THE STYLISTICALLY PALEOLITHIC PETROGLYPHS OF THE CÔA VALLEY (PORTUGAL) ARE OF PALEOLITHIC AGE. A REFUTATION OF THEIR «DIRECT DATING» TO RECENT TIMES

por

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Resumo: A «datação directa» das gravuras do vale do Côa por quatro especialistas em métodos cronométricos fundamentou a rejeição, pela empresa construtora da barragem que ameaça destruí-las por submersão, da cronologia paleolítica unanimemente atribuída a essas gravuras por arqueólogos de todo o mundo. Os resultados obtidos por esses especialistas são no entanto contraditórios, e apenas servem para pôr a nu as fragilidades teóricas e práticas dos métodos utilizados. A determinação, através da medição do respectivo teor em ^{36}Cl, do tempo de exposição à radiação cósmica das superfícies gravadas, é um método que está ainda em fase experimental de desenvolvimento, pelo que, quaisquer que venham a ser os resultados obtidos (o especialista em causa, F. Philips, ainda não apresentou relatório final), eles nunca poderão ser considerados como estimativas fiáveis da idade máxima das gravuras. A aplicação do radiocarbono à determinação da idade das crostas minerais ou das películas superficiais de alteração das rochas gravadas, por forma a obter, em função da relação estratigráfica das gravuras com essas formações, estimativas das respectivas idades mínima e máxima, não leva em conta que se trata, do ponto de vista da química do carbono, de sistemas abertos e não de sistemas fechados, pelo que os rácios $^{12}C/^{14}C$ das amostras analisadas não têm qualquer valor cronológico. Os resultados obtidos pelos dois especialistas que usaram o radiocarbono estão além disso em contradição total e absoluta, uma vez que, para A. Watchman, as gravuras seriam todas posteriores a 250 A.D., enquanto que, para R. Dorn, elas seriam todas anteriores a 19 A.D. O quarto especialista, R. G. Bednarik, recorreu à análise da micro-erosão dos sulcos gravados embora, como ele próprio admitiu, o método não possa ser aplicado nos xistos do Côa, tanto por causa da sua estrutura e composição mineralógica das rochas como por causa da inexistência de uma curva de calibração local. A sua opinião sobre a cronologia recente das gravuras baseia-se apenas em critérios estilísticos e contextuais totalmente infundados. A correcta aplicação de critérios deste tipo mostra que não é possível que as gravuras estilisticamente paleolíticas do Côa datem de época posterior a 10.000 BP, isto é, ao final do Paleolítico Superior. A validade destes critérios está confirmada, para a arte paleolítica de França e da Península Ibérica, pelos resultados da datação directa, pelo radiocarbono, de mais de duas dezenas de pinturas em gruta. Não há por isso qualquer razão válida para pôr em causa a cronologia estilística da arte paleolítica do vale do Côa.

Palavras-chave: Arte paleolítica. Vale do Côa. Datação de petróglifos.

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Abstract: Based on the «direct dating» of the Côa valley petroglyphs by four experts in chronometric methods, the company building the dam that threatens to submerge and destroy those petroglyphs rejected the Paleolithic chronology unanimously attributed to them by archaeologists from all over the world. The results obtained by those experts, however, are contradictory and, more than anything else, expose the practical and theoretical weaknesses of the methods they use. ^{36}Cl was used to determine the time of exposure to cosmic radiation of the engraved surfaces. Although F. Philips, who used this method in the Côa, has not yet presented his final report, it should be borne in mind that this technique is still in an experimental stage of development. The results derived from its application, no matter which, cannot be considered, therefore, as reliable estimates of the maximum age of the petroglyphs. Radiocarbon was used to determine the age of the mineral accretions and of the weathering rinds that can be found on some of the engraved rocks. In theory, this would allow, through the analysis of the stratigraphic relationship between such crusts and rinds, on one hand, and the petroglyphs, on the other, the establishment of minimum and maximum ages for the latter. This approach, however, incorrectly assumes that such formations represent closed systems from the point of view of the chemistry of carbon. Instead, they correspond to open systems. Therefore, the $^{12}C/^{14}C$ ratios measured in samples extracted from them are chronologically meaningless. In any case, the results obtained by the two experts who used this approach are in total and absolute contradiction: A. Watchman thinks the engravings are younger than 250 A.D., while R. Dorn thinks they are older than 19 A.D. The fourth expert, R. G. Bednarik, used microerosion analysis of the engravings. As admitted by himself, however, the method cannot be applied to the schists of the Côa, for reasons that have to do with the structure and the mineralogical composition of the rock as well as with the absence of a locally valid calibration curve. Bednarik's attribution of a proto-historic age to the engravings is entirely based on ill-founded stylistical and contextual criteria. A correct application of such criteria shows that the stylistically Paleolithic petroglyphs of the Côa cannot post-date the end of the Upper Paleolithic, *ca.* 10,000 years ago. The validity of those criteria has been demonstrated, for France and Iberia, by the direct dating, through radiocarbon, of more than twenty cave paintings. There is, therefore, absolutely no valid reason to question the stylistic chronology of the Paleolithic art of the Côa valley.

Key-words: Paleolithic art. Côa Valley. Petroglyph dating.

1. INTRODUCTION

The controversy regarding the Côa valley petroglyphs began in November 1994, when their existence was made public. Rock art experts and prehistorians from Portugal and all over the world immediately recognized them to be of Paleolithic style and, therefore, of Paleolithic age. This recognition was based on several readily apparent characteristics of the art: the animals represented (aurochs, horse, ibex, deer) were the large herbivores that make up the vast majority of Ice Age iconography; the large size of the figures (most being between 50 cm and 1.5 m long) and the conventions followed (profile view of the bodies, twisted rendering of the horns, sinuous cervico-dorsal lines, «pregnant» bellies, absence of a ground line) were identical to those documented in cave art sites, particularly in those well dated to the Solutrean period, around 20,000 BP; the animals that are rare in Paleolithic art (birds, reptiles, fish, etc.) were absent, as were any that could unquestionably be classified as domesticated (sheep, chickens, pigs, etc.); and, finally, representations of planets, stars, clouds and mountains, as well as of scenes with participating humans (hunting

stories, dances, etc.), were also absent. The patina of the engravings, identical to that of the surrounding rock, and the obvious damage by weathering, faulting and breakage that could be observed in many panels also testified to an ancient age and excluded the possibility of a modern forgery.

Given the number of engraved figures and the extent of their distribution along the valley (more than 15 km), this complex of sites implied a major revolution in our understanding of Paleolithic art. After the smaller discoveries made since 1981 in Portugal, Spain and France, it represented the final demonstration that major Paleolithic art was not restricted to caves and suggested that, in the past, most such art may have been done in the open air (Bahn 1995).

As a result of this recognition, a campaign to stop the construction of the dam that threatened to submerge and destroy this rock art complex was immediately started by Portuguese archaeologists and rock art researchers. This campaign was based on a characterization of the heritage value of the Côa valley that stressed several points (e.g. Zilhão 1995b): the open air situation of the Paleolithic petroglyphs and the extent of the site, which allowed us to have the first insights into the way Upper Paleolithic people conceptually organized the landscape they lived in; the continuity in the use of the valley for rock art representations throughout later time periods (petroglyphs and paintings dated to the Neolithic, the Chalcolithic, the Iron Age and historical times, up to the present century, were also known), making the valley a unique case, anywhere in the world, of an «open air art museum» with such a time depth; and the natural beauty of the valley and the need to preserve the art in the context that gave it meaning, which created an excellent opportunity to turn the area into a world heritage archaeological park.

This campaign received extraordinary support from the media and the public, both nationally and internationally. Between January and March 1995 important Portuguese political leaders, such as the President of the Republic and the leader of the opposition, called for a suspension of the construction work following the recommendations of a UNESCO mission that visited the area in February (according to which it was necessary to carry out a detailed archaeological study of the valley over several years). Finally, in May, the Portuguese prime-minister ordered the construction work to slow down and, at present (September 1995), the dam can be described as being technically (although not formally) stopped. A final decision on its future is to be made by the new government that will come out of the general elections of October 1st, 1995.

EDP (Electricidade de Portugal), the state owned electricity company building the dam, responded to this campaign by questioning the relevance of the engravings. As part of this response, they organized a «direct dating project» of the Côa petroglyphs that had been attributed to the Paleolithic on stylistic grounds, and hired four researchers to carry out those studies: Robert Bednarik, Ronald

Dorn, Fred Phillips and Alan Watchman. Preliminary results of the work by Bednarik, Dorn and Watchman were leaked to the press by the office of the Minister for Industry, and published in the July 7 issue of the Portuguese weekly *O Independente* (Sá and Ferreira 1995). This article interpreted such results as demonstrating that the art was post-Paleolithic and accused Portuguese archaeologists campaigning to preserve the site of incompetence and fraud.

In a press conference held by its board of directors on July 13, EDP confirmed that their official interpretation of the results was that the Côa rock art site was post-Paleolithic (EDP 1995). Therefore, it did not have the importance attributed to it by archaeologists and rock art researchers, and there was no reason to abandon the construction of the dam. According to several newspaper accounts, they even went as far as stating that, given these results, the site did not justify the money budgeted for the construction of a site museum next to the dam, which EDP planned to withdraw (*Jornal de Notícias,* Porto, 14/7/1995). These statements were backed by the Portuguese Minister for Industry.

As a result, political leaders who, previously, had expressed the view that the valley should be preserved, stated that they were puzzled with these developments, and that, if a late chronology for the Côa valley art was to be confirmed, they might change their position (e.g. António Guterres, general secretary of the Socialist Party and leader of the opposition, in an interview with *Público*, dated August 3, 1995). These circumstances transformed the scientific arguments concerning the chronology of the Côa valley art into a critical topic in the struggle to save it from destruction and led to the production of a report to the Portuguese authorities on EDP's «direct dating project» (Zilhão and Soares 1995). The methodological inconsistencies and internally contradictory results of EDP's project were exposed, and it was shown how such results did not constitute a valid basis for questioning the Paleolithic age of the stylistically Paleolithic petroglyphs. This paper is an elaboration of the arguments originally presented in that report, particularly as regards the work of Alan Watchman and Robert Bednarik, which is examined in further detail.

2. SCOPE, RESULTS AND PUBLICATION OF EDP'S «DIRECT DATING PROJECT»

The four researchers are supposed to have worked independently of each other, in what was referred to by EDP as a «blind test» experiment. However, no joint evaluation of the results of such a «blind test» by the dating professionals themselves has so far been produced. On the other hand, although EDP and the Minister for Industry have used these reports as the basis for policy definition on

such a major issue, they have so far refused to make them public. Portuguese archaeologists have thus tried to obtain copies directly from the authors who, so far, have refused to make them available, although a copy of Bednarik's report was obtained through independent channels.

The evaluation of EDP's «direct dating project» that is presented in the following lines therefore relies on the following data:

- The account of the results obtained by the different investigators that was given by *O Independente* (Sá and Ferreira 1995);
- Bednarik's report (Bednarik 1995b);
- Dorn's table of minimum ages as given to the press by the office of the Minister for Industry (Dorn 1995);
- Watchman's «Executive Summary» as given to the press by the office of the Minister for Industry (Watchman 1995a);
- Watchman's statements to the Portuguese newspaper *Público* (Salema 1995);
- Watchman's statements to *Science* (Fischman 1995);
- Watchman's abstract of the paper he presented at the September 1995 International Rock Art Congress of Turin (Italy) (Watchman 1995b).

