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The age of the Côa valley (Portugal) rock-art: validation of archaeological dating to the Palaeolithic and refutation of 'scientific' dating to historic or proto-historic times

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The dating studies of the 'modern rock-art scientists', when critically examined, are found not to show that the Côa valley petroglyphs are of recent age. Their Upper Palaeolithic characteristics, and therefore their likely late Pleistocene age, are consistent with their archaeological context.

Results of EDP's project

For the most part, the thousands of petroglyphs recently found in the Côa valley have been unanimously attributed to the Palaeolithic, on stylistic grounds, by archaeologists and rock-art experts from all over the world (Bahn 1995). EDP (Electricidade de Portugal), the state-owned electricity company building the dam that threatens to submerge and destroy the site, however, decided that stylistic analysis was not enough to establish the age and relevance of this art. They organized a 'direct dating project' aimed at obtaining 'scientific' chronological estimates and hired two Australian (Alan Watchman and Robert Bednarik) and two American (Fred Phillips and Ronald Dorn) dating professionals to carry out these studies.

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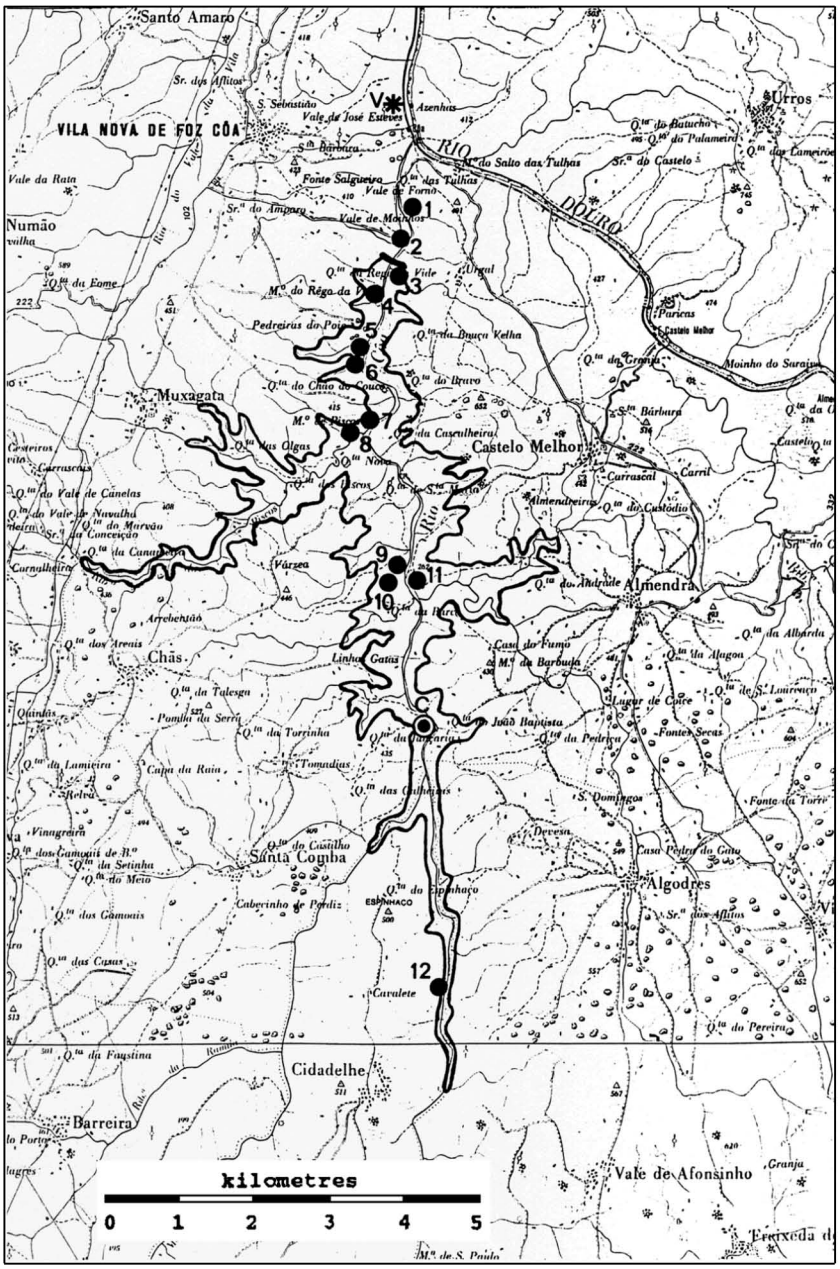

FIGURE 1. *The Côa valley, with location of the several clusters of rock-art attributed to the Upper Palaeolithic on stylistic grounds as they were known in March 1995.*

The limits of the lake that will flood the area if the dam is constructed are also indicated.

The base is the 1:100,000 map of the area published by the Instituto Geográfico e Cadastral.

After Rebanda (1995).

1 *Broeira.*
2 *Vale dos Moinhos.*
3 *Canada do Amendoal I.*
4 *Canada do Inferno.*
5 *Vale Videiro.*
6 *Vale de Figueira.*
7 *Foz de Piscos.*
8 *Ribeira dos Piscos.*
9 *Quinta da Barca I–II.*
10 *Quinta da Barca III.*
11 *Penascosa.*
12 *Faia VI.*

Letter V *represents the Iron Age and Palaeolithic art-site of Vermelhosa, on the Douro.*

Letter C *represents the Upper Palaeolithic camp-sites of Cardina, on the Côa.*

The four researchers are supposed to have worked independently, in a 'blind test' experiment. Although EDP (Electricidade de Portugal) and the Minister for Industry have used their reports as the basis for policy, they have not made them public. The authors have refused to make them directly available to Portuguese archaeologists, although a copy of Bednarik's report was obtained through independent channels.

Evaluation of EDP's 'direct dating project' therefore relies on the following:

- The results reported by *O Independente* (Sá & Ferreira 1995);
- Bednarik's (1995b) report;
- Dorn's (1995) table of minimum ages, given to the press by the office of the Minister for Industry;
- Watchman's (1995a) 'Executive Summary',

also given to the press by the office of the Minister for Industry;
- Watchman's statements to the Portuguese newspaper *Público* (Salema 1995);
- Watchman's statements to *Science* (Fischman 1995);
- Abstract of Watchman's (1995b) paper at the September 1995 International Rock Art Congress of Turin (Italy).

FIGURE 1 shows the location of the Côa valley art-sites known to contain stylistically Palaeolithic petroglyphs. The first 6·5 km upstream from the river-mouth have been partially flooded since the early 1980s by the Pocinho dam on the Douro. The engravings presently visible in localities 3–6 of FIGURE 1 represent just the tip of an artistic landscape now submerged under a few metres of water most of the year. The Foz Côa dam would drown these sites to a depth of more than 100 metres.

According to EDP (1995), all four researchers worked on the same three panels, at Canada do Inferno (FIGURE 1.4), Ribeira de Piscos (FIGURE 1.8), and Penascosa (FIGURE 1.11). As other illustrations in this paper, the tracings of these three panels (FIGURE 2) are partial and preliminary, intended only for independent stylistic evaluation by the readers, a purpose for which they are considered accurate enough. Systematic tracing and analysis of the Côa rock art is now under way by a government appointed team led by Portuguese rock-art researchers A.M. Baptista and M.V. Gomes.

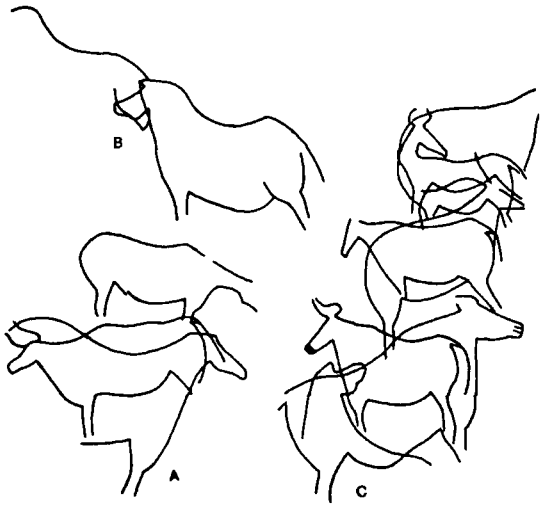

FIGURE 2. *The panels analysed in the framework of EDP's direct dating project:*
A *Canada do Inferno.*
B *Ribeira de Piscos.*
C *Penascosa.*
In this figure, as well as in FIGURES 4, 6 and 7, the tracings were done from photographs and cannot be taken as an exact rendering of the subjects (that from Canada do Inferno has been deliberately oversimplified by the elimination of the less complete or less clear animals).They are presented here as a means to illustrate the discussion, and we consider them to be accurate enough for that purpose. Most of the stylistically Palaeolithic figures from the Côa have sizes between c. *50 cm and* c. *1 m.*

Phillips' Chlorine-36 dating

Fred Phillips used Chlorine-36 studies to attempt a direct dating of the age of exposure of the engraved rock surfaces, that is, to obtain a maximum age for the petroglyphs.

