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Cosmogenic radiation nuclides in archaeology: a response to Phillips *et al.*

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Various methods have been employed in the attempt to date rock-art. Here Robert Bednarik offers a critique of the cosmogenic radiation nuclides method, and its application to the Côa petroglyphs.

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I am delighted that Phillips *et al.* (1997) have responded to my call for scientific attempts at refutation of the data so far offered concerning the age of *some* of the many petroglyphs in the Côa valley of northern Portugal. We have witnessed a great deal of non-scientific debate about this corpus of rock-art, and it is obvious that our understanding of the issue can be improved only through constructive scientific discussion, which may involve presentation of new data or logic. In continuing this productive mode of discourse I will respond to Phillips *et al.* (hereafter referred to as 'the Paper') and offer a critical examination of the use of their method in archaeology, and especially in rock art analysis.

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The quantitative determination of cosmogenic nuclides has been used experimentally for dating geomorphic surfaces (Davis & Schaeffer 1955; Phillips *et al.* 1986; 1990; Zreda *et al.* 1991). The most interesting results have come from a meteor impact crater in Arizona, ranging from 36,500 to 50,400 BP, which appear to correspond with a single TL date (Phillips *et al.* 1991: 2697; Zreda *et al.* 1990). However, a study of ^{36}Cl contents of boulders in a series of moraines of geologically known ages in California produced only ambiguous results which disagreed with the moraine sequence (Phillips *et al.* 1990: 1530).

Cosmogenic radiation (CR) products are said to provide information of the relative age of exposures (rock surfaces), and some of the results offered by the Paper seem to confirm this. However, there are several reasons why the method cannot provide numerical ages for rock exposure, such as those relating to its severe dependence upon erosion rates. The CR products, including ^{36}Cl, accumulate not only in the sampled surface layer, but also deep inside the rock. The amount of ^{36}Cl that was present in a surface at the time it became freshly exposed to the atmosphere (e.g. through exfoliation or slabbing) can never be known, meaning that the relationship between the present concentration of the nuclide and the age of the present rock surface cannot be ascertained or even estimated.

The Paper demonstrates this problem very well: a sample taken from a depth of 16 m below the Côa river provided an 'age' of between 800 and 2800 years, instead of a zero age. Mindful of the exponential accumulation curve of the nuclides (Kurz 1986; Lal 1991; Zreda *et al.* 1991), we might reasonably expect to obtain an 'age' of perhaps 10,000 years from a sample taken 2 m below present surface, and 20,000 from a surface that was formed a few centuries ago. Similarly, Phillips' sample from Castelo Melhor, a structure erected in the Middle Ages, produced a 'date' of about 30,000. He tries to explain this away by suggesting that the building blocks were collected from the ground level. There are hundreds of schist quarries in the area from which thousands of tons of such building blocks have been quarried over the past couple of millennia, precisely because the stone has such excellent construction properties. Rock from near the surface is inevitably weathered and fractured, and quite unsuitable for masonry work. Thus the more plausible explanation for the high concentration of ^{36}Cl in the Castelo Melhor sample is that it was quarried from a location where a stable geomorphological regime facilitated the accumulation of high contents of cosmogenic nuclides at some depth (from the plateau, i.e. the planation surface, from a depth of several metres).

There are two simple ways of testing this proposition. Firstly, blocks from historical masonry could be analysed, both at the surface exposed to the atmosphere and at a surface concealed in the masonry. If we made the reasonable assumption that the ^{36}Cl concentration was uniform in the block at the time of quarrying, the difference in present concentrations *might* provide a valid measure of age for the structure. A second way of testing the proposition would be to take samples from recent exposures of known ages, such as the 100-year-old railway quarry sampled by Watchman in 1995, at depths some metres below the land surface. It is surprising that Phillips did not attempt this, as it would have provided relevant data on the depth dependence of chlorine-36 concentrations.

