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RAR 20-635

Fact and fiction in the Côa valley

By ALAN WATCHMAN

A major problem with this paper is trying to sift the grains of fact from the chaff of fiction. Without a background and understanding of the Côa controversy, especially the independent scientific dating projects, the paper is believable. However, the biased reporting of the age of the rock carvings leads the informed reader to suspect that other aspects of the paper may not be critically represented.

Particular concerns relate to some inaccurate and misleading statements. For example, the contention that '*Raw-material proveniences* (my emphasis) show that the region was permanently inhabited by human groups which maintained geographically extensive networks of contact, circulation and exchange' is incomprehensible, illogical and unsubstantiated. How can the source of earth materials indicate levels of human occupation in an area?

Describing the rock art as Palaeolithic, but then saying that the motifs '*seem to date to the Gravettian and the Solutrean*' gives a glimpse as to the uncertainty in Dr Zilhão's mind about the real age of the carvings. Could they also seem to date to a much more recent period?

Labelling some of the carved animals 'species' as 'aurochsen' and 'ibex' reflects biased personal opinion. They could also be cows and goats!

The uncritical conviction that 'some very large figures are *certainly not* related to habitation. This *must* be the case, for instance, with the group of three "aurochsen"...' reveals passionate belief from personal interpretation of the carvings, but without any substantive evidence. There are other biases and errors, but to counter them individually establishes the paper as credible, which it is not.

Arguments concerning the probable age of the carvings have been proposed and debated. In 1995, during the political controversy in Portugal, many people believed that the dam should be stopped because the petroglyphs were Palaeolithic. The old age was the key reason why they needed to be saved from flooding. The scientific analyses carried out by Robert Bednarik (1995a) and I (Watchman 1995) to estimate the age of the so-called Palaeolithic carvings provided a much younger perspective. Dorn (1997) and Phillips et al. (1997) have provided support for the Palaeolithic hypothesis. However, the decision about protecting the Côa valley carvings was made by the Portuguese government based on the findings of a UNESCO panel of experts, who believe that stylistically the carvings are Palaeolithic. It is for this reason that there is now a UNESCO-sponsored World Heritage archaeological park and tourism in the Côa valley. The need for ongoing re-

search, conservation and management is unchallenged.

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RAR 20-636

Questions for Dr Zilhão

By ROBERT G. BEDNARIK

I am grateful to Dr Zilhão for offering his views on the research and management of the Côa petroglyph corpus for discussion. However, his report contains many inaccuracies that must not go unchallenged. Some relate to matters that one can argue about, being matters of opinion; some concern serious omissions that need clarification; and some *cannot* be argued about because they are matters of fact.

But first some points of agreement. Dr Zilhão mentions the planned but 'somewhat delayed' museum at the Côa dam site. Its construction has been forcefully demanded by IFRAO (to prevent recommencement of dam construction), most especially by Jack Steinbring in 1998. But ominously these delays continue, and as of early 2003 the museum project has not progressed at all. There is a privately owned, very well presented museum at Quinta da Ervamoira, within the Park, built after 1995 and fully completed in 1998. And concerning the wines produced at that property, I do agree with Dr Zilhão that they are superb.

I can also agree unreservedly on the question of the broad effects of the Côa campaign. Campaigners for preserving rock art anywhere in the world can take note that 100% of a sample of Portuguese high school students and 97% of the general population knew about the rock art. This extremely high level of awareness is without doubt attributable to the IFRAO campaign led by Mila Simões de Abreu. It demonstrates the value and potential long-term benefits for rock art protection of conducting high-profile media campaigns of this kind.

Matters of opinion

Dr Zilhão suggests that, 'originally, the valley's Palaeolithic representations were colour-treated', based on his identification of 'red paint' on one 'aurochsen' petroglyph at Faia. This illustrates his loose application of deductive reasoning. He ignores the dearth of painted petroglyphs in authentic Palaeolithic rock art (i.e. the Franco-Cantabrian cave art) and generalises from one instance to the whole corpus. He fails to show that what he sees on the Faia figure is indeed paint residue, here or in his other publications. But most importantly, how does he reconcile the complete and global lack of any Pleistocene paint residues on exposed rock surfaces with his extraordinary claim that the

'red paint' he perceives on the Faia image is of the Ice Age? His claim amounts to the proposition that this one figure is the world's only instance of surviving Pleistocene paint traces on an exposed rock panel. I reject it as extremely unlikely, and provided that what he claims to be paint is indeed applied pigment, this would very strongly imply a late Holocene antiquity.

