Semiotix Course 2008, The epistemology of Pleistocene archaeology

Robert G. Bednarik

Lecture No. 8. **Contingencies in Pleistocene archaeology**

Frameworks

Epistemology (from the Greek *episteme*, 'knowledge', and *logos*, 'theory'), or the *theory of knowledge*, is the branch of philosophy that deals with the nature and origins of knowledge. It addresses standards or norms for justification and reasoning (including logic and probability theory), ideals of rationality, and the effects of specific philosophies (e.g. empiricism, relativism), among other things. Specific canons of rationality are thought to be time-dependent (Lewis 1929: 253; Mannheim 1929-36: 57; Collingwood 1940: Ch. 6; Laudan 1977: 187) as well as culture-specific (Winch 1970: 97), and some authors have even defined them androcentric conflations biasing science in favor of male ways of experiencing the world. Descriptive epistemic relativism (e.g. deductive inference, causal reasoning; Swoyer 2002) has been improved in recent decades, but remains controversial. As historically and culturally situated creatures we cannot easily step outside our concepts, standards and beliefs to appraise their fit with some mind-independent reality of Kantian 'things-in-themselves'. The trap of extreme relativism, already convincingly opposed by Plato (in his *Theatetus*) can also be avoided by normative epistemic relativism. It holds that while there are no framework-independent facts about the veracity of inference, justification or rationality, there are facts about these variables relative to particular frameworks. Extreme relativism, on the other hand, invites solipsism: if one and the same thing can be true relative to one framework and false relative to another, true for some groups and false for others, there is no truth measure. This was countered by Plato (Fig. 1) thus: either the claim that truth is relative is true absolutely or else it is only true relative to some framework. If it is true absolutely, then at least one truth is not merely true relative to a framework, rendering the proposition apparently refuted.

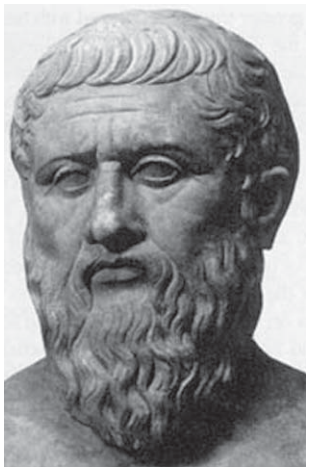

Figure 1. Plato (428/427–348/347 BCE).

A number of philosophers and social scientists (e.g. Quine 1960; Hollis 1967; Davidson 1984) have argued that we can only understand or interpret others if they largely agree with us about what is true, reasonable, justified or the like. The academic endeavor has resulted in a variety of schools, the disciples of which are separated by "logical gaps": "They think differently, speak a different language, live in a different world" (Polanyi 1958: 151). Or to quote Kuhn:

> In a sense that I am unable to explicate further, the proponents of competing paradigms practice their trades in different worlds. … Practicing in different worlds, the two groups of scientists see different things when they look from the same direction (Kuhn 1970: 150).

Some of these branches of the academic project have chosen to operate under a collective umbrella framework, called *science*; others have developed their own various frameworks. Science, today, favors a normative epistemic relativism over an over-simplified absolutism, but demands specific procedures of refutation and repeatability of experiments, and strives for refutable theories cast in terms of causes. After all, quantum theory implies that determinism fails and that objects need not always have determinate locations in space and time or determinate magnitudes (like a particular momentum or energy or spin; cf. Lecture 5). In all of this, the issue of testability of hypotheses is utterly paramount, involving two components: first, the logical property that is variously described as contingency, defeasibility or falsifiability (which means that counterexamples to the hypothesis are logically possible); and second, the practical feasibility of observing a reproducible series of such counterexamples if they do exist. Thus a hypothesis is testable if there is some real hope of deciding whether it is true or false of real experience. Yet the principal epistemological characteristic of archaeology as it has been conducted until now is its poor refutability. Relativism decrees that this does not render archaeology in some way inferior; archaeology is simply an epistemic framework that has chosen to eschew scientific demands in favor of a different framework.

