

Data and interpretation in the Côa valley, Portugal

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Substantial agreement

In view of the controversial status of the archaeological data from Portugal's Côa valley (Bednarik, 1995, Watchman 1995; Zilhão 1995) is perhaps more constructive to explore areas of agreement than to dwell needlessly on areas of disagreement between the warring factions. The paper of Aubry et al. (2002, henceforth referred to as 'the Paper') provides an excellent basis for exploring such areas of agreement. It shows that there is much more consensus than the polemic on this subject might suggest.

For instance, the Paper affords great care to the geological circumstances of the valley, its lithology, to how and when it was formed. It agrees that in geological terms it is a very young feature. That it has been cut into a Plio-Pleistocene planation surface has been universally accepted now it seems. The Paper even shows how the valley truncates a fluvial terrace of the Middle or Lower Pleistocene (in Fig. 4, Penascosa section), which grants the valley an age lower than that of these deposits. It is also noted how Acheulian handaxes and cleavers can be found in the Pleistocene sediments high along the Douro, where they occur also in the vicinity of the Côa valley. The complete absence of such finds in the lower reaches of the valley confirms that all sediments close to valley floors are very young, and that they are mostly Holocene is also the finding

of the Paper. The mention of occasional Pleistocene pockets and sediment residues on valley slopes agrees with the discovery of a Late Pleistocene deposit previously found at Penascosa, 40 m above the present river (Zilhão et al. 1997: Fig. 3). Since the formation of the valley began, apparently during the Middle Pleistocene, erosion of the soft schists and phyllites has cut over 300 m deep into the planation surface. So 20 000 years ago the river might have been perhaps 10 m above its present level. This illustrates once again the absurdity of the cosmogenic radiation results from the valley (Phillips et al. 1997), according to which rocks at its base would have become exposed to the atmosphere hundreds of millennia ago, when the river was in fact at an elevation at least 100 m higher than today.

Even on the subject of the Fariseu site, the Paper agrees largely with those sceptical of the precipitous Palaeolithic claims. It accepts, for instance, the criticism by Abreu and Bednarik (2000) that the stratigraphy consists entirely of layers of lake sediment, alluvial and colluvial deposits, and that much of this detritus postdates the establishment of the Pocinho dam about 15 years ago. But perhaps most importantly, the Paper concedes that there is currently no form of radiometric or other objective dating evidence from the excavation of Fariseu. It states quite explicitly that TL analysis of the Fariseu samples is currently still in progress (p. 71), three years after these samples were submitted. Therefore the Paper also agrees, at least implicitly (because Fariseu is the only site of dozens excavated where rock art has been claimed to be relatable to archaeological evidence), that there is currently no evidence linking any of the rock art of the Côa valley to any of the archaeological dates so far presented.

The Paper disagrees, however, with an earlier Instituto Português de Arqueologia (IPA) report on the nature of the lithic industry found in the Fariseu excavation. Whereas Anonymous (2000) reports that the lithic sample 'is not big enough to allow a precise diagnostic of the assemblage', the Paper is much more confident: the very few lithics are now attributed to the Early Magdalenian or Proto-Solutrean. It would help us to have confidence in these pronouncements if the purported artefacts had been illustrated, in the Paper or in any other publication. The only lithics ever published from the lower Côa valley (e.g. Carvalho et al. 1996; Zilhão 1997) are a few mostly microlithic pieces from Cardina 1 and Quinta da Barca, nearly all of them backed bladelets and geometric forms such as trapezoids, most being under 15 mm long (Bednarik 2003: Fig. 3). Again the Paper is in agreement with this concern by reporting that the few lithics found at lower Côa sites are largely microlithic. None of these specimens are diagnostic of an Upper Palaeolithic period, and bearing in mind that most were found in the same horizons as decorated ceramic shards (e.g. at Quinta da Barca, cf. Zilhão 1997: Fig. 4; and Salto do Boi - Cardina 1, cf. Zilhão 1997: Fig 5) it seems reasonable to assume that they are perhaps Neolithic. Here the

Paper disagrees significantly with previous evidence published by some of its authors. At Quinta da Barca, the ceramics clearly extend down to bedrock according to the section drawings they themselves provided in the past (Zilhão et al. 1997), yet the Paper now places them in the Palaeolithic.

