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ARCHAEOLOGICAL CONSTRUCTS OF PLACE IN THE PLEISTOCENE?

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***Abstract*:** *The best example of an archaeological construct of place from the Pleistocene is perhaps the definition of the Franco-Cantabrian cave art of western Europe as being endemic to a specific class of site. It is shown that this construct is a fallacy, and that most interpretations of the phenomenon are probably false. A discussion of the epistemological base of Pleistocene archaeology suggests that it is currently most inadequate to serve as the foundation of yet another niche archaeology, an 'archaeology of place'. Knowledge about the human past is not served well by expanding the current edifice of mythological explanations that Pleistocene archaeology essentially consists of.*

Introduction

The connection between archaeology and anthropology is not as obvious as it may seem at first sight. Traditional archaeology, the kind dominating academic discourse until the 1960s, held that culture is normative, i.e. that artefacts are expressions of cultural norms. Therefore one excavated 'cultural layers', created a taxonomy of their material finds and compared it with the contents of other layers to determine what was regarded as the geographical extent of a culture or as a sequence of cultures. Only with the advent of the 'New Archaeology' did demands arise that archaeology become 'more anthropological' — as well as 'more scientific'. Science was seen as progressing with time, whereas traditional 'culture history' seemed to be static, simply accumulating more 'data' about fetishes, i.e. objects that had come to represent something else, namely people. The reason of science's progress is its practice of testing hypotheses, of seeking falsification. But therein already lay the seeds of the later demise of the New Archaeology: the kind of archaeology practiced so far can import scientific propositions from other disciplines, but it is itself inherently non-scientific (not susceptible to falsification). Similarly, the view of culture as "man's extrasomatic means of adaptation" (Binford 1964) is doomed to failure: it presupposes that humans adapt through culture, whereas other animals do so through their bodies. But other animals, too, have culture, because the scientific definition of culture is the passing on of practice by non-genetic means, i.e. learning (Handwerker 1989). Thus a large range of learning does occur in the animal kingdom.

This is not to suggest that the New Archaeology was not an improvement over the previous approaches. The fetishes of traditional archaeology had been seen as evidence of the movement of the imagined carriers of the culture, such as ethnic groups (e.g. Childe 1929). For instance in Europe a *Glockenbecher*-culture was invented, based on the frequent occurrence of small ceramic beakers 'identifiable' by their bell shape. An itinerant 'beaker folk' was concocted as its carrier, an ethnic group that 'invaded' a variety of regions at various times (e.g. Abercrombie 1902; Harrison 1980). Alternative interpretations of the same data favour a social explanation of the phenomenon, involving no mass-movement of people (Lanting and Van Der Waals 1972; Sherratt 1994), but a movement of ideas ('memes'), perhaps about status. The same kind of colonisation logic is widely found in archaeology, for instance in the concept of a Celtic nation-like people.

Today we find it entrenched in the idea that the apparent diffusion of genes over enormous time spans, e.g. from Africa to Europe, proves the mass-movement of an ethnic group in the Late Pleistocene. This shows that archaeology can correct erroneous logic but in time goes on to apply it in a different context.

Nevertheless, the more positive aspects of the New Archaeology need to be made explicit. There was the demand to consider one's biases and to avoid simple intuition and implicit assumption. The concept of research design was given much more attention than previously: what were the specific questions to be addressed, how would one test specific hypotheses? Traditional archaeology had tended to focus on the more spectacular aspects of the past, the elites of civilisations, whereas the New Archaeology (or processualism) sought to secure more representative sampling to lead to more systematic description of past societies. With middle-range theory it hoped to build a platform of secure statements about the human past from which to infer and test theories.

It has not been the success it promised to be, and by the late 1980s it was considered to have introduced a narrow scientism into the discipline (Shanks and Tilley 1987a, 1987b). As Clarke (1978: 465) observes perceptively, the use of scientific techniques "no more make archaeology into a science than a wooden leg turns a man into a tree". Archaeology seemed to lack the most basic requirements of a science: there is no obvious way of satisfying the need for predictability and testability, and the independence from value judgments is hard to envisage in a discipline that is invariably humanistic and political. The experiment of creating a scientific archaeology failed because uniformitarian analogy, the basis of middle-range theory, cannot validly test propositions. There is no plausible reason to assume that all societies go through the same phases of cultural evolution, or that similar rules of their development should apply. Moreover, archaeology as practised essentially serves contingent political power, appropriating the human past to serve present hegemonies and constructs of reality, thus lacking scientific justification.