Fig. 1 shows the location of the presently known Côa valley art sites containing stylistically Paleolithic petroglyphs. The first 6.5 km upstream from the mouth of the river have been partially flooded since the early 1980's, as a result of the construction on the Douro, a few kilometers downstream from the confluence of the two rivers, of the Pocinho dam. The engravings presently visible in localities 3-6 of Fig. 1 represent therefore just the tip of an artistic landscape now submerged under a few meters of water throughout most of the year. The construction of the Foz Côa dam would entail the placement of all these sites at a depth of more than 100 meters.

According to EDP (1995), all four researchers worked on the same three panels, located at the sites of Canada do Inferno (Fig. 1, nº 4), Ribeira de Piscos (Fig. 1, nº 8) and Penascosa (Fig. 1, nº 11). As all others in this paper, the tracings of these three panels presented in Fig. 2 are partial and preliminary, and intended only as a basis for independent stylistic evaluation by the readers, a purpose for which they are considered accurate enough.

3. PHILLIPS'S ^{36}CL DATING

Fred Phillips used ^{36}Cl 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.

He was quoted by Sá and Ferreira (1995) as having obtained a maximum age of 3000 years for the Canada do Inferno panel. In a letter to the present author dated July 20, Bednarik also mentioned Phillips's results as part of the «proof» that the three panels were post-Paleolithic, and he repeated this in his oral presentation to the September 1995 International Rock Art Congress in Turin. Interestingly, a letter by Monty Flinsch, a collaborator of Phillips, dated July 7 and distributed in EDP's press conference of July 13, 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» what results ^{36}Cl had provided even before the chlorine had been extracted from the samples remained an unexplained mystery until Watchman informed the Turin Congress that the 3000 year old date had been obtained from a sample collected by Dorn, not by Phillips. It is quite clear, therefore, that such a date is the output of preliminary work and that all evaluations of its significance so far produced have been highly premature.

At the time of writing (September 1995), Phillips's final report does not seem to have been presented to EDP yet, and any speculation on what his definitive results may turn out to be is therefore useless. In any case, it should be stressed that, as is stated in the report from the Sydney 1993 Workshop on Production Rates of Terrestrial In situ-produced Cosmogenic Nuclides (Reedy *et al.* 1994), there are many uncertainties regarding the rates of production of ^{36}Cl. These rates are subject not only to past changes in cosmic-ray fluxes but also to local variations determined by the latitude and altitude of the site and by the composition and geometry of the rock. As a result, the verdict of the community of dating experts regarding this method is that it is still in an experimental stage of development. In these circumstances, the results, no matter which, to be presented by Phillips once his analyses are completed, cannot possibly be considered as representing reliable chronological estimates for the age of exposure of the engraved surfaces.

4. DORN'S AMS DATING OF ORGANIC MATTER ENCAPSULATED IN WEATHERING RINDS

Dorn obtained **minimum ages** for five figures from the three panels, all of them being (based on the upper end of the calibrated range) older than 19 A.D. (and one older than 2703 BC). Although EDP (1995) and Bednarik (*in litteris,* July 20, 1995) have interpreted these results as accurate assessments of the moment when the petroglyphs were made and, therefore, as demonstrating that the petroglyphs are not of Paleolithic age, nothing in Dorn's own writing (a table of minimum ages published by the Portuguese press — Dorn 1995 — and two letters

to the present author, dated July 18 and July 26) suggests that such is also his opinion. Any expert in absolute dating knows that dating of surface encrustations, accretions or patinae that cover and postdate petroglyphs only provide minimum ages for the art, which itself may be just marginally older than the surface «skin» or may be many times as old, as Bednarik himself acknowledged (Bednarik 1992a). On present evidence, it would therefore seem that Dorn's report is being abused by both Bednarik and EDP.

5. WATCHMAN'S AMS DATING OF ORGANIC MATTER ENCAPSULATED IN MINERAL ACCRETIONS

Watchman's procedure was to date organic matter that became fossilized in the crusts deposited by water flowing over the petroglyphs, thus obtaining a minimum age for them; similarly, he 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 such minimum and maximum ages (Fischman 1995). That is, therefore, how he is supposed to have obtained the results he gave EDP, which are the following: that the Côa engravings have a maximum age of 1700 years (Fischman 1995; Salema 1995; Watchman 1995a, 1995b); and that most of them were done only some one hundred years ago (Salema 1995). These statements were reasserted in Watchman's oral presentation to the September 1995 International Rock Art Congress in Turin, where he also attributed the authorship of the petroglyphs to the people working at the numerous water mills functioning along the river margins in the late nineteenth and early twentieth centuries.

5.1. Theory and assumptions

The first objection that needs to be raised against Watchman's approach is theoretical, because his procedure is based on several unverified assumptions which, upon closer scrutiny, reveal themselves as also somewhat naïve:

Assumption 1. The silica skins are formed once and for all, and have no subsequent history of weathering and erosion

According to Salema (1995), Watchman believes that these crusts start forming soon after the rock surfaces begin to be exposed to weathering agents. Since he thinks that he can use them to obtain a maximum age for the petroglyphs, it

necessarily follows that he works on the assumption that the crusts he samples in the present are the same that started to develop soon after the rock surfaces were first exposed in the past. However, it must be intuitive to any one with minimal experience in rock art that this assumption has to be wrong. That is at least the opinion of one leading rock art researcher. In a letter to the present author dated July 28, Andrée Rosenfeld wrote: «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

Radiocarbon dating consists of the transformation of a ratio between two isotopes (^{12}C and ^{14}C) into an age assessment. This transformation is based on the premise that the interaction of the sampled material with the environment (and therefore with the atmosphere of the earth, where ^{14}C is constantly being produced) ceased at a certain point in time (death of a living organism, for instance), and it is the age of that event that is measured by the technique. If, however, interaction continues after the event that one intends to date, the ratio between ^{12}C and ^{14}C in the analyzed sample does not accurately reflect the age of that event. In other words, radiocarbon dating can only be used as a chronometric technique when dealing with materials that, as regards the chemistry of carbon, have at a certain moment in their history passed from an open system state to a closed system state. Watchman's assumption is that the mineral accretions he sampled meet this condition, but most archaeologists, rock art researchers and dating experts would disagree with him.

It is well known to anyone that has worked in caves, for instance, that modern tree roots can penetrate stalagmitic crusts that are sometimes many centimeters thick. By analogy, it seems difficult to accept that lichen and fungi presently growing on mineral accretions that, according to Watchman himself (Fischman 1995), are only fractions of a millimeter thick, do not contribute to the «organic matter» encapsulated in such accretions. Although physical pre-treatment might conceivably eliminate rootlets and filaments belonging to such organisms, it would not be enough to eliminate the chemical effects of their activity, whose relevance becomes immediately apparent once we bear in mind the obvious analogy between these accretions colonized by live organisms and ordinary soils. This is all the more so since the

amounts used in AMS dating are extremely small and since, according to Werlhof *et al.* (1995), Watchman's use of laser extraction techniques precludes traditional chemical decontamination procedures. Werlhof *et al.* (1995) have also given some very clear verdicts on the issues raised above: «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 otherwise, he has to prove his point, not just assume it. Meanwhile, it is clear that unlayered mineral accretions, patinas, rock varnishes, etc., are open, not closed, systems. That is why it can be safely predicted that the dating of carbon of unknown (and, therefore, possibly composite) provenience contained in them is likely to provide random, meaningless, results. In one instance reported by Werlhof *et al.* (1995:266), filaments in a weathering rind under live epilithic lichens growing in a South Australian petroglyph gave a ^{14}C measurement of 687±84 BP (NZA 2275); but «organics» encapsulated by rock varnish gave an age, for the same petroglyph, of 14,910±180 BP (NZA 1367).

Assumption 3. The silica skin on rock surfaces adjacent to the petroglyphs was already there when the engraving was done

If one admits the validity of the objections to the first assumption, then it is quite clear that the silica skin on the adjacent rock surfaces can have exactly the same age as that covering the petroglyph, or can be even younger (Fig. 3). It is conceivable, for instance, that a mineral accretion covering a rock surface (and the petroglyphs engraved on that surface) goes through an erosion process that eliminates it, except inside the grooves defining the petroglyph, where it would have been sheltered from the erosional agent. 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, including by methods affecting not just the grooves but the entire rock surface. In

these circumstances, the idea that a maximum age can be derived from organic matter encapsulated in the silica skin covering a rock surface adjacent to a petroglyph is simply untenable. At best, if one disregarded the objections raised in the preceding paragraphs, one might concede that Watchman could have obtained maximum ages for the time the petroglyphs were last rejuvenated, but never for the time when they were first engraved on the rock.

In short, whichever dating results were eventually to be obtained for the Côa petroglyphs by Watchman's method, it could be predicted from the start that none could be reliably interpreted as maximum age assessments for the moment of their original production. In any case, by Watchman's own admission (Fischman 1995), when results ranging from 650 to 7000 BP 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 turned out to be younger than the minimum ages!** Although, as discussed above and as is shown in Fig. 3, such a scenario is *a priori* not entirely inconceivable, it is in absolute and total contradiction with the expectations of Watchman's model of crust formation and development and represents, by itself, enough evidence of the inadequacy of his approach and of the unreliable nature of his age estimates.

5.2. Watchman's «post-hoc accommodative argument»

In a letter to EDP dated January 19 (that is, 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 the deeds of «so-called archaeological experts professing to know the antiquity of the engravings without carrying out any scientific dating tests». He also made it quite clear to EDP that «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, according to Fischman (1995), as soon as he arrived in the Côa valley and began to examine the engraved panels, «several bells went off in my [his] head saying these things are young». So, it is fair to say that Watchman began his work convinced that the engravings not only were not of Paleolithic age but, instead, were probably very recent.

When he obtained results indicating minimum ages in the order of 7000 years he was therefore «puzzled» (Fischman 1995): his expectation that the art was modern, as well as his theory that the skins on surrounding rock were older than those covering the petroglyphs, were contradicted by such results. One might

think, therefore, that he would proceed to reexamine both theory and expectations or, at least, that he would use a certain amount of prudence and modesty in the presentation of his interpretation of the chronology of the petroglyphs. What he did, instead, was to issue categorical statements as to their very recent age, based on what is a text book example of what archaeologist Lewis Binford (1983) has named «post-hoc accommodative argument».

The first step in this argument was that of dismissing the dates on the skin covering the petroglyphs themselves as too old due to contamination. His microscopical examination of these skins showed that they were made up of a silty brown material «probably eroded from the hillsides above» (Fischman 1995). This material contained particles of graphite weathered out of the rock, and graphite, «formed from ancient carbon», would have been the contaminating material — «it made the engravings look anomalously old» (Fischman 1995). However, if the graphite was indeed weathering out of the rock, it should also be found in the silica skins covering the surface adjacent to the petroglyphs. But, according to Watchman (1995a), that was not the case: the latter are described by him as «uncontaminated», as opposed to the silty crusts inside the grooves defining the petroglyphs, which are described as «contaminated with ancient graphite and charcoal». No explanation is provided, however, for the apparently contradictory situation that arises from this: graphite was present in a crust formed by the accumulation of silty material coming from the erosion of the surrounding soils; but it was absent from the silica skins precipitated by flowing water over the surfaces of rocks where graphite is a natural component!

