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Concerns in rock art science

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Some archaeologists take an interest in rock art. This paper is intended to facilitate their appreciation of some of the difficulties that may be encountered through introducing archaeological modelling dynamics and epistemology into rock art science. The collaboration between archaeologists and rock art scientists is contingent upon communication: scientists seem sometimes incapable of explaining their epistemological premises effectively to archaeologists, and archaeologists have unrealistic expectations of scientific methodology, which can lead to over-interpretation of results, and to disillusionment. There are a variety of other problems, often apparently attributable to a lack of communication. This paper is an attempt to communicate, and to clarify some issues that I feel need to be canvassed.

These rather brief and superficial comments are not intended, in any way whatsoever, to belittle the great contribution archaeologists have made to rock art science. I would be very disappointed if my comments were seen as negative or discouraging. Rock art science is a field in its own right, and in the same way as archaeologists or astronomers or chemists surely are entitled to clarify their position relative to the practitioners of other disciplines, rock art scientists have I believe the same right. Some of the greatest and most distinguished rock art scientists are of course archaeologists, and especially so in Australia, but it is equally true that many archaeologists are practically uninformed about the field of rock art research. Since there is considerable overlap between these fields, or at least they share certain common borders, it is surely useful to reach an understanding of what each other's concerns, priorities and motivations are. I ask readers to see my comments in this light, and I ask archaeologists to respond constructively.

About petroglyph tools

The stone tools that were used in making percussion petroglyphs have been observed ethnographically, and researchers in Australia and other countries have found them for a long time (Bednarik 1998). At major petroglyph galleries around the world, a fair number of such tools have been studied. Some have even been used in replication experiments (e.g. Savvateyev 1976). Expertise exists in identifying such tools with great confidence, derived from replication work and detailed studies of the tools in question. Just as one tends to find stone tools in hunter-forager occupation sites, or quarry blanks and mining traces in sites of pre-Historic mineral extraction, petroglyph tools should occur at most percussion petroglyph sites. Where later sedimentation has taken place next to carved rock faces and pavements, as is often the case, one would expect to find such tools in stratigraphic contexts. Strata containing them may well comprise datable material (quartz or feldspar grains, charcoal etc.), which could then be used to estimate the time when these tools were used, i.e. when some of the petroglyphs were made. In such situations archaeology could make a valuable contribution to rock art science.

Around the world, many hundreds of archaeological excavations have been undertaken near petroglyph sites, but petroglyph tools have generally not been reported from them (there are a very few notable exceptions, however). This suggests that they were usually not recognised as artefacts and discarded with the spoil material. The amount of damage this is likely to have caused to rock art science is incalculable. Even from the archaeological point of view, these tools would have been the most important archaeological finds at the site, as they would have helped to link its archaeology with its rock art. It is therefore most unfortunate that this lack of knowledge has caused such damage to the research potential of petroglyph sites.

There are numerous cases on record (e.g. many dozens of excavations in one small part of just one valley of Portugal; Aubry et al. 1997; Zilhão et al. 1997) in which a principal purpose of the excavation was precisely the potential of linking the rock art with the archaeological deposit. This was usually attempted on the basis of seeking petroglyphs below ground level, in the hope that they would provide the means of minimum dating of the rock art. At a very few sites worldwide, e.g. in France, Canada and Australia, this did succeed, but it must be remembered that a minimum date does not provide an age estimate, only a *terminus post quem* date. Stratified petroglyph tools, on the other hand, can provide valid estimates of the times when petroglyphs were made at the site. Moreover, they are significantly more likely to be found than petroglyphs below ground level.

So in searching for concealed rock art to secure minimum dating of petroglyphs we have discarded the evidence that would have given us valid dating evidence, in favour of searching, usually in vain, for the ever elusive but inconclusive evidence (Swartz 1997a, b). In the process we have usually destroyed the most important archaeological evidence at the sites in question.

What archaeologists therefore need to know about petroglyphs and the tools used to make them is that they must not excavate at or near petroglyph sites, unless they can secure the support of an experienced rock art scientist who can identify such tools.