In examining the analytical data of Phillips' table 1 (1997: 101) several distinctive trends become immediately apparent. By far the most variable component is Cl, which fluctuates wildly (1·78–16·1 ppm), whereas the major component, silica, varies only from 59·7% to 68·5%. The sites producing low Cl readings are, predictably, 16 m below the river (i.e. in the aquifer) or locations most susceptible to leaching (i.e. hilltop, or outermost joint face). The site with the strongest presence of Cl, Canada do Inferno, provides the strongest evidence of vadose water activity, located at the foot of a steep cliff.

The Côa schists are, as observed in the Paper, highly pervious, so the most soluble salts (i.e. those containing Cl) travel freely through the myriad fine fissures. Where the egress of vadose water is most evident (from the often substantial precipitates, see Bednarik 1995a), the presence of chlorides is also most conspicuous. This pattern suggests that much of the Cl, probably most of it, has been derived from the interior of the rock, e.g. from bedding planes accessible to the highly active hydrological regime, which is consistent with Dorn's (1997) evidence for 'inherited weathering' on Côa exposures. Therefore, the ratio of Cl isotopes cannot be of any consequence to the age of the subsurface rock, because we have no way of knowing, from Phillips' data, which part of them was present at a particular exposure when it first became exposed, and which part is from deeper in the rock (migratory), and thus ^{36}Cl-poor. The net effect of this migration of Cl is that surface samples should provide excessively high 'ages', as is indeed the case.

This factor alone seems to refute the 'dates' provided in the Paper quite effectively, but there are several other fundamental problems with this report. For instance, the issue of erosion rates is fatal to the theory of its authors. In order to construct a plausible model they need to allow for a realistic surface retreat, but at the same time they are forced to exclude the possibility of such a retreat categorically: *any* rate of retreat is in conflict with the proposal of an Upper Palaeolithic age of the almost unweathered petroglyphs. In seeking to preserve the notion of a Pleistocene age, the Paper proposes both a zero erosion and a 2 mm/1000 years erosion scenario. Yet at the same time the authors suggest retreat rates of 10 and 15 mm/1000 years, explaining this by a model of 'episodic' erosion (slabbing every 100,000 years). This slabbing rate is determined from the claimed age of the panel exposure, and the Paper uses panel 'ages' based on zero erosion to calculate erosion rates. This would mean that we have rock surfaces in the Côa valley which retreat and do not retreat at the same time! While the authors are forced to assume that there is no retreat at all, in order to preserve the Palaeolithic age claim, they not only admit that erosion pitting of *c.* 5 mm occurs on one panel they analysed, but they also note the less developed state of erosion on an adjacent but younger panel. (On the subject of erosion pitting at Côa, see the much more comprehensive description of the processes at work in Bednarik 1995a.)

A counter-argument would be that the panel joint faces, on which the majority of the petroglyphs occur, are 'end of grain' surfaces protected by a thin veneer of case hardening (under 1 mm thickness). This might explain why such panels experience little weathering. It is, however, an invalid argument, because while the panel joint faces would be protected by such a veneer, all petroglyphs cut or hammered into them would extend into the softer, highly weathering-susceptible schist beneath the veneer. Therefore, the petroglyph surfaces would be expected to weather as rapidly as those of engraved dates and metal tool marks.

Nevertheless, I should not fail to mention that I agree with many of the ideas expressed in the Paper, whereas Dorn (1997), who relies entirely on the hypothetical dates being valid, is in significant disagreement with them. For instance, the Paper's proposition that the Côa data offer the 'opportunity to compare ^{36}Cl with ^{14}C' results conflicts fundamentally with Dorn's view that the accretionary deposits are open systems, and their carbon content cannot be used to deduce valid dating information. If Dorn (1996: 10) is right on this issue, as he may well be (see Bednarik 1979: figure 3 for relevant research results), then it is entirely pointless to compare his (or Watchman's) Côa radiocarbon results with those of an untested, untried and highly experimental method such as CR 'dating'.