This brings up the question of whether the graphite Watchman found in the grooves may not have a totally different origin. One possibility immediately comes to mind: that the graphite was introduced in the grooves in the context of the several procedures, using different materials (pencils, chalk, paint and wood charcoal), that were used by visitors to enhance the pictures before the Côa art sites were fenced. Since graphite is a component of pencils, its presence only in the grooves and not in the adjacent rock surfaces might be interpreted as a clue to the contamination of Watchman's samples by young carbon (particles of wood from the pencils, for instance), as well as old. On the other hand, the fact that many such episodes of enhancement took place at all the sites sampled in the framework of EDP's «direct dating project» obviously questions the basic premise of Watchman's approach — that of the integrity of the 0.1 mm thick crusts that he analyzed and sampled. Incidentally, that fact also provides the explanation for a pattern observed by Watchman, which he mistakenly interprets as indicating that the engravings are of a very recent age: the absence of lichen cover in the grooves as opposed to its presence on adjacent rock surfaces. In at least one instance, that of the panel 6 of Penascosa (Fig. 4), the present author can testify that such cover

was also present in the grooves when the panel was first discovered in late January 1995. In any case, given what Watchman himself had previously stated on the implications of such enhancement procedures, it is quite clear that, under the circumstances, AMS radiocarbon dating should never have been attempted in the first place: «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» (quoted from a letter dated January 19, 1995, sent by Watchman to the board of directors of EDP). Bednarik, Watchman's co-participant in EDP's «direct dating project», seems to be, or 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).

It should also be noted that Watchman's description of graphite as a «contaminant» contradicts his characterization of the age of the samples. If the petroglyphs are about one hundred years old, as he asserts, the crusts formed over them are therefore younger. But if, as a result of contamination by graphite, those crusts formed less than a century ago provide, as happened in at least one instance, radiocarbon ages of up to 7000 years BP, then it follows, given the laws of radioactive decay and the half-life of ^{14}C, that Watchman should describe his samples not as made up of modern organic material «contaminated» by graphite, but as the opposite. The carbon contained in them would have to be almost entirely made up of graphite «contaminated» by very small amounts of modern organic material! In the case of the «7000 year old» sample, graphite would have to represent 98% of its total carbon content!

Now if, as Watchman said in his presentation to the September 1995 International Rock Art Congress in Turin, the «organic matter» in the mineral accretions that he analyzed is essentially made up of diatoms encapsulated by silica, how does he explain, then, that the dated samples turn out to be essentially made up of graphite? In other words, Watchman is facing here what seems to be an inescapable dilemma: either his sampling procedures are adequate and the contamination cannot possibly be that extensive (and then the silty crusts are much older than he thinks); or they are indeed as young as he thinks they are (and then his sampling procedures are in clear need of substantial improvement). That such improvement may indeed be necessary is indicated by Watchman's statement on the specific *locus* of the graphite contamination problem: «graphite (...) occurs in thin yellow-brown weathering rinds at the base of the silty brown accretions» (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 sampling procedure mixed 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.

Even if one leaves aside the issue of the lack of integrity of the mineral accretions analyzed, it is quite clear that this admitted lack of precision in sampling, together with the extreme thinness of the mineral accretions, force us to bring up the question of what exactly it is that Watchman's samples actually represent (as opposed to what he thinks they represent). If it was not possible to separate the loose crust from the weathering rind, was it possible to separate the crust containing the carbon presumably contemporaneous with its formation from the surficial film of the crust upon which lichen and other organisms developed? And was it possible to separate the weathering rind from the unweathered rock itself? The implication of Watchman's explanation for the graphite problem is that such a separation was not done and is probably not feasible. In these circumstances, it is quite possible that the «organics» in his samples come essentially from only two sources: the old carbon from graphite in the rock and its weathering rinds; and the modern carbon from the living organisms that inhabited the grooves for the last few months or years. The greater or lesser weight of the latter would determine the specific «chronological» place of each sample in the spread of dates obtained.

Watchman might of course reply that graphite is a problem only in the case of the silty brown material found inside the grooves defining the petroglyphs, not as concerns the silica skins covering the adjacent rock surfaces. His interpretation of the radiocarbon ages obtained for the latter also implies, however, that they too were subject to contamination. According to Salema (1995), the oldest result Watchman obtained for «organic matter» encapsulated in the silica skins covering the rock surfaces adjacent to the petroglyphs was 1700 BP, and that was, therefore, in the framework of his assumptions, the maximum age the petroglyphs could have. This conclusion was reinforced by means of a contextual argument. «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» (Fischman 1995). That is, *ca.* 1700 BP the environmental change brought about by the beginning of agriculture implied that silica skins ceased to form; inhabitants of the area 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, 1995b).

Salema (1995) also reports, however, that not all of the silica skins gave the same radiocarbon age of 1700 BP: most gave younger ages and, in at least one instance, one such skin from an unengraved rock gave an age of 4300 BP. On the other hand, Watchman believes that these crusts «do not take very long to form», and he thinks that crust formation in the walls of a nearby 100 year old quarry is, in both thickness and duration, a good analogy for the processes that affected the kinds of rocks that were engraved (Salema 1995). Therefore, the range of dates he obtained for the silica skins covering those rocks carries a significant implication: if such skins are rapidly formed and if they ceased to form as a consequence of an environmental change that took place 1700 years ago (the beginning of agriculture in the valley), then the samples from them, with ages that are hundreds or thousands of years apart, have to be «contaminated» as well, some with younger material, some with older material! This, incidentally, is also admitted by Watchman in a passage where he states that «ancient carbon has contaminated the accretions in engravings and **on some rock surfaces**» (Watchman 1995b), and contradicts his other categorical statements that the silica skins formed before 1700 years ago are uncontaminated: «graphite (...) occurs in thin yellow-brown weathering rinds at the base of the silty brown accretions, but not in the hard, gray to white amorphous silica» (Watchman 1995b). In short: the silica skins covering the rock surfaces adjacent to the petroglyphs, which were supposed to provide the maximum age for those petroglyphs, also suffer from the same «contamination» problem that led Watchman to disregard the results obtained for the loose silty brown crust that were supposed to represent their minimum age!

After this, one cannot escape the conclusion that Watchman's explanation of why some of his radiocarbon results are correct age assessments and others are not contains too many inconsistencies and leaves too many unanswered questions to be acceptable. The presence or absence of graphite seems to be invoked according to the conveniences of the argument but, in an overall evaluation of the data supplied by him, it would seem that all of the crusts he sampled suffer from this problem to a greater or lesser extent. In other words, **everything is contaminated**! Since it is obviously impossible to quantify the extent to which this «contamination» affected the samples, and since he cannot exclude that «contamination» in the other direction (that is, by younger carbon) also occurred, it follows that his «dates» are nothing more than chronologically meaningless expressions of the values attained in the different samples by the ratio between the two carbon isotopes ^{12}C and ^{14}C.

In any case, even if one accepted that the formation of the silica skins he analyzed ceased around 1700 BP, that would not necessarily have to be relevant for the argument regarding the age of the petroglyphs. As discussed in the pre-

vious section, it is perfectly possible that a patina developed over a rock surface is younger than the petroglyphs found on that same surface. Watchman might object, however, by saying that if the engravings were already there when the silica skins began to form, they should also be covered by such skins (as in Fig. 3). Since that was not the case (inside the grooves defining the petroglyphs he claims that only the loose brown silty crust was present), the moment when the silica skins ceased to accumulate should indeed represent a maximum age for the petroglyphs, which must have been engraved after those skins formed. At the September 1995 International Rock Art Congress of Turin, however, Watchman stated that mineral accretions on the analyzed surfaces were only minimally developed, making it very difficult to obtain adequate samples (and that had also been the reason why he had suggested that Bednarik be invited to carry out microerosion dating). It seems fair to infer from this that the skins in question do not represent extensive and homogeneous covers and that their absence from the particular engravings Watchman studied may be, therefore, stratigraphically irrelevant. The basic problem, however, is that, as shown above, such skins are «contaminated» by non-contemporaneous carbon (as Watchman himself implicitly or explicitly admits), and the moment when they ceased to form (whether 100, 1000, 10,000 or 100,000 years ago), therefore, cannot possibly be determined by radiocarbon dating.

On the other hand, Watchman's attribution of a post-1700 BP genesis for the silty crust filling the grooves can only be interpreted as a minimum age for the petroglyphs defined by those grooves. If that attribution were to be accepted, it would only mean that the petroglyphs were older than the age of the silty crust, not younger. Therefore, if one accepted that the accumulation of the silty crust is a process resulting from the establishment of agriculture in the valley, then the petroglyphs would have to be considered as pre-dating that establishment. All the more so since the fact that he recognized «slightly weathered rock surfaces at the base of the silty accretions» (Watchman 1995b) implies that the moment of execution of the petroglyphs and the moment when the silty material began to accumulate would have to be separated by a significant amount of time!

Leaving this contradiction aside, it should also be stressed that Watchman's environmental model of the development of mineral accretions in the Côa valley rock surfaces is based on three contextual arguments that are totally unsubstantiated:

- **Two are unverified assumptions** – that the silt in the skin that covers the grooves eroded from the surrounding slopes (which is likely, but where are the analyses that prove it?); and that 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** – that such slopes were cultivated for the first time 1700 years ago (how does he know? what kind of research did he undertake to justify this assertion? how does he explain away the evidence for a Neolithic — that is, at least 6000 years old — settlement of the area by farmers?).

The unsubstantiated nature of these assumptions is a very important point in this discussion, because, upon closer scrutiny, it turns out that it is the argument relating to the moment when agriculture began in the area that constitutes the essential logical foundation of Watchman's chronology for the Côa valley art. As a matter of fact, from the point of view of the dating results, his conclusion is that only the ages obtained for the silica skins covering the rock surfaces adjacent to the petroglyphs are acceptable, providing a maximum age for the petroglyphs themselves. But, since his results for those silica skins cover a wide range of time, he also has to sort among them those that are «good» from those that are «bad» (or «contaminated»). And it is quite clear from the preceding discussion that the good ones are those in accordance with the 1700 BP date he presumes for the beginning of agriculture, which he equates with the moment when those skins ceased to develop. In other words, the maximum age Watchman allows the Côa art to have 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 the geochemical and archaeological assumptions (that silica skins ceased to form as a result of agriculture and that agriculture in the Côa valley only began 1700 years ago) used to either accept (in some cases) or disregard (in other cases) those radiocarbon results.

In short:

- Watchman's theoretical model of crust formation is based on false, or at least unverified, assumptions, as is the case with his environmental interpretation.
- The results obtained for the Côa rocks contradict the theoretical expectation derived from the model of crust formation.
- The lack of precision in the sampling procedure does not allow identification of the exact microstratigraphic provenience of the carbon present in the dated samples.
- The ratio between ^{12}C and ^{14}C in those samples is chronologically meaningless, since it consists 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.

