CONCEPTUAL PITFALLS IN PALAEOLITHIC ROCK ART DATING

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Abstract: The recent prodigious results in direct rock art dating have greatly advanced paleoart studies. However, the methodology being used remains in its infancy, and over-enthusiastic archaeological interpretation of these results could easily undermine the progress being made. This paper presents some examples from southern France in which scientific dating results have been utilized to generate interpretational hypotheses that are not necessarily warranted by the hard data. French examples of interpretation-embellished dating work are not unique, and instances from other continents are also cited. Some of the possible pitfalls are explained and a greater epistemological rigour in the interpretation of physical and chemical dating results is advocated, by explaining the limitations of currently used techniques.

Version française abrégée: La datation de l’art rupestre se fait traditionnellement par une datation approximative du contexte archéologique, par des comparaisons stylistiques, par l’identification d’objets représentés ou par la corrélation des pigments trouvés dans le sol avec ceux utilisés pour les peintures. La datation directe, peu à peu introduite depuis quinze ans, cherche au contraire à assurer le caractère scientifique de l’information relative à l’art rupestre. Une telle information est supposée fournir des données fiables à propos de l’art, et la relation chronologique établie entre l’art rupestre et les substances datées est réputée indiscutable de par sa liaison avec des phénomènes physiques immuables. Cependant, certains archéologues ont commencé à utiliser les résultats des méthodes de datation directe avec une démarche archéologique traditionnelle, construisant des modèles interprétatifs sur des bases scientifiques incorrectement comprises. Cet article commence par passer en revue une série d’affirmations concernant la datation de l’art rupestre attribué au Paléolithique supérieur en l’Europe occidentale et tout particulièremen pour le site de Cougnac en France. Ces affirmations sont examinées d’une manière critique et l’on s’aperçoit que, bien que fondées sur des données scientifiques au départ, les propositions déduites sont souvent contradictoires, incohérentes ou s’excluent mutuellement. La cause n’en est pas dans les données elles-mêmes mais bien dans la manière dont elles ont été utilisées et interprétées.

Plusieurs de ces chaussure-trappes sont analysées et mises en évidence. La vraie nature des datations radiocarbone et leur valeur statistique sont examinées et les difficultés inhérentes à la comparaison des datations sont expliquées. Une des méthodes directes privilégiées pour la datation de l’art rupestre paléolithique est l’analyse radiocarbone du charbon utilisé dans les dessins pariétaux, et il est expliqué combien de telles dates peuvent être trompeuses, notamment à cause de la taphonomie du charbon. Des exemples précis sont donnés et il est montre que des dates différentes obtenues pour un même motif, n’indiquent pas nécessairement que ce motif a été retouché ; d’autres explications sont également possibles pour de tels résultats. Les sources possibles de contamination pour ces échantillons sont discutées brièvement. Plusieurs exemples concernant l’Australie et les États-Unis d’Amérique sont ensuite cités afin d’illustrer les difficultés considérables déjà rencontrées dans l’interprétation rigoureuse des résultats des datations directes, y compris dans le cadre de travaux très semblables à ceux réalisés en France et en Espagne. Il est suggéré alors que bien que les datations faites en Europe soient réalisées avec autant de soins que dans le reste du monde, une minorité d’archéologues européens en attend des résultats irréalistes. Les archéologues qui interprètent de telles données doivent se familiariser avec les propriétés et les limites des mesures qu’ils obtiennent des laboratoires de datation. Dans de nombreux cas, ces données ne peuvent garantir les interprétations qui en sont faites, ce qui dans le cas d’informations aussi souvent cruciales peut aisément conduire à l’établissement de modèles fallacieux relatifs à l’archéologie de l’art rupestre.

INTRODUCTION

The concept of «direct dating» of rock art was formulated about fifteen years ago, in an effort to escape the traditional methodology of inductive rock art dating. Previous attempts to determine the age of rock art had been predicated largely on two approaches: archaeological minimum dating through excavation, and the correlation of stylistic traits of various types. The limitations of both these approaches have severely retarded progress in rock art research for a number of reasons, among them the following:
1. The conditions for archaeological minimum dating exist at very few sites, namely those where rock art is covered by datable sedimentary strata, or where rock fragments bearing art have become detached and are found stratified in supposedly datable sediments. There are almost no such instances of rock paintings, and even those of petroglyphs are not numerous world-wide.

