



# The chronology of the Aurignacian and Transitional technocomplexes. Where do we stand?

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**ABSTRACT** We present an assessment of the criticisms that have been raised against our argument that the emergence of the Aurignacian dated to no earlier than ca.36.5 kyr BP and post-dated the emergence of the so-called “Transitional” technocomplexes of Europe. In particular, evidence from the sites of El Castillo, Geissenklösterle and Grotte du Renne is discussed in the light of recently published reports. Analysis of newly published Castillo level 18 lithics confirms our diagnosis that this is by and large a Mousterian assemblage where a few Aurignacian items are evidence of mixing, not *in situ* local transition of one technocomplex into the other; this diagnosis is confirmed by a first hand study of the purported bone tools and symbolic items from this level. New radiocarbon dates and analyses from Geissenklösterle were recently used to suggest that the Aurignacian of the Swabian Alb dates back to 40 kyr BP and to support the *Kulturpumpe* model according to which cultural innovations of the regional Aurignacian and Gravettian predate similar developments in the rest of Europe. We provide a critical analysis of all the dating evidence, discuss the relevance of the “Middle Paleolithic Dating Anomaly”

hypothesis to explain inconsistencies in Geissenklösterle’s <sup>14</sup>C determinations, and conclude that the earliest Aurignacian occupation of the site cannot predate ca.36.5 kyr BP and in all likelihood took place between 35 and 33 kyr BP. This interpretation is consistent with what we know about the taphonomy and environmental context of the site and with the technology and typology of the bone and stone artifact assemblages, and conforms well to the <sup>14</sup>C dates on faunal remains modified by humans found in the Aurignacian levels. New analyses and <sup>14</sup>C dates from Bohunician and Châtelperronian sites confirm the precedence of these technocomplexes over the Aurignacian. We conclude with a word of caution on the scope of research on the origin of the Aurignacian. The amount of time required for a technical system to be invented and spread is probably too short to leave a detectable trace in the archeological record, and this transforms a search for the origin into a “Red Queen race”, valuable for expanding our horizons and gaining a better insight into earlier Upper Paleolithic technocomplexes, but of little use in attempting a social and historical understanding of this important cultural phenomenon.

## Introduction

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Since its original publication, our review of the evidence pertaining to the chronology of the Aurignacian, the Châtelperronian, and the other “Transitional” technocomplexes of Europe (Zilhão and d’Errico, 1999) has been the subject of much controversy. The symposium from which stem the different contributions assembled in this volume, organized by us in the framework of the Liège 2001 Congress of the UISPP, was conceived precisely as an opportunity to bring together colleagues conducting research on the different issues concerned by that review. Our purpose was to provide the stage for a lively and extended debate that clarified the controversy in as much as possible. The large attendance, the quality of the

thirty-two papers that were given and the intensity of the concluding discussion did indeed permit, in our view, substantial advances in the understanding of the questions involved in this extremely complex problem.

It was clear at the time, and is confirmed by the reading of the different contributions to this volume, that a significant number of participants used the main conclusions of our 1999 review as a set of testable propositions to be falsified through further empirical research. Much the same can be said of a number of studies published over the last four years (e.g., Hublin, 2000; Richter et al., 2000; Kozłowski and Otte, 2000; Bolus and Conard, 2001; Cabrera et al., 2001; David et al., 2001; Conard and Bolus, 2003). This is exactly what we intended, and it also explains to a large extent the fact that several of the papers assembled here focus on taking issue with particular aspects of our original assessment of the evidence. As a result of all this work, some empirical issues have been resolved, and the arguments presented by the different sides involved in this new “*bataille aurignacienne*”<sup>1</sup> have been clarified; many points of contention remain, however, while new ones were added.

We feel justified, therefore, in concluding this volume with an assessment of the criticisms and objections that have been raised against our original argument. The distinct empirical components of that argument will be dealt with on a one by one basis. At the same time, we will try to offer some thoughts on the logical and methodological problems that, in our view, help to explain the differences of opinion as much as, if not more than the empirical evidence itself. It will be recalled that the main conclusion of our 1999 review was that the emergence of the Aurignacian dated to no earlier than ca.36.5 radiocarbon years BP, and, therefore, that it post-dated the emergence of the first Upper Paleolithic technocomplexes of Europe and Asia. Contra the opinion of some, we believe that the data amassed since 1999 overwhelmingly vindicate our conclusions on the age of the Aurignacian and, hence, impose the acceptance of its different archeological corollaries.

## Who made what?

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Part of the reason why this problem is so difficult relates to the fact that it is deeply intertwined with the issue of modern human emergence. Models of a biologically-based superiority of moderns, defined as a separate species, dominated the field for most of the 1980s and 1990s, and are not yet completely extinct (e.g., Klein, 2003). In such models, the measure of the inferiority of the Neandertals was their apparent lack of language and symbolic thinking and, hence, their incapacity to become Upper Paleolithic (e.g., Stringer and Gamble, 1993; Noble and Davidson, 1996; Tattersall, 1995). The Aurignacian, in contrast, was considered to be the archeological proxy for the spread into Eurasia, triggered by the emergence of both modern anatomy and the Upper Paleolithic — “The Human Revolution” (e.g., Mellars and Stringer, 1989; Mellars, 1996; Noble and Davidson, 1991; Mithen, 1996; Bar-Yosef, 1998). Since the “transitional” technocomplexes of the European Upper Paleolithic were probably, if not demonstrably (in the case of the Châtelperronian), made by Neandertals, our conclusion that the Aurignacian post-dated them challenged the definition of the “Upper Paleolithic” as an integrated package of cultural, behavioral and biological innovation. If nothing else because it clashed head on with prevailing opinion, it was to be expected that this conclusion would not be widely accepted without significant resistance.

One aspect of that prevailing view that we did not, and do not, question is the association of the Aurignacian with modern humans. Such an association is widely accepted for

the later Aurignacian, and is plausible, even if so far undemonstrated, for the earlier Aurignacian (cf., for an extended discussion of this issue, Zilhão and Trinkaus, 2001). The results of recent research, much of it reported in different contributions to this volume, would seem to bring a demonstration closer to us. A few words of caution, however, are needed.

The modern human mandible with archaic features reported by Trinkaus et al. (2003) from the Oase cave, in Romania, dated to ca.34-36 ka BP, is at present the earliest secure evidence for anatomical modernity in Europe; its cultural-archaeological context and background, however, remain unknown. Svoboda et al. (2002) and Svoboda (this volume) report radiocarbon dates for Mladeč I obtained on samples extracted from remnant stalagmitic crusts adhering to the cave walls. Those remnants are inferred to be stratigraphically equivalent to the flowstone which originally capped the deposits containing the early modern human fossils (some of which are also variably encrusted). Together, those dates would suggest an age of ca.34-35 kyr BP, or slightly earlier, for the human remains, which are clearly associated with typical Aurignacian bone tools. However, it is well known that stalagmite is not a very reliable material for <sup>14</sup>C dating (Rutter and Catto, 1995; Schwarcz and Rink, 2001), and, therefore, as Svoboda himself points out, these results need independent corroboration through direct dating of the human bone itself.

Bolus (this volume) states that “with the new Vogelherd dates ... the human bones from layer V are the oldest fossils of modern *Homo sapiens sapiens* clearly associated with an Aurignacian assemblage in Europe”. Of the five AMS dates on samples of single bones with anthropic marks reported by Conard and Bolus (2003) for that layer, four are in the range of ca.32-33 kyr BP, in accordance with the “Typical Aurignacian” composition of the artifact assemblage, and one is of ca.36 kyr BP, which may be taken to suggest that an earlier Aurignacian component is also present in that assemblage. The evidence, however, does not allow us to say with which of those occupations the human remains were originally associated.

In fact, until the latter are directly dated, we cannot even be reasonably secure that they are indeed Aurignacian. Conard and Bolus (2003, p. 337), note that

“A degree of mixing between strata is not surprising given that the deposits of the cave were excavated over a period of less than three months in the summer and fall of 1931 before careful excavation methods and detailed studies of site formation processes were common. AMS dates of single bones from layer IV have also yielded dates of Magdalenian age, indicating that Riek’s excavation techniques did not succeed at rigorously separating the archaeological units. Several dates, including one new AMS date of ca. 26 ka BP, suggest that a Gravettian component is also present at Vogelherd.”

In this context, the possibility that the human material from Vogelherd’s layer V relates instead to intrusive burials from Gravettian or Magdalenian occupations of the site cannot be entirely ruled out at present. This is all the more so if we keep in mind the results reported by Smith et al. (1999), Svoboda (this volume) and Terberger and Street (this volume) for the direct dating of human remains of presumed early Upper Paleolithic age coming from geological contexts or from old excavations of a number of localities in Croatia, Moravia and Germany: all (Velika Pećina, Zlatý Kůň, Svitávka, Hahnöfersand, Paderborn-Sande) turned out to be significantly later.