Post-processual archaeology began to develop out of dissatisfaction with processual archaeology in the early 1980s, and also as a vehicle for a new generation of practitioners to create their own academic niches. It represents a distinctive retreat from the demand that archaeology become more like a science, in fact it explicitly acknowledges *that the discipline cannot be a science*. Instead, the many different factions of postprocessual archaeology claim that all data are inevitably theory-laden. Another admission inherent in postprocessual archaeology is that archaeologists merely *interpret* what they find, implicitly assuming that their hermeneutic interpretations are like those of the people of the past. It shares with Marxist archaeology an acceptance that the meanings produced by archaeologists are the results of political acts — the imposition of present-day values on the evidence. Postprocessual approaches are often called 'contextual archaeology' or 'interpretative archaeologies' (note the use of the plural). There is also an emphasis on the need to consider the values of the past, or the thoughts of the people being studied, and in that sense a similarity with the aims of cognitive processualism might be evident.

Judging from the results of the experiment, postprocessualism seems to be a questionable improvement on the preceding theory. One of the examples sometimes cited refers to the interpretation of rock art. Chris Tilley, a main protagonist of this school, has conducted a much-mentioned 'analysis' of petroglyphs at Nämforsen in Sweden (Tilley 1991). He begins by telling the reader that these 'carvings', as he calls them, are of the third millennium BCE — but no rock art in Scandinavia has been satisfactorily dated. He constructs an interpretation of an art of which he has only mediocre *interpretations* (much of the rock art had been destroyed since it was recorded almost a century earlier), only to then deconstruct it himself, and telling the reader that it is impossible to determine the meaning of the art. While he is very probably right, the question arises why archaeology bothers itself with a topic it is not equipped to deal with in any meaningful way.

The wave of 1990s postmodernism arrived at an inopportune time for archaeology. Weakened by its lack of underlying universal theory, by its internal divisions and epistemological contradictions, by its

inability to secure scientific standing, by its wrangling with the owners of cultural heritage and by many other factors, it now faces an intellectual climate that is sceptical, even hostile to finite claims of knowledge. Archaeology has little comprehension of its own theoretical underpinnings, debate of archaeological theory is "of a very low intellectual standard" (Johnson 1999: 182); endeavours to be scientific are defined as "vacuous" and the attempts of their presenters at learning as "pathetic" (Binford 2000-2001: 334). The new relativism of postmodernism haunts all of the 'social sciences' or humanities, but if it were allowed further inroads into archaeological thinking it would logically lead to the dissolution of the discipline.

The nexus between archaeology and anthropology is that both are trying to deal primarily with human culture. Traditionally the former addressed long-disappeared cultures, whereas the latter investigated extant ones. While the parameters of anthropology are much clearer, those of archaeology have become more confusing with time. Most archaeology today deals with 'historical' periods, so it should be a sub-discipline of History. In the United States it is indeed attached to anthropology, but in the British system it has acquired a puzzling identity in recent decades. The wide range of archaeologies dealing with recent or current issues belies the assumption that it addresses the distant past, and even the belief that archaeology is dominated by the method of excavation is a fallacy. Numerous branches of modern archaeology involve no excavation (e.g. aerial archaeology, rock art research, numismatics, most nautical archaeology), while several other disciplines (e.g. palaeontology, sedimentology, palynology, geology) share the method of excavation. It is therefore not apparent what it is that holds modern archaeology together as a discipline: there is no obvious common denominator for all niche archaeologies. Which brings us to landscape archaeology.

The interaction of archaeology with geography has long been given much attention; after all, archaeology is very much concerned with sites, and with the significance of their particular situation within the landscape. But by itself it is not an adequate justification for having a landscape archaeology. That is not to say that such an approach is to be discouraged; rather, the point is that at the simplistic level much of this has so far been presented it is most unlikely to lead to any credible insights. Where naive attempts to address the archaeology of landscape (i.e. the interpretation of past significance of places) fail is that they tend to ignore the severe limitations of this approach. Unless all places of significance to a particular society are known in a particular 'landscape', we are in no position to assume that we understand that society's interaction with a whole landscape (which, in any case, no longer exists, and needs to be 'reconstructed'). This presents us with two significant obstacles: taphonomy has ensured that many, even most, of the significant sites in an area are either not archaeologically available or not archaeologically known (Bednarik 2000). The older the society in question, the greater the probability that most of its sites have fallen victim to site destruction processes, of which we recognise many. Secondly, it is not always readily apparent which sites in a given area are directly related via culture, i.e. offer the evidence left by a particular society.