About rock art dating

There are a number of approaches available to estimating the age of rock art. Since the advent of 'direct dating', which has been developed over the past two decades, archaeologists have

consistently misunderstood what it is and what it provides (Bednarik 1996, 2002). 'Direct dating' of rock art is incapable of providing actual ages. Rock art scientists cannot 'date' rock art, and if any did claim that they could do this they would be charlatans. All scientists can do is provide falsifiable data concerning the age of rock art. Such data are in some fashion relatable to the true age of a motif, but the precise nature of this relationship is often very difficult to ascertain, and almost invariably misinterpreted by archaeologists. The reason is that these results are offered as refutable propositions, not as some sort of 'facts', which is how archaeologists often interpret them. I offer some examples.

Most 'direct dates' currently available from European rock art attributed to the Pleistocene are radiocarbon dates obtained from charcoal pigment. The only exceptions are the results from the Côa valley (Bednarik 1995a; Watchman 1995) and the radiocarbon ages from soot patches in Chauvet Cave (Clottes et al. 1995). It has been claimed or implied numerous times that these 'direct dates' from charcoal pigment provide secure dating of the painted cave sites in question (e.g. Bahn 1993). Here we have a classical example of archaeologically misinterpreted 'direct dates'. None of these radiocarbon determinations tell us the age of the art they were taken from. Assuming that there is no laboratory error or contamination—and ignoring that about one third of these dates must be statistically expected to be 'false' (i.e. they should be outside the tolerance margins), as well as several further qualifications (Ward and Wilson 1978; Wilson and Ward 1981; Ward 1994)—what such results tell us is that physicists have proposed that the charcoal that was used in the picture is from the wood of a tree that assimilated atmospheric carbon dioxide (i.e. lived) at the time in question. These results do not tell us when the wood was oxidised, nor do they tell us when the charcoal was collected and ground to a pigment. If the radiometric age of pigment did indicate the age of a painting, then red paintings made with haematite would be millions of years old. The same logic applies in both cases. Clearly pigment is usually not of the same age as a painting. Paint is, but archaeologists often confuse the terms 'paint' (or paint residue) and 'pigment'. These are two different terms, and they are certainly not synonymous. Pigment is merely a component of paint (others are solvent, binders, extenders, fillers, and numerous types of incidental inclusions).

To cite another recent example, let us consider the 'direct dating' evidence from rock varnishes, which has been championed by Dorn (1983, 1986, 1993; Dorn et al. 1992; Dorn and Whitley 1984) for many years. There is again no doubt that the evidence is 'direct', in that its physical relationship with the rock art is beyond dispute. However, once again the nexus between the analytical result and the true age of the feature in question is so complex that it seems almost impossible to quantify. After conducting extensive analytical and nanostratigraphical work during the late 1960s and through the 1970s, I arrived at the view that such ferromanganous accretionary deposits, which one often finds covering petroglyphs, cannot be dated by these simple means. My principal objection was that I found a wealth of organic matter in all weathering and accretionary zones I examined (Bednarik 1979). Organic matter is even present in apparently unaltered rock, but its concentration increases greatly towards the surface. This is an open system, and in the case of rock varnish and similar phenomena, we have long known that micro-organisms can recycle such crust material, and that the stratigraphy of such accretions is highly variable at a microscopic scale. Nevertheless, subsequent to my findings, Dorn proceeded to use such deposits routinely for rock art dating, first with his cation-ratio method, and after this became increasingly unpopular (particularly once Watchman [1992] failed to duplicate his results), by AMS radiocarbon analysis of bulk samples. After defending these methods for many years against the criticisms by Watchman, myself and others, he suddenly conceded that all his analytical results are doubtful, and that he had made two critical mistakes in all of the 15 years of his work. He admitted that both of these mistakes were classic errors of how 'the paradigm or mental framework that one operates under can blind a researcher' (Dorn 1996, 1997). At about the same time, Beck et al. (1997) presented their critical review of another aspect of Dorn's work (see Beck et al. 1998; Dorn 1998; Dalton 1998; Malakoff 1998).