Also, recent research (Bordes 2002a, 2002b, this volume) revealed that, in the Franco-Cantabrian region, Aurignacian assemblages stratigraphically underlying “classical” Auri-

gnacian I layers present technological features suggesting some degree of continuity with the Châtelperronian. Those assemblages include tool-types, such as the marginally retouched points from level K of Le Piage, that are not a part of the traditional Aurignacian tool-kit and have some similarities with Châtelperronian points. According to Bordes, these similarities may indicate a cultural influence of the Châtelperronian on the earliest Aurignacian of the region, whose authors remain completely unknown to us.

### **How early is the earliest Aurignacian?**

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Throughout the 1990s, the hypothesis that the earliest Aurignacian dated to ca.38-40 kyr BP, if not more, in central and western Europe, and that its first appearance predated the emergence of the Châtelperronian and other Transitional technocomplexes, relied almost entirely on two lines of evidence: the interstratifications at Roc de Combe, Le Piage and El Pendo; and the radiocarbon dates for l'Arbreda and El Castillo, in Spain, and Geissenklösterle, in Germany. In our 1999 paper, we rejected that evidence on a number of grounds. That no "interstratifications" exist at Roc de Combe and Le Piage, and that no Aurignacian earlier than the Châtelperronian exists at these sites is now demonstrated, exhaustively and we hope definitively, by the brilliant and methodologically innovative taphonomic work of J.-G. Bordes (2002a, 2002b, this volume). The evidence from El Pendo had already been conclusively dealt with by Hoyos and Laville (1982), and recent work at the site (Montes et al., 2002) confirmed that the stratigraphic sequence upon which the interstratification had been suggested corresponds entirely to an accumulation of eroded deposits, each containing a highly diverse mix of bones and artifacts derived from the many different occupations that took place at the site over the last 100 000 years. Where the radiometric data are concerned, we provide below an update of our arguments and discuss the new evidence published for l'Arbreda, El Castillo and Geissenklösterle.

#### *The age of the L'Arbreda sequence*

As in the case of El Castillo, we did not question the physical exactitude of the <sup>14</sup>C results obtained for l'Arbreda, and, therefore, that the deposition of the sediments in which they were collected took place during the time interval bracketed by those results. In our 1999 review, we simply noted that, because of the way the site was excavated (by artificial horizontal spits that could not account for the natural inclination of the strata), and because of the stratigraphic inversion of some results, picking only the earliest of them as those really associated with the first Aurignacian occupation of the site was an erroneous procedure. We concluded (p. 25):

"It is quite clear that L'Arbreda contains Mousterian, Châtelperronian and Aurignacian occupations, that the index fossils of the latter two were found in the expected stratigraphic order and that the AMS radiocarbon results obtained from individual samples of bone and charcoal contained in the deposit indicate that the sequence dates between ca.40 000 and ca.34 000 BP. On present evidence, however, the exact place of the Aurignacian in this chronological interval cannot be established beyond reasonable doubt and in all likelihood will not be known until the diagnostic bone points are themselves directly dated".

No such direct dating has yet been undertaken, so the issue remains unresolved. Harrold et al. (this volume), however, contend that their magnetic susceptibility results for l'Arbreda and El Castillo "support the implication of their radiocarbon dates that these sites indeed see early occurrences of the Aurignacian, dating to the Hengelo interstadial period". We disagree, because what is at stake at these sites is not the broad chronological placement of the sequence of levels (G, H and I, at l'Arbreda; 18 at El Castillo), which may well include the period of the Hengelo interstadial, but the putative association with the Aurignacian of individual samples of charcoal collected in those levels.

We must also point out that Harrold et al.'s results do not represent an independent assessment of the age of those deposits. In fact, their reasoning is circular, because the pan-European magnetic susceptibility (MS) chronology they propose is based on the assumption that the radiocarbon dates for the stratigraphic sequences used in the correlation are valid; therefore, the MS data cannot be used as supporting evidence for a validity that was assumed in the first place. As the authors themselves state (this volume):

"It is crucial to remember when using these methods, that while MSEC in combination with the graphic correlation method can be a powerful correlation tool, it cannot stand alone. Independently derived age estimates are needed for the sections sampled, so that correlation among sites depends not only on similar patterns of climate change, but also on chronometric dates".

### *The nature of El Castillo's level 18*

Cabrera et al. (2001) have now published the first description in some detail of the artifact assemblages associated with the extensive series of <sup>14</sup>C results presumably dating the Aurignacian occupation of the site published earlier on by Cabrera and Bischoff (1990) and Cabrera et al. (1993, 1996). We believe that this new information confirms our diagnosis: the 1 m thick "Aurignacian level" of Obermaier's excavations at El Castillo is a palimpsest containing at least two components, Mousterian and Aurignacian; and, in the area of Cabrera's new excavations, the rarity of diagnostic Aurignacian items suggests that the ca.38-40 kyr BP dates for level 18 are in fact related to the occupation of the site during the Mousterian.

### *The lithics*

The caption to Cabrera et al.'s (2001) Fig. 18 (reproduced here as our Fig. 1) reads "Level 18c: 1-10, bladelets from 1980 excavations, 11-15, prismatic cores from Obermaier excavations". This illustration graphically sums up our point. The morphology and technical features of the ten "bladelets" from the new excavations are not diagnostic: five simply correspond to chippage or small flake fragments, four are cortical over 50% or more of the dorsal face, and only one might conceivably be related to the reduction of a carinated core. Any production scheme, including those that were current in the Lower and the Middle Paleolithic, can generate such kinds of items; in contrast, the cores from Obermaier's excavations illustrated alongside are fine examples of prismatic blade/bladelet reduction processes.

The majority of the items illustrated elsewhere in Cabrera et al. (2001) are perfectly compatible with the diagnosis that this is by and large a Mousterian assemblage. Some (end-scrapers, burins, borers) are Upper Paleolithic-like, but these types are common occur-

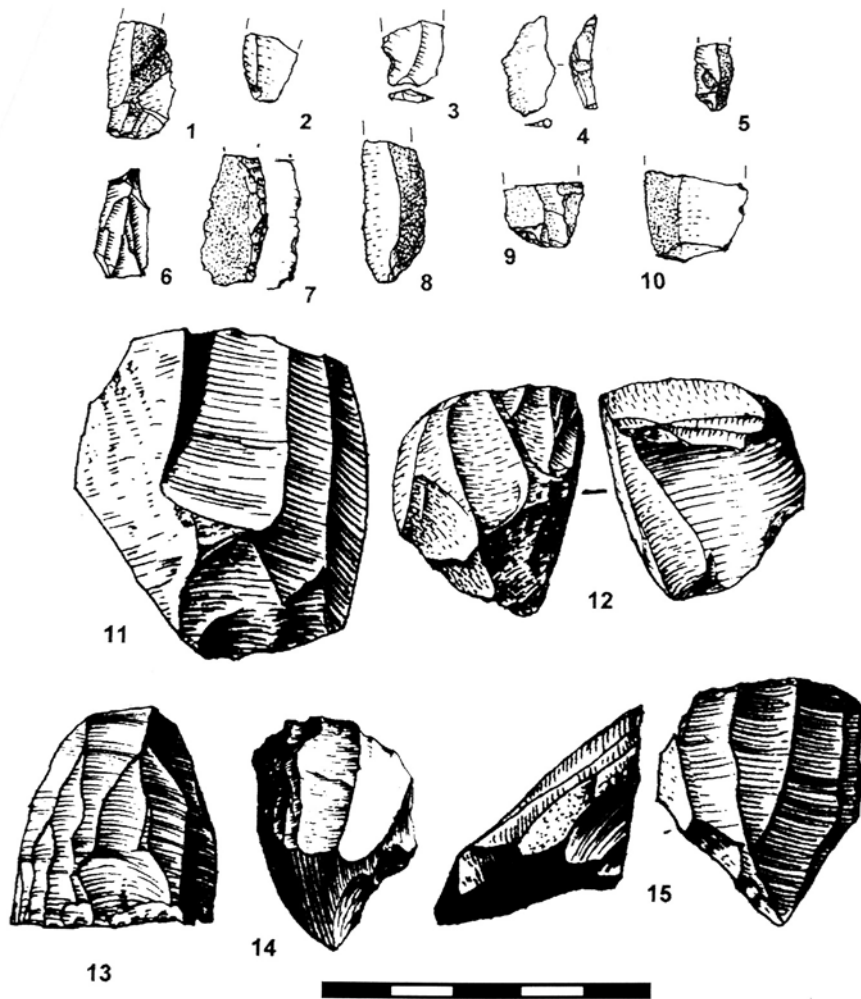


FIG. 1 – The “dual nature” of El Castillo level 18: 1-10. “bladelets” from Cabrera’s excavations; 11-15. Prismatic cores from Obermaier excavations (after Cabrera et al., 2001, Fig. 18, p. 525).

rences, albeit in small numbers, in Middle Paleolithic contexts (after all, Bordes’ type-list for the Middle Paleolithic does include an “Upper Paleolithic group” of retouched tools). Moreover, in numerous cases, the inclusion of many of the illustrated pieces in the categories to which they are assigned is debatable. Most “retouched blades”, for instance, seem to be, in fact, retouched flakes, and there are problems with the “endscrapers” too (e.g., Fig. 12, nos. 4 and 8; they are classified as endscrapers, but the former displays an irregular, continuous retouch, not a “front”, and the second is clearly a core). But, even if all such assignments are accepted, the fact remains that Upper Paleolithic-like items are significantly less numerous than the sidescrapers and other “archaic types”; the latter outnumber “endscrapers” 2:1 in Cabrera et al.’s counts for both sublevels 18c and 18b. On the other hand, given that, in this part of the site, “level 18” is located directly under a huge collapse, significant post-depositional mixing is to be expected in the first place, and may also account for the presence in the artifact assemblage of a few truly Aurignacian lithic items, as suggested in our 1999 critical assessment of the site’s stratigraphic sequence.