2. Archaeological minimum dating relies on various assumptions that are hard to validate (or, more appropriately, to refute), e.g. stratigraphical integrity (consider taphonomy of sediments), or that charcoal contained in a sediment is of the same age as that layer (Bednarik 1989). There are numerous natural processes that can account for charcoal being found in strata it is not related to chronologically.

3. Archaeologists cannot identify styles objectively (Conkey & Hastorf 1990), hence it is always tenuous to base any interpretations on their subjectively perceived styles, or to incorporate them in chronological modelling (Lorblanchet & Bahn 1993).

4. Excavated portable art may appear stylistically identical to a rock art motif, but there is no proof that it could not have been copied from the latter much more recently, hence it is not possible to conclusively correlate portable art and rock art stylistically.

5. Many of the assumptions in stylistic systems are iconographically derived, i.e. they are based on the subjective identification of objects, attitudes or activities believed to be depicted in rock art. Non-participants in the culture in question may not correctly perceive the iconographic meaning of art, and their interpretations are not falsifiable, hence not scientific.

6. It has often been attempted to correlate pigment traces found stratified in the ground with nearby paintings on rock (e.g. Macintosh 1965 in Australia; Wakankar 1983 in India; Combier 1984 in France). Unless such correlation is warranted by convincing chemical data it is not appropriate to attempt dating the rock art by charcoal supposedly associated with the pigment in the sediment, even if the stratigraphical integrity of the charcoal seems assured.

During the 1970s, the first serious endeavours at «direct» dating of rock art began, in which some form of information is obtained about the age of a feature that is physically related to the rock art, and indisputably either older, younger or the same age as the art (Bednarik 1979, 1981, 1992a). This work remained largely ignored by archaeologists, and in some cases was discouraged by them (Bednarik 1991a). Since 1990, however, archaeologists have begun to embrace direct dating with enthusiasm, using its various methods in several countries. The new methodology, developed outside of mainstream archaeology (as defined by Lewis-Williams 1993), was now used by archaeological practitioners, but at the same time its epistemology began to change, becoming diluted by traditional archaeological methods of model building as described below. Results supplied by scientists were once again interpreted by archaeologists, and used for purposes they were not suitable for. This has already led to excessive claims and unfounded announcements in the international specialist literature in many cases. It is the purpose of this paper to review some of these misinterpretations of dating results of rock art, and to place them in a more realistic and more rigorous perspective.

### A REVIEW OF SOME DATING CLAIMS

I begin by summarizing recent claims concerning the dating status of the Western European Upper Palaeolithic rock art traditions. There are currently 293 sites of this art known (Bednarik 1994a), although it must be cautioned that the attribution of many of these sites is tenuous. Initially, these traditions had been «dated» by considerations of perceived style, technique, location, claimed identification of motifs, perceived spatial association and so forth. However, most of the direct dates so far procured from these arts are at odds with the traditional chronology of European Palaeolithic art. In a recent debate, Lorblanchet (1993a, 1993b) has made a series of claims, which include the following statements:

a. «11% of European Pleistocene [rock art] sites have received an undisputed dating»;

b. «A negative hand [hand stencil] from Cosquer is dated as 27,000 years old, confirmed by the archaeological context»;

c. «Some hands from Gargas will shortly be dated to the same period [by] the bone fragments associated with these hands»;

d. «The existence of Aurignacian art ... on blocks and walls is well established [by] decorated pieces of stone or of fragments of fallen painted cave ceilings»;

e. «It is probable that art appeared at various points on the globe at roughly the same moment, at the time of the Middle/Upper Pleistocene transition»;

f. «If it is not] share the opinion of Bednarik and other researchers that would place the beginnings of art in the Middle Palaeolithic or even earlier»;

g. «Ancient art [such as that of Australia] may equally well be found in Europe»;

h. «Australia has the world’s oldest rock art».