- The proposed environmental interpretation represents the only basis to sort out the «bad» dates from the «good» dates but, contradictorily, carries, on one hand, the implication that the silica skins are also contaminated and, on the other, the implication that the petroglyphs are pre-agriculture, not post-agriculture.
- Radiocarbon should never have been used in the first place due to the open system nature of the unstratified mineral accretions present and the lack of integrity of the sampled panels.

In these circumstances, it is quite clear that Watchman's «maximum age» cannot be considered, from a scientific point of view, as a valid critique of the Paleolithic age of the petroglyphs engraved in the analyzed panels as determined by stylistic criteria. On the other hand, such a «maximum age» is in total contradiction with the results obtained by Dorn, who also used AMS radiocarbon dating but arrived at a completely different chronology: that all the engravings are older than 2000 BP, not younger than 1700 BP, as Watchman states.

5.3. 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. In these circumstances, the idea that thematically and stylistically Paleolithic art could have been done in the Middle Ages or in subsequent centuries is simply ludicrous and, in normal circumstances, its refutation would be a waste of time.

However, by categorically stating that a large majority of these petroglyphs were actually done no more than one hundred years ago (Salema 1995), Watchman aggravates his case. If that had been so, then several questions would have to be answered, all of which Watchman refuses to consider:

- Given that the grand-children and the great grand-children of the artists would still be alive today, why would no memory of such activity have survived the passing of only two or three generations?
- Given that late nineteenth and early to mid-twentieth century engravings are also known in the valley, their exact age being provided by the fact that they are signed and dated, and by the nature of the representations (castles, a train on a bridge, clocks, etc.), why would no such signing and dating behavior have been associated with the stylistically Paleolithic petroglyphs?
- Given that the stylistically Paleolithic petroglyphs represent animals that

have been extinct in the area for thousands of years, how could the nineteenth century millers who presumably would have engraved them have learned about the existence and aspect of such animals?

- Finally, how is it that the same millers were able to represent those species following conventions that are typical of an art that, at that time, still awaited rediscovery by modern science?

Not only does Watchman not answer these questions, he does not even realize that his theory that the engravings were done last century by the millers is a simple case of plagiarism. This theory was first 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 Fools tradition...

During the September 1995 International Rock Art Congress of Turin Watchman elaborated this argument even further. In an interview with the Portuguese news agency Lusa published by several Portuguese newspapers (cf. *O Primeiro de Janeiro*, September 6, 1995), he stated: «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]»; «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 twentieth century bathers drawing extinct Pleistocene animals is, however, one that most people will find 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 nearby 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,* July 16, 1995).

6. WATCHMAN'S OSL DATING OF RIVER GRAVELS

Watchman also used OSL (Optically Stimulated Luminescence) to date the river gravels near Penascosa. The results he obtained indicated to him that those gravels were only about 4000 to 6000 years old. From this he concluded that the rocks and their engravings could not be of Paleolithic age: «this [OSL] study indicated that the engraved rocks were first exposed 4500 years ago» (Sá and Ferreira 1995).

In his presentation to the September 1995 International Rock Art Congress in Turin, Watchman explicitly stated that the morphology of the valley indicated

that it represented a young, probably mid-Holocene, incision. The only reasoning that, in this context, might conceivably have led him to derive, from the OSL dating of the river gravels, a maximum age for the engraved rocks, is this: since the gravels at the bottom of the valley are only 4000 to 6000 years old, such is the maximum age of the incision of the valley itself; therefore, the petroglyphs could not possibly be of Paleolithic age, since the rocks where they were executed had only been exposed (by river incision of bedrock) in the mid-Holocene.

This is, however, utterly absurd. Wherever data on valley incision have been obtained for the Portuguese Quaternary, as is the case in Estremadura, the littoral region of central Portugal, they show that the last glacial maximum valley bottoms were lower than at present (Marks *et al.* 1994)! Furthermore, the torrential regime of the Côa implies that most sediment carried by the river tends to be transported downstream to the Douro, and very little can be found, even today, covering its rocky river bottom. If Watchman's OSL dates were to be confirmed by future research, they would simply indicate that the terrace at Penascosa dated to the mid-Holocene. It is obvious, however, that dating the fill of the valley only provides a minimum age for the valley incision itself, which, given our current knowledge of the Portuguese Quaternary, in all likelihood significantly predates the Upper Paleolithic.

The absurd nature of Watchman's reasoning is demonstrated by the results of the archaeological excavations carried out at Penascosa in August 1995 at the base of the engraved panels. The upper part of the gravels, which were buried under *ca.* 1 m of sandy deposits, contained iron horse shoes and iron horse shoe nails used early this century. This indicates that those gravels may be related to the changes in the regime of the river that resulted from the construction, in the early 1900's, of the water mill (and associated damming facilities) whose ruins are still visible at Penascosa. If the age of the gravels indicated the age of the incision, then the Côa would have excavated its valley only in the last few decades!

Archaeological survey also undertaken in August has in any case allowed the discovery of two Upper Paleolithic camp sites located on Pleistocene deposits some 10-20 m above the present valley bottom (see below). This suffices to demonstrate that the valley incision is indeed of pre-Holocene age, contrary to Watchman's opinion, which, it should be stressed, was not substantiated by a single piece of geological or geomorphological evidence.

7. BEDNARIK'S MICROEROSION DATING

Robert Bednarik is supposed to have used the microerosion direct dating technique, according to which none of the stylistically Paleolithic engravings

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

Microerosion was theoretically presented as a method of direct dating of petroglyphs in a 1992 issue of *Archaeometry* (Bednarik 1992a). It is based on the concept that there is a relation between the degree of erosion (measured through observation with a binocular microscope) suffered by some rock components (quartz grains, for instance) exposed by the engraving and the amount of time elapsed since the engraving was executed. The only concrete application so far published, however, is that of Besov Nos, Lake Onega, Russia (Bednarik 1992b). Moreover, no true «blind test» of this technique was ever undertaken. That is, no experiment in dating an engraving whose age was unequivocally established but unknown to the practitioner of the method has so far been reported. And the same is true for cases of replication of the same results by different practitioners in similar «blind test» conditions. Until these two conditions are met, microerosion should only be considered, at best, as an interesting possibility, but not as a dating method, not even at an experimental level. In any case, as Andrée Rosenfeld (*in litteris,* July 28, 1995) stated, microerosion «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 one agrees to discard these objections and accept that using Bednarik's variety of microerosion analysis for petroglyph dating may not be such a bad idea after all, one is faced with the fact that, according to Bednarik himself, «schist and other rocks of low metamorphism (slate, phyllite) are not well suited for microerosion analysis». There are many reasons why this is so, but one is readily apparent: the mineral components of these rocks are of microscopic size and the observation of microerosion features therefore impractical. However, «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 [him] to see them» (Bednarik 1995b). In short, 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 ones (the schists bearing the stylistically Paleolithic engravings).

Another precondition for the applicability of Bednarik's technique would be the availability of a locally valid calibration curve, that is, the establishment of certain parameters derived from engravings whose age is unequivocally established. In Bednarik's own words, «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» (Bednarik 1992a). That is, «without locally established calibration curves this method is of poor accuracy» (Bednarik 1995b). Since dated inscriptions are known in the Côa, one might be led to think that such a calibration curve would not be difficult to establish. As regards such historical petroglyphs, however, Bednarik (1995b) states that «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 locally established calibration curve, how can Bednarik legitimately claim that he applied microerosion dating? And how, then, does he arrive at an age estimate? As for the last question, the answer is very simple. His conclusion that «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 of the figures 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, in turn, derives from 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).

Even if one would find it intellectually challenging to play Bednarik's game, it should by now be well apparent how absurd the whole thing is: a reliable indication of magnitude of the age of the Côa slate petroglyphs based on a calibration curve for granites from Russia! More interesting, however, is that the application of the same curve to a quartz vein in panel 6 of the Penascosa site (Fig. 4) «would imply an exposure age of the panel of perhaps 30,000 years» (Bednarik 1995b)! And why would 6500 years be a reliable maximum age, and 30,000 years a non-reliable one? Because the latter «seems to high too 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 Paleolithic, which such an interpretation would stipulate» (all quotes from Bednarik 1995b).

The preceding paragraph immediately brings up several questions. For instance, why would the differential microerosion between abraded and pecked figures imply that a large amount of time would separate both kinds of motifs?

Why is this difference not just a byproduct of the different engraving techniques used in each case, and of the resulting differential exposure to weathering agents? 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? On the other hand, why should gelifraction be expected in the Côa valley? Incidentally, it should be noted that the reasoning regarding frost-weathering is nothing but a restatement, almost 120 years later, of the arguments put forward by some nineteenth century French positivists to sustain 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) also invokes, *en passant,* a series of other geological and archaeological, as well as stylistic, arguments, which he considers as «still more serious objections to the Paleolithic antiquity of the Côa art». Given the fact that the two maximum age estimates provided by the direct dating technique are so far apart, it must be considered that it is these arguments that, in the last instance, enable him to decide which is the «good» one and which is the «bad» one. In other words, since Bednarik's microerosion analyses of the Côa petroglyphs violated every single methodological requirement he himself had established in his own theoretical papers on the issue, it is such contextual arguments that must be considered as the real logical foundation of his dating of the stylistically Paleolithic figures to post-Paleolithic times. Let us now proceed to examine how sound such arguments are.

8. BEDNARIK'S ARGUMENTS ON CONTEXT

Most of the contextual arguments referred to by Bednarik in his dating report to EDP (Bednarik 1995b) are essentially the same which he developed in Australia before ever having set foot in Portugal, as stated in a paper published in the April 1995 issue of the *AURA Newsletter* (Bednarik 1995a). He had been informed in a reply sent to him by the present author, which he received before his trip to the Côa, that these arguments were ill-founded. Notwithstanding, he used them in his report to EDP and has developed them even further in a paper to appear in a coming issue of *Rock Art Research* (Bednarik n. d.).

Detailed information on Portuguese Upper Paleolithic faunas can be found in Cardoso (1993). The chronological, environmental and cultural evidence for the same period has been systematically reviewed in Zilhão (1995a). Marks *et al.* (1994), Póvoas *et al.* (1992) and Zilhão (1988, 1989, 1990, 1991, 1993, 1994) contain partial discussions of the relevant data in English or French.

8.1. Frost-weathering

In the littoral region of Estremadura, due to its proximity to the at times very cold waters of the Atlantic, last glacial cryoclastism of limestones is known almost down to present sea level (Daveau 1980). But, as demonstrated by recent work on slope (Rodrigues 1991) and cave (Zilhão 1995) deposits, below 400 m this process does not seem to occur after the last glacial maximum, coming to an end with the Upper Solutrean, at the time (*ca.* 17,000 BP) 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). Despite subsequent cold oscillations at sea, from then on the limestone hills and plateaus of this littoral region seem, on present evidence, to become covered by oak forests, and ibex and chamois disappear from the faunal assemblages. If, for the sake of argument, one used this littoral region as a model for the interior, and conceded that frost-weathering might have been a problem for the preservation of engravings of Solutrean age, the argument would still be simply untenable for the Magdalenian period. 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), periglacial features such as 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 (*ca.* 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 and 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 deposits containing a Late Gravettian (*ca.* 22,000 BP) archaeological context are coluvial sands entirely devoid of cryoclasts.