The technological observations provided by Cabrera et al. (2001, p. 516-517) strengthen these conclusions:

“The production is carried out following two debitage production schemes: a flake production scheme and a bladelet (rather than blade) production scheme. (...) The mentioned production schemes were developed on highly diverse raw-materials and following modalities similar to those of the preceding culture, the Mousterian from layer 20”.

Flakes are produced following centripetal strategies ordinary for the Middle Paleolithic. We reviewed above the evidence (or lack thereof) for bladelet production. It must also be pointed out, in any case, that, elsewhere in western Europe (for instance at Champ Grand, southeast France — Slimak, 1999 — and in levels 10-14 of Gruta da Oliveira, in the Almonda karstic system, Portugal — Zilhão, 2001b), the true production of bladelets from prismatic cores and carinated elements is documented as an extremely minority, even if well defined component of late Mousterian assemblages otherwise characterized by traditional levallois and discoidal flake production schemes. It may well be that such a component is also present in the late Mousterian of El Castillo, as is suggested by the descriptions provided in Cabrera et al. (2000) for levels 20 and 21 of the site — which, moreover, date to the same time range as those Oliveira levels, i.e., ca.39-46 kyr BP. Based on those descriptions, level 18 of the new excavations (for which the average of the ten <sup>14</sup>C dates available is 39 360 BP, i.e., also in that time range) does not seem to differ, technologically, from levels 20 and 21, as is also acknowledged by Cabrera et al. (2001).

#### *The organic products*

Therefore, we continue to see no reason, on the basis of the lithic assemblages from Cabrera’s excavations, to call levels 21-20 “Mousterian” and level 18 “Aurignacian”. Does the

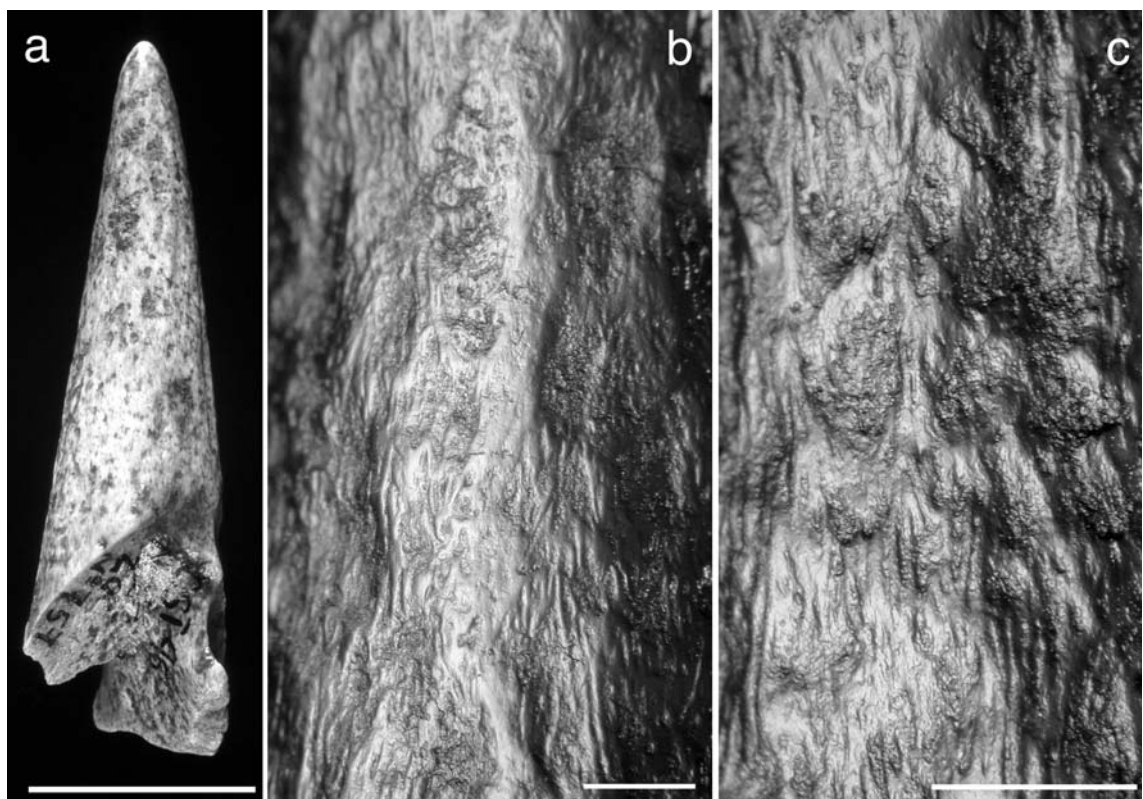


FIG. 2 – Antler tip from Castillo level 20 (a: scale = 1 cm; b-c: scale = 1 mm). The etched appearance of the bone fibers (b-c) reveals no traces of anthropic modification (photos F. d’Errico).

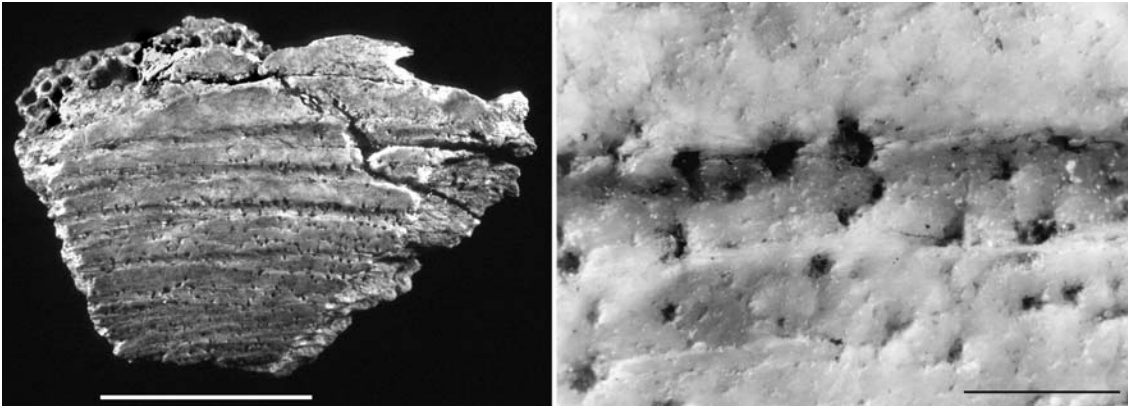


FIG. 3 – Fragment of horn core from Castillo level 18 bearing parallel vascular grooves mimicking anthropic engravings. Left: scale = 1 cm; right: scale = 1 mm (photos F. d’Errico).