Obviously these considerations do not deny that an archaeology of landscape can realistically become scientifically meaningful. The obstacles to this are very similar to those faced by archaeology generally: archaeologists would need to learn to couch their propositions in terms acceptable to taphonomic logic (Bednarik 1994a) and would need to apply the harsh rigour demanded by metamorphology (Bednarik 1995). This has so far not occurred in any systematic fashion and such a scientific archaeology is not likely to emerge in the near future. The difficulties are best illustrated with examples from the earliest periods archaeology takes an interest in, because it is there that the effects of taphonomic logic are greatest. Therefore the notion that constructs of place can be examined archaeologically are considered here in the light of what Pleistocene archaeology, as it currently stands, would be likely to contribute to this approach.

Cave art

Of all forms of archaeology, Pleistocene archaeology is the one most bedevilled by problems of epistemology. This is fully predictable, because the effects of taphonomic logic increase exponentially with age, and the Pleistocene, accounting for nearly all of human history, is the earliest period being considered

by archaeology (apart from some vague incursions into the late Pliocene). Of all the propositions ever formulated by archaeology about this period, since the first half of the 19th century, at least 90% need to be assumed to be false. This failure rate is unmatched in all academic disciplines, and it has been suggested that it might approximate predictions arrived at by divination of tealeaves or animal entrails. Ever since the entire discipline of archaeology rejected that humans coexisted with Diluvian fauna; that the remains from the Neander valley were those of a Pleistocene human; that Pleistocene people created cave art; or that an early human species had been found in Java, it has promoted nonsensical theories and finds (such as Piltdown) and ignored authentic ones (such as Australopithecus or the Glozel tablets). Its tendency to back false notions and to reject valid ones continues unabated to the present time. In recent decades the discipline has decreed that all humans were replaced during the Final Pleistocene by the progeny of some African Eve — even though there is not one iota of archaeological evidence to support this idea and it was initially based on the fraudulent data provided by Professor Protsch. It has accepted the Pleistocene age of a series of rock art on schist sites in Iberia — even though their petroglyphs are only a few centuries old. It has accepted the claim that a small-brained pygmy species lived in Flores in the final phase of the Pleistocene — even though there are numerous other endemic island populations in the region of fully 'modern' humans, some of which suffer from Laron Syndrome (a congenital deficiency of insulin-like growth factor, resulting from inbred genetic defects of the growth hormone receptor gene genetic defects).

The issue is not that Pleistocene archaeology consists of an endless litany of mistakes; these occur in other disciplines too, though less often. The issue is that the favoured models of Pleistocene archaeology are inevitably based on mistakes, and often these erroneous notions are embraced by the entire discipline. This is important in considering any claim concerning Ice Age evidence: practically all data from the period must be expected to be tainted by biases deriving from these countless epistemological and factual errors. Until the discipline rids itself of this accident-prone status, no information provided by it can be accepted on face value (Bednarik 2013a).

The example drawn on here to illustrate this encumbrance is one of the most-cited phenomena of Pleistocene archaeology, Franco-Cantabrian cave art. Here, the place, the physical nature of the site, is crucial in defining archaeological perceptions of the phenomenon as a whole. The corpus is called 'cave art' because it occurs only in limestone caves. It is hard to think of any other specific Pleistocene phenomenon whose definition is more profoundly locked to a highly specific *definition of place*. Therefore all explanation attempts we have seen refer in some way to the concept of a particular class of place. These have included vision quests, shamanism, hunting-magic rituals and a great many other interpretations, inevitably based on one overarching assumption determined by the apparent choice of site (its Crucial Common Denominator of the phenomenon category). Taking a series of art traditions underground to exclusively apply them in dark, forbidding spaces, often in places of difficult access, demands an explanation of profoundness, conjuring up notions of sacred and secret rituals. In some way, all explanations offered for these many cave-specific art traditions reflected the deliberate choice of placing their products in subterranean spaces.

