epistemologically sound framework they should be disregarded.
d. Cameron raises the subject of the Gönnersdorf female figures, and the question of their similarity with the female profile. It seems to me that there is an enormous range in contemporary female physique, and there may have been a similar range in Palaeolithic times. Therefore it might be preferable to reformulate the proposition thus: there are a number of motifs which, if our taxonomic designation is correct, are intended to depict the same object, and which resemble the profile of *some* human females. Classifications of artefacts are, after all, themselves artefacts, especially those of archaeologists, and are not objective entities by any standards (Adams and Adams 1991).

The concept of 'behavioural debris' perceived in archaeology is a useful starting point for debate. The question of how this so-called archaeological record relates to what really happened in the past is of course the crux of all archaeological interpretation. It is primarily a taphonomic question, because nearly all quantifiable characteristics of archaeological 'data' are the result of taphonomic processes (I use the term in its extended sense, including primary and secondary taphonomic processes; Bednarik 1993a), in rock art as in archaeology. The prospects of scientific study of palaeoart will remain very slim without the *coherent identification of that part of the extant characteristics of the evidence that is not the result of taphonomic processes*. This first prerequisite for a scientific study of rock art has remained largely ignored for the past hundred years and we need have no illusions of scientific rigour in this discipline until we come to terms with it. Without such reconciliation the scientific discipline of palaeoart studies will remain restricted to simple empiricist tasks: direct dating, paint analysis etc., i.e. safe, repeatable and readily falsifiable procedures.

Cameron raises several interesting subjects which serve to illustrate how a bias in archaeology can consistently select in favour of hypotheses that are unlikely to be correct. For instance, during the 1980s, a tendency developed among certain prominent archaeologists to view sceptically earlier claims for the cognitive or intellectual faculties of hominids. It became fashionable to question early fire use, dwelling building, language, hunting, non-utilitarian behaviour and self-consciousness. The hominids of the Lower Palaeolithic became once again wild beasts,

Refutability and taphonomy: touchstones of palaeoart studies

By ROBERT G. BEDNARIK

Refutation is the most rigorous epistemological tool we have, whatever its shortcomings may be, and it is Cameron's preferred approach as much as it is mine. I find his paper thoroughly convincing. A few rather unimportant points are mentioned in passing, before I will try to develop his arguments a little further:

a. Dating by archaeological stratification is by no means reliable. The dictum that the lower of two strata must always be the older is not true in geology, nor in archaeology, and the notion that the objects occurring in one stratigraphical unit must all be of the same period is often a fallacy (consider deflation phenomena).

scavengers without home bases, and ultimately, the Neanderthals' ability of speech was disputed. The result was an unproductive debate about the role of the hyoid bone and Broca's area, and at least one bold claim that the Neanderthals really belong to the apes rather than the humans.

This is not a new argument but it has been a very long time since we heard it previously. On close examination, these New Archaeology arguments were not about evidence, but about the perceived lack of it. Often the protagonists were inadequately informed about the subject, for instance the arguments about cognitive faculties such as human 'consciousness' could benefit from a much less humanistic emphasis. After all, ethological evidence of self-consciousness among apes is not new. We know that if paint is dabbed on the forehead of young chimpanzees, they will touch it when viewing themselves in a mirror. Humans recognise their own image at the age of about 18 months, gorillas take perhaps twice as long. To claim that bead manufacture in the Szeletian is the first evidence of human self-consciousness is not only based on inadequate archaeological facts (beads were produced much earlier, possibly up to 300 000 years ago; Bednarik 1992b: 34), it suggests also ignorance about primate ethology, which in contrast to archaeology is a science.

Confirmationism is itself an epistemologically unsound philosophy of science, but when it is based on negative evidence (the argument 'we have not yet found it, hence it does not exist') it is devoid of any value, unless a current absence of evidence is cited as part of a refutationist proposition. For instance, until 1990 it was correct to state that there is no evidence that art was practised in Pleistocene China; this is a refutable claim and it was duly refuted (Bednarik and You 1991). However, it would not have entitled one to postulate that Ice Age art did not exist in China, and then incorporate this claim into a model, for instance, a map of the world distribution of Pleistocene art. As soon as it is published, such a model is taken as gospel and will be included in all kinds of further models, hypotheses and theories, the authors of which have no regard for the tentative nature of most of the evidence they base their notions on. A great deal of what passes for 'archaeological knowledge' is based on negative evidence.