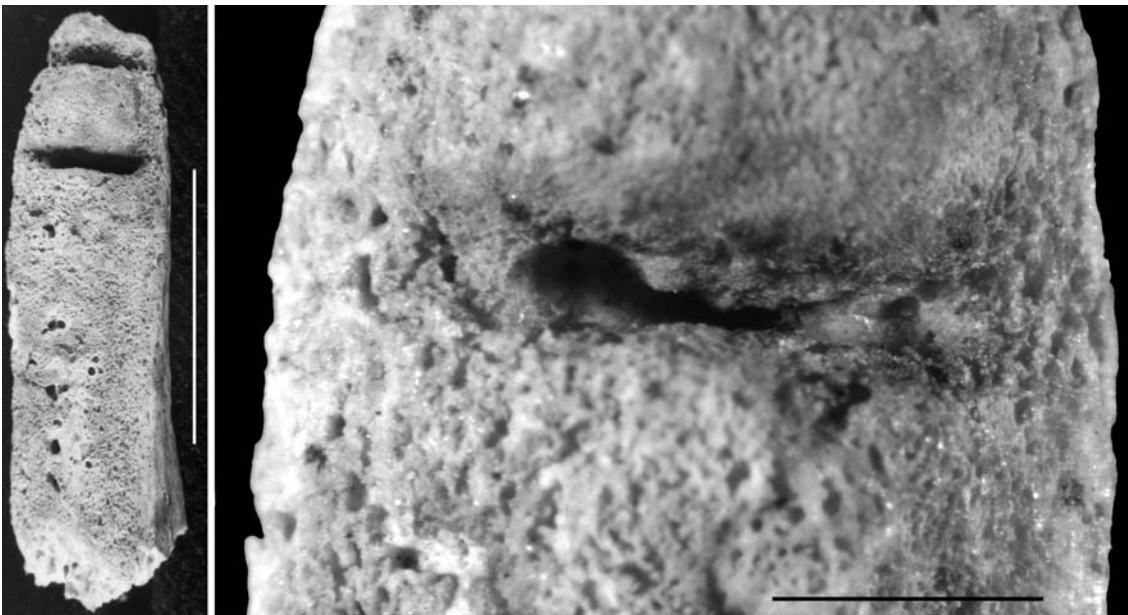


FIG. 4 – Spongy bone fragment from Castillo level 18 with natural elongated vacuums mimicking tool-made notches. Left: scale = 1 cm; right: scale = 1 mm (photos F. d’Errico).

latter contain bone tools, ornaments, or symbolic objects that might contradict this assertion? Cabrera et al. (2001) argue that such is the case, and substantiate their attribution of level 18 to the Aurignacian on the basis of a few bone tools which they consider “highly typical” (2001, p. 525), and on the basis of items of mobiliary art claimed to be similar to those found in Obermaier’s excavations. Before Cabrera and collaborators decided to publish their own observations independently, one of us was involved in the study of a part of this material and of a number of other specimens (d’Errico et al., 1998a), including the single engraved piece found by Obermaier in the Aurignacian level of Castillo kept in the Santander Museum. Our own hands-on analysis of this material, and a close evaluation of the evidence provided by Cabrera et al. for the pieces we have not directly examined, provides no support for their attribution of this assemblage to the Aurignacian.

D’Errico et al. (1998a) examined five bone objects, one from level 20 and four from level 18. Before analysis, the piece from level 20, a pointed antler fragment (Fig. 2), was seen

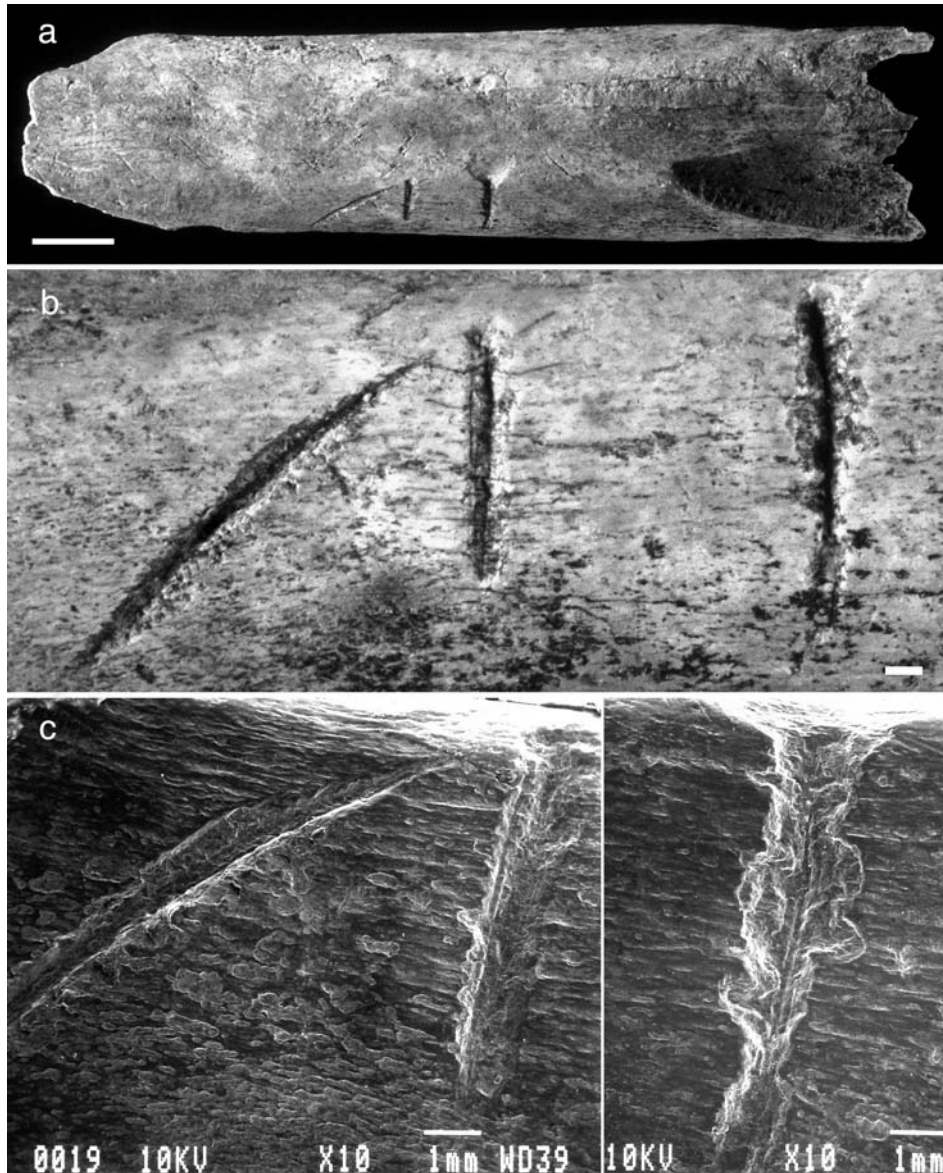


FIG. 5 – Diaphyseal fragment of red deer metapodial with three closely spaced incisions from Castillo level 18 (b-c: close-up views of the incisions). Notice their irregular outline, due to microflaking of outer circumferential lamellae, and differences in the sections of the marks. a: scale = 1 cm; b-c: scale = 1 mm (photos F. d’Errico).

as a possible broken spear tip. The occurrence of this tool in level 20 was considered important since it supported the model of a gradual emergence of Upper Paleolithic features within the local Mousterian. On analysis, this item was shown to be the tip of a deer antler with no traces of manufacture or use. The morphology of the fracture suggests that the break occurred when the antler was still relatively fresh. Subsequently, the piece underwent a chemical attack responsible for its slightly eroded surface and superficial micropits. Pseudo-points very similar to this specimen in size and microscopic appearance are found in Pleistocene hyena dens (Villa and d’Errico, 2001), and probably result from breakage of antler tips by chewing carnivores. Two putative symbolic objects from level 18 also appear to be the result of natural phenomena. One is probably a fragment of an horn core (Fig. 3) bearing parallel grooves that, on close examination, were identified as blood-vessel impressions, as demonstrated by their “U” shaped section and the presence of funnel-like open-

ings corresponding to the entrance of capillaries (d’Errico and Villa, 1997). The other (Fig. 4) is an unidentified bone fragment made of loose spongy bone carrying two natural vacuums which may be misinterpreted as anthropic notches.

The two remaining bone pieces from level 18 that we studied do show evidence of anthropic modifications. One (Fig. 5) is a shaft fragment from a red deer metapodial figured in Cabrera et al., 1993 (p. 93, fig. 11), but not mentioned in Cabrera et al., 2001. Each of the three incisions on the middle of the piece was produced by a single displacement of a cutting stone with a thick edge. SEM inspection reveals differences in the internal morphology of the marks, indicating that different tools or, more probably, different segments of the same cutting edge were responsible for the production of the marks. Their irregular outline, due to continuous microflaking of outer circumferential lamellae, suggests that these marks were inflicted on an already altered bone, well after the death of the animal. This is the only feature which might suggest a non-functional explanation.