It is immediately apparent that some of these statements are mutually contradictory: propositions (f) and (h) cannot both be true, because if Australian art were older than that of the European Upper Palaeolithic, it must be contemporary with the Middle Palaeolithic. Similarly, propositions (g) and (h) seem incompatible: one must be false, unless they both are. In reality, both are probably false. The world’s oldest rock art is certainly not in Australia: our present information suggests that the first colonizers of Australia possessed symbolic capacity
(Bednarik & You 1991; Noble & Davidson 1993), they probably had some form of art, in which case it must have existed in Asia earlier still. Moreover, the presently oldest known petroglyphs are in India (Bednarik 1993a).

Similarly, propositions (e) and (f) cannot both be true: the Middle/Upper Pleistocene transition occurred 100,000 years before the end of the Middle Palaeolithic, and Lorblanchet will have to decide which alternative he prefers. The difference is rather significant, as one alternative is only 23% of the duration of the other. In the same vein, propositions (e) and (h) cannot be reconciled, because there is no evidence that Australia was even occupied by humans at the time of the Middle/Upper Pleistocene transition.

His remaining statements are also at odds with a rigorous model: fragments of rock art found in sediment layers (proposition [d]) cannot establish the age of the art, at the very best they might establish a minimum age; similarly, there is no proof that the bones placed near hand stencils in Gargas (proposition [e]) are of the same age as the art (Clottes et al. 1992a); we know very well that some reactions to, or modifications of, earlier rock art occurred many thousands of years after the creation of the art, for instance in Cosquer Cave (Clottes et al. 1992b), but also at thousands of other rock art sites. As we will see below, Lorblanchet himself claims that he has evidence that some of the art was retouched much later, so his assumption that bones placed near paintings date the art is logically inconsistent with his own views. There is clearly no reason to exclude the possibility that the bones at Gargas are the result of a later response to the art. The two dates from Cosquer Cave (proposition [5]) refer to the age of the charcoal, or rather the event of assimilation of carbon in the tree from which the charcoal originates; they do not date the events of pigment preparation or painting, which are certainly more recent, and may well be many thousands of years more recent. There are large quantities of charcoal on the floor of the chamber, some 300 samples were collected in 1992. A few of these have yielded dates ranging over almost 13,000 years, and it is most likely that some of this charcoal was picked up from the floor in producing the charcoal stencils. The actual age of the charcoal is therefore entirely fortuitous, and represents no more than a maximum age for the painting it was derived from (Bednarik 1993b, 1994b).

This brings us to the first proposition (a), which I regard as wrong and seriously misleading. In response I had remarked wryly:

«I shall not comment here on [Lorblanchet's] statistics of «dated» European sites, except to say that it is a most impressive ... achievement if «objective» dating has been obtained at 11% of the sites».

To which Lorblanchet replied promptly:

«In response to Bednarik's contemptuous statement, I would like to point out that 285 European sites have been well dated to the Pleistocene and 11% of those have now been dated even more precisely».

**COUGNAC: A CASE EXAMPLE**

An examination of Lorblanchet's subsequent summary of his own work in the cave of Cougnac (Lorblanchet 1994) helps considerably in understanding his position as well as my objections. He has acquired a total of seven accelerator mass spectrometry radiocarbon dates from that site. Six are from charcoal or charred bone pigment taken directly from four painting motifs, the seventh is from a reindeer bone found on the floor of the cave. Lorblanchet accepts these dates as absolute statements of age, within their stated errors, and then proceeds to interpret them in terms of how the art was created, how the different motifs relate to one another, chronologically and culturally. For instance, he reports two different dates of 22,750 ± 390 and 23,615 ± 351 years BP from a male megaloceros figure, and assumes that this proves conclusively the figure was repainted. Similarly, he claims that the difference of his two dates from a female megaloceros figure shows that it was «renewed periodically». These dates are 19,498 ± 267 and 25,120 ± 390 BP, and his interpretation is that the figure was first painted in the Perigordian, and later renewed in the Solutrean. Two other motifs nearby were added in the Magdalenian, he says, at 13,810 ± 210 and 14,300 ± 180 BP respectively. He then observes, quite correctly, that «Such observations completely modify the perception prehistorians have had of the decorated cave of Cougnac».