The admission that, '[F]rom a stylistic point of view, the Palaeolithic art of the Côa presents some significant novelties, rare or unknown in Franco-Cantabrian parietal art' indicates that even Dr Zilhão himself finds it hard to reconcile many stylistic elements at Côa with his preferred interpretation. I had arrived at the same finding in April 1995, when I still 'shared the stylistic conviction of my colleagues' and when I was still 'confident that the Côa valley art will eventually be shown to be of Palaeolithic age' (Bednarik 1995b), but was sufficiently alarmed to call for scientific dating work. Instead of admitting that many if not most of the Côa zoomorphs are not of authentic Palaeolithic style or treatment, Dr Zilhão presents us with more personal opinions. Their attitudes indicate 'mating' and 'drinking scenes', he says, as if his visual perception could provide a measure of what a Palaeolithic artist perceived. He tells us which species were depicted, as if he had communicated with the artists. All of this belongs into the realm of archaeological mythology, or Bahn's (1990: 75) 'consensus fiction' of the past. It has no scientific currency, except for the study of Dr Zilhão's own visual perception and cognition. But his creative interpretations do not end here, he has even worked out the purpose of at least some of the motifs: they were territorial markers. At this stage I think we have well and truly arrived in the realm of science fiction.

Even if we do admit the possibility that contemporary Western perception can determine animal species in ancient rock art, it soon becomes apparent that Dr Zilhão's 'identifications' are of no value to his case. Aurochs, horses, ibex, deer, fish and chamois all occurred in the region in the Historical period, while typical Pleistocene species are completely absent at all the schist sites—as are the most typical Palaeolithic motifs, the so-called signs. Moreover, the bovids at Côa, Siega Verde and all other Iberian schist sites claimed to be Palaeolithic look to me like modern cattle breeds, including Spanish fighting bulls, and *Capra* sp. still survive in the region, contrary to Dr Zilhão's claims. Dr Thomas Wyrwoll (2000) has convincingly demonstrated that the ibex-like Côa figures Zilhão claims are Pleistocene closely resemble the coat markings on an extant species (Fig. 1). Horse images like the ones at these sites occur in their thousands in the area, in clearly modern contexts (Hansen 1997).

The shrill claims flaunting the importance of his Côa work are arguably irrational, and they seem to illustrate Dr Zilhão's preoccupation. For instance, his belief that the 'Côa finds ... crown a Copernican revolution', that they are as important as 'the revelation of Altamira', or his entirely unrealistic plans to cater for 200 000 annual visitors and his falsifying of previous visitor numbers all indicate a capacity for unwholesome grandiosity. These and other fac-

tors cannot be treated as mere matters of opinion.

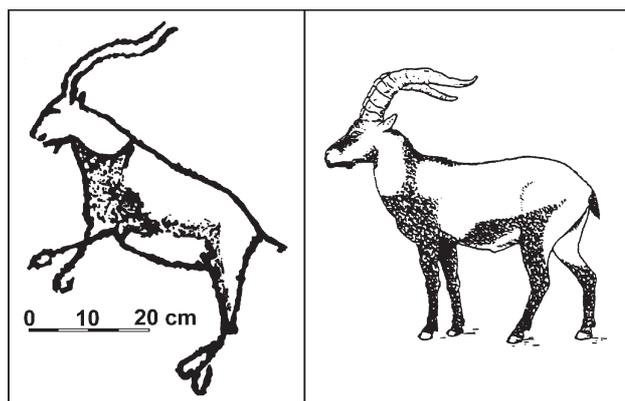


Figure 2. Zoomorphic petroglyph from Rego da Vale (on left, adapted from Zilhão et al. 1997) and drawing of *Capra ibex victoriae*, a Holocene subspecies (after Engländer 1986).