The last object we examined (Fig. 6) is a quadrangular long bone shaft fragment described by Cabrera et al. (2001, p. 526-527) as “... the distal end of a wedge showing a rhythmic series of horizontal traits (*“marques de chasse”*) similar to those found in classical Aurignacian levels...”. We see no obvious similarity between the pattern on this piece and that on the specimen from Obermaier’s excavation (Fig. 7), and this is confirmed by technical analysis. Optical and SEM inspection of the marks on the piece from Cabrera’s excavations (Fig. 6) reveals that each is composed by a set of thin, close parallel or intersecting, superficial incisions. Similar sets are produced experimentally when an irregular, sharp cutting edge is used to mark a fresh bone with a single motion or with two strokes inflicted

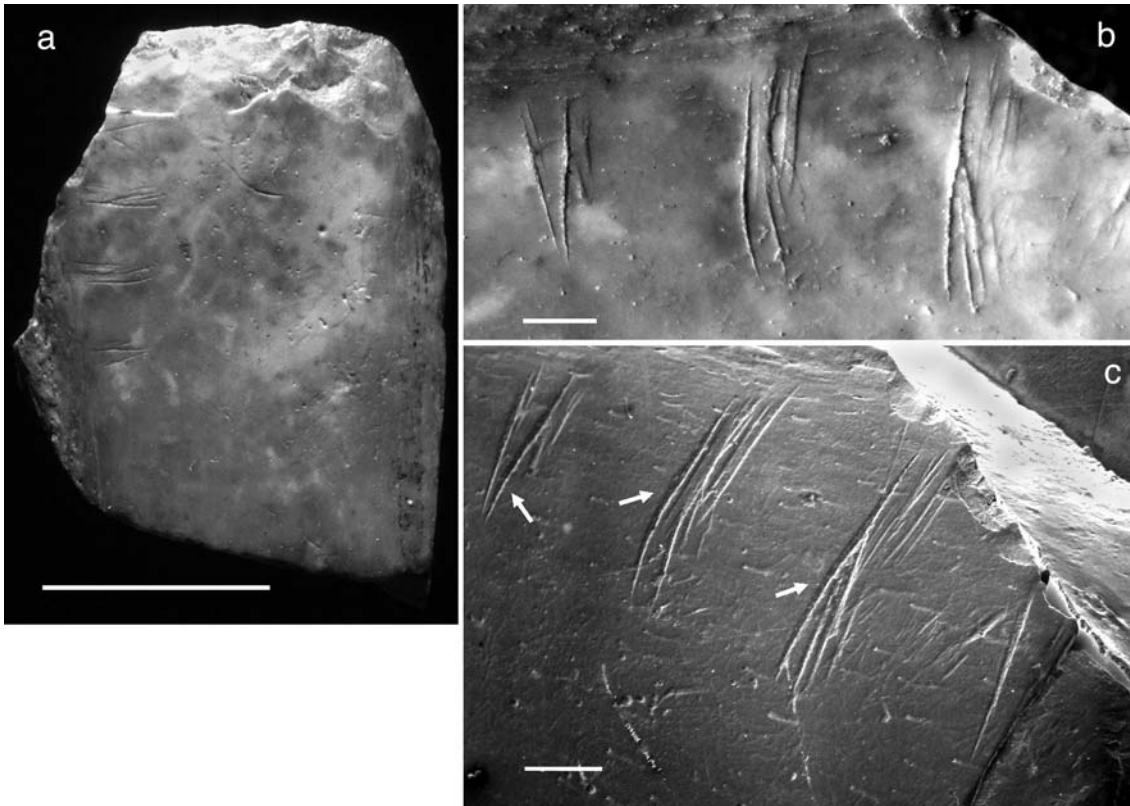


FIG. 6 – Bone fragment with four sets of closely spaced incisions from Castillo level 18 (b-c: close up views revealing the composite nature of the marks). The occurrence of similar incisions (arrows) in each set demonstrate the use of the same cutting edge; a: scale = 1 cm; b-c: scale = 1 mm (photos F. d’Errico).

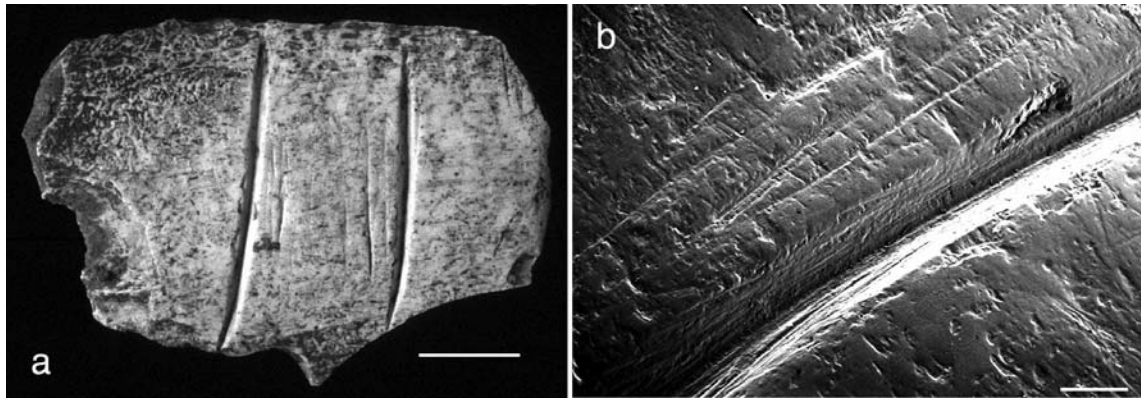


FIG. 7— Engraved bone fragment from the Aurignacian levels of El Castillo (Obermaier's excavations, Santander Museum) bearing two deep notches made with a straight edge blade (b: SEM micrograph with a detail of striations close to the main groove produced by the accidental exit of the tool during the grooving process). Left: scale = 1 cm; right: scale = 1 mm (photos F. d'Errico).

more or less in the same location (the irregularity of the edge creates multiple points of contact with the bone surface, hence, with motion, the pattern of closely grouped incisions). This procedure is very different from that used to produce the marks on the object from Obermaier's excavations (Fig. 7). The two deep long notches on this shaft fragment were produced by the repeated to-and-fro movement of a robust, straight, unretouched blade. The presence at some distance from the main grooves of striations produced by the accidental exit of the tool during the sawing process indicates a vigorous motion which has little to do with that observed on the piece from Cabrera's excavations.

The grooves in the object from Obermaier's excavations were produced by a straight cutting edge, a type of tool quite uncommon in the assemblage from level 18 of Cabrera's excavations. Moreover, those grooves do not seem to be the simple result of butchering activities — they are at least very uncommon occurrences in Lower and Middle Paleolithic sites. The marks on the object described by Cabrera et al. (2001) result from the same motions made to process carcasses and are virtually indistinguishable from marks made in the framework of such activities; the only feature possibly suggesting their symbolic nature, the equidistance between marks, also occurs when filleting (Defleur et al., 1999). Filleting is generally made in just one session and with the same tool, and SEM analysis of the object shows that at least three (indicated by the arrows in Fig. 6), and probably all four groups of marks were made by the same tool and in just one session. This is indicated by the identical striations, generated by the same zone of the cutting edge, observed in each set of marks. In sum, it is difficult at present to rule out the possibility that the marks on this object are the simple by-product of subsistence activities.

Four objects that we were unable to examine are described in Cabrera et al. (2001) and claimed to represent further support for the attribution of level 18 to the Aurignacian. Reproduced here as Fig. 8, they are: a "fragment of spear point made of red deer antler"; an "awl" also made of red deer antler; a fragment of hyoid bone with "a figure of an animal engraved on it" (2001, p. 527); and a fragment of a sandstone slab with some deep incisions. Judging from the photos, the object interpreted as a spear point appears to be a heavily eroded fragment of antler showing a number of longitudinally oriented vascular openings but no traces of anthropic modification. In size, shape and surface modification, this object is virtually identical, as argued above for the pseudo-point in level 20 (Fig. 2), to antler fragments modified by natural processes and, in particular, to antler tips digested by large car-