Over the past twenty years, many archaeologists have relied heavily on Dorn's results, and I sense that archaeologists are responding to his 'change of perception', as he calls it, by losing confidence in 'direct dating' methodology. This reaction would not be attributable to Dorn's errors, which were entirely predictable (as my 1979 paper shows), but to false expectations of archaeologists. 'Direct dating' does not produce 'better' 'dates' than traditional stylistic or archaeological dating methods, it merely produces falsifiable results, which are thus scientific. This does not necessarily make them better, safer, more reliable or more precise than archaeological/stylistic approaches. 'Direct dating' is not immune to human error, and it has not produced one single 'absolute' date for rock art. It is simply a *different* way of acquiring age information about rock art.

About the taphonomy of rock art

The most powerful theoretical tool developed in archaeology is taphonomic logic. This is not a theory; it is a framework of testing whether a particular archaeological proposition is worth pursuing further. It shares with taphonomy only its name, having been derived from that discipline initially, but the emphasis is on the operative word 'logic'. Rock art, like fossil bones, stone tools, pollen grains and the stars in the heaven, has been subjected to a variety of processes that are usually a function of time (in some complex fashion), and that have led to the present form of the evidence (Bednarik 1994a). One of the characteristics rock art shares with archaeological (and other) remains is that the composition and distribution of specific forms of evidence have been systematically distorted over time, and that this distortion increases with age. It may sound simplistic to say so, but it is perfectly correct to state that the distortion increases to the point where it cancels out the relevance of statistical consideration. In practical terms, statistics of Pleistocene rock art are irrelevant in most cases.

Everything that ever happened in the archaeological past constituted an 'archaeological event'. The probability of any detectable evidence of such an event surviving to the present time can never be 100% or 0%, but it can vary greatly between these two figures. To understand taphonomic logic we best begin by considering that, of much more than 99.9% of all archaeological events, no trace survived for more than one second. Of the still innumerable remaining instances, evidence survives in the long term only for a tiny fraction of one-millionth of a per cent. Of this, only an infinitesimal portion has been recovered by archaeology, of which an even smaller part has been correctly construed. The combined dynamics of preservation, recovery and interpretation all involve biases that are not just massive, they are also systematic in most cases. To treat any recovered sample of such evidence as if it were a random sample of some perceived entity, is a basic fallacy; archaeology cannot recover random samples of *any* class of evidence. Therefore the qualitative and quantitative distortions inherent in preservation, recovery and interpretation need to be accounted for.

The same applies to rock art, which is also a severely truncated and altered record that has been extensively misinterpreted. Taphonomic susceptibilities can create whole profiles of false rock art traditions (Bednarik 1994b) and thereby lead to systematic misinterpretation of corpora, chronological sequences and spatial distributions. For instance, McCarthy's (1988) petroglyph sequence for Australia, based essentially on early deep outlines and late shallow 'intaglios' (filled-in motifs), is

very likely attributable to taphonomic determinants. The filled-in figures are inevitably sgraffiti (i.e. produced by the sgraffito method), which often become indistinguishable once repatination is completed. The petroglyphs that survive longest are generally cupules and deep linear grooves. They are also the oldest found worldwide, dating back to the Middle and even Lower Palaeolithic (Bednarik 1995b). This, too, is hardly a coincidence, it is a very predictable distribution pattern and thus probably a taphonomic phenomenon. It means, in effect, that there is a high probability that other rock art was also produced at such early times. Whenever the oldest examples we have of a class of evidence are the ones that would be expected to survive longest, it should be assumed that they are neither the oldest made, nor the only ones made at the time in question.