Matters of factual distortion

According to Dr Zilhão, the existence of the Côa petroglyphs 'was officially announced by the responsible authorities' in November 1994. This is incorrect. In late November 1994, two IFRAO Representatives (Abreu and L. Jaffe) were asked by N. Rebanda, a consulting archaeologist who had conducted survey work on behalf of the Côa dam builders for years, to inspect the Canada do Inferno rock art site. Until then this discovery had been kept confidential. Abreu immediately notified IFRAO and began organising the campaign to stop the dam. IFRAO published a report about the issue in the same month (!), November 1994 (Bednarik 1994a), and it was only in the face of international censure orchestrated by IFRAO that the 'responsible authorities' admitted in early 1995 that they had concealed the existence of the rock art for years (Bednarik 1995c). Dr Zilhão avoids all reference to Abreu and her superhuman endeavours to save the Côa rock art, and he also avoids all reference to IFRAO's role in the Côa campaign. This is a serious distortion of the historical facts. The abandonment of the Côa dam was already a fait accompli by June 1995, when I was in the valley and had detailed discussions with EDP engineers. I learnt that an alternative site had been chosen already, and although its location was not disclosed to me we know today that it was the Sabor valley. (Now, here is a subject Dr Zilhão could address with authority, his role in the EDP's concealment of most of the rock art in the Sabor valley during the years he presided over the IPA.) The political process was somewhat slower, but the decision of November 1995, by the new government, was a foregone conclusion.

Dr Zilhão claims that no members of the typical Pleistocene fauna depicted in the cave art occurred south of the Ebro 'at the time'. His chronological qualification is itself interesting, since he does not specify 'the time' (Solutrean? Gravettian? Magdalenian?), but concerning the Final Pleistocene fauna of Iberia he does need to consult the palaeontological literature. For instance, *Coelodonta antiquitatis* (Kurtén 1968: Fig. 60), *Panthera spelaea*, *Crocota crocuta*

and *Ursus spelaeus* certainly occurred south of the Ebro (e.g. Altuna 1972, 1973; Cadeo 1956), and the latter species even in Portugal (at Furninha and Salemas; Musil 1981).

Similarly, Dr Zilhão's grasp of relevant geology has already led to his severe misinterpretation of the results from the Fariseu excavation. He continues to ignore his own statement that the presumed lithic artefacts from that site are all from colluvial strata (and hence have no stratigraphic context) (Anon. 2000). Not only has he made this cardinal error in the first place, apparently he is still not aware that colluvial detritus is of no stratigraphic relevance. It is the very nature of a colluvium that it comprises components of wildly different ages and is therefore totally irrelevant for dating. Moreover, in the years since the Fariseu excavation he has failed to report a single radiocarbon or luminescence date from that site, which others have predicted would contain only recent lake sediments (Abreu and Bednarik 2000). This is a crucial factor in the Côa debate and Dr Zilhão must make his dating results from Fariseu public—even though these 'Gravettian and Magdalenian' sediments are probably less than twenty years old because they were formed since the establishment of the Pocinho dam. After all, he informed us in 2000 that Norbert Mercier had sampled the site for OSL analysis, so where are the results? We also need to see illustrations of the so-called stone artefacts from Fariseu, and of the 'pebble engraved on both sides with geometrical stylised animal motifs that have parallels in the Azilian of France' (Anon. 2000). The lithic sample from that site, we were told in 2000, 'is not big enough to allow a precise diagnostic of the assemblage', yet here Zilhão states unambiguously that it consists of Gravettian and Magdalenian tools. Bearing in mind that no dates of any description have been disclosed from Fariseu (Aubry et al. 2002) and that no tools have been presented, the claim for its antiquity is spurious.

Dr Zilhão's distortions of the political aspects of the Côa campaign also require a response. As he wrested control of the campaign from Abreu during 1995, he made the preservation of the rock art conditional on acceptance of his hypothesis of its Pleistocene age. A social scientist who thoroughly analysed the public campaign concluded unambiguously that 'the political nature of the archaeologists' strategy influenced their scientific discourse' (Gonçalves 1998: 18). To preserve their claim that the rock art is of Palaeolithic age they tied its preservation to this age claim—and in fact demanded that it must be preserved *because* it is of Palaeolithic age. This fundamental error of strategy has haunted Dr Zilhão ever since, as his grotesque reactions to the almost identical Guadiana issue amply demonstrate (Zilhão 2001). Concerning this destruction of the largest rock art complex in Portugal, and one of Europe's greatest, the responsibility for this monumental act of vandalism rests squarely with Dr Zilhão. It is immaterial whether he did not know about the rock art's existence prior to April 2001, as he claims, or did conspire with others to have it destroyed by the largest man-made lake of Europe. It is beyond dispute that he was responsible for its protection and that he failed completely in this duty. There is no

doubt that he could tell us a great deal about rock art management in Portugal from 1996 to 2002, but we should not expect that we will ever get to know the whole truth about the rock art of the Sabor, Guadiana, or, for that matter, the Côa valley. Dr Zilhão has presided over the world's greatest bungle in public archaeology for half a century, that much is obvious.