It is perhaps in response to this non-scientific base that the discipline has developed a preference for authority. It

is widely considered inappropriate to challenge its upper echelons, simply because to undermine their eminence would impact on the credibility of the discipline. As they are the columns holding up its structure it is essential for its survival that they not be subjected to doubts. The principle is well expressed in countless reactions to challenges, of which I will cite just two. In Lecture 4 we have seen how archaeologist Dorothy Garrod, when caught attempting to salt the site of Glozel in France to discredit the discovery, at first denied this, but when confronted by several witnesses admitted the act. Many years later she confessed that she had done this "for the honor of the discipline—allowing Glozel to be recognized formally would have damaged too many careers and reputations". In other words, these careers and reputations (beginning with that of her mentor, Henri Breuil) were more important than the archaeology of the Glozel site, and they had precedence over the fate of the site's discoverer, Émil Fradin, whose life was at the time being destroyed by those whose reputations were at stake because of what he had unwittingly uncovered. This is of relevance to understanding the epistemology of the discipline.

The same attitude, that the credibility of professional archaeologists is more important than the veracity of their propositions, can be identified in countless other episodes in the history of the discipline. A more recent example, from my experience, was the reaction of the principal protagonist opposing scientific dating of the rock art in the Côa valley of Portugal. When I expressed my support for blind tests, he retorted in 1995 in the journal *Antiquity* (69: 889) that these were disrespectful, that one should have "consideration for colleagues" and that blind tests were "unethical". This illustrates not only the incommensurability gap (Whorf 1956; Feyerabend 1962; Kuhn 1970) between archaeology and science; it also manifests the self-corrupting paradigm that finds it is more considerate to allow the deliberate deception of colleagues and the public. Presumably it is also 'disrespectful' and 'unethical' to falsify the propositions of colleagues. This dimension of archaeology, its social construction (Berger and Luckmann 1966) of placing credibility above veracity, requires detailed analysis and exposure, because it defines its epistemology and explains the treatment of both dissenters and amateurs. They not only *may* be disproved, they *must* be disproved, even if that were to involve salting a site with fake objects.

On the issue of archaeology's status as a science, as a refutable system of knowledge claims, archaeologists are themselves very much divided. Those who favor post-processualism or post-modernism consider that the discipline is no science, and often even state that it does not need to be one. Many others, in an emotional need to see their discipline as a science, promote a cargo-cult-like scientism: if scientific data are imported it might become a science. Some have argued that archaeology is not fundamentally different from, say, geology. For instance in countering the contention that excavated and destroyed strata cannot be subjected to testing, one argument fielded is that geology (or any of a number of scientific disciplines) removes core samples, an activity similar to the excavation of archaeological sediments. This argument illuminates the relevant epistemologies. When a core of ice, sediment or rock is taken, a relatively homogenous deposit is sampled to gain an understanding of its composition, stratigraphy or other properties. If a second core hole were drilled next to the first, the results would be expected to be identical, hence the process is repeatable. This differs significantly from the project of archaeological excavation, where every square of sediment is expected to be different, have different contents (artifacts, interments, structures, occupation floors, etc.) and properties. If that were not the case there would be no point excavating. So each excavation unit is unique.

Therefore the difference between archaeology and, say, geology could not be more fundamental. In geology, the extent of the resource (the ore body, oil field or whatever) is mapped without exposing it. In archaeology the resource is exposed and destroyed completely, through the excavation of the part of a site explored. Once the sediment has been removed we only have the records: section and plan drawings, finds saved, and sediment samples taken. Most of what the archaeologist says about the resource, the 'cultural' deposit, is no longer falsifiable. We can only accept the report on authority, which in science is unacceptable. Records such as section drawings are merely artistic impressions; they are not hard evidence. As considered in Lecture 7, if we commissioned ten archaeologists to independently draw sections of a complex stratigraphy, we would elicit ten different drawings. This could be easily tested, but as noted, archaeologists are averse to such 'blind tests', which some of them consider to be "disrespectful to colleagues". One would be entitled to observe that the creation of such interpretations of stratigraphy is disrespectful to the discipline and to those who are misled into believing the claims of archaeologists about what they observed. Most stratigraphies at digs are determined by simple eyeballing of sections, and most archaeologists have an inadequate understanding of sedimentological factors and properties. Most would not be able to conduct even simple scientific tests on site during excavations, and therefore their pronouncements of what they saw in the excavation can be questioned. The fact that their determinations cannot be tested, cannot be falsified, is therefore a fundamental concern.