8.2. River erosion

Bednarik (1995a) argues that the fact petroglyphs occur right down to the floor of the Côa valley, only a few meters above water, makes it difficult to understand how Upper Paleolithic art could have survived the many fluctuations in river level that, based on evidence from other European rivers, must have happened in the Côa since the end of the Pleistocene. This is simply not a problem. Preservation of last glacial maximum archaeological remains in the flood plain of modern Portuguese rivers is demonstrated, for instance, at Terra do Manuel, where a radiocarbon dated 22,000 year old living floor located *ca.* 1 m

below the surface was excavated in 1988-89 (Zilhão 1995a). In this part of the country, this is explained by the pattern of downcutting caused by eustatic response to lowered sea levels. Fluviatile terraces accumulated at the beginning of isotope stage 2 were thus exposed as beaches that were made available to human occupation, and were indeed occupied. Coluvial and eolian accumulation of sediments eroded from the extant slopes largely denuded of vegetation subsequently buried and protected these sites from Tardiglacial and Holocene erosion. The lowest panels at Penascosa and Ribeira de Piscos are, as far as elevation above the river is concerned, in a topographical position even more favorable, from the point of view of preservation, than that of the Late Gravettian and Proto-Solutrean habitation site of Terra do Manuel. There is therefore no reason to suggest, until detailed geological studies of the valley (as yet unavailable) eventually show otherwise, that such a position is incompatible with a Paleolithic age for the engravings. In other words, elevation above the river is, *per se,* a totally irrelevant issue for the argument concerning the chronology of the Côa petroglyphs.

8.3. Absence of cold adapted species

It is true that, as Bednarik (1995a) notes, 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 any of the many Upper Paleolithic paleontological and archaeological sites that are known in Portugal and in Mediterranean Spain (Fig. 5), although Cardoso (1993) does cite 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 because other bones from the same site (Algar de João Ramos) were radiocarbon dated to ca. 14,000 BP, making an attribution to *Elephas antiquus* impossible. This reasoning, however, assumes that the faunal assemblage from this purely paleontological site is homogeneous and dates to a single period, which does not seem reasonable in a stratified cave environment poorly excavated in the late nineteenth century. In this context, it seems more reasonable to admit that the bone fragment in question does belong to *Elephas antiquus,* a species which, as Cardoso (1993) shows, survived in Portugal up to *ca.* 30,000 BP and could well have been represented in the otherwise quite banal Upper Pleistocene faunal assemblage from Algar de João Ramos.

All available evidence therefore suggests that the Atlantic and Mediterranean façades of Iberia, south of the Ebro, may have constituted a separate faunal province, where cold adapted species, even at the level of micromammals (Póvoas *et al.* 1994), did not penetrate (Aura and Villaverde 1995). Those species are also

completely absent from the thousands of engraved slabs found in the Gravettian, Solutrean and Magdalenian levels of the deeply stratified cave site of Parpalló (Valencia), which were found in very rich, and radiocarbon dated, archaeological deposits, spanning the Gravettian, Solutrean and Magdalenian periods (Villaverde 1994). Horse, aurochs, red deer and ibex, plus the occasional chamois, bird or carnivore, that is, exactly the same species as those whose bones have been recovered in the archaeological deposits (Davidson 1983), are the animals represented in these works of mobiliary art (Table 1). Stylistically, such representations are also strikingly close to those found in the Côa and, given their archaeological context, are known to be of Paleolithic age beyond any reasonable doubt.

TABLE 1
Parpalló Upper Paleolithic decorated stone slabs
Animal species represented (a)

	G	LS	EMS	LMS	US	SGI	SGII	SGIII	EMA	EMB	UM	GAL	Other	Total
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 (b)	–	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

(a) after Villaverde (1994: Table 26, modified); G – Gravettian; L – Lower Solutrean; EMS – Early Middle Solutrean; LMS – Late Middle Solutrean; US – Upper Solutrean: SGI – Solutreo-gravettian I; SGII – Solutreo-gravettian II; SGIII – Solutreo-gravettian III; EMA – Early Magdalenian B; UM – Upper Magdalenian; GAL – galleries (surface)
(b) chamois, fox, Iynx, wild boar, wolf, mustelids and birds.

Bednarik (n.d.) objects to these observations at two levels, one is factual, the other theoretical. At the factual level he restates, based on outdated (more than twenty year old) references, that cave bear is indeed present in two Portuguese faunal assemblages from the Pleistocene (those recovered at 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»: «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 an 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 they were first studied by Harlé (1910-11). As a result, both Harlé

(1910-11) and all subsequent authorities have given a categorical verdict on *Ursus spelaeus* 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). Therefore, Bednarik's taphonomic argument is irrelevant: the issue at stake is not why cave bear remains have not been found in non-limestone areas of Portugal, but why the species has never been found in Portuguese limestone cave deposits.

Since Cardoso (1993) reviews 21 cave sites spread all over the country, the absence of such remains is not easily explained away as due to deficient sampling. This brings up Bednarik's theoretical argument regarding cave bear and cold-adapted species: that «absence of evidence» does not equal «evidence of absence», that is, 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». It is also a common argument in anti-evolutionist thinking, for instance under the form of statements like this: «the fact that trilobites are absent from post-Paleozoic beds and dinosaurs are absent from pre-Mesozoic ones does not mean that their remains will not eventually be found in such deposits». If Bednarik were right in that criteria of absence cannot be accepted as scientific evidence, not only archaeology but also geology and paleontology would fall outside the scope of science.

The fact, however, is that, whether Bednarik likes it or not, these are well established disciplines that have developed their own scientific methodology and, in particular, have learned to deal with the issues regarding patterns of presence and absence in terms of probability statements (Dawkins 1991). For instance, given our present data base of Paleozoic and Mesozoic deposits, the probability that trilobites and dinosaurs were actually contemporaneous is so small that, for all practical purposes, such contemporaneity can be assessed as an impossibility. The data base of Portuguese Quaternary faunas is not as large as that of fossiliferous Paleozoic and Mesozoic beds all over the world, so the probability that bison, cave bear, mammoth, reindeer and woolly rhino may one day be found is not as small as in the dinosaur/trilobite example, and it cannot be considered a total impossibility, particularly in those parts of the country that are closer to the known past ranges of those species. Such a data base is, however, large enough to suggest that, if present at all, those cold-adapted species would probably have occurred only as very small and marginal populations (or even as stranded individuals) that one would not be correct in considering as part of the daily environment of Upper Paleolithic hunter-gatherers living

along the western and southern shores of Iberia. The tentative (and questionable) identification of bison, megaloceros, reindeer and woolly rhino among the fine lined 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 be taken, if confirmed by future research, as an indication of such infrequent occurrences. Meanwhile, «absence of evidence» should indeed be considered, in this case, as «evidence of absence».

8.4. Survival of aurochs, deer and horse in the «bleak tundra» of the Côa region

Bednarik (n.d.) argues that, being only 35 km away from the mountain glaciers of Serra da Estrela, «the [Côa] valley 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 Paleolithic 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 corresponds necessarily to a «bleak tundra». A simple look at a map will also show that most Cantabrian Upper Paleolithic 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.

8.5. Ibex as a chronological marker

Bednarik (1995a) also states that «by 11,000 BP, 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 are rare in Portugal, and all come from cave sites. In the examples known — all located in Estremadura — ibex, chamois and horse are entirely absent and the fauna is dominated by red deer and rabbit with marginal amounts of wild boar and roe deer. This is particularly the case at the cave of Caldeirão (Fig. 5), which was excavated by the present author and is located at a distance of 20 km from the top of Serra d'Aire, the highest elevation of Estremadura (678 m). At this site, the absence of horse and wild caprids contrasts markedly with their abundance in the underlying Solutrean lev-

els (Póvoas *et al.* 1992; Zilhão 1995a). This, together with other paleoenvironmental indicators, namely the rodent and the land snail faunas, suggests that, after 16,000 BP, the low altitude limestone elevations of Portuguese Estremadura were already covered by forest, and that alpine species and horse were not present in those areas. Given the proximity of the Côa region to the Meseta and its drier climate, it is quite possible, however, that ibex may have survived into the Magdalenian in this more inland part of the country, although such a survival is hard to conceive after 9500 BP. 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 Paleolithic petroglyphs in the Côa, was already covered by a *Quercus pyrenaica* forest up to an elevation of 1600 m (Mateus and Queirós 1993). Since ibex is not known to inhabit these forests, **it seems very unlikely that it survived in the Côa region after the end of the Upper Paleolithic and, therefore, that the representations of the animal found in the valley art postdate the Tardiglacial/Early Holocene boundary**. It should be stressed, in any case, that ibex is not represented in the faunal inventories from the late Holocene archaeological sites (for instance, those from the Neolithic or the Chalcolithic) of the Douro basin (Jorge 1993).

8.6. Use of metal tools

Bednarik (1995b) categorically states that one stylistically Paleolithic anthropomorphic figure engraved in a panel from Ribeira de Piscos (one that was not analyzed in the framework of the «direct dating project») had been made with a metal tool, which obviously excluded a Paleolithic age for this figure. Francesco d'Errico, a world authority on this kind of problem, contributed the following comments on this issue (*in litteris,* August 23, 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 (...) 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 totally without basis.

8.7. Absence of archaeological context

Bednarik (1995a) states that the Paleolithic, Epipaleolithic, or even Mesolithic, settlements closest to the Côa art sites are more than 120 km away and that, therefore, there would be no archaeological context for that art, if it were indeed Paleolithic. The fact that he uses this as part of the argument against the Côa art being of Paleolithic age (Bednarik n.d.) is intriguing since, contrary to what he had to say on last glacial faunas, this time he does equal «absence of evidence» with «evidence of absence». In this case, however, the equation is not appropriate, in the first place because, although still few and far between (due to the traditional bias towards caves and the fact that there are no limestone outcrops in the area), several Upper Paleolithic sites have been found in the Spanish Meseta since the early 1980's. Bengoechea *et al.* (1986) and Fabian (1986), for instance, report Solutrean and Magdalenian open air settlement sites in the Valladolid and Salamanca provinces, across the border from Portugal (Fig. 5). So, if the issue of archaeological context is looked at from a regional perspective, it is quite clear that interpretation of absence is not what is at stake here.

