

NOTES AND COMMENT

ARCHAEOLOGY: EMPIRICIST DETERMINISM OR CULTURAL SYNTHESIS?*

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The following comments have been prompted by a recent paper by Lewis-Williams in this journal (1993). His inspiring review is of rare scientific integrity and insight, so what follows is clearly not a critique in any sense of the word. I believe that archaeology has much to gain from critical self-examination, from sound review of its practices, prejudices, biases, techniques, even the effects of its established jargon. A renaissance of epistemology would do a great deal to strengthen the discipline scientifically. Archaeology has become exceedingly difficult to criticise from without, because of its restrictive practices: only university-qualified practitioners may practise archaeology, as established by legislation in most countries. I am not questioning this practice, I merely observe that such exclusive executive control exists in no other discipline (except, for obvious ethical reasons, in medicine), which creates a deontological conflict of interest: it impairs disciplinary independence, which is so essential in science. Therefore practitioners, particularly those in positions of influence, need to be most active in vigorous self-assessment.

Lewis-Williams (1993:47) shows that the term 'archaeological analysis' is essentially an oxymoron: a conjunction of two incongruous terms. An analysis of taxonomies or entities created by archaeologists can only be an analysis of the way the minds of archaeologists work, reflecting the stylistic divisions they create among the material remains they have decided are of archaeological significance, and the value-laden concepts they form (Conkey & Hastorf 1990).

Another frequently used term with which I have difficulties is the phrase 'archaeological record'. If it referred to the total sum of all that is known by all archaeologists, it would be useless as a concept: who could possibly gain access to all these knowledge claims, and what would guarantee us that they would be free of conjecture? If the phrase referred only to that archaeological knowledge which has been published, no one person could read and memorise all the hundreds of thousands of publications in the discipline. One might argue that this record is simply the kind of received knowledge one can be assumed to have acquired during archaeological training. Such training differs enormously according to where in the world it was received. If we referred to some sort of lowest common denominator definition, it would, in my view, express a level of archaeological knowledge that would be inconsequential.

In some contexts, 'archaeological record' evidently refers to the supposedly uninterpreted data from just one site, or a series of sites. But we know that this 'record' is merely selective observations obtained on the basis of biased research models and questions, by researchers who

have been conditioned by cognitive, perceptual, epistemic, political and cultural biases, and academic elitism. Such data do include an objective component, but how does one effectively excise that part which is not sound? Even the objective data distilled from it are not suitable for direct interpretation without intensive taphonomic screening (Bednarik 1992, 1994), which is a technique of logic applied only in its crudest forms so far.

It is often assumed that there is such a thing as a world archaeology: an international network of specialists with a reasonably uniform philosophy, epistemology, deontology and methodology. I have worked closely with practitioners in numerous countries, in most continents, and have not observed such a uniformity. The methodologies of various schools differ sometimes more than those of different disciplines. One needs only to consider the differences of chronometric approaches: some traditions have become so dependent on just one method that they have invented artificial cultural plateaus on the basis of its limitations, e.g. the Middle/Upper Palaeolithic demarcation and first occupation of Australia and Americas. They consider as somehow 'inferior' research traditions which prefer more holistic approaches to dating or rely on more secure, if less accurate and 'absolute' methods, such as geochronological dating.

Not only are there enormous differences in university curricula across the world, but most practitioners are, in some way, employees of the state. In some countries they work within hierarchies that permit little departure from officially sanctioned directions. Oligarchies exist in all of academe. In many countries they are perhaps very subtle, but they are nevertheless influential, through research funding policies, research priorities, and various political influences. In addition, and superimposed on these power-related factors, there are those related to the researcher's 'social, political, intellectual and academic milieu', which Lewis-Williams (1993:46) describes so well.