All of this is probably a mythology of scholars smitten by the perceived profundity of their subject. The first warning signs that something seems to be wrong with this parody of an explanation are that there are no other known occurrences of rock paintings or shallow engravings, which is what the cave art consists of, that are of the Pleistocene — but there are earlier traditions of deep percussion petroglyphs, as modelling within taphonomic logic would predict. The only rare exceptions of finding painting or shallow engraving traditions from the Pleistocene are where 'fluke conditions' apply, such as paint traces surviving under skins of mineral accretions (carbonate, oxalate or silica skins), or in limestone caves which offer highly unusual preservation conditions. Secondly, it seems rather odd that such societies, many of which may have been culturally unconnected, would share this strange predilection for placing all of their rock art inside caves. In fact, when we consider that there are much less than 10,000 such motifs known across western Europe, it would appear that much less than one image was

produced per year (Bednarik 1986). It seems absurd that such a lengthy series of traditions could have survived on that low frequency of being externalised. Again it appears more appropriate to look for a systematic taphonomic bias: the art occurs in caves because that is the only environment permitting its preservation.

However, there are far more decisive factors to be considered. Two types of this cave art are capable of providing sound empirical data about the age of the artists, finger flutings (Bednarik 1986, 1987; Sharpe and Van Gelder 2006) and prints or stencils of body parts (especially hands; Guthrie 2005). The comprehensive metrical evidence shows unambiguously that the finger flutings in European as well Australian caves are largely, and often exclusively, of children, sometimes even of small infants. More decisively still is the data derived from hand stencils: of those of 30 Palaeolithic caves of western Europe, 92.5% are by children under the age of 17, and not a single one is by a person over the age of 20 (Guthrie 2005). There is one more secure index of the age of the cave visitors: imprints of human body parts on soft cave floors and walls. Again the hard evidence is perfectly clear: the sample available today comprises the foot, heel, finger or handprints of no fewer than eleven caves (e.g. Clottes 1997: 31; Clottes and Courtin 1995: 175; Roveland 2000). Although many of the tracks are rather faint and not well suited for metric determination, it is amply evident that the overwhelming majority, certainly over 90%, derive from children or teenagers, and very few can be attributed to adults (Bahn and Vertut 1997; Bégouën and Vallois 1927; Cathala 1949; Delteil et al. 1972; Clottes and Simonnet 1972; Clottes 1973, 1985, 1986, 2001; Clottes et al. 1995; Duday and Garcia 1983, 1985, 1990; Garcia 2003; Garcia and Duday 1993; Kiparissi-Apostolika 2000; Pales 1954, 1960, 1976; Roveland 2000; Ucko and Rosenfeld 1967; Vallois 1962). Some of these cave clay markings, for instance in Tuc-d'Audoubert, are thought to have been occasioned by infants as young as three years. The great majority appear to represent children and adolescents ranging from perhaps 9 to 15 years of age, i.e. they seem to reflect the data derived from most finger flutings; and they are slightly lower than the ages deduced from hand stencils.

To test this finding we might consult the data from another source, portable art of the same many cultural traditions. One form of mobiliary art allowing the determination of approximate age of the artist is paint that has been applied with fingertips on plaques in the form of dots. Again, the empirical evidence shows conclusively that this dot art was made by juveniles (Bednarik 2002). To maintain, in the face of such overwhelming evidence, that perhaps all of it is pure coincidence, and that it does not necessarily preclude the possibility that *all* other cave art was still made by adults for 'adult' purposes, would seem untenable. Science cannot take into account the wishful thinking of archaeologists, it is obliged to weigh the evidence and favour the most economic explanation. In the case of this art corpus, the evidence favours the proposition that a *significant part of it is the work of juveniles*. Science operates by refutation of hypotheses; this hypothesis can be refuted by providing empirical evidence that a significant part of the art is the work of adults. At this time, no such evidence of any description is available, hence all previous interpretations of Franco-Cantabrian art are probably false.

This proposition is supported by other factors the discipline has ignored to maintain its fantasies. The most startling aspect of the Upper Palaeolithic cave art of western Europe is its prominent component of figurative graphic art, which is almost unique in Pleistocene palaeoart. It contrasts sharply with the almost complete lack of confirmed finds of graphic figurative art east of the Rhine. Across most of Eurasia, only two clear exceptions are known (Bednarik 1994b, 2013b), which begs the question: if at least some Asian artists of the period did know how to draw figuratively, why did their societies not develop similar traditions? One possible answer is that non-figurative art is the more sophisticated form, and figurative art was the preserve of young 'graffiti artists' of the Pleistocene. Startling new evidence indicates that among extant traditions using exclusively non-figurative graphic art, children may be capable of producing perfect figurative images when prompted (Sreenathan et al. 2008; Bednarik and Sreenathan 2012). This could then be seen as a 'juvenile' form of expression, and it should have always been obvious

that 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 2003a: 408, 412) and is therefore based on lower levels of perception and neural disambiguation than non-figurative art.