If looked at from a local perspective the fact that, until recently, no Upper Paleolithic archaeological sites had been found in the Côa valley (or, more generally, in Northern Portugal) was in itself meaningless because no survey for Paleolithic sites had ever been carried out. In other words, since no one had ever looked for the evidence, any assessment regarding the presence or absence of such evidence would in this case be scientifically illegitimate. Recent developments illustrate the point better than any further considerations on logic. On August 14, 1995, at 10 a.m., after only 20 minutes of the first serious archaeological survey of the Côa valley by experienced Paleolithic archaeologists, an open air camp site was found at Cardina, on a Pleistocene platform *ca.* 3 km upriver from Penascosa (Fig. 1). The lithic assemblage so far recovered in the test excavations already carried out at this site 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 BP). Testing carried out in September at a second nearby platform revealed an extremely rich archaeological level at a depth of *ca.* 1 m below the surface. The thousands of lithic artifacts recovered in the 4 m^2 test make up an assemblage that is typical of the Late Gravettian: similar assemblages excavated in several cave and open air settlement sites located in Portuguese Estremadura have already been radiocarbon dated to *ca.* 22,000 BP. It should be quite clear from these examples that there must be many more sites in the region waiting to be found.

9. BEDNARIK'S ARGUMENTS ON STYLE

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

- **Regarding bovids**
  «several bovids have internal markings on muzzles that do not resemble Paleolithic art»; «the horns on bovids do not resemble those on most of the supposed aurochs figures of Paleolithic 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 aurochsen.

- **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 Paleolithic animal pictures».

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

squarish shape of the muzzle. As for the caprids, the four representations in Fig. 5 (one from Penascosa, one from Canada do Inferno, and two from Quinta da Barca) should be enough to 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 of these horn representations show the characteristic twisted shape that allows the unmistakable identification of these caprids as belonging to the iberian variety of ibex, *Capra pyrenaica* (cf. Altuna and Apellániz 1978).

In short, the specific conventions mentioned by Bednarik (1995b) conform completely with what is known from well dated Paleolithic art and 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 is obviously absurd, and deserves no further comment except that, as was the case with Watchman, 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 on the post-Paleolithic age of the stylistically Paleolithic petroglyphs; and to make an innovative point regarding preservation policies for rock art sites since, according to them, «if the art were to be shown to be post-Paleolithic, its importance would diminish dramatically and the controversy concerning its preservation would be largely resolved» (quoted from a letter by Bednarik and Watchman offering their services to EDP's board of directors, dated March 24, 1995).

10. VALIDITY OF DATING BY STYLE IN SOUTHWESTERN EUROPE

Since AMS dating of pigments was introduced, 25 individual paintings from the sites of Altamira, Chauvet, Cougnac, Cosquer, Covaciella, El Castillo, Le Portel, Niaux and Pech Merle have been directly dated. The results obtained ranged from 12,000 to 32,000 BP; that is, except for two unpublished contaminated samples from Gargas and Bédeilhac, there are no instances of rock art attributed to the Upper Paleolithic in Southwestern Europe that turned out to give Mesolithic, Neolithic, Chalcolithic or even later ages, when directly dated by ^{14}C AMS (Clottes and Lorblanchet, personal communications). The probability that this is due to simple chance is so infinitely small that it can be considered nil.

The reason why is that, unlike the case in most other regions of the world, archaeologists in Southwestern Europe are fortunate enough to have at their disposal an independent standard against which the characteristics of rock art can be compared: that provided by mobiliary art. It cannot be argued, therefore, that stylistic dating of rock art is non-scientific because it cannot be refuted: it can. To refute the stylistic dating of the Côa and other Iberian open air petroglyph sites

to the Upper Paleolithic 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 in Southwestern Europe. Therefore, in what concerns this part of the world, and in the present state of our knowledge, the proper scientific stand can only be that of accepting what has been confirmed by one hundred years of research: that stylistically Upper Paleolithic engravings are indeed of Upper Paleolithic age. This is all the more so in the case of the Côa region because its late Holocene history is relatively well known, particularly after recent work carried out in the area by S. O. Jorge (Jorge 1993; see also Alarcão 1990): megaliths, as well as rock shelters painted in the same style as some of such megaliths, are a well known feature of the archaeology of central Portugal; important Chalcolithic settlements and Bronze Age sites with decorated *statues-menhirs* exist only a few kilometers from the Paleolithic rock art sites; Iron Age art in the Côa valley is recognizable not only by style but also by the fact that the horses are mounted by humans that carry characteristic weapons found in burials from that time. The hypothesis of the survival in the Côa, into late pre-historic times, of a Paleolithic group with a Paleolithic economy and a Paleolithic art is therefore pure nonsense.

This conclusion is also supported by the other chronological method traditionally used in art history: the analysis of the stratigraphic superposition of figures. At Vermelhosa, a newly discovered rock art site close to the confluence between the Côa and the Douro (Fig. 1), there is a panel with a very fresh Iron Age engraving (a mounted warrior with characteristic weapons) superimposed on a very patinated fine lined deer filled with *grabado estriado.* The latter is virtually identical to those found in engraved bones from the Early Magdalenian levels of Altamira and Castillo (Cabrera and Giménez 1989). It is quite clear that, if the Iron Age figure is 2000 to 3000 years old, then the stylistically Paleolithic one has to be much older. Similar fine lined engravings are known in association with the larger sized pecked engravings from the better known locations in the Côa valley itself. One particular instance is an aurochs from the panel at Ribeira de Piscos that also contains the anthropomorphic figure Bednarik claims to have been made with a metal tool. The interior of this aurochs is also filled with *grabado estriado,* which Bednarik (1995b) claims to be unusual for Paleolithic art. Instead, this technique is characteristic of Solutrean and Early Magdalenian engravings in both the parietal and the mobiliary art of Iberia.

One of the main objections raised by Bednarik (1995b) against the validity of stylistic dating is the fact 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». He uses this as an example that «the idea

that naturalistic art is a typical Paleolithic phenomenon is clearly false», and as another argument against a Paleolithic age for the Côa petroglyphs. This line of reasoning is, however, misleading. First, nobody attributed the Côa figures to the Paleolithic on the basis that they were naturalistic in general but because of their particular naturalistic style, which is typical of European Paleolithic art and very different from Levantine. Second, the attribution of Levantine art to the Paleolithic by Breuil 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 and was immediately criticized, particularly by Spanish researchers such as Hernandez-Pacheco, who as early as the 1920's presented a strong case in favor of a post-Paleolithic age for the Levantine art. Fourth, the final demonstration that such was the case has come about in recent years through the application of the traditional art history methods which, according to Bednarik (1995b), «rock art science outside Europe does not accept»: the realization that, in some panels, Levantine figures were stratigraphically superimposed on «macroschematic» themes which, in turn, were identical to those found in the decoration of Early Neolithic ceramics (Beltrán 1982; Marti and Hernandez 1988).

Bednarik's misuse of the evidence pertaining to the Holocenic rock art of Iberia is also apparent when he quotes approvingly from a paper by Portuguese rock art researcher A. M. Baptista on Vale da Casa, a site with several Iron Age engraved panels located near Pocinho, a few kilometers downstream from the confluence between the Côa and the Douro (Bednarik n.d.). In this work, Baptista (1983) 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 Paleolithic art. Based on this example, he went on to caution against the use of stylistic criteria alone in rock art dating and to stress the need for considering the archaeological context as well. As a further example of this, he mentioned a horse figure from Vale da Casa that, according to him, would show some stylistic similarities with the Mazouco horse (the first open air engraving to be attributed to the Paleolithic — Jorge *et al.* 1981) and that, were it not for the clear Iron Age context of the site (where horses are often mounted by warriors carrying characteristic weapons), might have been considered, therefore, of Paleolithic age as well. Had Bednarik actually read Baptista's paper and looked at the illustrations that accompany it, he would have immediately realized that these were not suitable grounds on which to seek support for his case. Apart from the sinuous cervico-dorsal lines of the horses, nothing else in Vale da Casa resembles Mazouco or Paleolithic art in general: the figures are all 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. To use Vale da Casa as a further argument against the stylistic dating of the Côa art to the Paleolithic, and as another example of the unreliability of stylistic dating in general, is therefore a gross distortion of the evidence, as is also Bednarik's omission of Baptista's opinions on the Côa: he is one of the Portuguese rock art researchers hired by the Portuguese government to study the art of the valley and he has always sustained the Paleolithic age of the stylistically Paleolithic engravings which Bednarik «dated» to recent times.

The other argument used by Bednarik (1995b) to substantiate his affirmation that stylistic dating should be abandoned (and should never have been accepted in the first place) is that, in some instances, AMS radiocarbon dating of pigments has shown that the age predicted by archaeologists on the basis of stylistic analysis has had to be corrected. The most striking case of that would be Chauvet, where an Aurignacian age was obtained, instead of the Solutrean age predicted by stylistic dating (Clottes *et al.* 1995). More than anything else, however, this case provides an illustration of the shortcomings of Leroi-Gourhan's system which, for the earlier periods, is not based on the rule of following parallels with mobiliary art but, instead, on the assumption that animal representation evolved from simple to complex. It is this assumption that is proven wrong by the dates for Chauvet, not the methods of stylistic analysis. Actually, comparison with the mobiliary art of the German Aurignacian shows that, both as concerns style and the animals represented (rhino, horse, lion, bear), the dates for Chauvet make perfect sense.

Even if, however, one accepts for the sake of argument that the error of 50% that occurred in the extreme case of Chauvet is typical of the stylistic dating of Southwestern Europe Paleolithic rock art, stylistic dating would still be a much better tool than the combined «direct dating» techniques used in the Côa by Bednarik and Watchman: their results diverge not by 50%, but by at least 3000%! In any case, it is quite clear that the typical error involved in stylistic dating is far less than 50%, and more often than not the results obtained by AMS direct dating of paintings have been in accord with archaeological expectations. One well-known example was the experiment in dating bison from Altamira, Castillo and Niaux that were all considered to be of «Early Style IV», that is, to date between 16,000 and 13,000 BP. 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 — confirmed the stylistic age (Valladas *et al.* 1992). The same happened recently with the bison from the new site of Covaciella, in Asturias, where two «Style IV» bison were directly dated to *ca.* 14,000 BP (J. Fortea, *in litteris,* September 10, 1995).

This should not be interpreted as indicating that archaeologists are content with stylistic dating and that new direct dating techniques should be considered unwelcome and unnecessary. The present author's position in this regard is exactly the opposite: better dating tools are indeed badly needed. This does not mean, however, that one should throw the baby out with the bath water. Stylistic analyses of pre-historic art have resulted in sound achievements that cannot be readily dismissed by techniques whose reliability is unknown or questionable. The insistence by some practitioners of those techniques, such as Bednarik and Watchman, on refusing to recognize the limits of their own methods and the merits of classical approaches represents a more serious obstacle to the development of reliable direct dating technologies than the reluctance of more traditionally oriented archaeologists in accepting methods derived from the experimental sciences. Such insistence in any case leads those practitioners to a paradox and a methodological dead end: since the direct dating of every single one of the millions of figures known from rock art all over the world is obviously impossible, they have to accept that the direct dating of some is meaningful only if their age can be used, through stylistic and contextual criteria, to substantiate a chronological attribution of those that it will never be possible to date directly. If stylistic dating is totally invalid, why, then, would anyone want to develop direct dating techniques? Or are Bednarik and Watchman implying that only the dated figures should be accepted and that, therefore, the overwhelming majority of the evidence should be discarded? If so, would not that be the equivalent of transforming rock art studies, not into the «science» that they claim to practice, but into a dilettante «hobby»?