A few questions

Much of the present paper resembles Dr Zilhão's rhetoric of recent years, so it may be preferable to present specific questions for him to deal with in his reply. I would be most grateful to him if he could address the following specific questions:

1. He has conducted numerous excavations at the bases of Côa petroglyph panels, seeking motifs that had been covered by sediment. With the exception of the infamous Fariseu site, where the panels were covered by recent sediments, this effort was entirely unsuccessful. In all of these many excavations, why was there not a single stone tool reported that had been used in making these petroglyphs (Swartz 1997a, 1997b; cf. Bednarik 1994b)?
2. After excavating hundreds of trenches at dozens of sites, why have no faunal remains, human remains, typical Upper Palaeolithic stone tools, palynological or proper sedimentary data ever been reported?
3. Dr Zilhão makes the point that he has excavated many hearths at Côa sites. If that is so, why has no radiocarbon date ever been reported from the Côa valley (other than Watchman's and Dorn's direct dates from the art panels, and Zilhão's one sample from the Penascosa terrace of about 1000 BP that refuted his own claim that it is a Pleistocene feature)?
4. How does Dr Zilhão explain that microlithic stone tools he defines as Palaeolithic occur stratigraphically together with ceramics at all levels at Quinta da Barca and at most levels at Cardina 1 (Carvalho et al. 1996; Zilhão 1997), the two principal occupation sites he claims are Palaeolithic?
5. How does he reconcile the complete absence of any Pleistocene sediment deposit in the lower part of the valley with his claim that he has demonstrated the presence of Palaeolithic occupation sites?
6. How does he reconcile his view that all zoomorphs on the Côa are of Palaeolithic age with the determination of others that some or many of them were made with metal tools (Eastham 1999; Bednarik 1995d)?
7. Is he willing to withdraw his claim that ibex did not exist in the region during the Holocene (Wyrwoll 2000)?
8. Why does an equine motif at Fariseu which he places in the Gravettian appear to wear a bridle (Abreu and Bednarik 2000)? Is he suggesting that the Gravettians had domesticated horses (Fig. 2)?
9. How does he explain that those zoomorphs he claims are Palaeolithic are usually much less weathered and patinated than inscriptions of the 18th century at the same localities (Bednarik 1995d)?



Figure 2. Detail of equine figure, suggestive of a horse head with bridle, Fariseu petroglyph site.

10. How does he account for the complete absence of patination on all of the Fariseu petroglyphs (see his Fig. 4 as well as the above Fig. 2)?
11. If this corpus is Palaeolithic, why does it completely lack the most typical of the Palaeolithic art of south-western Europe, the so-called Palaeolithic signs?
12. Why do the Côa petroglyphs only feature species that existed in the region well into Historical times, or indeed until today?
13. Why does the distribution of Côa petroglyph sites coincide exactly with the distribution of water mills of recent centuries?
14. How does he explain that the very similar Mazouco equine motif is not of the Palaeolithic (Baptista 1983)?
15. How does he account for the almost complete lack of fluvial wear on the supposedly Palaeolithic petroglyphs where they occur within the river's flood zone, all being on exceedingly soft rock (Bednarik 1995d)?
16. How does he explain the survival of all of this rock art on schistose surfaces that hydrate and recede rapidly, i.e. at a rate of up to 10 mm per millennium (Bednarik 2001b)?
17. How does Dr Zilhão explain that there are numerous instances where supposedly Palaeolithic engraved lines dissect lichen thalli, and that the largest thalli occurring over petroglyphs are only a few centimetres in size, corresponding to an age of two or three centuries?
18. How does he account for the hundreds of equine petroglyphs on a 2-km-long stone wall near Castro (Hansen 1997)?
19. Since the petroglyph corpus of Siega Verde cannot possibly be older than Roman times (Bednarik 2000) and in the opinion of the local population is the work of recent shepherds, how does he reconcile this age of a very similar nearby rock art corpus with the age he claims for the Côa art?
20. How does he reconcile his claim that some Côa bovids resemble certain Lascaux bovids with the Holocene age suggested for the latter (Zilhão 1995; cf. Bahn 1994, 1995b)?
21. How does he now view his own role in the destruction of the scientific value of all of the Côa rock art as described by Jaffe (1996)?
22. How does he explain his role in the destruction of the rock art in the Sabor and Guadiana valleys? Does he have any retrospective regrets?