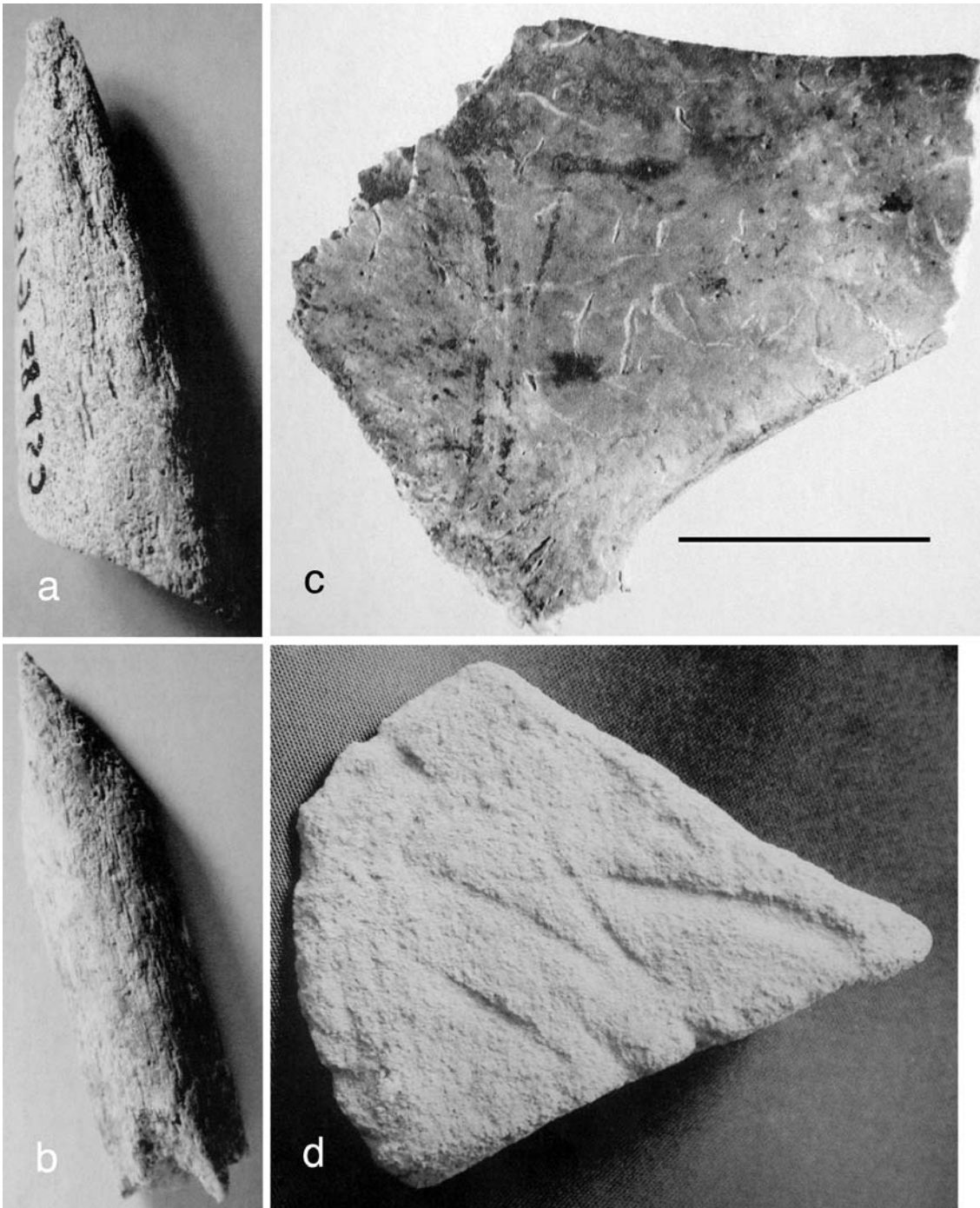


FIG. 8 – Objects from Castillo level 18 interpreted as a broken spear point (a), an awl (b), an engraved animal depiction on a hyoid bone (c), and an engraved slab (d). Scale in (c) probably 1 cm, not provided for the others (modified after Cabrera et al., 2001, p. 527-528, Figs. 21-24).

nivores (Villa and d’Errico, 2001, p. 101). The specimen interpreted as an awl bears no visible anthropic modification either, and its morphology may well also be the result of natural processes. The contrary would be surprising, since red deer antler is too soft to manufacture awls and we do not know of a single awl made on this raw material from any Upper Palaeolithic site (for the Aurignacian, cf. also Liolios, 1992). We see nothing in the way of an engraved animal depiction on the bone whose photo Cabrera et al. publish, just grooves

probably made by roots and elongated patches of dark staining. Is this staining anthropic and, if so, are the two vertical discontinuous patches indicating a leg and the horizontal one a belly? Difficult to say with the evidence at hand. We must note, however, that if Cabrera et al. are correct, this would be one of the smaller animal representations of the Upper Palaeolithic, the leg of the “deer” being only 1 cm in length. Finally, the claim that the few wide grooves in the sandstone slab represent decoration is, in our opinion, unsupported. These grooves may well be anthropic, but we know that such kinds of objects bearing traces of use in the form of grooves are known in Mousterian contexts (De Beaune, 2003); this object does not attribute level 18 to the Aurignacian.

In sum, the osseous objects from the new excavations that we have directly analyzed are either the result of natural processes or, when anthropogenic, provide no support for the attribution of level 18 to the Aurignacian. Objects with marks similar to those from level 18 are known in Mousterian contexts (for instance, at Morin, Isturitz, Noisetier, Ermitage, Ferrassie, Marillac, Suard, Vaufray...). As for the three new bone objects presented by Cabrera et al. (2001), and considering the controversial nature of level 18 and the unconvincing evidence they provide, their artifactual nature cannot be accepted until publication of a microscopic analysis of those objects validates the diagnosis.

#### *An issue of definition?*

Perhaps because they acknowledge the problems inherent in their first presentations of the material from El Castillo, Cabrera et al. (2001, p. 529-530) no longer consider the assemblage from level 18 as equivalent to the true Aurignacian. Instead, they now call it “Transitional Aurignacian”, and they consider it to be the equivalent of the different pre-Aurignacian “Transitional” assemblages of Europe:

“... the industry reveals mosaic traits in relation with the Mousterian from the technological point of view, but comprises a sufficient percentage of lithic materials characteristic of the Aurignacian to allow its placement in a stage of transition towards the archaic Aurignacian. For this reason we call it Transitional Aurignacian.” And: “Thus, to the existence of transitional industries such as the Châtelperronian, the Uluzzian, the Bachokirian, etc., we must now add the Transitional Aurignacian of Castillo 18b- and 18c-type”.

We still believe that the most parsimonious explanation for the majority of the Aurignacian elements in the assemblage from new level 18 is post-depositional mixing, and we reassert that we find Fig. 18 of Cabrera et al. (2001) (reproduced here as Fig. 1) an excellent graphic illustration of the dual nature of that level. Rigaud (1996) and Lucas et al. (this volume) pointed out that “Châtelperronian assemblages rich in lithics of Mousterian manufacture were systematically superposed stratigraphically on a Mousterian layer” and that “Châtelperronian levels poor in Mousterian artifact forms were either lacking Mousterian levels below or isolated from lower Mousterian layers by other Châtelperronian units rich in Mousterian types”. This coincidence led them to conclude that “the stratigraphic integrity of many Châtelperronian assemblages” had to be called into question, especially (but not exclusively, as they discussed for Saint-Césaire and Grotte XVI) those from old excavations. It is clear, therefore, that our interpretation of El Castillo is not a case of special pleading but, instead, conforms to a widespread situation in many cave and rockshelter sites (cf. also Bordes, 2002, this volume). Much as in those French “Châtelperronian assemblages rich in lithics of Mousterian manu-

facture”, the assemblage recovered in level 18 of El Castillo — Cabrera et al.’s “Aurignacian rich in Mousterian types” or “Transitional Aurignacian” — in all likelihood corresponds in fact to the mix of a few Aurignacian elements in an otherwise Mousterian context.

Even if Cabrera et al.’s (2001) reinterpretation of level 18 as “transitional” is eventually vindicated by future research, we would continue to argue that their choice of name was unfortunate and confusing. Maintaining the designation “Aurignacian” (even if now qualified) for that assemblage amounts to assuming that its subsequent (undemonstrated) “evolution” to the Aurignacian was somehow predestined. In fact, it is a good example of the traps involved in the use of the word “transition” that are eloquently dealt with elsewhere in this volume by Kuhn, with whom we are in complete agreement. Common ground on our understanding of the significance of El Castillo’s level 18 would be much easier to achieve if the excavators had avoided loading the name they chose for the assemblage contained therein with the meaning implied in the use of the word Aurignacian. After all, why not call it something else equally compatible with the logic of their argument, like, for instance, “Evolved Mousterian”, or “Transitional Mousterian”?

### *The Geissenklösterle’s radiocarbon dating record*

In the most recent overall presentation of the data from this site, Conard and Bolus (2003, p. 360) conclude that the chronometric results available “suggest that the Aurignacian of the Swabian Jura dates back to 40 ka BP”. In order to support their conclusion, those authors present new stratigraphic and taphonomic data, whereas Conard et al. (this volume) provide additional micromorphological evidence. Taken together, this evidence would warrant the integrity of the site’s stratigraphic sequence and refute the claims that, according to these authors, Zilhão and d’Errico (1999) and Zilhão (2001a) would have made in previous assessments of the site’s sequence: “that considerable mixing has occurred between horizons II and III” (Conard and Bolus, 2003, p. 350); “that the assemblage from layer III is the result of contamination from the overlying upper Aurignacian deposit” (Conard and Bolus, 2003, p. 360). Bolus (this volume) also states that the information upon which such assessments were sometimes based was simply “incorrect”.