Before prehistorians modify their perception of the Cougnac art, they might need to contemplate the data in a more rigorous fashion. To begin with, Lorblanchet's date from a reindeer bone is not cited here, because it is irrelevant to the discussion. Bones occur often in caves, usually because they were brought in by animals. Lorblanchet offers no explanation why he considers this particular bone to be important. It bears a few small incisions purportedly made with a stone tool (Lorblanchet et al. 1990, fig. 8), but this suggests in no way that the bone could not have been brought in by an animal. Unless we can show that it was used in the making of a painting, it is simply not relevant, nor are the incisions on it to the rock art. The ages of the stalactites some paintings occur on would be more relevant to the art: at least they would provide maximum ages for the paint they support.

The earlier «perceptions» of the age of the Cougnac figures range from Aurignacian-Perigordian (Sieveking & Sieveking 1962, p. 67) to Early Magdalenian (Leroi-Gourhan 1971, p. 267). The general impression of the Cougnac art is one of comparative stylistic uniformity, much in contrast to some other French sites where even a cursory examination suggests the presence of two or more, very distinctive episodes of art production (e.g. Baume Latrone, Cosquer Cave). Lorblanchet now claims that
some Cougnac figures date from the Perigordian (in the sense of Gravettian), some from the Solutrean (or were retouched then) and some of the Middle Magdalenian. These propositions may be correct, but whether they are is not really relevant here. What is relevant is whether the dating evidence warrants these deductions derived from it, and their use in further archaeological model building. We shall attempt to separate fact from fiction here.

To begin with, we must remember that there were no identifiable Palaeolithic «cultures» in the sense of the meaning of the word culture: the skills, arts, beliefs and customs of populations maintained over a long period of time. Terms such as Perigordian or Magdalenian are simply techno-chronological pigeonholes, referring to taphonomically distorted evidence of non-perishable tools, and its arbitrary taxonomy created by specialist «analysts» of an alien society, our society (cf. Lewis-Williams 1993 for an excellent analysis of the term «archaeological analysis», which is in fact an oxymoron). Such a taxonomy is based on the entirely subjective stylistic perceptions of contemporary people, it has no objective justification. There can be no doubt that the Upper Palaeolithic period (itself an archaeological artefact) would be divided into entirely different phases if different types of information had become heuristically available, and in a different historical sequence (e.g. social, ethnic or artistic information). Yet here we are, engaged in the almost grotesque activity of trying to force artistic (i.e. cultural) information into a chronological framework predicated on subjectively interpreted technology of stone tools! This shows the absurdity of forcing rock art into an archaeological straitjacket.

Technically more important than these epistemic considerations is the question: are Lorblanchet’s deductions scientifically warranted? Are six radiocarbon dates taken from four rock art motifs adequate evidence to justify his claims? Each radiocarbon date is reported at one standard deviation (SD), which means that there is a statistical chance of 68.26% that the sample’s true age lies between the stated tolerances. In practical terms, two of Lorblanchet’s six dates should be expected to lie outside their stated values if his sample were fully representative, and we know neither which ones, nor how much wrong they might be. But the sample is most unlikely to be truly random, each result refers to an independent event. We could adopt values of two SD, but that would still not guarantee that all dates are correct, and it would expand the stated error correspondingly. Moreover, there is no guarantee that only two dates are wrong: it is perfectly possible that all of them are wrong, just as it is statistically possible that all of them are right.

To make matters worse, Lorblanchet compares individual dates with others, i.e. he compares one set of statistical probabilities with other such sets. The two «dates» from the female megaloceros can be related spatially, because they are from the same motif, but that does not mean that they can be convincingly correlated in a statistical sense. The probability that this is valid is only about 46.5%. The probability that all six «dates» are correct is only 10%, and thus the same applies to the probability that Lorblanchet’s propositions are all correct. It is therefore prudent to assume that only a number of his claims can be reasonably maintained, and one would be tempted to discard those that seem the least judicious. I am not suggesting that this would be statistically or epistemologically sound; it is in fact this sort of common logic approach that facilitates false conservatism in the discipline, by favouring statistical clustering. Nevertheless, since there is a very high probability that at least some of Lorblanchet’s deductions are wrong, it might be sensible to examine them more closely.