If we add to this already serious encumbrance of the discipline the observation that archaeologists have an inadequate understanding of taphonomic logic (Bednarik 1994, 2007) and its effects on their interpretations, the impact could not be more serious on our confidence in claims about the Pleistocene. This is because, undeniably, the effects of taphonomy increase linearly with antiquity. By the time we arrive in the Pleistocene, most archaeological pronouncements must be expected to be invalid. Which is a major factor in explaining why the history of Ice Age archaeology comprises so many mistakes and controversies.

Social realities

Our next epistemic encumbrance is the fact that the crucial common denominator (CCD; first proposed in Bednarik 1990–91) of phenomenon categories is difficult enough to determine in today's world, but is probably impossible to identify in past cultural systems. Today's objects in our

perceptible world do not exist independent of conceptual frameworks (Putnam 1981: 52):

> If, as I maintain, 'objects' themselves are *as much made as discovered*, as much products of our conceptual *invention* as of the 'objective' factor in experience, the factor independent of our will, then of course objects intrinsically belong under certain labels because those labels are just the *tools we use to construct a version* of the world with such objects in the first place (Putnam 1981: 54, his emphases).

Examples would include biological species (which are often debatable categories, even though we agree that viable reproductive ability is their CCD) or rock types (which often lack an objective taxonomy). To then extend an epistemologically questionable practice to an epistemologically challenged field such as Pleistocene archaeology is to court disaster. Consider, for instance the intricate stone tool nomenclatures we have invented, on which our hypothetical cultural categories depend, and on whose veracity much of Pleistocene archaeology stands and falls. Does anyone seriously believe that these entirely arbitrary and etic stone tool types are real? Do we believe that one hypothetical 'Aurignacian' person (there is no proof that such a group actually existed, as a tribe, nation or ethnic entity) said to another: "Pass me the keeled scraper, this waisted blade is unsuitable for making this wooden thingy"? Or that he exclaimed: "Look what a lovely Acheulian handaxe I just found in the streambed! I can knap it into at least six of those Abri Audi points that have been so fashionable lately." All of the designations and taxonomic units of Pleistocene archaeology are simply inventions, of relevance in one framework, false in many others, and almost certainly false in the cognitive framework of the people of the time when Aurignacian-type tools were made:

> Quite literally, men of those days lived in a different world because their instruments of intellectual interpretation were so different (Lewis 1929: 253).

Searle's (1995) illumination of social realities distinguishes between the brute facts of an object's intrinsic characteristics and those that are observer-relative, or 'institutional' facts. For instance, an object may be made partly of wood, partly of metal. Its property of being a screwdriver exists only because the person who makes or uses it represents it as such. Precisely the same applies to an object made in the Pleistocene; it has factual properties, and socially constructed, observer-relative properties. However, there is no evidence that the latter are shared between the ancient maker and user of the object, and its modern-day archaeologist interpreter. The term *déformation professionnelle* refers to this issue: professional training also results in a distortion in the way the world is perceived. Confirmation bias (Wason 1960; Evans et al. 1983) can only add to the sophistry. Observer-relative definitions, attributions and claims about the distant human past are clearly not in themselves of scientific utility; they need to be subjected to metamorphological analysis, which so far has not occurred in a systematic fashion.

But again, this does not automatically show that all of Pleistocene archaeology is nonsense; it merely shows that, on the basis of reasonable probability, a certain proportion should be assumed to be false. According to Kuhn (2000: 30), scientific revolutions occur through "change in several of the taxonomic categories prerequisite to scientific descriptions and generalization". However, correcting taxonomic categories in Pleistocene archaeology would not be an easy task. Firstly, there is the intransigence of the discipline to contend with; and secondly, how would one set about creating a superior taxonomy? This, of course, would be the subject of a separate series of lectures. Here we are only concerned with the much less ambitious project of explaining how metamorphology would need to approach these matters.