Sá & Ferreira (1995), published on 7 July, report that Phillips obtained a maximum age of 3000 years for the Canada do Inferno panel. In a letter to the present author, dated 20 July, Bednarik mentioned Phillips' results as part of the 'proof' that the three panels were post-Palaeolithic; he repeated this in oral presentation to the Turin congress. But a letter by Monty Flinsch, a collaborator of Phillips, dated 7 July and distributed at a press conference held by the EDP on 13 July, stated that it had not yet been possible to process the Côa samples, and that it would not be possible to do it until late August. How anyone could 'know' the Chlorine-36 results before the chlorine had been extracted from the samples was explained when Watchman informed the Turin congress that the 3000-year-old date had been obtained by Phillips from a sample previously collected by Dorn. Such a date is the output of preliminary work, and evaluation of its significance is premature.

At the time of writing (early October 1995), Phillips' final report does not seem to have been presented to EDP, and any speculation on what it may say is useless.

The report from the Sydney 1993 Workshop on Production Rates of Terrestrial In Situ-produced Cosmogenic Nuclides (Reedy *et al.* 1994) records many uncertainties regarding the rates of production of Chlorine-36, which are subject to past changes in cosmic-ray fluxes, and to local variations determined by latitude and altitude, and by rock composition and geom-

site	petroglyph	age range (1σ error)
Canada do Inferno	FC-95-1: Left-Oriented Ox	1513–1022 BC
	FC-95-3: Upper Horse	
	Analysis a	**1912–1513 BC**
	Analysis b	1731–1324 BC
Penascosa	FC-95-4: Top Goat	
	Analysis a	**3343–2703 BC**
	Analysis b	3028–2487 BC
	FC-95-6: Big Ox, Right Oriented	
	Analysis a	193 BC–AD 135
	Analysis b	**373 BC–AD 19**
Ribeira de Piscos	FC-95-7: Horses with Heads Together	
	Analysis a	**1893–1405 BC**
	Analysis b	1401–910 BC
	Analysis c	1878–1396 BC

Because these are minimum ages, the oldest sub-sample (bold) is the closest age estimate.

TABLE 1. *Five petroglyphs analysed from Foz Côa have minimum ages (Dorn 1995).*

etry. The community of dating experts regard this method as in an experimental stage of development. The results, no matter which, to be presented by Phillips once his analyses are complete, cannot possibly be fully reliable chronological estimates for the age of exposure of the engraved surfaces.

Dorn's AMS dating of organic matter encapsulated in weathering rinds

Dorn obtained *minimum ages* for five figures from the three panels, all (based on the upper end of the calibrated range) older than 135 AD (and one older than 2703 BC (TABLE 1).

EDP (1995) and Bednarik (letter, 20 July 1995) interpret these dates as accurate assessments of the *moment* when the petroglyphs were made and, therefore, as demonstrating that the petroglyphs are not of Palaeolithic age.

Nothing in Dorn's own writing (the table of minimum ages published by the Portuguese press (Dorn 1995; here TABLE 1) — and two letters to the present author, dated 18 and 26 July 1995) suggests this is his opinion.

Absolute dating of surface encrustations, accretions or patinae that cover and post-date petroglyphs only provides minimum ages; the art itself may be just marginally older than the surface 'skin' or it may be many times as old, as Bednarik (1992a) himself acknowledged.

On present evidence, it seems that Dorn's report is being abused by both Bednarik and EDP.

Watchman's AMS dating of organic matter encapsulated in mineral accretions

Watchman's study dated organic matter fossilized in the crusts that had been deposited by water flowing over the petroglyphs, to obtain a minimum age for them; and it dated identical skins on adjacent rock surfaces, which would provide a maximum age; the true age of each petroglyph would therefore be bracketed by the results of each pair of ages (Fischman 1995). That is how Watchman is supposed to have obtained the results he gave to EDP:

- the Côa engravings have a maximum age of 1700 years (Fischman 1995; Salema 1995; Watchman 1995a; b); and
- most were done only some 100 years ago (Salema 1995).

These statements were reasserted in Watchman's presentation to the Turin congress, where he said the petroglyphs were made by people working at the numerous water- mills functioning along the river margins in the late 19th and early 20th centuries.

Watchman's approach is based on several unverified assumptions which, upon scrutiny, reveal themselves as somewhat naïve.

Assumption 1: The silica skins, formed once and for all, have no subsequent history of weathering and erosion.
If the dated crusts on adjacent rock surfaces are to provide a maximum age for the petroglyphs, they must be the same that started to develop soon after the rock surfaces were first exposed in the past. Experience in rock-art research shows this assumption is wrong. A leading rock-art researcher, Andrée Rosenfeld writes (letter to the present author, 28 July) :

> I have . . . come across petroglyphs covered by one type of patina, which then partly erodes, and is partly replaced or covered by different material clearly reflecting a history of changing weathering conditions at the site. . . . Weathering conditions and patina formation are likely complex dynamic processes, and I doubt that we have fully understood them. It is for that reason that I have not attempted to involve patina dating methods in my own research.

Assumption 2: No organic matter can penetrate these skins after they are formed.
All radiocarbon dating depends on the premise that the sampled material ceases interaction with atmospheric ^{14}C at a certain time (death of a living organism, for instance), and it is the age of that event which is measured. Watchman assumes that the mineral accretions meet this condition, having passed from an 'open' to a 'closed' carbon system. Most archaeologists, rock-art researchers and dating experts would disagree.

In caves, modern tree roots commonly penetrate stalagmitic crusts that are sometimes many centimeters thick. It seems difficult to accept that lichen and fungi presently growing on mineral accretions do not contribute to the 'organic matter' encapsulated in them; the accretions, according to Watchman (Fischman 1995), are only fractions of a millimetre thick. Physical pre-treatment might eliminate rootlets and filaments belonging to such organisms, but it would not eliminate the chemical effects of their activity (bear in mind the obvious analogy between these accretions colonized by live organisms and ordinary soils). The amounts used in AMS dating are extremely small and — according to Werlhof *et al.* (1995) — Watchman's laser extraction techniques preclude traditional chemical decontamination. Werlhof *et al.* (1995) give clear verdicts on these issues:

- 'unlayered varnish is not a closed system';
- 'available empirical data reveals that organic matter that is encapsulated by rock varnish is younger than independent controls';
- 'small samples are easily "contaminated" by noncontemporaneous organics';
- 'until [current] uncertainties are addressed . . . all ^{14}C ages on surficial rock-art must be viewed as experimental'.

If Watchman believes that unlayered mineral accretions, patinas, rock varnishes, etc., are closed systems, he has to prove this point, not assume it. The experience in dating carbon of unknown (and, therefore, possibly composite) provenience contained in these deposits is that it provides random results. Werlhof *et al.* (1995: 266) report that filaments in a weathering rind under live epilithic lichens growing in a South Australian petroglyph gave a ^{14}C measurement of 687±84 b.p. (NZA 2275); but 'organics' encapsulated by rock varnish gave an age, for the same petroglyph, of 14,910±180 b.p. (NZA 1367). A composite sample mixing carbon from both proveniences would have given an intermediate, meaningless, age.

Assumption 3: The silica skin on adjacent rock surfaces was already there when the engraving was done.
If objections to the first assumption are valid, then the silica skin on adjacent rock surfaces can have exactly the same age as that covering the petroglyph, or can be even younger (FIGURE 3). A mineral accretion covering a rock surface (and the petroglyphs engraved on that surface) could go through an erosion process that eliminates it, except where sheltered from the erosional agent inside the grooves defining the petroglyph. Subsequently, a new skin could develop over the exposed surface, and cover the petroglyph as well. In that case, the skin on the rock adjacent to the petroglyph would be *younger* than the lower part of the stratified skin preserved inside the grooves defining the petroglyph.