Archaeology and indigenes

The influence of political predisposition, for instance, is of considerable complexity, and yet many practitioners would scoff at the suggestion that they are unable to free themselves of such biases. This is particularly relevant in this day and age, with its world-wide resurgence in ethnicity, prompting an increase in nationalism and jingoism even among researchers. To suggest that such currents have no influence on researchers is historically invalid. In a recent case, an Australian was severely reprimanded by a French colleague after reporting very early direct radiocarbon minimum dates for Australian rock art, pointing out that these are considerably greater than the known age of Upper Palaeolithic rock art in south-western Europe. The French scholar warned against practising an 'inverse racism' that 'minimises the great antiquity and importance of the first European art' (Lorblanchet 1993); and he warned against encouragement of 'ethnic pride' among the first Australians, the Aborigines. What he seems to be saying is that the Australian report was by a person of European extraction,

who should practise racial selectivity in what he reports, and not challenge the perceived precedence of European culture. To know of such views among prominent and influential practitioners renders it impossible to accept archaeology as a politically neutral, idealistic search for truth about the past. It is not politically neutral in any one country. To prove this one does not need to cite such notables of the past as Professor Gustaf Kossinna, we can find examples today in our own 'enlightened' times.

I recall the surprise among many of the overseas delegates attending the Second AURA Congress in Cairns, Australia, in 1992, when they realised that the fifty-nine Aboriginal delegates at the conference (one third of the entire Australian contingent) were there not for decoration, but to present papers, chair two symposia, and to tell archaeologists what they expected of them. For Australian archaeologists this was not a new experience. They had learned to adjust to new ideas flowing from such a working relationship with their principal client group, or they had abandoned their local aspirations in favour of offshore ones. Some delegates seemed concerned that the discipline could allow itself to be hijacked by indigenous interests; presumably, as long as it serves European concepts of reality, it is an objective discipline. Lewis-Williams recognises the professional power of archaeology, and I would be interested in his views on moderation of that power by the interests of client groups, particularly by the indigenes in those countries where nearly all archaeology relates to the histories of non-European indigenes. After all, this is the case in both South Africa and Australia: post-contact archaeology is of limited scope there, although the effects of colonisation on indigenous societies would be relevant.

Effects of heuristic history

Archaeological beliefs are often more subjective than the recent data provided by rock art researchers, whose work archaeologists have traditionally viewed with reservation. Lewis-Williams considers this point, arriving at the conclusion that 'mainstream' archaeology will have to adjust to the results of rock art research. The practitioners he so addresses are used to considering rock art as being of peripheral interest, limited largely to its role in providing decorative cover images for books on 'proper archaeology'. I would like to consider his proposition here.

During the last century, the history of archaeology was determined by the finds progressively becoming available, by the interpretations of this evidence, by personal and political factors and by the models of influential scholars. In other words, there could not have been any method in the heuristic progress of the discipline, in the order in which its knowledge was acquired. We all know how the powerful archaeological establishment of France flatly rejected the rock art of Altamira for decades without even examining it (Bahn 1992). Let us assume, for the sake of argument, that Altamira had been discovered before the identification of Upper Palaeolithic tool industries, and that it was soon dated and recognised for what it is. (This is not the absurd suggestion that it may seem: the existence of some Palaeolithic art was known for centuries before Palaeolithic tools were recognised.) No doubt science would have formed a concept of the people who had produced the art, based on its perceived aesthetic merits. Next let us assume that, several decades later, a non-archaeologist discovered stone tools and attributed them to the same period. His notions would have been rejected with considerable scholarly indignation: how could the

sophisticated artists of the era be expected to have led the miserable lives suggested by such a wretched technology of primitives! Such a preposterous attempt to discredit the established scientific fact that advanced civilisation had begun in Europe 30 000 years ago would have been resoundingly rejected.