To select from the myriad misconceptions of Pleistocene archaeology just one for a representative analysis, let us consider the common conception that Paleolithic cave art is dominated by zoomorphs (Fig. 2). Bearing in mind that animal images from that corpus are in fact outnumbered four or five times by apparently non-figurative motifs, the question arises why there should be a conception among the public, and even among archaeologists, that animal images prevail numerically. There are many palaeoart traditions around the world whose iconography is dominated by zoomorphs, so this is not at all a variable defining the Franco-Cantabrian cave art. Such traditions include European (e.g. Spanish Levantine), African (e.g. San, specific Saharan cultures) as well as other examples, including European rock art of

Figure 2. Paleolithic zoomorphs, Lascaux Cave, France.

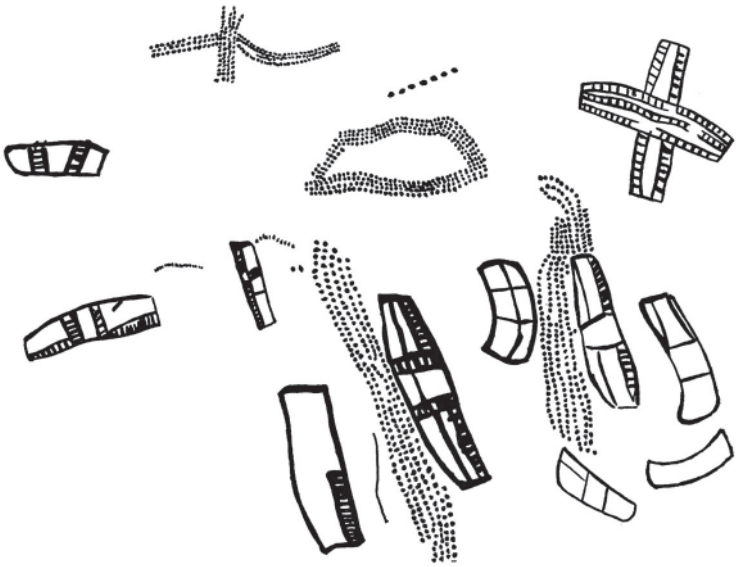

Figure 3. Sample of non-figurative Paleolithic rock art, popularly known as 'signs', from the cave of Castillo, Spain.

recent millennia. But by far the most specific type of motifs that are exclusive to the Upper Paleolithic corpus are the so-called signs, which are repeated many times (Fig. 3). Their meaning remains unknown and they have attracted far less attention than the zoomorphs. These occur in no other rock art tradition; hence they are reliable cultural markers. And yet there is a perception among archaeologists that zoomorphs are the prime indicator of Paleolithic rock art. This has led to many unsuccessful searches for Pleistocene rock art across Eurasia and even in North America, including a tendency to label any animal imagery in Eurasian rock art as having to be of the Paleolithic (Breuil 1952; Okladnikov 1959; Mori 1974; Balbín et al. 1991; Kohl and Burgstaller 1992; Molodin and Cheremisin 1993; Zilhão et al. 1997; Bahn et al. 2003).

How did this misconception arise? While it is obvious that publications tend to overemphasize the figurative content of this cave art, more subtle factors may also be at work here. Judging from the response patterns of visitors of publicly accessible cave sites, the public prefers that part of the art it believes it 'can identify', and has rather less interest in 'un-interpretable signs'. This resembles the reaction of children when viewing rock art panels. It seems that either researchers react in the same fashion and form priorities on that basis, or they are good judges of what the public appreciates and simply adapt their priorities accordingly. While this preoccupation no doubt curries favor with the public, it is detrimental to research. From a scientific perspective, the non-figurative component is likely to be the more important. As the apparent part of a communication system it is much more culture-specific than the invented 'styles' of researchers, because figurative images appear to communicate across cultures—which is precisely the point. So we tend to ignore the culture-specific motifs in favor of what we think we can comprehend (in spite of being historically and culturally situated creatures that cannot correctly identify alien iconographies), and have instead invented 'styles' of animal images as a means of defining 'cultural identities'. Meanwhile, the complex communication systems of the Upper Paleolithic remain unexplored, even though we know that all symbol systems (be they computer languages, conventions for diagrams, styles of painting) influence perception and thought (Goodman 1978). In short, archaeology fails to clarify; it only serves to muddle issues that would be much better left to *semiotic study*.