To appreciate the magnitude of the problems the CR method faces (never having been successfully used in archaeology; *cf.* attempts at Stonehenge), especially when it is seen in the context of erosional retreat, it is pertinent to consider the following points. Assuming that the production of the nuclides does occur as the theory predicts, it would be subject to numerous variables — as Phillips and colleagues well appreciate. The changing topography through time of a canyon of soft and unstable rock has a major impact on a site's exposure to radiation, but for the past it is an unknown variable. The shielding factor we assume for each sample may be valid for the present, but probably not for the past, since cosmogenic radiation itself is not uniform and would have varied according to extraterrestrial and atmospheric factors. Erosion rates (which have such a great influence over relative nuclide concentrations) also differ with local topography and lithology, and differed even more in the past. Variables of past topography, climate and hydrology cannot effectively be accounted for. What we do know with some degree of certainty are the general retreat rates of natural stone, as determined for engineering purposes (e.g. from the extensive work conducted at the Institut für Allgemeine und Angewandte Geologie at the University of Munich, under the direction of W.-D. Grimm, from 1986 to 1988). According to such engineering data, which are derived entirely from structures of known ages, schists (second only to carbonates) are among the most rapidly retreating rock types, receding typically at 1–10 mm/1000 years. While this corresponds with some of the alternative rates suggested in the Paper, of 10 or 15 mm/1000 years, it must be emphasized that the engineering data do not relate to slabbing events over geological periods, but to granular or solution erosion at a micro-scale over the past two millennia. As will be obvious to even the most stubborn advocate of an Upper Palaeolithic age for certain of the Côa petroglyphs, these basic engineering data render such an age simply impossible: the petroglyphs would have experienced a rock surface retreat of between 20 and 200 mm in 20,000 years, the antiquity demanded by 'stylistic daters'. I think we can all agree that the 'Upper Palaeolithic-style' (hereafter UP-style) petroglyphs on the Côa are eroded only at a microscopic scale, in the order of microns, and that even the much older, non-UP-style petroglyphs on the same panels have erosion pitting of only <1 mm (Bednarik 1995a: figure 10a).

Perhaps we should consider the possibility that the engineers and architects got it all wrong, or that the Côa schists are somehow exceptional. But this possibility is countered by the many inscribed dates in the Côa valley, alongside the numerous Christian symbols and written inscriptions. The engraved dates provide good reference points concerning degree of patination and weathering. Whereas the date of '1944', occurring with a neat petroglyph of a train on a bridge, is almost unpatinated and offers little weathering evidence to the unaided eye, the two engraved dates of the 18th century on panel 7 at Canada do Inferno, occurring among panels of 'UP-style' petroglyphs, are so badly weathered and deeply patinated that their last two digits are almost undecipherable. They are thus much more weathered than the 'Palaeolithic' images just a few metres away. It is therefore a grave misconception to assume an absence of weathering on the Côa schists, even for periods of a few centuries. Conversely, lichenometric indices from some of the supposedly UP-style Côa petroglyphs suggest ages of 200–300 years.

When discussing the Côa petroglyphs, most authors — myself included — have glossed over the fact that the vast majority of the valley's rock-art motifs are either historical or Christian symbols, inscriptions, or were made with metal tools, or are of a style not remotely resembling any known Palaeolithic art. The number of 'stylistically Palaeolithic' figures is quite minor (Zilhão *et al.* 1997), and they are usually among the least weathered or patinated motifs. They are the only ones sampled by both Dorn (1997) and Watchman (1995; 1996), whose brief it was to try and determine the ages of UP-style images, not those of other styles.