These statements are in several respects at odds with the intellectual history of the issue. The information we used is not incorrect, and the claims that considerable mixing occurred across the Aurignacian levels of Geissenklösterle are not our own but the original excavator’s, Joachim Hahn. We simply used his information and echoed his claims as published in the site’s monographic publication (Hahn, 1988). Based on that, we went on to point out: (1) that, in a cave sequence with a very low sedimentation rate and where the fauna is dominated by cave bear, a degree of post-depositional disturbance is to be expected in the first place; (2) that the typology of horizons II and III published by Hahn was based on a post-excavation construct whose validity could not be independently assessed at the time because the exact stratigraphic provenance of the different items was not reported; (3) that the dated samples could come from bones accumulated by both carnivores and humans; (4) that there was no reason to restrict to only two (those corresponding to reconstructed horizons II and III) the number of moments when the cave was used; (5) that, in fact, the pattern of radiocarbon dates indicated at least three different episodes of human use; (6) that the cultural affinities with the Typical Aurignacian of the latest of such episodes (reconstructed horizon II) were clear; (7) that the cultural affinities of the earlier were clearly not with the Proto-Aurignacian as defined in l’Arbreda, Fumane or Isturitz; (8) that reconstructed horizon III was clearly

Upper Paleolithic, given the presence of bone tools and ornaments and the technology based on blade production, but its assignment to the Aurignacian was questionable because it relied solely on the presence of a few carinated pieces whose exact provenance was not reported and could conceivably be derived from overlying deposits; and, (9) that even if these carinated pieces were eventually shown to be *in situ* and the stratigraphic integrity of horizon III eventually accepted, such pieces were not sufficient to warrant an attribution to the Aurignacian because similar items were reported from pre-Aurignacian “Transitional” complexes of central Europe such as the Szeletian and the Bohunician.

Were our doubts and questions unreasonable? Since Conard and Bolus (2003) rely on the research carried out by Liolios and Teyssandier (this volume), the opinion of the latter should carry some weight in answering this question. Let us hear them: “There is therefore good cause to call into doubt the integrity of reconstructed level III and to question the origin of the supposedly related carinated/nosed pieces (Zilhão and d’Errico, 1999): do they really belong to this level or were they moved down by turbation from reconstructed level II? The point we want to make (...) is that, in spite of explicitly acknowledged disturbance, two specifically patterned assemblages can be distinguished”. Moreover, Liolios and Teyssandier point out that their results, which finally solved the issue, could only be achieved because they were able to use all the artifacts recovered up to 1991 for their study, whereas Hahn’s monograph had taken into account only the results of excavation until 1984. This simple fact substantially changed the background against which we wrote our 1999 paper. We therefore reject Bolus’s claim that we used “incorrect information”; the facts are that, at the time, the “correct” (i.e., currently available) information simply did not exist.

We believe that part of the problem is that Conard and Bolus’s perception of our position is related to the still widespread feeling among archeologists that pointing out the limits to the interpretation of a site caused by formation and disturbance processes somehow implicitly amounts to detracting from that site’s scientific value. We believe the opposite is true: all archeological contexts are post-depositionally disturbed to a greater or lesser extent, so the better we understand what happened, how it happened, and with what consequences, the more we can trust inferences related to the associational, chronological and behavioral properties of the anthropic components of the sites we study. What makes the Geisenklösterle an extremely valuable site is the in-depth taphonomic work initiated by Hahn and now continued by Conard and the rest of the Tübingen research group; and, consequently, the fact that, at this site, we can see a disturbance that, in most other sites of the same period, remains hidden under such deceptively simple descriptive categories as the excavation level. Because we can see that disturbance, we can evaluate its impact and use that evaluation to “calibrate” excavation-derived units of association. That in no way did we ever mean to detract from the site’s value should in any case have been quite clear from the section of the Acknowledgments of our 1999 paper that we feel justified in reproducing here:

“We dedicate this paper to the memory of the late Joachim Hahn, whose pioneer work on the taphonomy and absolute chronology of the earliest Aurignacian was a constant source of inspiration throughout this endeavor” (p. 60).

#### *Implications of the acknowledged post-depositional disturbance*

This said, to what extent does the new evidence presented by Conard and Bolus (2003), Bolus (this volume), Conard et al. (this volume) and Liolios and Teyssandier (this volume) clarify our original questions? First, the recognition that two different kinds of assem-

blages are represented in horizons II and III and the cultural affiliation of horizon III are not at stake any more. It seems unquestionable that the anthropic component of the latter is for the most part related to the Aurignacian and earlier than that represented by horizon II. At the same time, however, and contra Conard and Bolus (2003), those data confirm Hahn's concern that the post-depositional vertical displacement of items at the site was significant, and that such processes explain the numerous stratigraphic inversions in the dating. Moreover, as we will try to demonstrate below, the new evidence confirms what we said in our 1999 review: that, as is the case elsewhere in Europe and the Near East, the Aurignacian of the Swabian Jura in fact is no earlier than ca.36.5 kyr BP.

Conard and Bolus (2003) and Conard et al. (this volume) state that post-depositional vertical displacement is minimal based on plots presenting the distribution of items in refitting groups A3, A9, A11, A16 and A20, which they believe "demonstrate the outstanding context of the Aurignacian finds from Geissenklösterle and show that only a small portion of the finds underwent significant vertical displacement" (Conard and Bolus, 2003, p. 350). It must be borne in mind, however, that these groups are only a small fraction (<20%) of the 30 refitting complexes examined. As shown by Teyssandier et al. (2002), many of the other refitting groups display the pattern of connections across the whole of the Aurignacian sequence already documented by Hahn (1988). Most importantly, the difference between Hahn's view, which we believe still stands, and Conard and Bolus's, does not relate so much to the quantitative assessment of the amount of disturbance that occurred at the site, but rather to the qualitative impact that even a quantitatively small amount may have at certain scales and for certain kinds of analysis. To illustrate the point, we will accept in the following discussion, as do Conard et al. (this volume), Hahn's (1988) estimate that only some 7% of the lithic artifacts moved between archeological horizons II and III, and that 60% of the pieces have not moved from their original subunit of deposition.

Such a level of post-depositional vertical movement is perfectly compatible with the preservation of both the "evident features" (such as ash lenses) and the "latent features" (such as horizontal concentrations of worked bone and ivory, or lithic scatters) reported by Conard and Bolus (2003, p. 350) and Conard et al. (this volume). Since it implies that the bulk of the material remained *in situ*, such a level of vertical displacement is also perfectly compatible with the recognition of global assemblage patterns, allowing legitimate between-horizon comparisons and assessments of overall differences and similarities. For instance, in such a context, and once we learn (Liolios and Teyssandier, this volume) that there are six carinated or thick-nosed scrapers in horizon II, but 40 in horizon III, it becomes unreasonable to sustain that the presence of carinated and thick-nosed scrapers in the latter may be explained by downward displacement from the former. At this level of analysis (i.e., the level of assessing the technological relevance that carinated reduction schemes have in horizon III), the possibility that 7% (i.e., three) of those 40 carinated and thick-nosed scrapers may actually derive from horizon II obviously is of no relevance.

Where such a possibility becomes of relevance is when the exact original stratigraphic position of individual items is at stake, i.e., when items have a meaning in themselves and not simply as singular manifestations of a certain category of finds. This is the case, in particular, with samples of bone selected for radiocarbon dating. If Hahn's estimates were correct, we would expect post-depositional movement to have little impact where conventional bulk samples are concerned, because the opposite effects of the potentially displaced contaminants contained in such samples (towards aging or towards rejuvenating) would more or less cancel each other out in the end. But, if only some 60% of items remained *in situ*, and if 7% of items moved between horizons II and III, then two things are to be expected,

provided a sufficient number of results is available, when attempting to date the two horizons by AMS: that a small percentage of the samples will give results for II instead of III, or for III instead of II; and that a significant percentage of the samples in II and III will be out of order in terms of the internal stratification of the different subunits making up each of those reconstructed archeological horizons (IIa, IIa, and IIb for horizon II; IIc, III, IIIa, and IIIb, for horizon III).

When the ages of the 33 samples (25 AMS and eight conventional) from the Aurignacian levels of the Geissenklösterle whose validity is accepted by Conard and Bolus (2003) are plotted against their stratigraphic provenance, both these expectations are confirmed (Fig. 9). The conventional results are in the expected stratigraphic order, and they present a clear and gradual pattern of increasing age with increasing depth. The opposite is true with AMS results: a random pattern, not one of increasing age with increasing depth, becomes apparent. Moreover, the upper limit of the 95% confidence interval for the earliest of the seven AMS results for reconstructed horizon II only slightly overlaps the earliest of the conventional results; and five of the 18 AMS results for reconstructed horizon III do not overlap at all, even at two sigma, with the most recent of the conventional results.