This example using the incidence of iconography is only one of many I could choose from. In Lecture 6 we began exploring the generic reasons why paradigms of Pleistocene archaeology essentially have to amount to distortions of what really happened in history. There are myriad factors, to be untangled by metamorphology, mindful of the contingency of our modes of thought and evaluation. The most parsimonious system of modern science, normative epistemic relativism, concedes the lack of framework-independent facts about general veracity, but preserves the veracity of inference, justification or rationality relative to specific frameworks. Therefore the knack is to rank the frameworks themselves according to their validity. The relativist admits that he inhabits one of them, and thus acknowledges that his claims are only true relative to it. The extreme relativist, or postmodernist, courts a solipsism capable of accepting that the Sun rotates around the Earth, because "everything is only a social construction".

The normative epistemic relativist, then, needs to ask: what is the relative epistemic strength of the received knowledge of Pleistocene archaeology as a framework of knowledge claims? This, of course, is the subject I have sought to address in this series of lectures, and it has become evident that this is a comparatively poorly established framework. Indeed, in comparison to some of the best-performing disciplines, this field seems almost lost in the wilderness.

Towards an epistemology of Pleistocene archaeology

As an epistemologist of archaeology I investigate where the knowledge archaeologists believe they possess originates, and what its intrinsic nature is. If I were to do the same in a field of science, such analyses would be welcomed as providing useful testing of frameworks. But in archaeology, because of its great dependence upon authority, such attention is not only unwelcome, it is vigorously discouraged, and I know from my experience that it can incur the wrath of much of the discipline. This is in fact one of the most fascinating epistemological facets of the field. While favoritism and nepotism are encouraged, through a whole raft of practices (e.g. to be admitted to a lucrative club of archaeological consultants, one is required to be nominated from within, i.e. by existing members), critical analysis is frowned upon. Archaeology simply does not wish to confront its epistemology, and it implicitly rejects the principles of a meritocracy.

Should such a discipline be supported by society, by the public and by public institutions? If we consider the

primarily political content and thrust of the discipline, its neocolonialist basis and nature, its practices of destroying cultural heritage sites, and its unhealthy curatorial aspirations and monopoly-forming practices—what, precisely, are the benefits archaeology offers society? The endless series of blunders over the last couple of centuries, the always controversial 'explanations' of the past are not persuasive factors. The books and television films for the public may be good entertainment, but their frequent lack of veracity warrants improvements, not endorsement. On balance, one might be tempted to advocate the discipline's closure. Or, perhaps, that it should revert to its previous state, a hobby or scholarly interest of gentlefolk researchers, people who lack the missionary zeal of modern Pleistocene archaeology—and who have, as history shows, performed rather better than their modern 'professional' counterparts.

But a thorough epistemological analysis of the field also shows that many researchers in Pleistocene archaeology do make extraordinary efforts to provide quality work. Moreover, in spite of the many shortcomings, the discipline would in my view have the potential to improve, and to improve quite considerably. In this series of lectures I have canvassed some of the potential improvements, especially a destruction of dogmas and their replacement with weak propositions of falsifiable formats. Pleistocene archaeology at present knows very little of scientific veracity; many of the claims it has so far presented were precipitate, and the picture we have of Ice Age hominins is so profoundly inadequate because the kinds of models that grow, mushroom-like, in this dark are often only designed to create reputations and careers (Henneberg and Schofield 2008). It is this system that needs to be dismantled, not the discipline per se. It needs to be fully appreciated that, in a few centuries hence, our present knowledge of our Pleistocene ancestors will look just as inconsequential as such knowledge 200 years ago appears today. It is from that historical perspective we need to approach this subject. And we need to appreciate why it is that Pleistocene archaeology makes so little progress, or merely stands still, while the hard sciences progress at a breathtaking pace