Heuristic history not only determines acceptability of models, it also stipulates the cognitive framework within which they may be developed. Unaware of its tool technologies, archaeologists would have divided the Upper Palaeolithic period into cultures as perceived through the art, and this taxonomy of cultures would have been totally different from the one we have inherited. Since art remains the only tangible cultural dimension we have of the period, we can assume that the cultural pigeonholes so created would have been far more pertinent than the ones we have in fact inherited: they would have been derived from conventions of depiction, iconicity, perspective, technique, subject, graphic conventions, forms of syntax, semiological markers, modes of use and re-use and from similar, culture-mediated traces of behaviour of the societies concerned. Culture is, after all, defined by the collective beliefs, values and customs of a society. These are expressed in numerous characteristics (architecture, literature, mime, mythology, apparel, semantic qualities of otherwise utilitarian objects, to name but a few), but not in utilitarian objects themselves. Tools as such do not define cultures, be they screwdrivers, spoons, computers or lithics. Artefacts often are culturally diagnostic, if they bear stylistic variables of semiotic value, and the very thin argument of archaeology is essentially that such information incorporated in items such as stone tools, etc. can be defined archaeologically. If we cannot falsify the attempts of archaeologists to extract and interpret such perceived information, the claims that typologies of stone tools, etc. are valid and can define cultures are not scientifically testable. The only *cultural* information we have of early people is that provided by palaeoart, besides a few archaeologically perceived but doubtful cultural practices.

Despite this, most archaeologists have not shown a great interest in palaeoart. Their principal objection was that the art could not be dated securely. This may be chronocentrism, as Lewis-Williams points out, but there is also much truth in it. While archaeologists belaboured the need for rock art dating, others strived for ways of achieving it. When non-archaeologists began to develop direct dating methods, they were sometimes discouraged and rejected by archaeologists. But direct dating was introduced in 1980 (Bednarik 1984, 1993) and in 1987 the world's first AMS radiocarbon date from a rock painting appeared in South Africa (Van der Merwe *et al.* 1987). Since then it has taken only a few years for the results of this method to be misused and misinterpreted by archaeologists elsewhere (Bednarik in press; Clottes in press).

The materialist approach to what is ethnocentrically described as 'prehistory' has its origins in the early collection of antiquities. C.J. Thomsen divided prehistory into three periods, during which tools were made of stone, bronze and iron respectively. In 1865 Sir John Lubbock introduced the term Neolithic for the period during which people began to use ground stone tools, cultivate plants, produce pottery and domesticate animals. We know that these hallmarks of the Neolithic are irrelevant in most parts of the world, but we still use the term regardless. Ground stone tools (e.g. in Australia) and pottery (Incipient Jomon of Japan) of the Pleistocene occur in various regions; the advent of plant cultivation is often

unknown or does not coincide with that of other 'Neolithic innovations' and the question of first domestication remains unresolved. What use is the concept of a Bronze Age in southern Africa, or a Mesolithic in Australia? Once microliths and Levantine shelter art characterised the Mesolithic, now the former occur in the Howieson's Poort industry, the latter have become Neolithic. But the most absurd result of archaeology's penchant for pigeonholes must be the Middle-Upper Palaeolithic transition, when modern humans are supposed to have 'exploded onto the scene' - history's first colonisers bringing beads to the natives (White 1989).

Archaeologically perceived pigeonholes have become serious impediments to progress in the discipline: taxonomic systems exaggerate differences as well as similarities. They are simply not factual (for well-informed recent views, see Lindly & Clark 1990; Duff *et al.* 1992; Hayden 1993; Vishnyatsky 1994) and they have led to strange if not absurd treatments of the evidence. To illustrate by example: two female figurines 20 000 years apart chronologically and 6000 km spatially can be considered components of a single cultural tradition, even if there is no evidence of cultural affinity, while two figurines from the same site, only a few millennia apart, cannot if one of them happens to be post-Palaeolithic. And yet, the concepts we formed of the Palaeolithic are probably substantially erroneous. We think today that mammoths were still hunted less than 4000 years ago (Vartanyan *et al.* 1993), at a time when a few thousand kilometres to the south, Bronze Age Chinese were inventing writing. So much for our simplistic concepts of 'Palaeolithicity'! They are merely the product of archaeological needs to categorise, to taxonomise, and may need to be challenged from time to time. Had these taxonomies been based on a totally different heuristic history of the discipline from the beginning, they would be totally different today. Where, then, is the objectivity in archaeological interpretations, derived as they are from a historically random sequence of discoveries?