Assumption 4: The petroglyphs were never rejuvenated in time-periods subsequent to the first episode of groove formation.
There is ample evidence that rock-art was frequently rejuvenated, with methods affecting

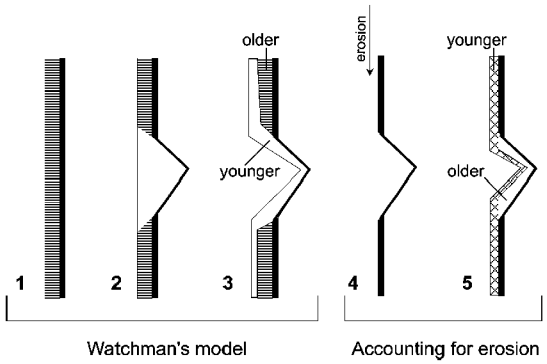

FIGURE 3. *Models for the genesis of mineral accretions on rock surfaces. In Watchman's model, the material filling the grooves is younger than that in adjacent rock surfaces:*
1 *Mineral accretion forms on freshly exposed surface.*
2 *The incision by petroglyph execution eliminates the accretion from the affected area.*
3 *Subsequent accretions fill the groove and bury the former in the adjacent rock surface.*
If erosional processes are accounted for, the stratigraphic relationship may be reversed.
4 *Erosion eliminates all mineral accretions on the rock surface, except in the sheltered grooves.*
5 *Subsequent accretions are younger in the adjacent rock surface than at the bottom of the groove.*

not just the grooves but the entire rock surface. So the idea is untenable that a maximum age can be derived from organic matter in the silica skin adjacent to a petroglyph. At best — if one disregards the objections raised above — Watchman could have obtained maximum ages for the time the petroglyphs were *last* rejuvenated, not for the time they were *first* engraved.

In short, no dating of the Côa petroglyphs by Watchman's method would make a reliable maximum age assessment for their original production. By Watchman's own admission (Fischman 1995), when results ranging from 650 to 7000 b.p. were actually produced, the mineral accretions on the engravings turned out to be *older* than those on the surrounding rock; that is, the *maximum* ages were *younger* than the *minimum* ages! This scenario, explicable when one considers the issues noticed here, is in absolute and total contradiction with the expectations of Watchman's model of crust formation and development; it represents, by itself, enough evidence of the inadequacy of his approach and of the unreliable nature of his age estimates.

Watchman's 'post-hoc accommodative argument'

In a letter to EDP dated 19 January 1995 (four months before setting foot on the Côa valley), in which he offered his services as a dating professional, Watchman said how 'greatly disturbed' he was to know of 'so-called archaeological experts professing to know the antiquity of the engravings without carrying out any scientific dating tests'. He made it clear to EDP, 'I therefore do not accept the general consensus, that seems to pervade Europe, that engravings and paintings of horses, bulls, etc., are necessarily 20,000 years old.' Also, reports Fischman (1995), as soon as Watchman arrived in the Côa valley and began to examine the engraved panels, 'several bells went off in my head saying these things are young'. So, it is fair to say that Watchman began his work convinced that the engravings were not of Palaeolithic age but, instead, probably very recent.

When he obtained results indicating minimum ages in the order of 7000 years he was therefore 'puzzled' (Fischman 1995): he had expected the art to be modern, and the skins on surrounding rock to be older than those covering the petroglyphs. Did he re-examine both theory and expectations, or show prudence and modesty in presenting his interpretation of the petroglyph chronology? Instead, he issued categorical statements as to their very recent age, based on what Binford (1983) named 'post-hoc accommodative argument'.

In the limited space the ANTIQUITY editor permits me, it is only possible to sketch the inconsistencies, contradictions, unsubstantiated assumptions and unanswered questions in Watchman's further reasoning. The first step was to propose contamination by graphite weathering out of the rock, 'formed from ancient carbon' — 'it made the engravings look anomalously old' (Fischman 1995). But why should adjacent surfaces be 'uncontaminated', while crusts inside the grooves defining the petroglyphs are 'contaminated with ancient graphite and charcoal' (Watchman 1995a)?

Where does the graphite Watchman found in the grooves come from? Was it introduced from the materials — pencils, chalk, paint,

wood charcoal — used by visitors to enhance the pictures before the Côa art-sites were fenced? The many such 'enhancements' at the sites sampled in the EDP dating project question a basic premiss of Watchman's approach — the integrity of the 0·1-mm-thick crusts he sampled. And Watchman had said, in his letter of 19 January 1995 to the EDP board: 'any form of chemical addition to the surface, especially paint, will significantly affect the chances of ever obtaining a reliable radiocarbon date for carbon-bearing substances that may be present in the engravings' . Bednarik, Watchman's co-participant in EDP's 'direct dating project', seems to be, or to have been, of the same opinion: 'the introduction of foreign carbons, by any means and in any amounts, renders AMS radiocarbon dating invalid' (Bahn *et al.* 1995: 31).

We do not know what is the history of the carbonaceous material Watchman dated. If graphite — from what source? Or, if the 'organic matter' in the mineral accretions is essentially made up of diatoms encapsulated by silica, as Watchman told the Turin congress, how do the dated samples turn out to be essentially made up of graphite?

In short — and the arguments and counter-arguments are not short — it would seem that all of the crusts Watchman sampled suffered from two grave problems to a greater or lesser extent. The true origin and history of the carbon dated is unknown. And the results do not show that clear pattern which would follow from his idealized model of how the crusts are formed and behave. The carbon may consist of a mix, in varying proportions, of:

- old carbon (graphite) from the rock itself and its weathering rinds;
- carbon that is penecontemporaneous with the formation of the different types of mineral accretions covering the rock surface and the petroglyphs (organic carbon from dead organisms encapsulated in those accretions); and
- recent carbon incorporated through pedogenetic and anthropic processes in the rock, the weathering rinds, and the mineral accretions.

This is implicitly admitted by Watchman himself when he states that 'graphite [. . .] occurs in thin yellow-brown weathering rinds at the base of the silty brown accretions' found inside the grooves defining the petroglyphs (Watchman 1995b). Since he had told us before that the dates obtained referred to the silty brown accretions themselves ('carbon from silty brown accretions developed in engravings gives ages ranging from 3000 years to almost 7000 years ago'), that statement implies that his samples mix carbon-bearing substances with two different proveniences: the loose brown silty crust filling the grooves; and the weathering rind of the rock at the bottom of those grooves, under the crust. The lack of integrity and extreme thinness of the unstratified mineral accretions analysed, together with the admitted lack of precision in sampling, force us to conclude that Watchman's 'dates' are chronologically meaningless expressions of ^{12}C to ^{14}C ratios with no relevance to the real age of the petroglyphs and the real age of exposure of the engraved rock surfaces.

This explains the logical importance, in Watchman's construct, of the contextual reasoning explained by Fischman (1995), according to whom 'A clue to their true age came when Watchman learned that the remainder of the brown layer consisted of silt probably eroded from the hillsides above when farmers began cultivating. That happened about 1700 years ago — which he thinks is the maximum age of the images.' That is, *c.* 1700 b.p., under the environmental change brought about by the beginning of agriculture, silica skins ceased to form; inhabitants subsequently engraved the rock surfaces covered by these silica skins; the grooves were then filled with a loose silty brown crust made up of material eroded from the surrounding slopes and containing varying amounts of old charcoal, notably graphite, responsible for the anomalously old 'minimum ages' (Watchman 1995a; b).

Since his results for those silica skins cover a wide time-range — most gave younger dates than 1700 and at least one for an unengraved rock was 4300 b.p. (Salema 1995) — Watchman sorts those that are 'good' among them from those that are 'bad' (or 'contaminated'). The 'good' ones are in accordance with the 1700 b.p. date he presumes for the beginning of agriculture in the Côa valley; this he equates with the moment when those skins ceased to develop.

Watchman's environmental model of the mineral accretions developing in the Côa val-

ley rock surfaces is based on three contextual arguments. Two are *unverified assumptions*:

- the silt in the crust that covers the grooves eroded from the surrounding slopes (which is likely, but where are the analyses that prove it?); and
- such erosion only took place as a result of farming (why not before forest development, in late Last Glacial times or in the early Holocene?).

The other is an *outright invention*:

- such slopes were cultivated for the first time 1700 years ago (how does he know? what research did he undertake to justify this assertion? how does he explain away the evidence for a Neolithic — that is, at least 6000-year-old — settlement of the area by farmers?).