We have been told by western European rock art specialists for decades that the various technological phases of the Upper Palaeolithic are reflected in distinctive art styles, and that these can be reliably recognized. Here we have a competing claim: that artists of the «Solutrean» renewed paintings of the «Perigordian», faithfully maintaining precisely the same style. If this were true, why should we have to consider stylistic pronouncements concerning Palaeolithic art in future? Lorblanchet et al. (1990) have previously stated that the male megaloceros was first drawn in red, then the female was added in black. Now Lorblanchet says that the female was first drawn in black, then the male in black some 1500 years later, then the male was retouched after a further millennium, and finally the female was retouched after another 3000 years. So presumably he means that the male was first drawn 25,120 years ago or earlier, but in red pigment. This means it would also have to be of the «Perigordian». However, if we accept that a few of the dates are not correct, this entire construct becomes a house of cards: for instance, we are left without evidence of a Solutrean retouch if we disregard just the younger of the dates from the female figure. By ignoring the older of these two dates, we need not postulate that the red male must be over 25,120 years old, and by ignoring both the oldest of the six dates, the evidence for repainting almost evaporates. If we consider the two dates from the male megaloceros, we see that they overlap by 617 years at two SD, and that the two probability expressions begin to overlap at only 1.16 SD. There is certainly no adequate proof that the motif must have been retouched. Hence Lorblanchet’s claim relies on a single date from the female megaloceros.

**CONTAMINATION OF ROCK PAINTS**

But there are even more serious qualifications. In accepting the radiocarbon dates on face value we would be assuming that the charcoal had been produced very shortly before it was used as pigment. We possess no proof at all for this to be the case, and even if it were so in many instances, there would certainly be others where charcoal from an earlier period was used. Charcoal survives even the
most adverse chemical environments exceptionally well, and it is often found on the surface of former occupation sites. Because of its very low specific gravity it tends to "float" on a loose deposit that is subjected to trampling, and it is similarly affected by both wind and water transport. Therefore we have not even a logical reason to assume that the dates obtained from charcoal must relate to the age of the paintings. We know in fact that a radiocarbon date refers to the time when the tree that provided the wood for carbonization lived, and not to the time when the painting was created. We could speculate about the number of instances where the charcoal used was several centuries old, even millennia. If we were to assume, for instance, that this applied in half the cases, only two of Lorblanchet's six dates would remain statistically valid. It would then become impossible to defend any of his hypotheses, and that is still before considering the possibilities of sample contamination, laboratory error, or the many inherent uncertainties of the radiocarbon dating method itself (deVries effect, isotopic fractionation, incorrect half-life, etc.). Finally, Pleistocene radiocarbon dates remain of course uncalibrated, although there is a dendrochronological project underway in Australia to extend calibration back to possibly 20,000 BP, with the help of the Tasmanian huon pine.

Cougnc Cave is a heavily visited tourist cave, and the effects of frequent visitation are well known: microorganisms are introduced, artificial lighting encourages algal growth, and speleo-atmospheric carbon dioxide levels increase significantly. The contamination required in such a minute sample to distort the result of 22,000 by 1000 years is minimal. The Cougnac art is often impregnated by a calcite skin (explaining the site's unusually good preservation), which contains a composite carbon ratio, made up of two ratios: one half of the calcite is (presumably) free of radiocarbon, the other half is younger than the painting. Lorblanchet's samples are treated in hydrochloric acid (to remove carbonate), aqueous ammonia (eliminating humic acid) and oxygen (to remove organic components) (Valladas et al. 1990). In the case of the first sample dated from Cougnac Cave, this treatment resulted in retaining only c. 1% of the original carbonate sample (1.2 mg). The alternative method of eliminating contamination, selective oxidation in a low-temperature, low-pressure oxygen plasma (Russ et al. 1990) was not used. It would be impudent to attribute any great precision to these results, and to use such tentative values in any far-reaching archaeological constructs. Lorblanchet's bold pronouncements stand in stark contrast to the cautious interpretation of an Australian team that also secured two disparate dates from a single painting (near Sydney), of 6085 ± 60 and 29,795 ± 420 BP respectively. These authors discussed the implications of their results in detail and offered four alternative explanations for the conflicting dates (McDonald et al. 1990, p. 90). Their finding must be seen in the context that in Australia, the practice of rock art retouch has been documented on numerous occasions, both ethnographically and archaeologically, whereas that practice has hardly ever been demonstrated in the European Palaeolithic art. For instance, the incomplete rendering of the male megaloceros in red is certainly no evidence of later repainting in black, it is more plausibly explained by assuming the artist ran out of red paint and decided to execute the figure in black instead. The findings of McDonald et al. are most relevant, and they need to be considered in the context of the above speculations:

"The demonstration of conflicting dates from the same motif clearly indicates that caution is required in interpreting any AMS date received from charcoal pigments. Given the constraints on sample collection and the costs involved in AMS, it would be easy to rely on single sample dates. However, despite the definition and accuracy of the AMS technique itself, its application to the potentially complex taphonomy of art pigment on rockshelter surfaces remains at an early developmental stage" (McDonald et al. 1990, p. 91).

What I find even more disconcerting in Lorblanchet's report from Cougnac is that pigment was sampled from the entire outline of each figure, and that no further sampling will be possible in that important site. I assume that no nanostratigraphic studies of the type conducted in U.S.A., Canada and Australia were undertaken to resolve the question of retouch. Therefore we have no way of testing any of the hypotheses proposed, and all of the datable substance was sacrificed to produce only precarious results in terms of interpretation. To destroy the entire available sample is not sound archaeological practice, and it brings to mind the debates concerning the ethics of destructive sampling of rock art (Bednarik 1992b, 1994c; Watchman 1992; Loy 1994). It also raises again the question of establishing proper procedures of peer review before the removal of samples is authorized. We are occasionally reminded that world rock art belongs to all of humanity, but it seems to me that in this case, the research potential of an important site was seriously compromised for a paltry return in terms of scientific knowledge gained, and without the consultation of the world community of researchers.

OTHER PITFALLS IN DIRECT ROCK ART DATING

It should be emphasized that problematic interpretations of rock art dating results have been promoted by various other researchers in recent years, and I must take this opportunity to apologize to Lorblanchet for apparently singling out his work for criticism. I am merely using his recent pronouncements to illustrate certain points in showing how archaeological misinterpretations may begin, but the problem itself is quite widespread, and it seems to be endemic to archaeologists working with rock art. My point is well illustrated by Clottes' (1994) recent announcement that
European archaeologists have reported scientific dating results selectively, publishing those they considered acceptable and withholding those they thought to be inappropriate. On the other hand, dates from samples known to be contaminated have been published, apparently because they met the researchers' expectations. Clottes reviewed all twenty dates published for European Palaeolithic rock art as of February 1994, and found that there were some problems with archaeological interpretations of practically all of them - including those he himself had produced. If we contemplate the possibility that the discussion of direct dating results has been prejudiced in this fashion, the reservations expressed in this paper become even more pertinent. Clearly, the effects of selective presentation of data with which it is intended to confirm specific interpretative models become superimposed over those of data misinterpretation or over-interpretation. The cumulative effect of uncritical application of traditional archaeological modelling strategies is that it results in unsupportable constructs. Given archaeology's history of then ardently defending such confirmation-derived models of the past, their introduction into rock art research can only be to the detriment of that discipline.

The seriousness of archaeological misinterpretation of rock art is well illustrated by a recent archaeological fiasco in Australia. At Laurie Creek, a painting site in Northern Territory, a «weathered dark red pigment» was reported to contain human blood traces (Loy et al. 1990), and these proteinaceous remains were dated to c. 20,000 years BP. At the time, this result represented the oldest direct date from rock art pigment in the world. But the radiocarbon scientist of the team, D.E. Nelson, had «second thoughts» about the dating. After re-sampling the site he reported that the material contained neither protein nor human blood, and that the «pigment» was in fact a natural weathering deposit of iron oxides. Not only was the date archaeologically meaningless, he found that organic material was often present on the surface of natural rock faces at the site (Nelson 1993). This demonstrates two points: that it needs to be determined what organic matter is being dated and how it relates to the art production; and that archaeologists sometimes identify rock markings as rock art that are in reality non-anthropic markings. This is a surprisingly common problem: I have reported literally thousands of misidentifications of surface markings (on rock or portable objects) by prehistorians, and it seems advisable to consult specialists on matters of discriminating between natural and anthropic markings (Bednarik 1991b, 1994d).