Some disagreement

The Paper generally fails to address the concerns of sceptics of the claims of Palaeolithic occupation evidence at the base of the Côa valley. Besides failing to present any of the claimed lithics, particularly those from Fariseu, it also provides no details of the dating claims implied in its Figure 8. Indeed, if we look at this graph closely we see that there is a line under Fariseu, implying that dates of between 10 000 and 11 000 BP were obtained from samples C3 and C4a. Yet in the text it is stated unambiguously that no dates are available from this site. So which evidence does this mark refer to? Other samples from this site are implied to be 18 000 to 19 000 years old, yet they are from the very same level, C4. In other words, the upper two thirds of the site's alluvial/colluvial series is agreed to be under 15 years old, the lower third is claimed to be tens of thousands of years old without any dating evidence. In the case of Quinta da Barca Sul the basis are apparently three TL determinations, also of 10–11 ka, but unless it is proposed that the accompanying ceramics are also of the Pleistocene these three TL 'dates' are of little consequence. Not only is the detail of much of Figure 8 perplexing, the value of the TL analyses and purported stone tools is itself questionable. The Fariseu sediment consists entirely of a series of colluviums and alluviums, lacking any definable occupation levels. It is agreed that most or all of the constituents have been transported from somewhere else, especially from upslope, so their position within the sediment is fortuitous and of little or no archaeological significance. Moreover, we know from other examples how severely misleading TL results from poorly stratified deposits tend to be. For instance the claims of up to 176 000 TL years for Holocene sediments at the Jinmium site in Australia were conclusively disproved by OSL and radiocarbon analyses, and such error sources are well understood (Fullagar et al. 1996; Roberts et al. 1998). The complete lack of any reported ¹⁴C results, not only from Fariseu but from all the dozens of Côa excavations (except a 1000-year-age from the Penascosa terrace), is particularly disconcerting, and OSL analysis might have provided more secure luminescence results (the site was sampled for OSL by Norbert Mercier, but the results are not mentioned; cf. Anonymous 2000). The truncation of Figure 8 at 10 000 BP points to yet another major concern: in view of the large number of carbon and luminescence samples processed from Côa sites since 1995, why are only about two dozen TL results summarised in this graph? It would have been appropriate to list *all* results secured, and not only those implying Pleistocene ages. Many of those shown in

Figure 8 are not related to purported archaeological finds, and almost none are even suggested to be related to any rock art. So the relevance of these incompletely presented data needs to be questioned, and until the Holocene dates are located on the section drawings and listed in tabular form, the data presented in the Paper are far too incomplete to be considered, particularly in this controversial context.

The archaeology from the Côa sites seems to be best characterised as a series of very small microlithic assemblages found either stratified with ceramic remains, or in poorly stratified, largely colluvial deposits. No radiometric dates from charcoal have been reported, and all dates the Paper presents seem to be from supposedly heated quartzite detritus. There are no identified faunal remains, no palynological analyses or other support for these dubious results. None of the stone artefacts of which illustrations have been provided display any diagnostic features one can reasonably attribute to a Palaeolithic industry. Instead these backed bladelets and tiny trapezoids match precisely the Neolithic assemblages that are so plentiful elsewhere in northern Portugal (Silva 1993).

Aubry et al. turn the customary convention of presenting empirical data and then arguing for one or the other interpretation on its head. They base their interpretation of the Côa valley's archaeology on omitting or excluding most of the crucial data (such as all Holocene dates). In proposing to demonstrate Pleistocene occupation of this Holocene valley they present no radiometric or other dates, they offer no illustrations of stone implements, report no occupation floors, faunal or human remains, pollen, sedimentary analyses or any of the other forms of documentation usually expected from Upper Palaeolithic excavation reports. Nowhere in the world would such a reluctance to depict lithics or present dates be accepted as adequate evidence for Pleistocene occupation.

