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A UNIFIED THEORY FOR PALAEOART STUDIES

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Abstract. Like archaeology, palaeoart studies has been based largely on uniformitarianism: the geological theory explaining phenomena as being the result of extant forces having operated uniformly in the past. This is argued to be inadequate as an epistemological model for the discipline. Taphonomic logic is explained as a superior model forming the logical core of metamorphology, a theoretical framework for testing the admissibility of hypotheses. This framework is argued to provide the discipline of palaeoart studies with the unified theory needed to facilitate its further development.

Introduction

The purpose of this proposal is to take a fresh look at studying palaeoart, and to create an analytical tool hopefully suitable to take the discipline into the future. For as long as the interpretation of palaeoart has been attempted, it was generally based on a loose combination of iconographic evaluation and a form of uniformitarianism introduced by archaeology. Uniformitarianism itself derives from geology, where it represents the theory that geological phenomena can be explained as the results of still observable processes having operated uniformly in the past. Archaeology has adopted this idea as its general theory (Cameron 1993), which has led to a reductionist discipline in which taxonomic styles are determined subjectively (see preceding paper by Mario Consens), ethnographic or replicative analogies dominate interpretation, and the statistical definition of artificial taxonomies is generally seen as providing a valid basis of interpretation.

Since uniformitarianism as applied in geology seems to provide a sound unified theory in that discipline, the reasons for its failure in archaeology need to be considered. While both disciplines do deal with the past, the major difference between them is that the geological past is determined entirely by empirical laws. The human past, on the other hand, is not. Like history, it may be assumed to have been affected by human decisions in many cases, by cultural, political, social, cognitive, religious and other choices that are not readily accessible to procedures of mechanistic reductionism. Hence a universal theory may serve one discipline very well, but that does not necessarily warrant its application in another discipline. This applies particularly to the human past, where choice and conscious decisions are legitimate variables.

There are significant epistemological problems with the orthodox form of archaeology, but it was introduced into palaeoart research, which was traditionally seen to be a sub-discipline of archaeology (Lorblanchet 1992). It is in

the particular field of rock art studies that the shortcomings of this form of archaeology have become most obvious, because the application of many of its procedures to rock art can be shown to be false. This raises the question of how useful or valid the theory of uniformitarianism is in rock art research, and by extension in archaeology generally.

Representativeness of ‘samples’

It is my contention that, while our discipline is extremely well served by the kind of new techniques and approaches developed in recent decades, their effectiveness will depend largely on having an all-embracing, general or unified theory within which these approaches can be deployed. Such a theory should be epistemologically sound and provide a framework within which data can be located and tested by refutation.

One of the greatest heuristic stumbling blocks of orthodox archaeology is that it tends to treat evidence — or what it calls the ‘archaeological record’, an essentially meaningless concept (Bednarik 1994a) — as a kind of random sample, as if it amounted to a *representative* selection of variables defining a particular culture. In the case of rock art, this is rendered particularly incongruous by two factors: that major rock art sites offer usually cumulative records in two-dimensional space, and that the scientific dating of their chronological components continues to remain extremely difficult (Bednarik 1990–91, 1996). In other words, the rock art at such sites may belong to different periods, the artists of which contributed to the same corpus, perhaps reacting to pre-existing art at the site. Traditional archaeological approaches are practically pointless here, and the determination of what actually constitutes a valid sample is often extremely difficult, if indeed possible.

Expressed in epistemological language, there is a dependency relation called a supervenience: one set of properties, forming the historical event, is supervenient on a

second, represented in the selected sample. There could not be a difference in the first without there being a difference in the second, though there could be a difference in the second with no difference in the first. Thus the relationship of the two sets of properties has to be explored by alternative means, not by direct deductive reasoning.

Faced by these very valid objections, the archaeologist tends to retreat to the argument that, irrespective of a site's chronology or cultural attributions, one can still describe what is on the rock, in an objective fashion. But even this is a fundamental fallacy. Pure description of the present state of a rock art site is not only subjective, its statistical analysis is almost meaningless (Bednarik 1995a). Since diverse traditions may have contributed to the assemblage, the samples of each of them are not only themselves culturally distorted, they cannot be readily identified — contrary to some archaeological claims. Moreover, they are so distorted taphonomically that archaeological pronouncements must be expected to be misleading (Bednarik 1994b): we are likely to recognise a much greater portion of that part of the art that is the more recent, than of the older, taphonomically more distorted part. It is to be emphasised that many factors contribute to these crucial distortions, they are all systematic, and most of them are of cumulative effects. It is therefore a massive fallacy to assume that one can describe typical samples of any entity in rock art.

Let us briefly dwell on this argument. Generally speaking, the degree of taphonomy experienced by rock art is related to its age at a given site. But if we cannot effectively separate chronological entities at the site, as is often the case, we cannot even estimate the degree of taphonomy any given motif or group of motifs may have been subjected to. So a crucial variable is unknown, while affecting another crucial variable. In effect, in describing rock art one describes very distorted samples of entities that could not even be validly described if they were not subjected to these systematic distortions.