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, the acceptance of this condition turned out to be 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 (such 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 how inconceivable it is (at least outside the paradigms of «scientific creationism») that the incision of the Côa valley, which is more than 200 m deep, could have begun only in the mid-Holocene. Had they «bothered» to do their background homework on Iberian history and prehistory, they would immediately have realized that their dates could not possibly be correct. Thus, the spectacular failure of Bednarik and

Watchman's «direct dating» methods (which is not without parallel in the past — cf. Glozel) also conveys a moral: that norms of professional ethic 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 the best possible insurance against making a fool of yourself.

11. CONCLUSION

Watchman's maximum age is not directly based on the radiocarbon results he obtained but on an interpretation of those results that is based on false or unverified assumptions. It is in any case contradicted by Dorn's minimum ages. The latter can only be used to refute Watchman's attribution to the historical period of the figures analyzed, they do not refute the attribution of the Côa petroglyphs to the Paleolithic: it is obvious that the statement that a figure was made before 2000 or 5000 BP does not contradict the statement that it is older than 10,000 BP. 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 Paleolithic. On present evidence, it would seem, for instance, that it is difficult to define a specifically Early Magdalenian style as opposed to a specifically Late Gravettian one (or, at least, that archaeologists and rock art experts have not yet been able to devise adequate criteria to recognize and differentiate stylistic conventions exclusive of each of those time periods). However, AMS radiocarbon dating has shown that, in Southwestern Europe, the criteria used to attribute rock art to the Upper Paleolithic in general have stood the test of direct dating.

In this context, only if the results of a detailed archaeological and geological study of the valley showed that the Côa valley petroglyphs could not possibly date to the Paleolithic, would it be scientifically legitimate to question such an age. Those studies have just begun, and the first results obtained are entirely in agreement with the stylistic dating. Therefore, the only possible conclusion that on present evidence can be extracted from the controversy regarding the dating of the stylistic Paleolithic petroglyphs of the Côa valley is that there is absolutely no valid reason to question their chronological attribution to the Upper Paleolithic, that is, to the period between 10,000 and 30,000 years ago.

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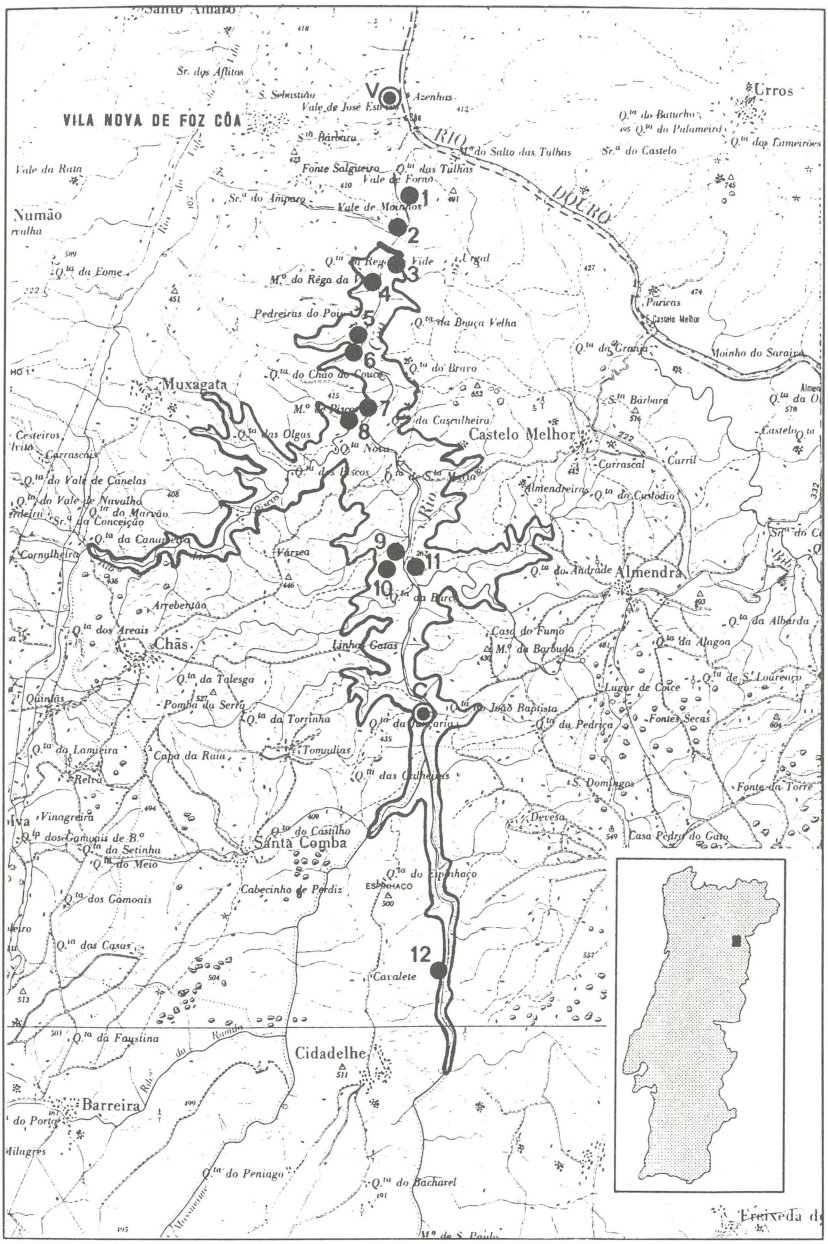


Fig. 1 – The Côa valley, with location of the several clusters of rock art attributed to the Upper Paleolithic on stylistic grounds known until March 1995. The limits of the lake that will flood the area if the dam is constructed are also indicated. The base is the 1:100,000 map of the area published 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. The letters V and C represent, respectively, the Iron Age and Paleolithic art site of Vermelhosa, on the Douro, and the Upper Paleolithic camp sites of Cardina, on the Côa.

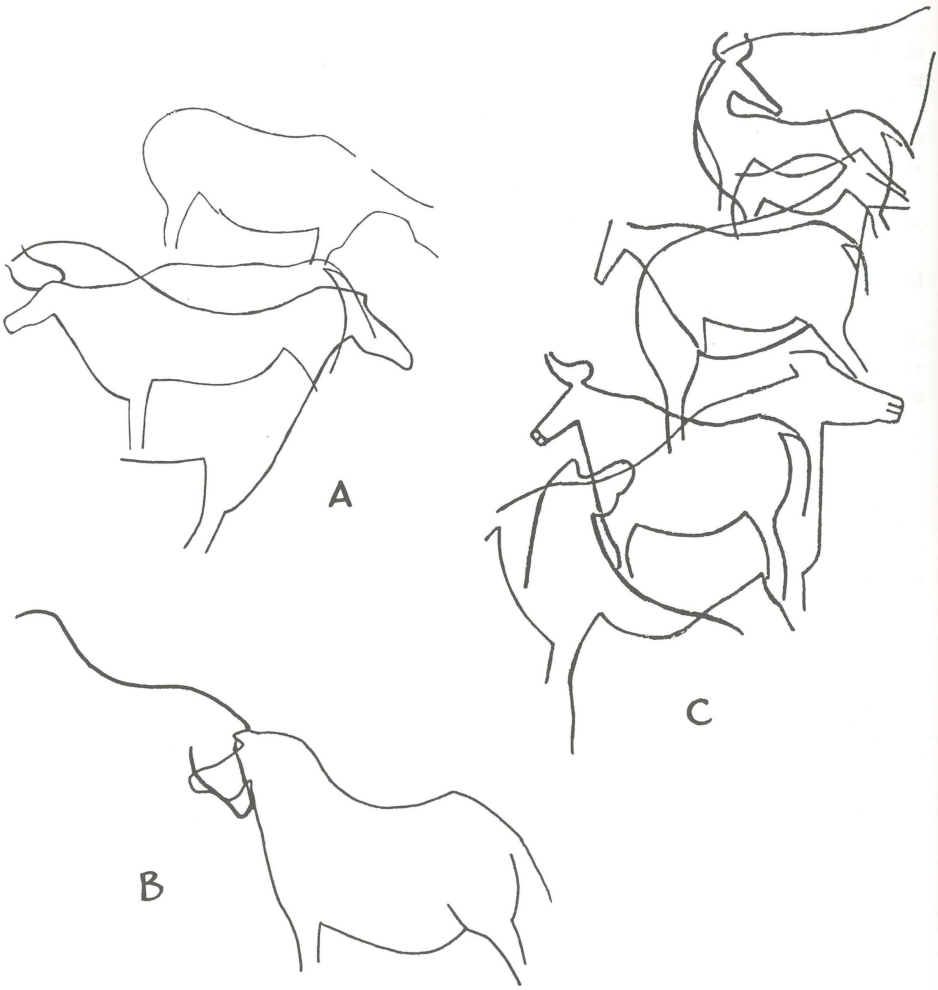


Fig. 2 – The panels analyzed in the framework of EDP's direct dating project. A. Canada do Inferno; B. Ribeira de Piscos; C. Penascosa. In this figure, as well as in Figs. 4, 6 and 7, the tracings were done from photographs and cannot be taken as an exact rendering of the subjects (that from Canada do 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 Paleolithic figures from the Côa have sizes between *ca.* 50 cm and *ca.* 1 m.

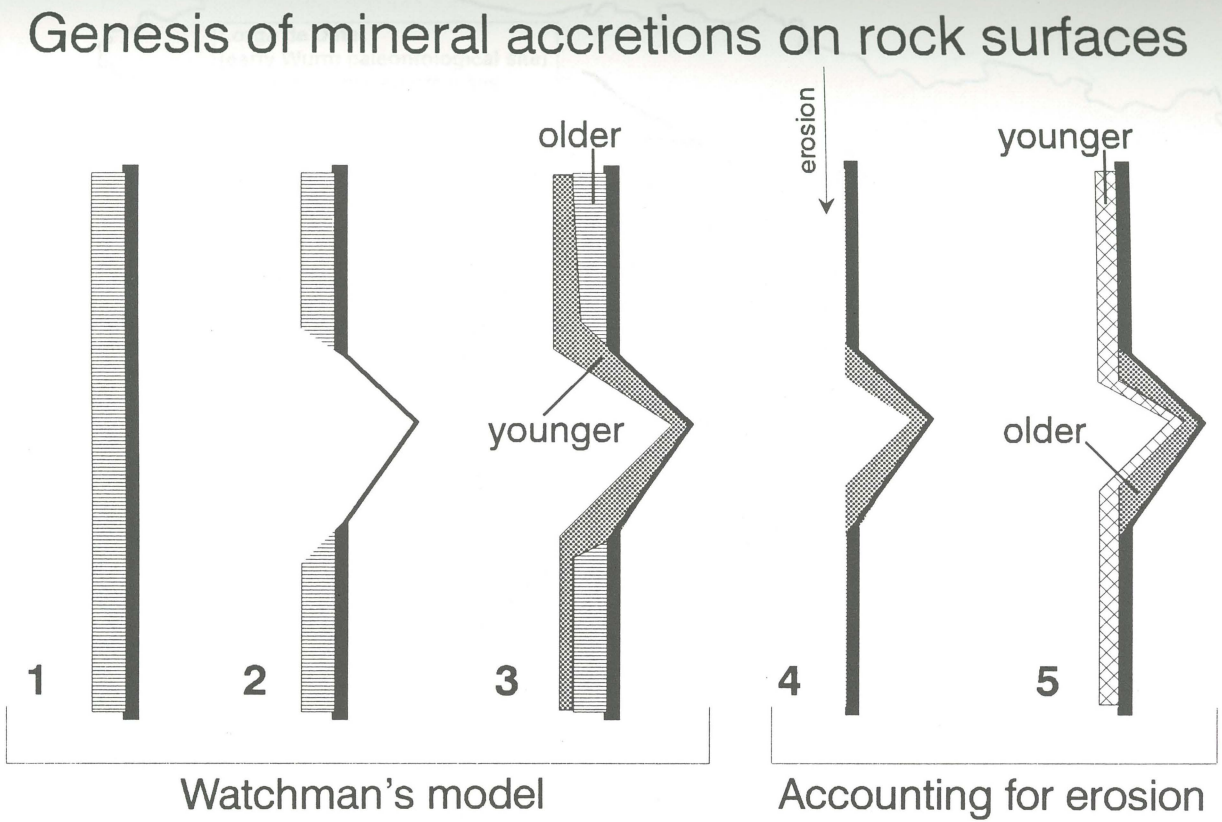


Fig. 3 – Models for the genesis of mineral accretions on rock surfaces. In Watchman's model, the material filling the grooves is younger than that in adjacent rock surfaces: 1. mineral accretion forms on freshly exposed surface; 2. the incision by petroglyph execution eliminates the accretion from the 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.