Finally, Aubry et al. make no attempt to respond to the dozens of objections to a Palaeolithic age of the rock art or the occupation evidence at the base of the Côa valley. They need to respond to the evidence that most engraved motifs were made with metal implements (in one case the claim is that carbonised steel was used; Eastham 1999); that the distribution of Côa petroglyphs matches precisely the distribution of historical water mill structures; that the 'Palaeolithic' images are often much less weathered or patinated than engraved dates and inscriptions on the same or adjacent panels, that one of the horse pictures at Fariseu is shown wearing a bridle (Abreu and Bednarik 2000; Bednarik 2003: Fig. 2); that the petroglyphs within the annual flood-zone of the river bear very little or no fluvial wear; that their weathering and patina is no more than a few centuries old; that the schist hydrates and disintegrates rapidly; that all of the animals depicted in the valley occurred there in the most recent history; that there is a complete absence of the diagnostic form of Upper Palaeolithic rock art, the so-called signs; that the grooves of numerous purported

Palaeolithic motifs dissect lichen thalli and must thus be younger than these thalli; that the style of most Côa images is not Palaeolithic, but Roman or later; that the specific motifs identified as the oldest are geometric and schematised zoomorphs, not remotely resembling any art of the Upper Palaeolithic; that the Vermelhosa figures are of the Iron Age (Abreu et al. 2000); that the Mazouco figures are not Palaeolithic (Baptista 1983); that all of the nearby and very similar Siega Verde petroglyphs must postdate the Roman period and have now been shown to be all under 200 years old, dating mostly from the 20th century (Bednarik 2009); that the local villagers at Siega Verde claim that the petroglyphs were made by shepherds and 'had a good laugh when archaeologists told them that the art was Palaeolithic' (Hansen 1997); that a 4-m-high and 2-km-long stone wall near Castro, in the same area as Siega Verde, bears literally hundreds of horse pictures like those at Siega Verde and Côa; that the Lascaux late phase, with which some Côa motifs were compared to show that they must be Pleistocene (Zilhão 1995), is in fact of the Holocene (Bahn 1994, 1995). In considering just one of these objections, the archaeozoologist Thomas Wyrwoll has examined all semi-naturalistic animal images in the Côa valley and has concluded that

the idea that some of the Côa rock engravings would date to the Palaeolithic, as expressed by some Portuguese archaeologists because of the mere existence of ibex representations amongst them, is based on incorrect assumptions regarding the distributional history of this species. There is also no other zoological reason to date any piece of Côa rock art to the Palaeolithic (Wyrwoll 2000: 95, my translation).

Wyrwoll explicitly rejects Zilhão's (1995) claim that there were no ibex in the region during the Holocene, arguing that the Côa ibex figures must be of the Pleistocene. Wyrwoll points out that all the ibex-like figures in the Côa valley resemble *Capra ibex lusitanica* or *victoriae*. The Portuguese ibex, *C. i. lusitanica*, became extinct only in 1892, and not as Zilhão (1995) claims at the end of the Pleistocene. The Gredos ibex (*C. i. victoriae*) still survives in the region. The body markings depicted on one of the Côa zoomorphs, a figure from Rego da Vide, resemble those found on *C. i. victoriae* so closely that this typical Holocene sub-species rather than a Pleistocene sub-species (notably *Capra ibex pyrenaica*) is almost certainly depicted (Bednarik 2003: Fig. 2).

These issues need to be addressed by those claiming a Palaeolithic age of Côa rock art. But most of all, what we need especially from Fariseu are illustrations of the purported lithics; a complete list of all dating results, relating these to the stratigraphy; and comprehensive sedimentary data of the type usually provided by large projects such as this one.

What Aubry et al. (2002) present is a case for Palaeolithic occupation of the planation surface overlooking the valley, extending to the Acheulian, and perhaps sporadic residues at elevations well above the river. Their intensive search for Pleistocene sediments on the valley floor has been negative, apart from occasional

transported residues that may or may not contain older colluvial lithics. Hopes to find occupation floors in situ near the river are thus fading, and with them the hopes of relating such deposits to engraved rocks. The Côa research also suggests that undisturbed Neolithic deposits seem to occur above the present valley's flood-zone, but not in its current flood-zone. All sediments of any substance found on the valley floor appear to be of the late Holocene, and there are thus no Pleistocene sediments in the vicinity of the rock art sites. Bearing in mind that even the presence of preserved Palaeolithic occupation floors has little significance for the question of the rock art's age if it cannot be related stratigraphically, this means that a first precondition for dating the Côa rock art archaeologically has not been met so far. In 2003, twenty-two scientific questions were addressed to Zilhão in this journal. In the decade since he has not responded to a single one of them.

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