The maximum age of 1700 years that Watchman allows the Côa art is not based on the radiocarbon results, that is, on the direct dating technique that was supposed to give him superior powers of chronological estimation, but entirely on these geochemical and archaeological assumptions — that silica skins ceased to form as a result of agriculture beginning in the Côa valley only 1700 years ago — which are used to accept (in some cases) or to disregard (in others) those radiocarbon results.

It is quite clear that Watchman's 'maximum age' cannot be considered, from a scientific point of view, as a valid critique of the Palaeolithic age of the petroglyphs engraved in the analysed panels as determined by stylistic criteria. And Watchman's 'maximum age' of 1700 years contradicts the results obtained by Dorn, who also used AMS radiocarbon dating but arrived at a different chronology: that all the engravings are *older* than 135 AD.

Implications that Watchman refuses to face

In Portugal the last 1700 years are fully historical, and written documentation on art and religion is available for the entire period. The idea that thematically and stylistically Palaeolithic art could have been done in the Middle Ages or subsequent centuries is ludicrous.

By categorically stating that a large majority of these petroglyphs were actually done no more than 100 years ago (Salema 1995), probably by the millers of the valley, Watchman aggravates his case:

- Given that the grand- and great-grandchildren of the artists would still be alive today, why has no memory of their art survived the passing of only two or three generations?
- Given that late-19th- and early- to mid-20th-century rock-engravings are also known in the valley, their exact age provided by their being signed and dated, and by the nature of the representations (castles, a train on a bridge, clocks, etc.), why is no signing and dating associated with the stylistically Palaeolithic petroglyphs?
- Given that the stylistically Palaeolithic petroglyphs represent animals extinct in the area for thousands of years, how could 19th-century millers have learned about the existence and aspect of such animals?
- Finally, how were the millers able to represent those species following conventions typical of the cave-art that, at that time, still awaited rediscovery by modern science?

This theory — of the engravings being done last century by the millers — is not of Watchman's own devising. It was presented more than one month before Watchman came to the Côa, as a major feature in the main news report of a Portuguese TV station. As they explained the day after, that had been their way of respecting the April Fool's tradition. Watchman elaborated this argument in an interview to the Portuguese news agency *Lusa* published by several Portuguese newspapers (*cf. O Primeiro de Janeiro*, 6 September 1995), stating: 'at Canada do Inferno there are petroglyphs in areas where water mills for flour production and even a nice sand beach used to exist' [before flooding by the Pocinho dam]; and 'it is possible, therefore, that these engravings are no more than 20 to 50 years old and were made by the bathers for entertainment and amusement'.

The spectacle of 20th-century bathers drawing extinct Pleistocene animals is difficult to contemplate. . . . It is in any case contradicted by historical evidence: letters and other documents recently found in the Municipal Library of Mirandela (a near-by town) demonstrate that the engravings in the Côa valley already existed in 1939, when some were identified and described (but never published) by a local doctor, José Silvério de Andrade (*O Comércio do Porto*, 16 July 1995).

Bednarik's micro-erosion dating

Robert Bednarik, supposed to have used a micro-erosion direct-dating technique, finds none of the stylistically Palaeolithic engravings

could be older than *c.* 6500 BP, and most were done only some 3000 years ago.

Micro-erosion was theoretically presented for direct dating of petroglyphs in *Archaeometry* (Bednarik 1992a). It is based on the concept of a relation between the degree of erosion (measured through observation with a binocular microscope) suffered by some rock components (e.g. quartz grains) exposed by the engraving and the amount of time elapsed since it was executed. The only application published is for Besov Nos, Lake Onega, Russia (Bednarik 1992b) — engravings which are otherwise insecurely dated. No true 'blind test' of this technique was undertaken; that is, there has been no experiment in dating an engraving whose age was unequivocally established but unknown to the practitioner of the method. Nor has there been replication of results by different practitioners in similar 'blind test' conditions. Until these two conditions are met, micro-erosion can only be considered, at best, as an interesting possibility; it is not a dating method, even at an experimental level. In any case, as Rosenfeld (letter, 28 July 1995) states, micro-erosion 'was developed by geographers in the UK to measure the erosion rate of large natural rock surfaces — it depends on taking numerous readings and was intended to arrive at an average rate for extensive areas. It was not designed to allow for the many uncontrollable variables that may operate at any one spot on a rock surface — as e.g. in a petroglyph.'

Even if Bednarik's variety of micro-erosion analysis for petroglyph-dating turns out not to be a bad idea, one is faced with the fact, according to Bednarik himself, that 'schist and other rocks of low metamorphism (slate, phyllite) are not well suited for micro-erosion analysis'. Of many reasons why this is so, one is readily apparent: the mineral components of these rocks are of such microscopic size that the observation of microerosion features is impractical. Actually, 'there are a few petroglyphs on granite in the Côa valley, which is an ideal rock for this method, but they are of difficult access and time did not permit me to see them,' reports Bednarik (1995b). Bednarik *could* have used his method on what, in his own terms, would be suitable rocks (the granites bearing stylistically Neolithic petroglyphs), but chose instead to use it on the unsuitable schists bearing the stylistically Palaeolithic engravings.

Another precondition for Bednarik's technique is a locally valid calibration curve, with certain parameters established by study of engravings whose age is unequivocally established. In Bednarik's own words (1992a), 'in a practical application of dating a geomorphic or petroglyph surface through assessing microerosion phenomena it is requisite to determine the rate of wane development, initially by establishing a calibration curve for surfaces of known, or approximately known, ages. These rates may vary in different climates, rock types, and even mineral compositions.' That is why 'without locally established calibration curves this method is of poor accuracy' (Bednarik 1995b). Since dated inscriptions are known in the Côa, one might think that such a calibration curve could be established. Of these historical petroglyphs, Bednarik (1995b) states, 'the extreme anisotropism of the rock would render it difficult to accept the engraved date for calibration purposes, which applies also to historical inscriptions I observed elsewhere in the region'.

If the rocks are not appropriate, and if there is no local calibration curve, how can Bednarik legitimately claim that he applied microerosion dating? How does he arrive at an age estimate? To the last question, the answer is simple. His conclusion — 'among the figures I have seen, none can be older than 6000 to 8000 years at the most' — derives from the maximum age he gives to one figure in the main panel of Canada do Inferno, which 'would be between 4500 and 8500 years old, with the highest probability at about 6500 years BP'. This number follows the 'experimental' application of the Lake Onega calibration curve, 'ignoring the differences in climate and lithology', to the pattern of measurements made by Bednarik on this figure. According to him, such an application 'does not provide us with a true age of the motif but does offer a fairly reliable indication of magnitude of age for this figure' (all quotes from Bednarik 1995b).

How absurd the whole thing is! An 'age' for slate petroglyphs in Portugal is based on a calibration curve for granites from Russia!

Application of that same calibration curve to a quartz vein in panel 6 at Penascosa (FIGURE 4) 'would imply an exposure age of the

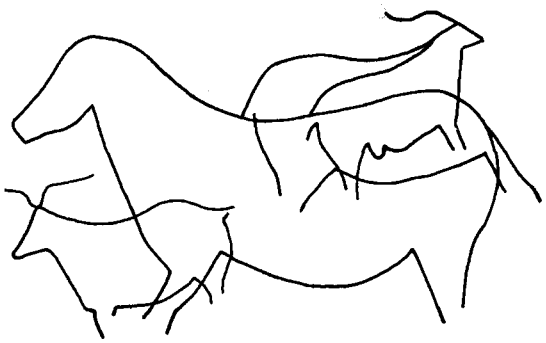

FIGURE 4. *Penascosa: tracing of panel 6 (see caption to* FIGURE 2*).*

panel of perhaps 30,000 years'. Why is 6500 years a reliable maximum age, and 30,000 years not reliable? Because, Bednarik says, the older date 'seems too high to me in view of the absence of gelifraction damage'. And because the stratigraphy and differential microerosion of abraded and pecked figures would imply that the pecked ones 'would have to be several times as old', something impossible to accept on the basis that 'we have no indication of an iconic rock-art tradition in Europe from the Middle Palaeolithic, which such an interpretation would stipulate' (all quotes from Bednarik 1995b).

The preceding paragraph immediately brings up several questions. Why does a differential microerosion between abraded and pecked figures imply a large time-span would separate both kinds of motifs? Why is it not just a by-product of the different engraving techniques, and of the resulting different response to weathering? Where are the studies proving that weathering — or, for that matter, accumulation of crusts, or the formation of patinas — is a linear function of time? Why should gelifraction be expected in the Côa valley? (Bednarik's reasoning restates, almost 120 years later, 19th-century arguments that Altamira was a fraud: the art could not possibly be that old because it simply would not have been able to survive for so long!)