Fig. 4 – Penascosa: tracing of panel 6 (see caption to Fig. 2).

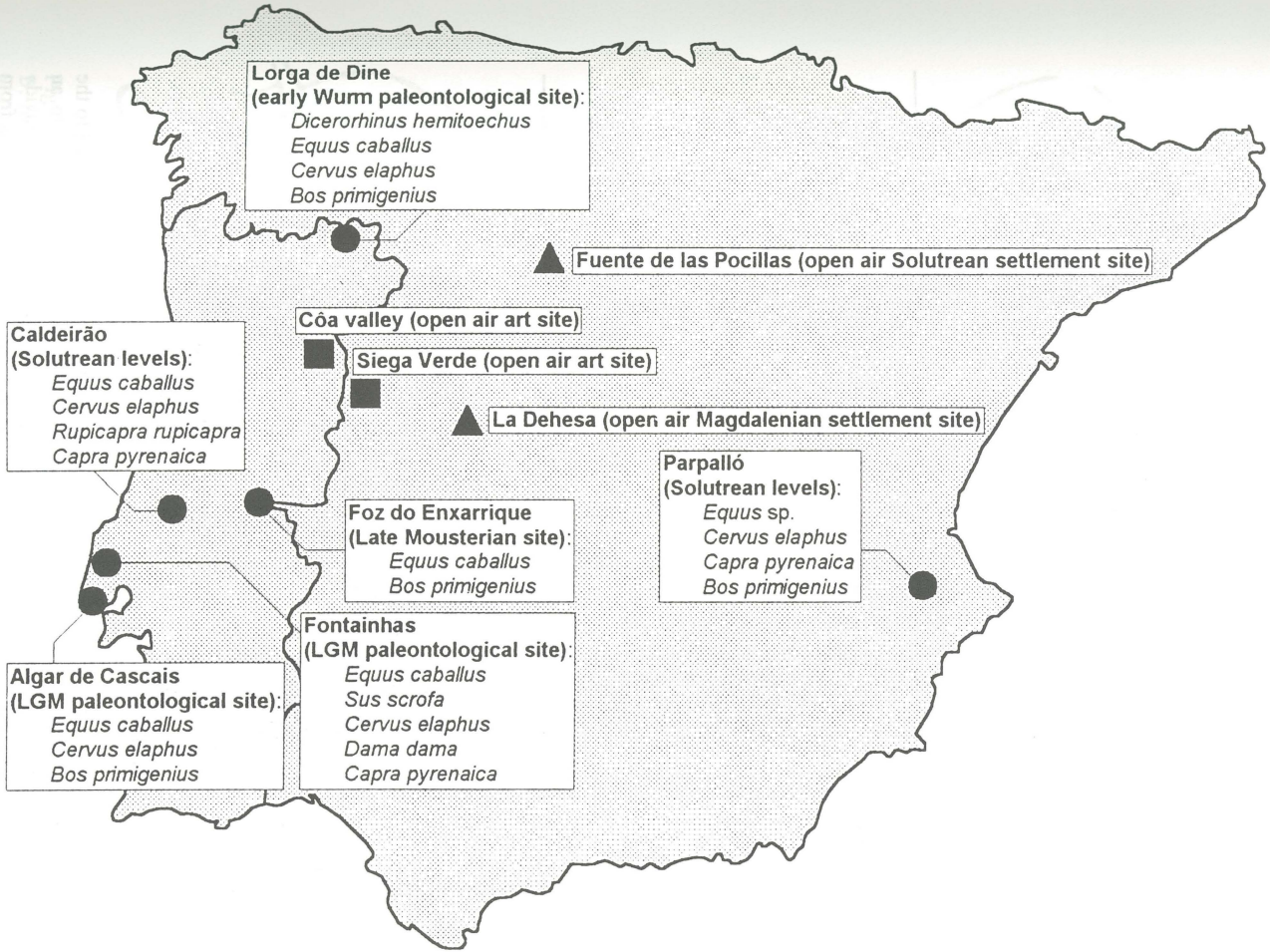
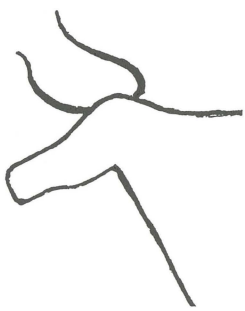


Fig. 5 – Selected Portuguese Upper Paleolithic sites with archaeological and paleontological large herbivore faunas, and settlement sites in Spain close to the Côa valley complex of Paleolithic open air rock art sites.

Shapes	Parpalló	Côa
	Middle Solutrean	present
	Upper Solutrean Solutreo-gravettian Early Magdalenian	present
	Upper Magdalenian	absent



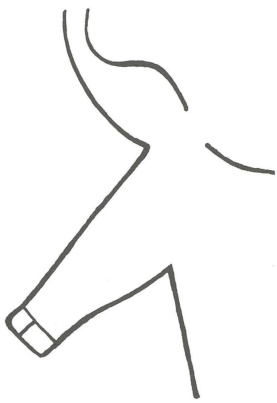


Fig. 6 – Top: horn shapes of the aurochsen in the engraved slabs from Parpalló compared to the Côa figures. Middle: the head and horns of the Tête du Lion aurochs (left) compared to an aurochs from the Canada do Inferno panel, which Bednarik describes as a «domestic bovid» (right). Bottom: head and horns of a Lascaux aurochs (left) compared to that of an aurochs from the Penascosa panel analyzed by Bednarik, for whom the shape of its horns and the internal markings on its muzzle «do not resemble Paleolithic art» (right).

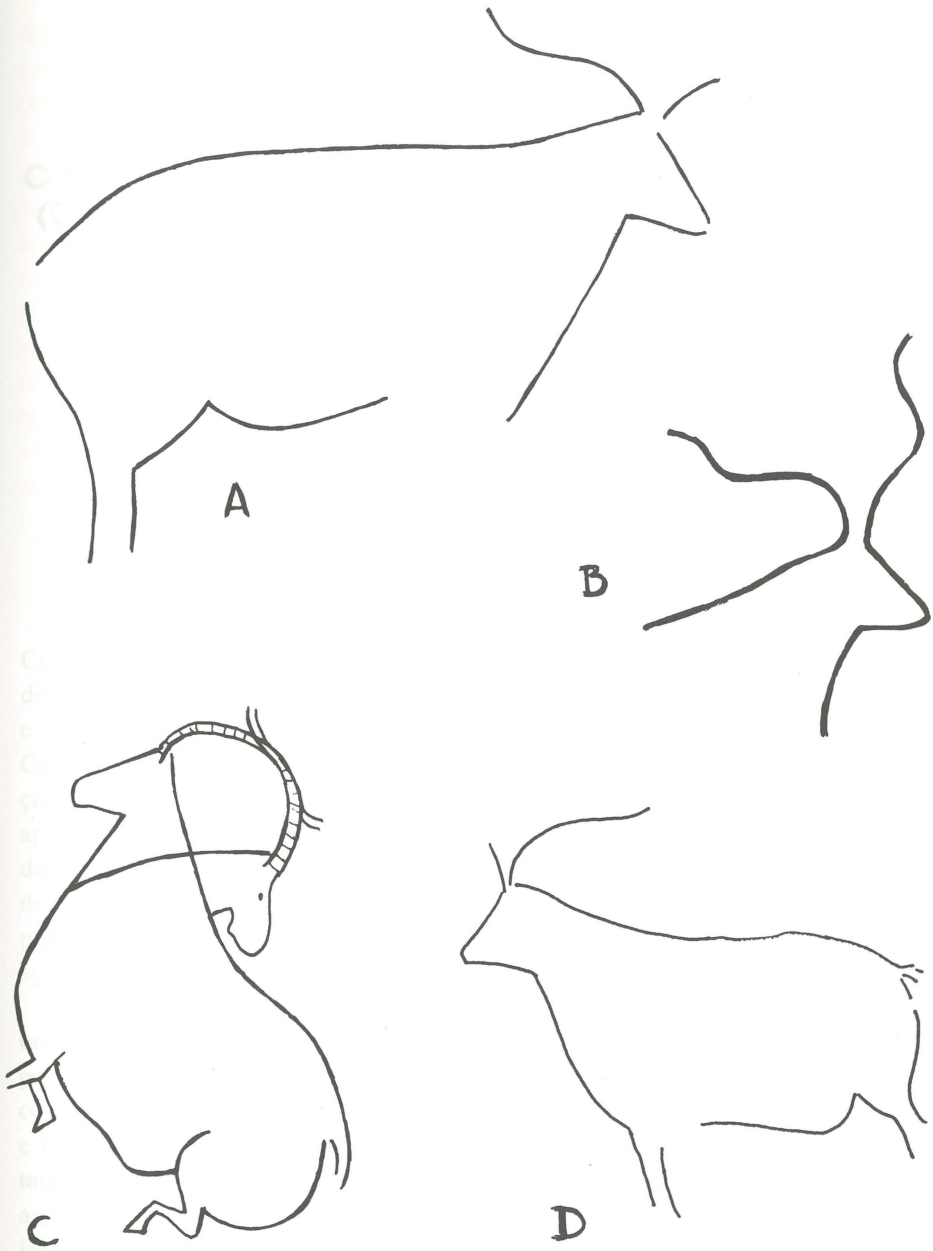


Fig. 7 – Ibex representations in the Côa valley rock art: A. from a panel at Canada do Inferno; B. from a panel at Quinta da Barca I; C. from a panel at Quinta da Barca III; D. from a panel at Penascosa. According to Bednarik, «the large and distinctly curved horns of the ibex are not present, instead the horns found in the petroglyphs resemble those of certain domestic goats».