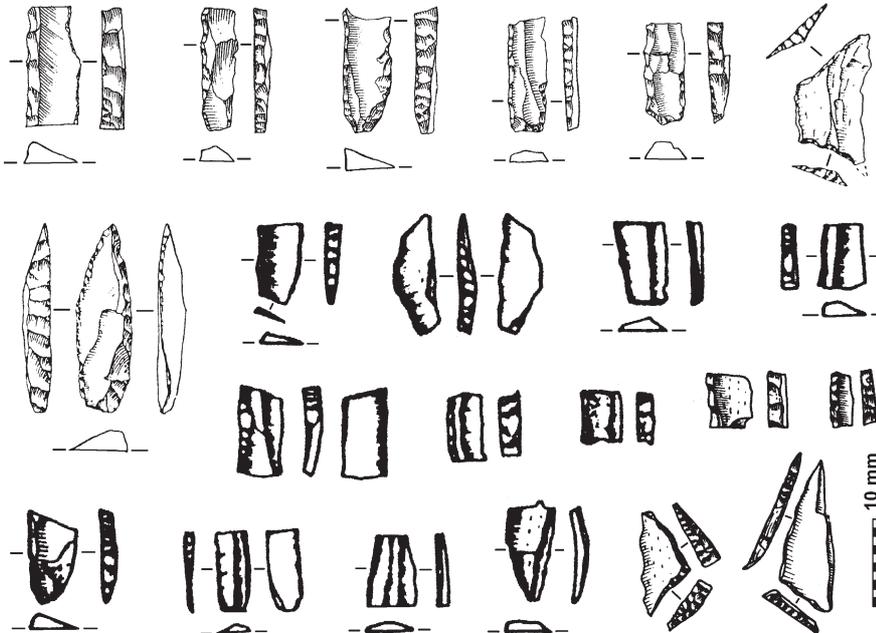


Figure 3. A selection of twenty-two stone implements excavated at Côa sites, claimed to be Palaeolithic. Most are from Cardina, the four closest to the scale are from Quinta da Barca. They resemble Neolithic assemblages of the region. (Adapted from Zilhão 1997 and Carvalho et al. 1996.)

Finally, I ask readers to reflect on the following point: would we accept a similar claim for Pleistocene antiquity of a rock art corpus anywhere else in the world, based on the same level of proof? In the Côa valley we have no occupation carbon or OSL dates, no faunal or human remains, no typical Palaeolithic stone tools, no Pleistocene sediments, no unambiguous Pleistocene human occupation evidence, no stratigraphic connection between rock art and a Pleistocene living floor, no sedimentary data, no pollen analyses, and probably no Pleistocene hearths. What has so far been presented as archaeological evidence is a very small number of mostly microlithic stone tools that resemble early Neolithic industries elsewhere in northern Portugal (Silva

1993), and which in the Côa valley were in nearly all cases found together with ceramics. To claim that they are Palaeolithic is absurd (Fig. 3), and to deduce from such flimsy evidence the age of a rock art is something we would not tolerate anywhere else. Even if a Palaeolithic occupation of the valley were demonstrated, it would still not fol-

low that any rock art present must also be of such age.

This is not the first time Portuguese archaeologists have made unsupported Pleistocene age claims for rock art. The cave of Escoural in southern Portugal contains only Middle Palaeolithic and Neolithic occupation evidence (Lejeune 1997), yet its rock art has long been claimed (and accepted) as being of the Upper Palaeolithic. So here we have a case where rock art antiquity was accepted on the basis of a lack of corresponding occupation evidence, whilst on the Côa, Zilhão tries the opposite approach. Perhaps archaeologists need to understand that *neither the presence nor the absence* of occupation horizons demonstrates the age of any rock art that happens to occur at the same vicinity. In the Côa case, they have yet to demonstrate the existence of Pleistocene occupation floors in a 40-m zone above the river (within which the rock art occurs), but with the complete absence of Pleistocene sediments in that zone that might be very difficult to do.

Robert G. Bednarik

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In accordance with standard *RAR* policy, Dr Zilhão has been asked to respond to these comments. Regrettably we have not received a reply at the time of going to press. Any response received from him will appear in the next issue of *RAR*.

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