Besides frost-weathering, Bednarik (1995b) invokes, *en passant*, geological, archaeological and stylistic arguments, which he considers as 'still more serious objections to the Palaeolithic antiquity of the Côa art'. Since his two direct-dating estimates, at about 6500 and about 30,000 years, are so far apart, it is these arguments that decide which is the 'good' and which is the 'bad' one. Bednarik's microerosion analyses at Côa having violated every single methodological requirement established in his own theoretical papers on the issue, it is those contextual arguments that are the real logical foundation of his dating of the stylistically Palaeolithic figures to post-Palaeolithic times.

Bednarik's arguments on context

Contextual arguments in Bednarik's dating report (1995b) to EDP are essentially those, developed before ever having set foot in Portugal, which he stated in the April 1995 *AURA Newsletter* (Bednarik 1995a). He has developed them further in a paper forthcoming in *Rock Art Research* (Bednarik in press).

Information on Portuguese Upper Palaeolithic faunas can be found in Cardoso (1993), and on chronological, environmental and cultural evidence in Zilhão (1995). Marks *et al.* (1994), Póvoas *et al.* (1992) and Zilhão (1988; 1989; 1990; 1991; 1993; 1994) contain partial discussions of relevant data in English or French.

Frost-weathering

In the littoral region of Estremadura, Last Glacial cryoclastism of limestones is known almost down to present sea-level (Daveau 1980). But below 400 m this process does not seem to occur after the Last Glacial Maximum (Rodrigues 1991; Zilhão 1995), coming to an end with the Upper Solutrean, *c.* 17,000 b.p., when a very warm oscillation is recorded in sea surface temperatures off the Portuguese coast, which by then reached almost modern levels (Bard *et al.* 1992; Duplessy *et al.* 1992). In the interior regions of north and central Portugal, however, as a result of extreme dryness and different lithology (schists and granites instead of limestones), cryoclastic slope deposits are unknown below an elevation of 700 m above modern sea level, so there is absolutely no basis to believe frost-weathering would have affected the low-lying (*c.* 100 m) and sheltered valley of the Côa, especially after deglaciation in the Cantabrian mountains and in the Serra da Estrela was completed, some time between 16,000 and 14,000 years ago (Turner & Hannon 1988). In any case, test excavations carried out at the newly found archaeological site of Cardina, located in the Côa valley itself (see below), have now settled the issue: here, the Late Gravettian (*c.* 22,000 b.p.) deposits are colluvial sands entirely devoid of cryoclasts.

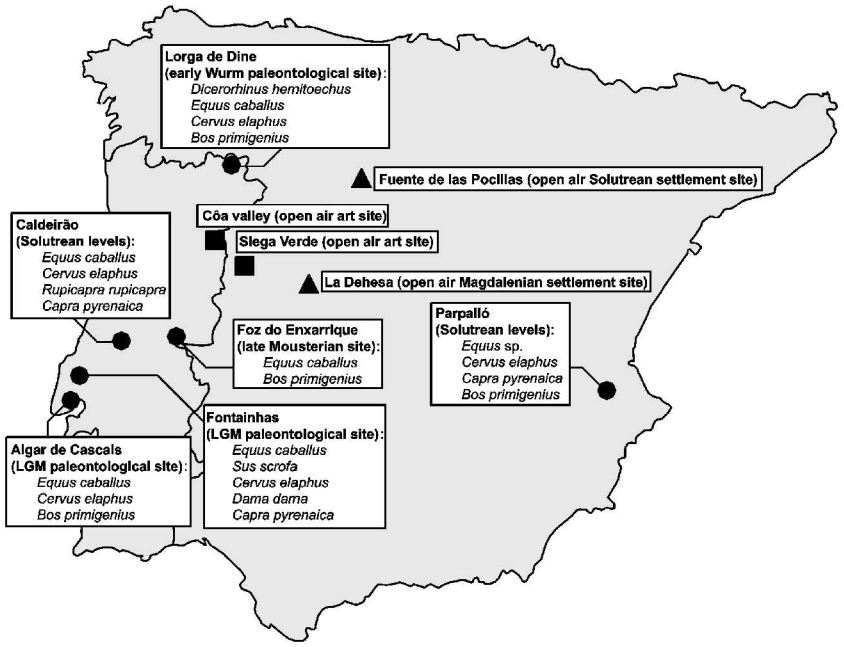

FIGURE 5. *Selected Iberian Upper Palaeolithic sites with archaeological and paleontological large herbivore faunas, and settlement sites in Spain close to the Côa valley complex of Palaeolithic open-air rock-art sites.*

Absence of cold-adapted species

It is true, as Bednarik (1995a) notes, that cave bear, bison, mammoth, woolly rhino and reindeer are not present in the Côa art. But this is exactly what should be expected: they are not present either at the many Upper Palaeolithic palaeontological and archaeological sites known in Portugal and in Mediterranean Spain (FIGURE 5).[1] All good evidence suggests that the Atlantic and Mediterranean façades of Iberia, south of the Ebro, may have constituted a separate faunal province, which cold-adapted species did not penetrate (Aura & Villaverde 1995; also Póvoas *et al.* 1994). Those species are also completely absent from the thousands of engraved slabs from the deeply stratified cave-site of Parpalló (Valencia), found in very rich radiocarbon-dated archaeological deposits, spanning the Gravettian, Solutrean and Magdalenian periods (Villaverde 1994). The animals represented in this mobiliary art — horse, aurochs, red deer, ibex, the occasional chamois, bird or carnivore (TABLE 2) — are exactly the same species as those whose bones have been recovered in the archaeological deposits (Davidson 1983). Stylistically, the representations are strikingly close to those in the Côa.

Bednarik (in press) objects to these observations on factual and on theoretical grounds. In the facts, he is wrong.[2] Since Cardoso (1993) reviews 21 cave-sites spread all over the coun-

1 Cardoso (1993) cites one occurrence of mammoth in his list of Quaternary faunal remains from Portugal. The bone in question is a large shaft fragment classified as part of the femur of an elephantid, which he reasons should be a mammoth. There is better cause to think it belongs to *Elephas antiquus*, which survived in Portugal up to *c.* 30,000 b.p. and could well have been represented in the otherwise banal Upper Pleistocene faunal assemblage from Algar de João Ramos.

2 He says, with outdated (more than 20-year-old) references, that cave bear is present in two Portuguese faunal assemblages from the Pleistocene (Furninha and Salemas), and that the geographical distribution of cave bear remains, which 'resembles the distribution of limestone karsts in Europe', indicates 'a massive taphonomic bias'. Accordingly, 'the apparent absence of cave bear remains in regions lacking limestone caves (such as most parts of Portugal) tells us absolutely nothing about the former range of the species'. This argument insists on a error of fact and entirely misses the point. The references to cave bear at Salemas are based on mistaken preliminary identifications by Zbyszewski (1963), subsequently accepted by Ferreira (1964) and Roche (1971; 1972); they have been corrected since by Torres (1979) and Cardoso (1993), who referred those remains to *Ursus arctos*. The bear remains recovered at Furninha have always been attributed to *Ursus arctos* since their first study by Harlé (1910–11). As a result, both Harlé (1910–11) and all subsequent authorities have given a categorical verdict in respect of *Ursus spelaeus* occurring in Portugal: the only bear species that can be recognized in the Quaternary faunal remains from the country is *Ursus arctos* (Torres 1979; Cardoso 1993). Bednarik's taphonomic argument is irrelevant.

	Gravettian		Solutrean			Solutreo-gravettian			Magdalenian			gal.	other	total
	G	LS	EMS	LMS	US	SGI	SGII	SGIII	EMA	EMB	UM			
aurochs	2	6	6	1	3	2	1	1	2	10	7	15	3	59
horse	1	7	14	8	12	20	6	9	9	12	15	10	5	128
deer	-	14	19	7	6	6	3	-	8	16	8	14	4	105
ibex	2	8	19	5	10	8	16	6	8	16	9	22	11	136
other	-	1	4	-	-	-	-	-	1	2	6	2	2	18
undetermined	2	27	42	29	19	19	9	14	19	29	25	51	32	320
total	7	63	104	50	55	55	35	30	47	85	70	114	57	766

TABLE 1. *Animals represented on Parpalló Upper Paleolithic decorated stone slabs.*

G	*Gravettian*	US	*Upper Solutrean*	EMA	*Early Magdalenian A*
LS	*Lower Solutrean*	SGI	*Solutreo-gravettian I*	EMB	*Early Magdalenian B*
EMS	*Early Middle Solutrean*	SGII	*Solutreo-gravettian II*	UM	*Upper Magdalenian*
LMS	*Late Middle Solutrean*	SGIII	*Solutreo-gravettian III*	gal.	*galleries (surface)*

'Other' animals comprise chamois, fox, lynx, wild boar, wolf, mustelids and birds.
After Villaverde (1994: table 26, modified).

try, the absence of such remains is not easily explained away as due to deficient sampling. His theoretical argument is that 'absence of evidence' does not equal 'evidence of absence': that remains of those species may eventually be recovered in other regions of the country or in new, as yet unknown, sites. This is a quintessentially non-scientific style of reasoning, one that is commonly found in Christian fundamentalist literature under the form, for instance, of statements such as, 'God exists because no one can prove that he doesn't.'