Finally, the extensive archaeological work conducted in the valley refutes the 'dates' presented in the Paper. Zilhão *et al.*'s (1997) only radiocarbon result from the 62 sites they have investigated is 1000±60 BP (Sac-1322), confirming as they note Watchman's TL dates from the same terrace. The earliest occupation found in the valley consists of a distinctively microlithic industry, found together with pottery remains. Only one site, Cardina I, yielded a ceramics-free microlith assemblage, which may well date from the Mesolithic. This agrees precisely with the initial age estimates for the oldest rock-art component (Bednarik 1995). Zilhão's team accepts my proposition (Bednarik 1995a: 87) that the lower Côa valley, cut 300 m deep into the Plio-Pleistocene planation surface, is very recent, and an entirely Quaternary feature (Zilhão *et al.* 1997: 11). They suggest an age of 90,000 years BP for a terrace remnant now 40 m above the river, although without testable evidence. However, geologically such an age is realistic, as it corresponds roughly to the relative valley depth. So the river has cut down another 40 m since this terrace was formed, and yet according to table 1 in the Paper, a cliff now exposed at the same site, just above the river, became exposed 355,000 years ago! This is simply a physical impossibility, because none of the present cliff exposures near the bottom of the valley could have existed when the river was 40 m above its present elevation. It shows dramatically that the 'dating' results presented in the Paper are meaningless random numbers.

Cosmogenic radiation products cannot provide any dating for rock-art, nor has anyone claimed this. Even if we could share the belief of Phillips to have produced numerical *exposure* ages, these would only represent maximal restraints on petroglyph ages. In this context I am surprised that these authors failed to cite the first-announced exposure age estimation of a Côa rock surface, of 'perhaps 30,000 years' (Bednarik 1995b: figure 1), especially as it agrees with their belief that the Côa exposures were available for engraving during the Late Pleistocene. This rough age estimate was secured from a large quartz vein at Penascosa, but I remain sceptical about its validity, for precisely the reason Dorn (1997) cites on the question of 'inherited weathering'. It seems possible that substrate weathering occurred before exposure to the atmosphere, provided that this quartz surface had been in a fissure long accessible to water flow. If my exposure age of *c.* 30,000 were valid it would indicate the availability of *some* rock exposures during Upper Palaeolithic times, at geomorphologically stable sites such as Penascosa (where exposures are clearly older than at Canada do Inferno). So the Paper would merely support my earlier finding, apparently without the authors realizing this.

However, I have to disagree emphatically with them when they say (Phillips *et al.* 1997: 102) that after spalling, 'the accumulation of cosmogenic nuclides *will be initiated* at the fresh face at the time of the spall event' (my italics). Nuclides such as ^{36}Cl have accumulated in the rock in question long before it became exposed at the surface, as the authors themselves amply demonstrate, and no method has been offered to quantify either the pre-spalling or the post-spalling component of any nuclides present (consider Phillips' data from 16 m below the Côa river). Therefore this method, as it is currently used, is fundamentally incapable of determining rock exposure duration. This does not necessarily mean that it will join that other CR method (cation ratio) of dating geomorphic exposures and become a redundant oddity. There may be valid applications for cosmogenic radiation products analysis (preferably of ^{3}He, ^{10}Be, ^{21}Ne, ^{26}Al, ^{41}Ca), especially where the exposure of deep rock occurred instantaneously, on a geological time scale (e.g. through major meteor impact, large-scale geomorphic or geophysical adjustment, or human action such as mining or deep quarrying). Such events are likely to expose material of negligible 'background signal'. Another potential application of this method refers to rock of uniform background signal that has been unevenly exposed to radiation since an archaeological event. For instance, one might analyse two blocks in a pyramid, one at its surface and one in its interior, provided they can be matched up by their sedimentary strata or other petrological characteristics. In this way, one could presumably estimate the time-span since the blocks were quarried, or even the depth they were quarried from. This method may still have useful applications in archaeology, although certainly not in rock-art dating. But even for less intricate applications, much work still needs to be done in calibrating results in blind tests, and in securing a better understanding of numerous variables. The ^{36}Cl method, in particular, cannot be used where vadose water travels through a higher-lying rock mass. While this may not present a problem on rock pavements or in manmade structures, it renders the results from the Côa rock exposures entirely meaningless.

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