Consider just two of the newest disciplines, both only half a century old: plate tectonics and ethology. In a matter of decades they progressed from embryonic stages to incredible complexity. Or consider genetics: after its introduction by Mendel (1866) it remained stagnant, if not ignored, for much of a century, but look at its sophistication today! By comparison, Pleistocene archaeology and palaeoanthropology are both static: if skeletal remains of a small primate are found on the island of Flores today, the discipline erupts into a cacophony of competing interpretations, ranging from a gibbon to modern human, and any conceivable intermediate form. As noted, this is the same as an astronomy that, in our time, still argues about whether the Sun or the Earth rotates around the other. As we saw in Lecture 4, anyone can see that the Flores creature was a primate, and if the experts of the world form opinions representing the entire possible range, it becomes starkly apparent what happens when authority in a non-falsifiable discipline is challenged. Not only do we now have a proposal that the specimen may have been planted in the sediment, presumably as part of an elaborate hoax (Henneberg and Schofield 2008), as well as the documented academic misconduct and 'skullduggery' relating to this discovery; there is also the spectacle of a bitter battle between competing schools of thought. This is precisely the same pattern that marred previous palaeoanthropological discoveries. Reliance on authority does not seem to be the answer.

The question arising from such observations is: what is it that renders Pleistocene archaeology so accident prone and unreliable? Today I am probably the most avid debunker of archaeological blunders and mistakes, and I have been asked whether there is a system in how I choose foci of interest. The answer is that taphonomic logic tends to identify the most mistake-prone areas quite readily, and it is then merely a question of homing in on false claims on the basis of data. But there is one other factor, which is related to a false logic implicit in the extreme conservatism of the discipline. Because of the impossibility of subjecting most archaeological claims to systematic refutation, this extreme conservatism has developed as a defense system. It led to a reliance on, and preference for, authority, which is itself already an epistemological impairment. But more relevantly, it fostered a specific brand of minimalist dogmas. These are based on the assumption that to protect the paradigm against unsound challenges to what provides a 'semblance of certainty', it is best to resist changes to a dogma. The more we resist, the closer the process resembles an inverted form of falsificationism. In other words, if the dogma says humans at a certain time acquired a specific technology or ability, any data supporting an earlier introduction has to be resisted strenuously, until the evidence becomes overwhelming. This protects the received knowledge against frivolous claims, which have to be opposed vigorously. Claims that do not challenge the dogma, on the other hand, could be readily accepted, irrespective of whether they are false; they could not damage the dogma.

This provides a key to understanding the false epistemology of Pleistocene archaeology. It shows that anything can be proposed and will be accepted, provided it leaves the dogma intact: *compliance with dogma, not veracity, is the criterion of acceptance*. But therein lies the problem: the dogma is itself likely to be false. To see this, we need to recall (see previous lecture) how the discipline is entirely dependent upon a random historical sequence of discoveries: if that sequence had been different, our received Pleistocene archaeology also would be very different. To defend a randomly acquired model on no basis other than its historical precedence is demonstrably fallacious. Moreover, the practice of retreating as reluctantly as possible from such an incomplete model is logically unsupportable. It argues for a top-down retreat strategy—a regression from a *contingent* state of limited validity, instead of starting with the null hypothesis that sampling errors are inherent in all archaeological work.

A better alternative than adherence to a flawed model would be to start with the null hypothesis that our ancestors of around 5 to 8 million years ago were not fundamentally different from chimps or bonobos, and that since then they

developed into what we are today. Since we are not certain how this development occurred, it would be judicious to assume that, half way through, we might expect to find creatures about half way between apes and us. In a purely anatomical sense, that is indeed what we are finding, and it is generally agreed that physical evolution of hominins occurred fairly gradually, over the entire period. Similarly, encephalization (enlargement of the brain) was undeniably a gradual process. Why, then, is it that Pleistocene archaeology assumes that cognitive or intellectual development is almost entirely a feature of the last third of the Late Pleistocene (the last 40,000 years of it)? Bearing in mind the enormous cost of encephalization to mothers and whole societies (consequences of the need to expel large skulls through the birth canal, long-term dependency of infants), it is illogical to maintain that these large brains were not used. It is also evolutionary humbug: like any other evolutionary change, organ size must be selected for; it does not just increase randomly. So instead of insisting, as most Pleistocene archaeologists do, that pre-modern hominins were fundamentally primitive, because the dogma demands so, it would be far more realistic to postulate that frontal lobes were used for thinking in proportion to their size. This makes biological sense, but it is entirely irreconcilable with the model archaeology offers us. Therefore the conundrum seems to be due to the archaeological dogma more than any other factor: it simply does not fit. It claims the evidence for earlier cognitive sophistication is too patchy, too sparse.