If present at all, those cold-adapted species would probably have occurred only as small and marginal populations (or even as stranded individuals), not in the daily environment of Upper Palaeolithic hunter–gatherers along the western and southern shores of Iberia. The tentative (and questionable) identification of bison, *megaloceros*, reindeer and woolly rhino among the fine-line engravings of Siega Verde (Balbín *et al.* 1995), already in Spain but only some 60 km southeast of the Côa complex of art-sites, might record such infrequent occurrences.

Survival of aurochs, deer and horse in the 'bleak tundra' of the Côa region

Bednarik (in press) argues that the Côa valley, only 35 km away from the mountain glaciers of Serra da Estrela, 'experienced severe periglacial conditions which only very few species could be realistically expected to have survived . . . in this bleak tundra landscape'; 'aurochs, deer and horse were almost certainly not among them, and ibex only in summer'. This statement is an excellent illustration of Bednarik's ignorance of the Palaeolithic record of Iberia, and of the geographical realities of the present-day world. Any postcard from Switzerland suffices to refute the concept that the landscape 35 km away from a mountain glacier must be a 'bleak tundra'. Most Cantabrian Upper Palaeolithic cave-sites were less than 35 km away from the glaciated mountains of northern Spain but, notwithstanding, contained faunal assemblages entirely made up of the animals which, according to Bednarik, would not have survived in the Côa region.

Ibex as a chronological marker

Bednarik (1995a) states, 'by 11,000 [b.p.], ibex does not seem to be present in low-altitude occupation deposits, but it would continue to occur in the mountainous areas, so it is not a chronological marker'. This statement needs correction. Magdalenian faunal assemblages, rare in Portugal, come from cave-sites in Estremadura: ibex, chamois and horse are entirely absent; the fauna is dominated by red deer and rabbit with marginal amounts of wild boar and roe deer. At the cave of Caldeirão (FIGURE 5), excavated by the present author and located 20 km from Serra d'Aire, the highest elevation of Estremadura (678 m), the absence of horse and wild caprids contrasts markedly with their abundance in the underlying Solutrean levels (Póvoas *et al.* 1992; Zilhão 1995a). This, together with the palaeo-environmental indicators of rodent and land-snail fau-

nas, suggests that, after 16,000 b.p., the low-altitude limestone elevations of Portuguese Estremadura were already covered by forest, and alpine species and horse absent. Given the proximity of the Côa region to the Meseta and its drier climate, ibex *may* have survived into the Magdalenian in this more inland part, although such a survival is hard to conceive after 9500 b.p. By then, even the Serra da Estrela, which reaches the highest elevation in Portugal (2000 m) and is located some 50 km south of the southernmost occurrences of Palaeolithic petroglyphs in the Côa, was already under *Quercus pyrenaica* forest up to an elevation of 1600 m (Mateus & Queirós 1993). Since ibex is not known to inhabit these forests, it seems *very unlikely* that ibex survived in the Côa region after the end of the Upper Palaeolithic; the representations of the animal found in the valley art are unlikely to post-date the Tardiglacial/Early Holocene boundary. And ibex is missing from faunal inventories of the late Holocene archaeological sites — for instance, the Neolithic or Chalcolithic — of the Douro basin (Jorge 1993).

Use of metal tools

Bednarik (1995b) categorically states that one stylistically Palaeolithic anthropomorphic figure engraved in a panel from Ribeira de Piscos (but not analysed in the 'direct dating project') had been made with a metal tool, which obviously excluded a Palaeolithic age for this figure. Francesco d'Errico, a world authority on this problem, contributed the following comments on this issue (letter, 23 August 1995):

> Experimentation carried out by me and other colleagues shows that stone tools with sharp points can produce engraved lines which display none of the features characteristic of stone tool use and that can be hardly distinguished from those produced by metal points.
>
> My unpublished technological analysis of Fornols-Haut engravings . . . [has] shown that clues demonstrating the use of lithic implements are present on these figures but that they occur rather rarely. The research was carried out in the laboratory using high resolution casts of the engraved panels examined by mean of low-angled light as well as a scanning electron microscope. If such an analysis had to be limited to field observations the majority if not all of these clues would have remained unnoticed.

These statements seem sufficient to demonstrate that the categorical nature of Bednarik's conclusion as regards the tool used to engrave the human figure from Ribeira de Piscos is without basis.

Absence of archaeological context

Bednarik (1995a) states that the Palaeolithic, Epi-Palaeolithic, or even Mesolithic, settlements closest to the Côa art-sites are more than 120 km away; there would be no archaeological context for the art, were it Palaeolithic. Contrary to his reasoning over Last Glacial faunas, he here equates 'absence of evidence' with 'evidence of absence' — wrongly. Although still few and far between (due to the traditional bias towards caves and there being no limestone outcrops in the area), several Upper Palaeolithic sites *have* been found in the Spanish Meseta since the early 1980s. Bengoechea *et al.* (1986) and Fabian (1986) report Solutrean and Magdalenian open-air settlement-sites in the Valladolid and Salamanca provinces, across the border from Portugal (FIGURE 5).

Until recently, no Upper Palaeolithic archaeological sites had been found in the Côa valley (or, more generally, in northern Portugal) — because no survey for Palaeolithic sites had ever been carried out. On 14 August 1995 at 10 a.m., after only 20 minutes of the first serious archaeological survey of the Côa valley by experienced Palaeolithic archaeologists, an open-air camp-site was found at Cardina, on a Pleistocene platform *c.* 3 km up-river from Penascosa (FIGURE 1). The lithic assemblage recovered in test excavations is mostly made up of flint and rock-crystal tools and cores whose typological and technological characteristics are consistent with an Upper or Terminal Magdalenian age (10,000–12,000 b.p.). Testing subsequently carried out at a second near-by platform revealed a rich archaeological deposit (artifact densities are in the order of 500 g/cm/sq. m) at a depth of *c.* 1 m below the surface. The soft hammer blade debitage prepared by abrasion of the edges of core platforms and producing small lipped butts, the microgravette points and the backed and truncated bladelets found among the thousands of lithic artifacts recovered in the 4 sq. m excavated are typically Late Gravettian. This kind of assemblage has been radiocarbon-dated to *c.* 22,000 b.p. at cave and open-air sites in Estremadura. It

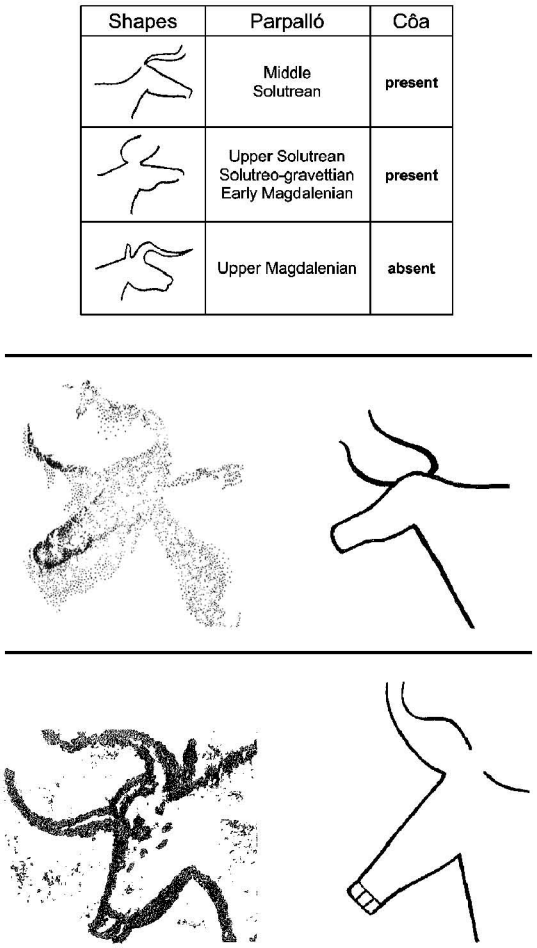

FIGURE 6. Above *Horn shapes of the aurochs in the engraved slabs from Parpalló compared to the Côa figures.*

Middle *The head and horns of the Tête de Lion aurochs* (left) *compared to an aurochs from the Canada do Inferno panel, which Bednarik (1995b) describes as a 'domestic bovid'* (right).