There are still other problems with the supposedly 'neutral' description of rock art, which I will only briefly touch upon. For instance, theory-free, pure description is not possible, but perhaps more relevantly, the use of statistics is highly questionable in rock art. Apart from the obvious restraints posed by chronology (i.e. we often do not know what types of figures belong to a specific tradition) and taphonomy (i.e. we observe only residue samples, and nearly all of the factors determining what survives or what survives best are not related to culture), there is the taxonomic barrier. It is simply impossible to subject the contents of a rock art corpus to statistical treatment unless the 'analyst' creates a taxonomy of the motifs (Bednarik 1990–91). In the case of pre-Historic art, for which we have no valid motif taxonomies, the researcher creates categories that are supposedly etic (but in fact also relate to his or her cultural, cognitive or academic conditioning); they are probably in most cases not those the creator or original consumer of the art concerned would perceive (emic). Manipulating statistical indices obtained from this reflection of the analyst's own ontology may provide valid information about his or her perception, but it tells us nothing reli-

able about the art, its artist or its former consumer.

The distinction and definition of the neologisms *etic* and *emic* are not as straightforward as they may appear, as is also evident by the debate between Kenneth Pike and Marvin Harris. Pike (1954), a linguistic anthropologist, coined the terms as an analogy to the words 'phonetic' and 'phonemic'. According to him, the emic perspective is that of members of the given society under study, in the same way that phonemic analysis is concerned with the intrinsic phonological distinctions that are meaningful to speakers of a given language. Therefore the native members of a culture are the sole judges of the validity of an emic description. The etic perspective, on the other hand, refers to the extrinsic concepts and categories that have meaning to the outsider observer, including the researcher. While this distinction is a valuable tool of epistemology, there are various pitfalls. For instance, the etic observer also must have an emic dimension, derived from conditioning of various types, among them academic. It therefore seems that the less academically conditioned the researcher is, the less rigorous s/he may be, but the more academically conditioned s/he is, the less capable of securing etic veracity s/he is likely to be.

These examples show us some of the severe limitations applying to traditional, mainly archaeological approaches to rock art. These approaches, essentially, lead nowhere in a scientific sense. I would like to present an alternative here.

Introducing metamorphology

Although taphonomy was introduced in palaeontology sixty-six years ago (Efremov 1940), it took thirty or forty years for archaeologists to realise its significance to their discipline, but in adopting it they promptly misused it by applying it as actupalaeontology, which is precisely what Efremov originally intended to replace with it (cf. excellent discussion in Solomon 1990). Similarly, the adoption of taphonomic logic in rock art research was long delayed, although it is obvious that any scientific access to rock art is contingent on the *coherent identification of that part of the extant characteristics of the evidence that is not the result of taphonomic processes* (Bednarik 1990–91). In fact, until very recently rock art was studied entirely by naive, non-taphonomic means: as if it survived as a true reflection of what was once created.

The acceptability of what this discipline has established in terms of interpretation of rock art has been challenged by my introduction of taphonomic logic (Bednarik 1986, 1994b). It demonstrated decisively that many of the traditional explanations of palaeoart could not possibly pass the taphonomic acid test. In fact, some long-accepted beliefs about rock art looked decidedly naive the instant they were subjected to even the most cursory taphonomic review. This suggests that such logic would be a superior test in deciding whether a hypothesis could be expected to be of scientific standard. It does not, I emphasise, determine whether a hypothesis is right, but it does identify those hypotheses that should never have been considered seriously, because they are not amenable to refutation, and thus to scientific

testing.

Once we realise the effectiveness of taphonomic logic as a preliminary tool in grading hypotheses, it becomes obvious that it might offer a general theoretical framework for our discipline. The problem, essentially, is that there is a gap between the reality of what really happened in the distant past, and the record of it *as perceived by the individual researcher today*. If we are to bridge this gap, so that we may see how the perceived record relates to what happened, we need to understand the processes that account for the gap. This should have always been self-evident in archaeology, and that any archaeological interpretation that ignores this gap should be expected to probably be invalid. Taphonomy does account for some of the processes or dynamics accounting for this gap, namely those related to preservation, and in this sense it is perhaps the easiest to appreciate. However, there are many other factors that distort what the individual researcher perceives. These include the way data are collected, stored, disseminated and interpreted. They include the biases of the researcher, of specific schools or the discipline as a whole, and many other external factors that have a bearing on how the so-called evidence is individually perceived as well as interpreted. For instance, the researcher's own limitations are a powerful factor in how evidence may be reported. These may be limitations of knowledge or of language. I have shown repeatedly that ignorance of researchers concerning existing data, language barriers, and biases through preconceived models have not only severely influenced hypotheses and their defence, they have also stifled the flow of information in palaeoart studies and general archaeology (e.g. Bednarik 1992, 1995b, 1995c, 1999). The academic system itself, which is so crucial to the dissemination of knowledge, can also stifle that very process and act as a filter in quite a number of ways. All of this can cumulatively add up to such distortions in dominant models that these bear very little resemblance to what historically happened in the past. This is because many of the distortions are not random, *they are systematic*.