Nelson's results are of importance to most other direct dating of rock art. The naive idea of archaeologists, that rock surfaces are somehow sterile and free of organic substances, contributes to their misinterpretation of analytical results. Most known misidentifications of natural surface markings as rock art or portable art refer to perceived petroglyphs: twelve types of such rock markings have been defined, of which true petroglyphs (including engravings) are only one (Bednarik 1994d). Misidentifications of natural rock markings as paintings is much less common, the above Australian example being one. Another is the misidentification of a natural discoloration, caused by the dehydration of geothite to haematite through heat, at the American site Rochester Creek. That «motif» had been radiocarbon dated to about 2000 BP (Loendorf 1986), until it was shown that it is not a painting at all, but a natural feature (Bednarik 1987).

Another recent study emphasizing the need for restraint in interpreting direct dating results has illustrated the considerable complexity involved in construing dates from reprecipitated carbonates (Bednarik & Head 1995). The dating of Australian petroglyphs sandwiched between laminar speleothems seems to be a simple and promising procedure, and yet the many qualifications that apply to its results render these findings almost ineffectual. Rejuvenation of porous samples has been shown to occur; in volcanic regions, non-biological CO₂ may enter the system; and past compositional fluctuations of atmospheric and biological carbon are intricately linked to climate. Plant communities can be very sensitive to atmospheric CO₂ levels, which Antarctic ice cores have shown to have fluctuated in the past, and the δ¹³C value of reprecipitated carbonate in equilibrium with respiratory CO₂ can differ significantly according to vegetation type (e.g. steppe grasses vs shrub communities) above the cave in question. The uncertainty introduced by environmentally determined δ¹³C values adds to the effects of other factors, increasing the uncertainties applicable to Pleistocene radiocarbon determinations.

CONCLUSIONS

Pitfalls in the direct dating of rock art generally fall into two categories: on the one hand, those attributable to inappropriate methods or the misapplication of techniques, which can only result in invalid results. On the other hand, there are the pitfalls which are attributable to the misuse of presumably valid analytical results. Here we have only considered the second category. A typical example of the first category would be cation-ratio dating of rock varnishes, a method used for over a decade but now widely regarded as ineffectual. This type of problem is largely unavoidable: in such a new and developing, but very difficult field of research it is inevitable that many promising possibilities be considered, explored and tested. Provided that we appreciate the tentative nature of this work, it is not only legitimate but indeed essential. However, the second category of pitfalls, through misuse or over-interpretation of data, would be avoidable.

In this paper I have briefly addressed my concern that the new methodology of direct dating of rock art might be compromised by its integration into the standard practices of orthodox archaeology. «Mainstream archaeologists"
who avail themselves of these methods have an obligation to acquaint themselves with the limitations and qualifications that apply to the results they acquire from scientists. In particular, they need to consider statistical questions of resolution (Lanteigne 1991), taphonomic and deductive logic (Bednarik 1994e), aspects of contamination, the qualifications applicable to laboratory results, and the realistic limits of the method being used. Confirmationist archaeologists tend to be less concerned with the scientific interpretation of data supplied by support disciplines, than with utilizing these data in underpinning their theories and pronouncements. In doing so, they are sometimes oblivious to the integrity of the dating results, using them for the construction of apparently logical and soundly based models of interpretation that may appear convincing, but are in reality misapplications of the hard data. This does in no way suggest that their interpretations are necessarily invalid, they may even be shown to be correct in the future; it merely means that they are without scientific basis at the present time. Faith in one’s beliefs is a great religious attribute, but it is of no help in creating scientific constructs of reality.

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