The logic this is based on has been explained and can be quantified as an integral function (Bednarik 1994b). Its effect is best described by example. We have no direct evidence of hominid navigation older than about 8500 years, but we have indirect evidence of it that is about one hundred times as old (Bednarik 1997). In this case, the phenomenon category's 'taphonomic threshold' is the early Holocene, and its 'taphonomic lag' is the time between that point in time and the historical commencement of the phenomenon in question. In other words, in this example the taphonomic lag is about 99% of the phenomenon category's historical duration. The perdurable evidence forms of most archaeological events have taphonomic lags of between 99% and 100%, very few fall significantly under 99%, hence it is most inappropriate for archaeology to assume that it can know the beginnings of most human practices. *Any* statistical interpretation of rock art that is not qualified by taphonomic logic is grossly misleading.

About discriminating rock art and natural markings

The form of rock art that was produced by some reductive process is called petroglyphs, but its reliable identification has been problematic. Archaeologists have frequently identified other types of rock markings as petroglyphs, or petroglyphs as natural markings of some kind (Bednarik 1994a). Among the non-rock art markings that have most frequently been misidentified are animal markings in limestone caves, xenoliths, rillenkarren and rock marks made by machinery. On portable objects, numerous types of markings have been archaeologically misidentified as art, including mycorrhyzal grooves, animal claw marks, clastic movements marks, tooth marks on bones, cracks and solution marks, incidental tool marks, and various types of natural perforations, such as those caused by parasitic organisms (on shells) or gastric acids (on bone). In some cases, archaeologists have consistently refused to accept the corrections of specialists, and have insisted that their identifications are correct. There have been instances of portable natural markings being described as mobiliary art, and of man-made non-utilitarian markings being declared to be natural. It can readily be demonstrated that the discrimination of anthropic and non-anthropic rock markings is a subject for specialist attention.

In the case of rock art, this is a complex aspect that must be considered within its context, the related weathering and modification processes affecting rock surfaces (e.g. speleothem precipitation in caves), and not within the context of traditional archaeological preoccupations. Many archaeologists lack the training or field experience of judging the results of these processes, and some have been found to be very defensive when corrected.

There have also been problems in misidentifying natural colouration of rock surfaces as rock paintings, but these are far less common. Nevertheless, some spectacular instances do exist and have been discussed, including in Australia. In general, however, the problems are far more common in the identification of engravings and other petroglyphs. When even the slightest doubts are possible it is always advisable to consult a specialist rock art scientist. After all, to provide published dating evidence from some coloured rock surface that turns out to be a natural discolouration, as has happened on at least two occasions, is not just embarrassing for the researchers concerned, it is also a waste of limited resources and publishing space. It is similarly embarrassing when some natural rock markings are pronounced to be petroglyphs, and declared a protected archaeological site, as has happened on occasion (e.g. in Victoria).

About interpreting rock art

Some archaeologists tell us what is depicted in rock art, and what archaeological conclusions we can draw from *their* identifications. My children were able to tell me what they thought was depicted in rock art soon after they learnt to speak, and I found their identifications more illuminating than those of archaeologists. This is because their perception was less conditioned than that of academic sophisticates, who have undergone massive conditioning throughout their lives.

When a person who is not a participant in the cultural system the rock artist in question belonged to tells us what he or she thinks is depicted in rock art, or what the meaning of a rock picture is, we are in fact learning about how that person relates to physical reality, and how that person's occipital visual centre reacts to input information. Whether the person in question is an infant or an archaeologist is not particularly relevant to the veracity of the perception. If one were a cognitive psychologist and had decided to study the perception of archaeologists, one would certainly take a keen interest in such information. To the rock art scientist, however, it is of very limited relevance, because such a practitioner would in most cases only be interested in the perception and cognition of the rock artist, or of those who shared the perception of reality the rock artist possessed. In short, it would be helpful if researchers, to whom the cultural and iconographic norms and values of the rock artists are alien and usually incomprehensible, would abstain from cluttering publications with their opinions of what is depicted in rock art, and what rock art means. There is, for instance, no reason to assume that an archaeological or other academic training would equip a person to better understand the meaning of rock art (Macintosh 1977). It is appropriate to cite here the founder of modern Australian archaeology: 'A prehistorian may infer methods of application or techniques of engraving, from observation, but comment concerning motivation and meaning is beyond the scope of normal archaeological activities' (Mulvaney 1969: 174). We are most fortunate in Australia in that we have, in our society, people who still possess the knowledge to interpret and understand the meaning of rock art, and they are the only ones whose opinion counts in this instance. Unfortunately, Western researchers are not capable of truly comprehending alternative worldviews, and in the translation of probably untranslatable information, meanings can only be understood in corrupted forms. Throughout history, non-Western societies have always had to make allowances for the ideological righteousness of Westerners, and that, sadly, also applies to much of what passes as 'science'.