Below *Head and horns of a Lascaux aurochs* (left) *compared to that of an aurochs from the Penascosa panel analysed by Bednarik, for whom the shape of its horns and the internal markings on its muzzle 'do not resemble Palaeolithic art' (Bednarik 1995b)* (right).

should be clear there must be many more sites in the region waiting to be found.

Bednarik's arguments on style

The most forceful statements regarding the stylistically non-Palaeolithic nature of the Côa petroglyphs in Bednarik's dating report (1995b) are:

Regarding bovids

'several bovids have internal markings on muzzles that do not resemble Palaeolithic art'.

'the horns on bovids do not resemble those on most of the supposed aurochs figures of Palaeolithic times, particularly those presumed to be of the Solutrean. They do, however, resemble the forward position and twist found in modern Iberian cattle breeds.'

'These [modern Iberian cattle breeds] also share the slim head shape found in the Côa figures.'

In sum, given their muzzles, their head shape, and their horns, the Côa bovids are domestic oxen, not aurochs.

Regarding caprids

'Even more inappropriate is the description of the caprid-like figures as ibexes. The large and distinctly curved horns of the ibex are not present, instead the horns found in the petroglyphs resemble those of certain domestic goats.'

Regarding equids

'The line indicating the overlap of the haunch in the horse at Ribeira de Piscos is not normally found in Palaeolithic animal pictures.'

Although it is really hard to be wrong on all counts, not one of Bednarik's above-quoted affirmations has any correspondence in reality. 'The line indicating the overlap of the haunch' occurs in painted horses from Ekain (Altuna & Apellániz 1978; Apellaniz 1987) or Niaux (Clottes 1984), just to mention two cases. As for the bovids, the comparison in FIGURE 6 between an aurochs head from Lascaux and one of those Bednarik claims to have analysed at Penascosa resolves the issues concerning 'internal marks on muzzles' and 'forward position and twist' of the horns. As regards horns, comparison (FIGURE 6) with the engraved slabs from Parpalló (Villaverde 1994) actually confirms the attribution of most Côa art to pre-Magdalenian times. In FIGURE 6, the 'slim head shape' of some Côa bovids is also compared to that of the famous aurochs of the Grotte de Tête de Lion, in Ardèche (radiocarbon-dated, by association with a close-by hearth containing fragments of the pigment used in the painting, to *c.* 21,500 b.p.: Combier 1984). This figure, as well as several of the aurochs from Pech

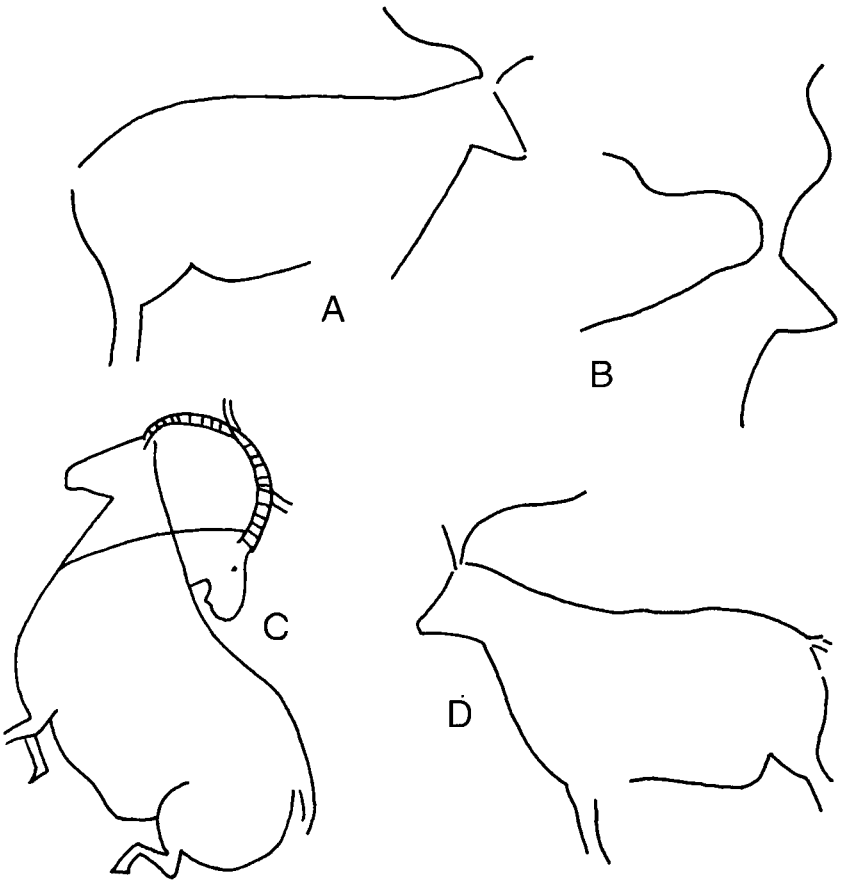

FIGURE 7. *Ibex representations in the Côa valley rock-art.*
A *From a panel at Canada do Inferno.*
B *From a panel at Quinta da Barca I.*
C *From a panel at Quinta da Barca III.*
D *From a panel at Penascosa.*
According to Bednarik (1995b), 'the large and distinctly curved horns of the ibex are not present, instead the horns found in the petroglyphs resemble those of certain domestic goats'.

Merle (Lorblanchet 1984), also shares with many of Bednarik's 'domestic' bovids of the Côa the squarish-shaped muzzle. As for the caprids, the four representations in FIGURE 7 (one from Penascosa, one from Canada do Inferno, and two from Quinta da Barca) should enlighten the reader as to Bednarik's affirmation that the 'large and distinctly curved horns of the ibex are not present'! Even the more schematic show the characteristic twisted shape that unmistakably identifies these caprids as belonging to the Iberian variety of ibex, *Capra pyrenaica* (*cf.* Altuna & Apellániz 1978).

In short, the specific conventions mentioned by Bednarik (1995b) conform completely with what is known from well-dated Palaeolithic art; the criteria he uses to describe the Côa animals as domesticates would imply that bovids and caprids had been domesticated in the Franco-Cantabrian region as early as 20,000 years ago! This obvious absurdity deserves no further comment.

It seems fair to conclude that Bednarik came to the Côa essentially for two reasons: to prove himself right, that is, to find 'evidence' of the correctness of his previous statements (Bednarik 1995a) on the post-Palaeolithic age of the stylistically Palaeolithic petroglyphs; and to make an innovative point regarding preservation policies for rock-art sites since, according to Bednarik and Watchman, 'if the art were to be shown to be post-Palaeolithic, its importance would diminish dramatically and the controversy concerning its preservation would be largely resolved' (from a letter by Bednarik and Watchman offering their services to EDP's board of directors, dated 24 March 1995).

Validity of dating by style in southwestern Europe

Since AMS dating of pigments was introduced, 25 individual paintings from the sites of Altamira, Chauvet, Cosquer, Cougnac, Covaciella, El Castillo, Le Portel, Niaux and Pech

Merle have been directly dated. The results obtained ranged from 12,000 to 32,000 b.p. (Clottes, Fortea & Lorblanchet pers. comm.), that is, there are no instances of rock-art attributed to the Upper Palaeolithic in southwestern Europe that turned out to give Mesolithic, Neolithic, Chalcolithic or even later ages, when directly dated by ^{14}C AMS. The exceptions are two samples from Gargas and Bédeilhac, but they remain unpublished and their significance cannot be assessed. French rock-art experts consider them to be contaminated, particularly that from Bédeilhac, a site that was used as an air-base during the Second World War and where paintings are known to have been recently 'retouched' (Lorblanchet pers. comm.).