It is fairly self-evident that the objective study of contemporary human societies is not entirely without difficulties, and yet one would assume that in the case of living cultures, all the phenomena, rules and processes defining them are knowable. (The endless controversies of anthropologically reported meanings among non-Western peoples suggest that one could question Davidson and Noble's naive belief that language clarifies shared meanings. It would be easy to construct a viable argument that the communication between ethologists and apes is scientifically more precise than the communication between anthropological interviewers and subjects.) When examining pre-Historic societies, more than 99.99% of all the variables one would need to be familiar with are not accessible to us, and most are unlikely to ever become knowable by empirical deduction. Primary taphonomic processes have erased them from the record, and since these processes are far from random it is obvious that they selected in favour of certain phenomena. Archaeologists often treat what they regard as data as if they conveyed evidence approaching a one-to-one relationship with the circumstances of their historical context. This is not being said as a criticism of archaeology, rather I use archaeology as a metaphor for the reality-building processes of humans: through imposing taxonomies on the physical world accessible to human sensory faculties, humans have constructed a confirmationist framework of reality (Bednarik 1990/91). In both cases, all we really have are artificial constructs: both archaeological interpretation and anthropocentric reality are conceptual artefacts (Bednarik 1992b). Provided that they are couched in self-confirming systems of knowledge, they confer confidence, but that is no proof for their validity.

Of particular concern should be the tendency of some confirmationists to oppose contradictory evidence, not because of some objective arguments, but simply because it tends to refute their favoured theories (usually their own). Motivated by human vanity, some scholars go to extraordinary lengths to discredit data which they had not been aware of at the time they presented their theory, and rather than conceding that they were inadequately informed they retreat to a strategy of trying to reject any contradicting data — not because they favour refutation, but because they are unable to accept the effects of their own superficiality. As I have said before, the dynamics of academe are at fault, not the individuals concerned: if academe rewarded those who excel in self-critique, rather than those who strive most to be 'right', science would gain enormously.

Cameron clearly favours a philosophy championed by Karl Popper (e.g. 1968) and Imré Lakatos (1970), who believed that a scientific methodology for judging when one set of empirical beliefs is better than another is possible. I do agree with him in principle, but not without cautioning that my personal experiences in science suggest Thomas S. Kuhn's model to be probably closest to the truth. Kuhn (1962) has examined episodes in the history of science, which led him to argue that change from one theory to another is essentially non-rational: there is no method in it, and science is itself a contingent social activity. Choice of paradigm is presupposed by scientific rationality, not founded upon it, and Kuhn distinguishes between 'normal' and 'revolutionary science'. Feyerabend (1975) goes even one step further, denying that there is a 'scientific method' and arguing that scientific establishments are as effective in closing minds as were the religious powers of past centuries (though, admittedly, they are not at liberty to use physical torture nowadays). A courageous statesman among rock art specialists, Professor Jack Steinbring, has recently addressed the role of personal weaknesses and faulty, often weak, egos of influential academics, and of how the academic setting facilitates their influence (Steinbring 1992).

Both Kuhn's and Feyerabend's propositions are fundamentally correct in my experience. In the sense of the former's terminology, palaeoart studies have recently reached a 'crisis point' and are now undergoing a scientific 'revolution'. There is no method in this revolution, nor can academe control it (although it seeks to), changes are not due to some conscious program, but are affected by random factors: the predispositions and priorities of some individuals, personal enmities and alliances, various classes of external factors, social and intellectual currents in society, to name just a few. The visible manifestations of the current revolution in palaeoart studies are conspicuous enough: from a ludicrously backwards sphere of interest just ten years ago, a fully fledged discipline is emerging. It is my hope that it will adopt a refutationist philosophy, combined with a taphonomically guided methodology (Bednarik 1993b). If it does, it will become

as scientific as any of the hard sciences. To preserve its integrity it will be necessary for the discipline to be under the aegis not of academe, but of a body lacking vested interests and seeking no power of its own. Power corrupts, and the power of academe is enormous. If it cannot be moderated by scholarly societies in the future, it will continue its march towards a universal technocracy ruled by academic mediocrity (academic excellence derives from individual initiative, while the dynamics of academic institutions favour mediocrity and compliance).

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