About favourite models

Moreover, the epistemological dynamics of Western 'science' tend to foster in researchers a predilection towards preferring evidence that is thought to support some favoured model, or that is thought to refute some model they dislike. In the hard sciences, this is much less of a problem than in disciplines that are generally based on non-refutable propositions. This is so because propositions in the hard sciences are usually readily testable. Rock art research certainly has experienced more than its fair share of enthusiastically defended or attacked models, and the great difficulties rock art specialists sometimes experience with ardently defended but non-refutable and thus unsci-

entific archaeological views need to be highlighted. They are often attributable to some of the issues highlighted above, and to the lack of refutability of archaeological propositions generally.

There is, however, a very simple and readily available method of testing whether an archaeological proposition is worth considering further. The first role of taphonomic logic is precisely to eliminate the need to pursue enticing but ultimately worthless propositions in archaeology, as well as in rock art research and any other disciplines that deal with events and phenomena of the past. Most propositions can be formulated in such a way that their transit through the filter of taphonomic logic is possible, and after emerging from such treatment those that have no future within a scientific epistemology can easily be identified.

REFERENCES

AUBRY, T., CARVALHO, A. F. and ZILHÃO, J. 1997. Arqueologia: Salto do Boi - Cardina I, pp. 1–23. In *Arte rupestre e pré-história do Vale do Côa (trabalhos de 1995–96)*. Relatório científico ao Governo da República Portuguesa, Lisbon.

BAHN, P. 1993. World's oldest cave art. *Archaeology* May/June 1993: 37.

BEDNARIK, R. G. 1979. The potential of rock patination analysis in Australian archaeology — part 1. *The Artefact* 4: 14–38.

BEDNARIK, R. G. 1994a. The discrimination of rock markings. *Rock Art Research* 11: 23–44.

BEDNARIK, R. G. 1994b. A taphonomy of palaeoart. *Antiquity* 68: 68–74.

BEDNARIK, R. G. 1995a. The age of the Côa valley petroglyphs in Portugal. *Rock Art Research* 12: 86–103.

BEDNARIK, R. G. 1995b. Concept-mediated marking in the Lower Palaeolithic. *Current Anthropology* 36: 605–34.

BEDNARIK, R. G. 1996. Only time will tell: a review of the methodology of direct rock art dating. *Archaeometry* 38: 1–13.

BEDNARIK, R. G. 1997. The earliest evidence of ocean navigation. *The International Journal of Nautical Archaeology* 26: 183–91.

BEDNARIK, R. G. 1998. The technology of petroglyphs. *Rock Art Research* 15: 23–35.

BEDNARIK, R. G. 2002. The dating of rock art: a critique. *Journal of Archaeological Science* 29: 1213–33.

BECK, W., DONAHUE, D., BURR, G. and JULL, A. J. T. 1997. AMS ^{14}C dating of Early Anasazi Petroglyphs from the North American Southwest desert region. Abstract, *Conference handbook, Sixth Australasian Archaeometry Conference*. Australian Museum, Sydney.

BECK, W., DONAHUE, D. J., JULL, A. J. T., BURR, G., BROECKER, W. S., BONANI, G., HAJDAS, I. and MALOTKI, E. 1997. Ambiguities in direct dating of rock surfaces using radiocarbon measurements. *Science* 280: 2132–5.