To correct this we need to be able to understand the nature and effects of these distortions, taphonomic and otherwise. This would provide the kind of framework we require to account for the gap between what happened in the distant past, and the abstraction or reified construct of it as it is perceived by the individual researcher interpreting a specific sample of the remaining evidence of this event. I have called this framework *metamorphology*: the logic of how the form of the evidence is altered to become reified constructs in time and space (Bednarik 1995d). This is the unified theory our discipline requires to deal scientifically with the evidence available to us.

One of the principal factors of metamorphology is taphonomy, which can be explained in a quasi-quantifiable fashion, at least in the form of integral functions (Bednarik 1994b). Other factors are more difficult to deal with. I have taken an interest in the effects of lack of relevant knowledge, not to be critical of individual researchers, but purely to illustrate real, documentable and hopefully quantifiable dynamics within the discipline. The ef-

fects of false hypotheses and of their ardent defence need to be understood if we are to obtain a valid reflection of metamorphology. I cannot see one good reason why the dynamics of knowledge acquisition or academic power politics in a discipline should be immune from scholarly analysis. Our discipline often does not hesitate to study the taboos of the societies we investigate, be they extant or extinct groups, so why should the study of itself be taboo? These are realities, they have significant effects on the discipline, and these dynamics need to be understood like any other process contributing to our knowledge. Therefore this aspect should be studied as carefully as any other that contributes to metamorphology. Our discipline would be in a sorry state if such research would be discouraged because the 'reputation' of individuals is considered to have precedence over the integrity of the discipline (as has occasionally been suggested).

Even without any level of quantification, metamorphology has already shown how effective it is in eliminating absurd propositions from consideration. Some of the most popular and durable notions about palaeoart have already been debunked, for instance the idea that 'art' began with a Big Bang with the Aurignacian in France, or the idea that Pleistocene rock art was restricted to caves (Bednarik 1986). Many other entrenched models are likely to fall victim to metamorphology even without developing this science, by simply applying its logical framework to them. This should prevent us from wasting any more precious time or effort on hypotheses that may look persuasive enough, but have in reality no scientific, refutable or testable basis. Once this unified theory has been refined through the study of the phenomena and processes that contribute to metamorphology, it will provide us with an entire framework of introducing and pre-testing information in the discipline scientifically. Every part of this framework will be refutable, and by its very nature it will be of a thoroughly transparent epistemology: it directly addresses the question of how we acquire and manipulate our 'knowledge' and strives to illuminate its dynamics.

Conclusion

I expect the introduction of this scientific framework in palaeoart studies to be a rather painful process for the discipline. It should be remembered that, among all scholarly pursuits, our field of inquiry may well have the highest ratio of secure information to number of interpretative hypotheses. The number of such hypotheses we have in rock art is incredible, whereas the amount of truly convincing, hopefully objective data we have is minuscule. Nearly all the interpretations we have of palaeoart, especially of rock art, are highly precarious. They are often without a refutable basis, dealing largely with subjective impressions, meaning, stylistic claims of all sorts, unsupported chronologies and the like. What many of them have in common is that they are not refutable, i.e. they are couched in such vague, abstract or idiosyncratic terms that they cannot be tested effectively. Indeed, when it is attempted to introduce scientific methodology such as 'blind tests' in archaeological interpretation of rock art, practitio-

ners may object to them on the basis that such procedures are 'unethical' and lack 'consideration for colleagues' (Zilhão 1995: 899). This profound opposition to testing and scientific procedures shows clearly enough that archaeological approaches can only retard epistemological progress in rock art science.

The alternative approach I have presented here differs significantly from the traditional. Where archaeology attempts to give us certainty, its scientific alternative, metamorphology, will create *systematic uncertainty*. It will provide us with complex multiple-scenario interpretations of data, with almost instant probability profiles — weeding out hypotheses based on false crucial common denominators of phenomenon categories (Bednarik 1990–91) very effectively. Unfortunately, this means that the vast majority of existing hypotheses about palaeoart will be rendered obsolete, either because they will become profoundly improbable, or their probability ratings will be so poor that no-one will be prepared to be seen supporting them. Therein lie both the strength and the weakness of metamorphology.

Could metamorphology ever become just another dogma? This is difficult to predict, but I tend to think not. It is, after all, not itself a theory, but simply an alternative framework of presenting and considering data. Since its fundamental logic is obviously valid, whereas that of traditional archaeology is flawed (as can be demonstrated), it ought to be an improvement. It offers us the means of creating a unified theory of palaeoart studies, and once this is convincingly developed in our discipline, archaeology may well follow our example. For the first time in the relationship between orthodox archaeology and rock art research, in which the latter was traditionally seen as the poor relative, rock art science will lead and archaeology, hopefully, will follow.

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