Had the cultural sequence invented by prehistory been based, instead, on art, it would be definite, except where resolution of the evidence had been inadequate. Admittedly, such a system might still be prone to deficiencies, but its shortcomings would be attributable only to faulty interpretation and amenable to correction, resulting in progressively better models. They would not be due to fundamental errors of theory, such as the idea that utilitarian aspects of material remains provide reliable criteria for the identification of cultures. So the difference between a palaeoart-guided prehistory, and one guided by empiricist taxonomies of utilitarian data, is that the former results in models of progressively greater validity as it is being tested and improved, while the latter will never result in a valid history of past cultures, whatever level of resolution we employ and however rigorously we seek to test our hypotheses. The theory itself is flawed. In short, an archaeological periodisation predicated on sound parameters of palaeoart is superior to one based on perceived artefact types.

Conclusion

The majority of practitioners are well aware that the discipline's neo-colonialist historical background, its deontological problems and the need for epistemic revision all suggest that structural and fundamental change is inevitable. Lewis-Williams discusses southern African

archaeology of the 1990s in this light, but should we not consider the direction well beyond the present decade? Can we expect (or permit?) archaeology to survive into the 21st century in the same shape in which it has floundered through the present century? What does a major restructuring entail, and what would be its effects?

This brief review is not the place to discuss post-processualist archaeology. The writing is on the wall concerning the need of the discipline to abandon European/androcentric metaphysics in favour of multidimensional models, and a recognition that indigenous concepts of reality are more relevant to understanding the past than pseudo-science predicated on European religion, mythology and epistemology. The first rule in real science must be that its claims of knowledge need to be relevant and logically acceptable to any *conceivable* intelligent organism in the universe (whether it exists or not is irrelevant), not just to what mere humans think, consider significant, or hope to be true, at a particular time in their history. True science is an altruistic and idealistic search for truth and in archaeology, which deals largely with vanquished and destroyed human societies, it is particularly important to resist the temptation to interpret the past in terms of one's own cognitive, social, political and academic conditioning. This is not to suggest that intellectual honesty or epistemic integrity are easy to accomplish; consider rock art, for instance. It must be studied outside of human reactions to it (Bednarik 1991/92), and yet human reactions to rock art seem to provide most of our data: the reactions of rock art interpreters, conservators, indigenes, tourists, archaeologists, connoisseurs, art historians and many others. Perhaps their reactions ('interpretations') are of relevance to real science (by recourse to cognitive universals; Bednarik 1990/91), but so far no-one has demonstrated this scientifically. Scientific approaches are certainly possible, but in palaeoart studies they are only now beginning to evolve.

Nevertheless, these developments suggest that palaeoart studies will supersede 'mainstream archaeology' as the preferred approach to much of the human past. To illustrate this point I mention just one example, the nano-stratigraphy of rock paintings as pioneered by Watchman (e.g. 1992). It promises a more secure chronology than perceived sedimentary stratigraphy of soil deposits, and one that is tied directly to cultural rather than technological indices. It offers considerable research potential for future centuries, as do other recent developments in rock art studies. Traditional archaeology, with its emphasis on 'backwardness' of past societies by contrasting their technologies with that of the researcher, may itself become a thing of the past. Granted, archaeology has, in recent decades, been at pains not to appear patronising in its language and approach, but the power disequilibrium will remain as long as technological faculties remain its criteria of culture, and cross-cultural dialogue falls short of Habermas' (1979) 'ideal speech situations' (Leone & Potter 1992). Therefore it needs to be supplanted by a system emphasising culture rather than technology. Rock art research is doing this for 'mainstream archaeology', and it is quite capable of creating its independent, separate archaeology, should the technocracy of establishment archaeology force it in that direction. But one would hope that orthodox archaeology appreciates that Lewis-Williams's (1993:48) dictum 'the history of a community is constructed by the working out of conflicting interests within it' applies also to the archaeological community.

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