CLOTTES, J., CHAUVET, J.-M., BRUNEL-DESCHAMPS, E., HILLAIRE, C., DAUGAS, J.-P., ARNOLD, M., CACHIER, H., EVIN, J., FORTIN, P., OBERLIN, C., TISNERAT, N. and VALLADAS, H. 1995. Les peintures paléolithiques de la Grotte Chauvet-Pont d'Arc, à Vallon-Pont-d'Arc (Ardèche, France): datations directes et indirectes par la méthode du radiocarbone. *Comptes Rendus de l'Académie des Sciences de Paris* 320: 1133–40.

DALTON, R. 1998. Dating in doubt as researcher is probed. *Nature* 392: 218–9.

DORN, R. I. 1983. Cation-ratio dating: A new rock varnish age-determination technique. *Quaternary Research* 20: 49–73.

DORN, R. I. 1986. Rock varnish as an indicator of aeolian environmental change. In W. G. Nickling (ed.), *Aeolian geomorphology*, p. 291–307. Allen and Unwin, London.

DORN, R. I. 1993. Dating petroglyphs with a three-tier rock varnish approach. In D. S. Whitley and L. L. Loendorf (eds), *New light on old art*, pp. 13–36. Institute of Archaeology, University of California, Los Angeles.

DORN, R. I. 1996. A change of perception. *La Pintura* 23(2): 10–11.

DORN, R. I. 1997. Constraining the age of the Côa valley (Portugal) engravings with radiocarbon dating. *Antiquity* 71: 105–15.

DORN, R. I. 1998. Response to Beck et al. 1998. *Science* 280: 2135–9.

DORN, R. I., CLARKSON, P. B., NOBBS, M. F., LOENDORF, L. L. and WHITLEY, D. S. 1992. New approach to the radiocarbon dating of rock varnish, with examples from drylands. *Annals of the Association of American Geographers* 82(1): 136–51.

DORN, R. I. and WHITLEY, D. S. 1984. Chronometric and relative age determination of petroglyphs in the western United States. *Annals of the Association of American Geographers*, 42: 308–22.

MACINTOSH, N. W. G. 1977. Beswick Creek Cave two decades later: A reappraisal. In P. J. Ucko (ed.), *Form in indigenous art*, pp. 191–97. Australian Institute of Aboriginal Studies, Canberra.

McCARTHY, F. D. 1988. Rock art sequences: a matter of clarification. *Rock Art Research* 5: 16–42.

MALAKOFF, D. 1998. Rock dates thrown into doubt, researcher under fire. *Science* 280: 2041–2.

MULVANEY, D. J. 1969. *The prehistory of Australia*, 1st edition. Thames and Hudson, London.

SAVVATEYEV, J. A. 1976. *Petroglify Karelii*. Nauka SSSR, Petrozavodsk.

SWARTZ, B. K. 1997a. An evaluation of rock art conservation practices at Foz Côa, northern Portugal. *Rock Art Research* 14: 73–5.

SWARTZ, B. K. 1997b. An investigation of the Portuguese government policies on the management of the Foz Côa sites. *Rock Art Research* 14: 75–6.

WARD, G. K. 1994. On the use of radiometric determinations to 'date' archaeological events. *Australian Aboriginal Studies* 1994/1: 106–9.

WARD, G. K. and WILSON, S. R. 1978. Procedures for comparing and combining radiocarbon age determinations: a critique. *Archaeometry* 20: 19–31.

WILSON, S. R. and WARD, G. K. 1981. Evaluation and clustering of radiocarbon age determinations: procedures and paradigms. *Archaeometry* 23: 19–39.

WATCHMAN, A. 1992. Investigating the cation-ratio calibration curve: evidence from South Australia. *Rock Art Research* 9: 106–10.

WATCHMAN, A. 1995. Recent petroglyphs, Foz Côa, Portugal. *Rock Art Research* 12: 104–8.

ZILHÃO, J., AUBRY, T., CARVALHO, A. F., BAPTISTA, A. M., GOMES, M. V. and MEIRELES, J. 1997. The rock art of the Côa valley (Portugal) and its archaeological context: first results of current research. *Journal of European Archaeology* 5: 7–49.