But why should one expect the dogma to fit? It is probably false inherently; it is a minimalist interpretation of largely distorted data; it was arrived at by non-random sampling and by inadequate methodologies. Moreover, bearing in mind the effects of taphonomy, the available, intrinsically incomplete record is precisely what is to be expected, especially from the earliest periods of human history.

In other words, the dysfunctional relationship between the models of Pleistocene archaeology as disseminated by the great journals and institutions on the one hand, and those derived by more scientific and more critical approaches on the other is fully predictable. It illustrates the difference between grandiose story telling and the more sobering probability scenarios of science. There can be no science without the facility of falsification or testability, yet there are several fundamental impairments preventing the testing of archaeological propositions:

1. The principal method, excavation of sediments, cannot yield falsifiable knowledge claims, because the resource itself is destroyed in the process. Therefore all claims about what excavation has shown are based on authority alone, which in science is not acceptable.
2. Propositions of archaeology about what happened in the past cannot be falsified directly by other purely archaeological claims (although in many cases they may be susceptible to indirect falsification by scientific methods).
3. Archaeology cannot produce predictive postulates (e.g. about societies that no longer exist) capable of being subjected to testing.
4. The classes of nomenclatures or taxonomies invented by archaeology, especially Pleistocene archaeology, are non-falsifiable; they are etic and free-standing constructs. This applies, for instance, to artifacts, motivations, beliefs, intentions, social models and practices.
5. Archaeology cannot, with any credibility, cast hypotheses in terms of causal relationships.

The inability of archaeology to accommodate the canons of science is illustrated by the inevitable misuse and misinterpretation of scientific data or modes of discourse when these are introduced. For the purpose of illustrating this point with a randomly selected example, consider the use of Voronoi diagrams or Dirichlet tesselation in archaeological theory (although here it is mistakenly referred to as Thiessen polygons, an application of the principles to meteorology). For instance Clarke (1978: Fig. 116) treats Iron Age sites as Voronoi sites, which means that each of these occupation sites has a Voronoi cell consisting of all points closer to that site than to any other. This is in principle nearly correct, but the model's application in archaeology can only give rise to falsities. In Clarke's example of Iron Ages sites, the following factors show this:

a. We can never consider all Iron Age camp sites in a given area, but only those that have survived, and only those that have been located so far.
b. Unless we can consider only those that coexisted, comparing the geometric distribution of all known Iron Age sites would serve no useful purpose.

This example shows how taphonomic logic and metamorphology debunk an archaeological misapplication of a scientific method that has numerous valid applications in other disciplines. Its denouement can be extrapolated to most of Pleistocene archaeology, demonstrating that much of it amounts to a mythology (e.g. Bednarik 1992, 2008).

At the end of my deliberations I would like to return to the question I posed in Lecture 2: does archaeology understand its role in needing to explore how the cognitive niche of hominins might have been established? Does it appreciate this need if we are to bring any light to bear on how human constructs of reality (which in the final analysis determine all epistemology) came into being? And can we confidently rely on this discipline's capability of extracting the kind of information needed in such a quest? All factors considered fairly, I think we would be obliged to point to the extensive list of epistemic deficiencies I have sought to canvass.

And there is one more final point to be made here: academic freedom is a Trojan horse when it is applied selectively (e.g. to exclude amateurs or indigenes) and when the knowledge it yields is used specifically to enhance the power of the state. Indeed, if one had a choice between the competing opinions on some matter of two archaeologists of exactly identical knowledge, one being independent and one being dependent, one would still have to prefer the independent researcher's view.

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