The simplistic evolutionism purporting that figurative art is more sophisticated than non-figurative has no foundation in reality, and yet it has been a guiding light of Palaeolithic art research for over a century. As if the points raised above were not enough to question all established ideas about this corpus of palaeoart, it has also been assumed that this cave art marks the 'origins of art', another fundamental fallacy of Eurocentric Pleistocene archaeology. Not only was it preceded by hundreds of millennia of palaeoart production, especially in other continents, there is in all probability far more surviving Middle Palaeolithic rock art in the world than Upper Palaeolithic (an observation Eurocentric archaeologists seem to have not yet learnt to appreciate). The presumed Pleistocene and early Holocene petroglyphs of Australia alone, all of which are the products of mode 3 technologies (cf. Foley and Lahr 1997), may be over a hundred times more numerous than those of Europe's Upper Palaeolithic (Bednarik 2003b). They are purely non-figurative. By the time we arrive at the absurd claim that the advent of the European cave art marks the arrival of mythological African invaders, the traditional explanation of this corpus has already unravelled. It contains nothing useful to any scientific consideration, and the proposition (Bednarik 2007) that some of it was created by robust Europeans (the kind of humans defined as Neanderthals) only adds to the general impression that *all interpretative commentary about this cave art is severely flawed*.

Constructs of Pleistocene places

Upper Palaeolithic cave art is a classical Pleistocene example of place being crucial to determining archaeological interpretations of a phenomenon. And yet the preceding deconstruction of the traditional models of meaning, purpose or authorship of this example shows that, on the basis of actual evidence in lieu of savants' wishful dreams, all interpretations relating to the place, the cave, are probably false. The same process of deconstruction can be applied to any other Pleistocene phenomenon archaeology has tried to interpret, which shows that the discipline simply lacks the ability to interpret the human past soundly. This is not because such phenomena cannot be interpreted validly, but because the epistemology employed in this endeavour has been largely faulty. If we are to consider the role of landscape or site or place in the human past during the Ice Ages, we need to begin by appreciating, firstly, that the data available to us are heavily contaminated by dogma and falsities, and secondly, that the interpretations we have inherited of such entities are probably not adequate to consider such complex dimensions as spatiality. For instance, if we were to consider the constructs 'Aurignacians' had of caves on the basis of traditional archaeological dogma, we would perhaps be guided by the following notions: that they were related to strong belief systems, e.g. of a shamanistic orientation, introduced by 'anatomically modern' invaders from Africa, connected to secret rituals held in these caves. If this palaeoart were the work primarily of juvenile graffiti artists who resembled 'Neanderthals' rather than 'Moderns', as the actual evidence implies, any pronouncement we would have made about the construct of place as it relates to caves would be an absurdity. It would be the result of our reliance on numerous notions about the context of this art that would all be erroneous. Creating an archaeology of place or landscape on such a flimsy basis would only add to an already overabundant supply of mythological explanations about the Pleistocene. It would only serve to discredit the already very troubled discipline further.

The question that needs to be foremost in the mind of every archaeologist who is interested in what really happened in the past is therefore how to resist the temptation of creating more fantasies based on other unsubstantiated beliefs. The most constructive way to proceed is to resist these epistemic shortcuts, and to accept that veracity of our interpretations of the

human past demand a completely different level of rigour than that which Pleistocene archaeology has so far furnished. The overconfidence this field has developed since the mid-19th century is completely unjustified; the imagery it has painted of early human history is largely a product reflecting contingent prejudices. It does not stand up to scientific scrutiny in most respects.

This raises the issue of how archaeology might attain the status of a science, a topic this author has addressed frequently. Certainly in the case of the Pleistocene period this would not be by adding to the interpretative edifice already erected by archaeology, as would be the agenda of any 'archaeology of place' based on currently held ideas about the humans of that period. The first condition to create *an archaeology of place* for the Ice Ages would be to systematically excise all mythologies from the discipline, and to begin it anew, virtually from scratch, and this time in a scientific format. This would be an immense task, and far beyond the discipline as it currently stands. If we were to construct a landscape archaeology on the present, wholly inadequate paradigm, it could only be as worthless as the puerile models archaeology has presented so far. The ramshackle constructs mainstream archaeology offers are not likely to result in any credible model for the entire duration of the present century.

Some other century, perhaps ...

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