To refute the stylistic dating of the Côa and other Iberian open-air petroglyph sites to the Upper Palaeolithic is actually straightforward: find stylistically identical figures in contexts well dated to later time-periods, for instance, in rock slabs from Mesolithic settlement sites, in Neolithic pottery, in dolmenic art, or in Bronze Age sculpture. Such findings have never been reported. What has been confirmed by 100 years of research remains true: stylistically Upper Palaeolithic engravings are indeed of Upper Palaeolithic age. The late Holocene of the Côa region is relatively well known, particularly after recent work by S.O. Jorge (1993; see also Alarcão 1990): megaliths, as well as rock-shelters painted in the same style as some megaliths, are well-known in central Portugal; important Chalcolithic settlements and Bronze Age sites with decorated *statues-menhirs* exist only a few kilometres from the Palaeolithic rock-art sites; in Iron Age art in the Côa valley, recognizable also by style, the horses are mounted by humans who carry characteristic weapons found in burials from that time. The hypothesis of a Palaeolithic group with a Palaeolithic economy and a Palaeolithic art surviving in the Côa into late prehistoric times is nonsense.

Analysis of stratigraphic superposition of figures supports this. At Vermelhosa, a newly discovered rock-art site close to the confluence between the Côa and the Douro (FIGURE 1), a very fresh Iron Age engraving (a mounted warrior with characteristic weapons) is superimposed on a very patinated fine-line deer filled with *grabado estriado*. The deer is virtually identical to those found in engraved bones from the Early Magdalenian levels of Altamira and Castillo (Cabrera & Giménez 1989). If the Iron Age figure is 2000–3000 years old, then the stylistically Palaeolithic one has to be much older. Similar fine-line engravings are associated with the larger pecked engravings from the better-known locations in the Côa valley itself. The interior of an aurochs — from the same panel at Ribeira de Piscos as the figure Bednarik claims was made with a metal tool — is also filled with *grabado estriado*, which Bednarik (1995b) claims to be unusual for Palaeolithic art. In truth, this technique is characteristic of Solutrean and Early Magdalenian engravings in both the parietal and the mobiliary art of Iberia.

Bednarik (1995b), arguing against stylistic dating, notices that such naturalistic art traditions as the Spanish Levantine 'were initially attributed to the Pleistocene, then to the Mesolithic, and are now considered to be Neolithic', and decides that 'the idea that naturalistic art is a typical Palaeolithic phenomenon is clearly false'. This reasoning is misleading. First, nobody attributed the Côa figures to the Palaeolithic on their being naturalistic in general but because of their *particular* naturalistic style, typical of European Palaeolithic art and very different from Levantine. Second, Breuil's Palaeolithic attribution of Levantine art was not based on parallels with well-dated mobiliary art but entirely on archaeological speculation regarding historical links between Mediterranean Spain and North Africa. Third, such attribution never represented a scientific consensus; Hernandez-Pacheco as early as the 1920s presented a strong case in favor of a post-Palaeolithic age for the Levantine art. Fourth, the final demonstration of a later date has come about in recent years through the application of those traditional art-history methods which, according to Bednarik (1995b), 'rock-art science outside Europe does not accept': in some panels, Levantine figures were observed to be stratigraphically superimposed on 'macro-schematic' themes identical to those found decorating Early Neolithic ceramics (Beltrán 1982; Marti & Hernandez 1988).

Bednarik's misuse of the evidence pertaining to the Holocenic rock art of Iberia is also apparent when he quotes approvingly (Bednarik in press) from Baptista (1983) on Vale da Casa, a rock-art site located near Pocinho, a

few kilometres downstream from the confluence between the Côa and the Douro. Baptista commented on the very sinuous cervico-dorsal lines of the Vale da Casa horses as representing the resurgence of a convention that Leroi-Gourhan considered typical of the early stages of Upper Palaeolithic art and cautioned that use of stylistic criteria in rock-art dating must consider the archaeological context as well. At Vale da Casa, such horses are often mounted by warriors carrying characteristic Iron Age weapons and, in spite of the sinuous cervico-dorsal line, are certainly not of Palaeolithic age. Nothing else in those figures resembles Palaeolithic art: they are very small (10 cm or less, for the most part); the perspective is entirely different (the hindquarters are depicted as seen from the back, the rest of the body in profile); and the overall proportions of the bodies (often very elongated and with short limbs, that is, 'dog-like') are non-naturalistic.

Bednarik (1995b) sees that, in some instances, AMS radiocarbon dating of pigments has corrected the age predicted by stylistic analysis. The striking case is Chauvet, where an Aurignacian age was obtained, instead of the Solutrean age predicted by stylistic dating (Clottes *et al.* 1995). This case actually illustrates the shortcomings of Leroi-Gourhan's system which, for the earlier periods, is based not on parallels with mobiliary art but on the assumption that animal representation evolved from simple to complex. Comparison with the mobiliary art of the German Aurignacian shows the dates for Chauvet make perfect sense, both as concerns style and the animals represented (rhino, horse, lion, bear).

Bednarik, in celebrating that Chauvet divergence of some 50%, overlooks how his and Watchman's results diverge by at least 3000%! The typical error involved in stylistic dating is far less than 50%. Bison from Altamira, Castillo and Niaux were all considered of 'Early Style IV', that is, to date between 16,000 and 13,000 b.p. The results obtained — 12,890±160 for Niaux, 12,910±180 and 13,060±200 for Castillo, and an average of 14,000±400 for three samples from Altamira — confirm the stylistic age (Valladas *et al.* 1992). At the new site of Covaciella, in Asturias, two 'Style IV' bison were directly dated to *c.* 14,000 b.p. (J. Fortea, letter, 10 September 1995).

Conclusion

Archaeologists are not content with stylistic dating; new dating techniques are welcome and necessary. Yet stylistic analyses of prehistoric art have resulted in sound achievements that cannot be readily dismissed by techniques whose reliability is unknown or questionable. The insistence by some practitioners, such as Bednarik and Watchman, in refusing to recognize either the limits of their own methods or the merits of classical approaches is the more serious obstacle to the development of reliable direct-dating technologies.

This attitude, of absolute rejection of the methodological and substantive achievements of mainstream archaeology and rock-art research, led Bednarik and Watchman to accept the condition — set by EDP — that their work should be carried out in total isolation from Portuguese prehistorians. Inconceivable on purely ethical terms, this condition turned out fatal also on purely scientific grounds. Had they 'bothered' to follow the traditional rules of international scientific cooperation, they would have realized from the start that many expectations derived from their Australian experience were totally unreasonable in the Iberian context (as, for instance, the idea that, if old, the engravings should be covered by thick layers of rock varnish). Had they 'bothered' to consult with their Portuguese colleagues, they would immediately have realized that the incision of the Côa valley, more than 200 m deep, could not have begun only in the mid Holocene, as Watchman argued in Turin. Had they 'bothered' to do their background homework on Iberian history and prehistory, they would have realized that their dates could not possibly be correct. The spectacular failure of Bednarik and Watchman's 'direct dating' methods (which is not without parallel in the past — remember Glozel) also conveys a moral: the norms of professional ethics should be followed not only out of respect and consideration for your colleagues but also, and perhaps even more importantly, for the very selfish reason that they are an insurance against making a fool of yourself.

Watchman's maximum age is based, not directly on the radiocarbon results he obtained, but on an interpretation of those results that uses false or unverified assumptions. It is in any case contradicted by Dorn's minimum ages.

Dorn's age estimates refute Watchman's attribution to the historical period of Côa petroglyphs without refuting their attribution to the Palaeolithic; the statement that a figure was made before 2000 or 5000 b.p. does not contradict the statement that it is older than 10,000 b.p.

As for Bednarik, his arguments for a late age of the Côa valley art are based entirely on demonstrably false stylistic and contextual arguments.

The shortcomings of stylistic dating mean that it cannot be used with absolute certainty to attribute individual figures to a specific period of the Upper Palaeolithic. Present evidence does not define a specifically Early Magdalenian as opposed to a specifically Late Gravettian style (or, at least, it can be argued legitimately that the scholars have not yet devised adequate criteria to recognize and differentiate stylistic conventions exclusive of each time-period). But AMS radiocarbon has shown that, in southwestern Europe, the criteria used to attribute rock-art to the Upper Palaeolithic in general have stood the test of direct dating.

There is absolutely no valid reason to question the chronological attribution of the engraved figures of the Côa valley to the Upper Palaeolithic, that is, the period between 10,000 and 30,000 years ago. Stylistic analysis suggested that a majority of those figures had been made in Late Gravettian and Solutrean times, around 20,000 b.p. Human settlement of the valley at that time is now proved at the site of Cardina and provides an archaeological context for those petroglyphs.

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