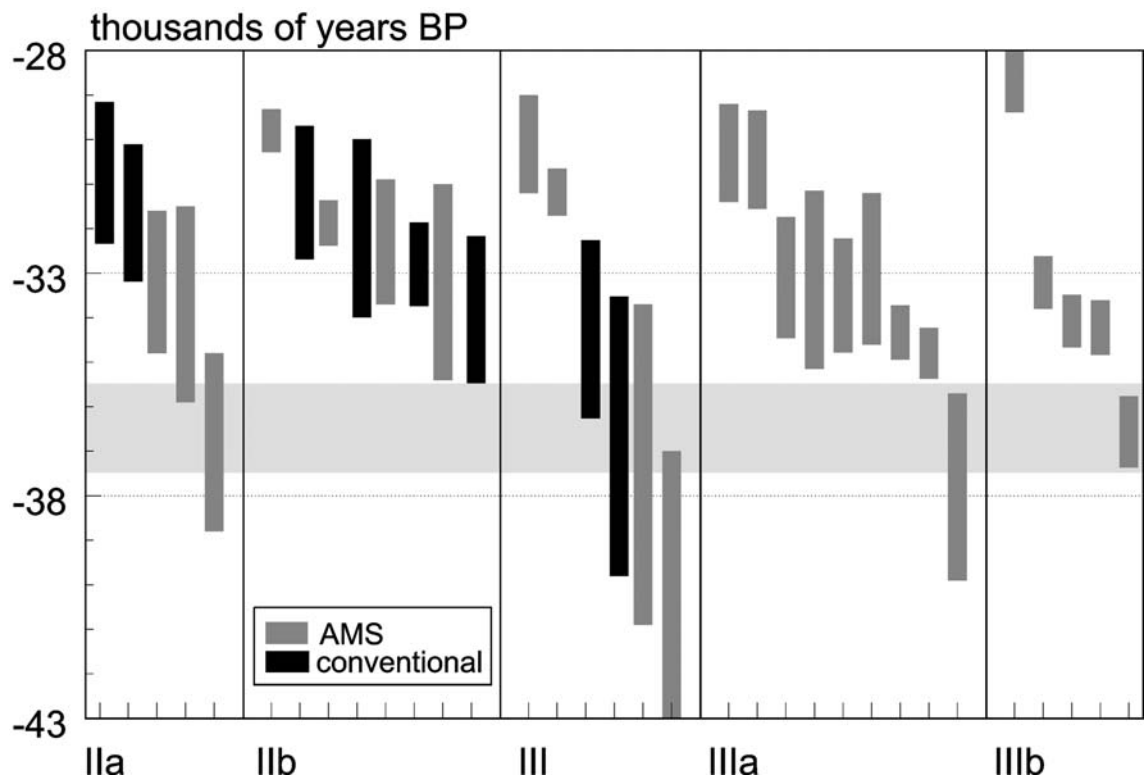


FIG. 9 –  $^{14}\text{C}$  dates (at 2 sigma) accepted by Conard and Bolus for the Aurignacian levels of Geissenklösterle. In this and subsequent graphs using uncalibrated dates, the grey band represents the time interval between 35.5 and 37.5 ka BP, the 95% confidence interval of the  $36.5 \pm 0.5$  ka BP age for the emergence of the Aurignacian proposed by Zilhão and d'Errico (1999).

Put another way, six of the 25 AMS results, i.e., 24%, seem to fall outside the range of results that would be expected if no post-depositional processes of vertical displacement had affected the integrity of horizons II and III. We explained these patterns, which we had identified in 1999 based solely on the then available AMS results, as caused by the same disturbance processes responsible for the vertical scatter of items in the refitting groups:

“given the vertical spread of the artifacts (...), a similar spread of the bone component of the deposit is to be expected, and the AMS dates which are apparently too young in respect to the reconstructed archaeological level of provenance are more likely to represent correct assessments of the age of vertically displaced samples than results rejuvenated by chemical contamination”.

*The impact of fluctuations in atmospheric radiocarbon production*

Conard and Bolus (2003) did not recognize these patterns, and, hence, could not have provided any direct and straightforward explanation for them. Their line of reasoning, however, suggests they believe that this spread of dates is a simple consequence of the fluctuations in atmospheric <sup>14</sup>C content which are known to have occurred between ca.40 and ca.30 kyr BP:

“For years researchers in Tübingen sought to explain the odd patterns in the radiocarbon dates from archaeological sites, including Geissenklösterle, by invoking taphonomic arguments about the mixing and reworking of finds. Now it seems more plausible that the explanations for the irregularities of the <sup>14</sup>C dates during the Aurignacian relate to fluctuations in radiocarbon production and transport” (p. 358).

And:

“The changes in North Atlantic planktonic foraminifera reflect extreme peaks in <sup>14</sup>C production that correspond to temporal offsets of more than 6000 years and perhaps as much as 10 000 years. Given that the marine signal for <sup>14</sup>C variation is attenuated due to reservoir effects, we must expect even greater fluctuations in radiocarbon concentrations in terrestrial archives including archeological sites” (p. 356).

The first thing to note where such an explanation is concerned is its inconsistency with the three basic and central claims of broader anthropological significance that Conard and Bolus (2003) derive from the Aurignacian of the Geissenklösterle: that “the Aurignacian of the Swabian Jura dates back to 40 ka BP” (p. 360); that “from the onset the Swabian Aurignacian shows a fully developed lithic and organic technology and the presence of ornaments by ca. 40 ka.” (p. 366); and, that “the western European Aurignacian postdates similar and analogous developments in the upper Danube region” (p. 362). If all <sup>14</sup>C dates in that time range can be off the true calendar chronology by as much as ten millennia or more, then all dates between ca.30 and ca.40 kyr BP are in fact the same date. Put another way, if that were true, we would lack the temporal resolution to assess what sites or processes are earlier or later, and all that happened in those long ten thousand years would be contemporaneous to our eyes.

In such a context, it would be simply impossible to define, as in the authors’ *Kulturpumpe* model, “the Swabian Jura as a region of early Upper Paleolithic colonization and cultural innovation” (p. 363). On what basis? Certainly not on the basis of the radiocarbon dates, if one wants to be consistent with the interpretation of the Geissenklösterle scatter of dates as caused by fluctuations of atmospheric <sup>14</sup>C; but such dates are in fact the only empirical basis of the model. The TL results published by Richter et al. (2000) are of no use here, because no other Aurignacian sites in this time range have been TL-dated, and because the very large standard deviations inherent to that method only allow us to discriminate between

events separated by amounts of time in the order of five to ten millennia; i.e., they have an even greater impact on the resolution of time scales than do any major fluctuations in atmospheric  $^{14}\text{C}$ . Moreover, invoking TL dates in this context would aggravate the inconsistencies of Conard and Bolus's position, given their initial statement, with which we fully agree (and, in fact extensively elaborated in our 1999 paper): "at present, despite well documented variations in  $^{14}\text{C}$  production in this period (...), radiocarbon measurements provide the only broadly applicable means of dating find horizons in the critical period between 30 and 50 ka calendar years BP" (p. 335).

On the other hand, the most significant anomaly in  $^{14}\text{C}$  production discussed by Conard and Bolus (2003, p. 356-359) is that identified by Beck et al. (2001) when studying a stalagmite from the Bahamas. This anomaly, if confirmed by future research, may be responsible for a radiocarbon rejuvenation of up to 8000 years in samples with a real age of ca.41-42 000 calendar years. On the basis of this, Conard and Bolus propose a general "Middle Paleolithic dating anomaly" to explain the fact that the two results obtained for the Mousterian levels underlying the Aurignacian sequence of the Geissenklösterle are in the range of ca.32-34 kyr BP. This is a possibility well worthy of further exploration, and one that might explain, for instance, the anomalous result of  $31\,900 \pm 1400$  BP (OxA-8541) for Context 19 of Gorham's cave (Barton et al., 1999; Pettitt and Bailey, 2000), which is associated with an otherwise coherent series of ca.42-47 kyr BP results. We will not dispute that this  $^{14}\text{C}$  production peak may be the fundamental cause of apparently anomalous ca.32 kyr BP results which, as in Gorham's, are interstratified with coherent series of multiple dates in the range of ca.35-45 kyr BP. We will not dispute either that this peak may be invoked legitimately as the cause underlying anomalous ca.32 kyr BP results obtained on isolated samples or isolated levels. It is clear, however, that it cannot explain ca.32 kyr BP results interstratified with coherent series of multiple dates in the range of ca.30-35 kyr BP, and all the more so if the latter are independently confirmed by U-series, TL and other dating methods. Hence, even if Conard and Bolus's "Middle Paleolithic dating anomaly" may explain to some extent the "coexistence effect" between Neandertals and modern humans in central and eastern Europe, it certainly does not refute the late survival of the Iberian Mousterian and of its Neandertal makers (Zilhão, 2000).

We must point out also that this anomaly occurs significantly earlier in time and, therefore, is not directly relevant to the chronology of the Upper Paleolithic of the Geissenklösterle. Indirectly, however, the explanation provided by Conard and Bolus in fact supports the argument that the earliest Aurignacian of the site is significantly later than ca.41-42 000 calendar years ago, i.e., significantly later than ca.38-39 000 radiocarbon years BP. As they describe it (2003, p. 353), the two anomalous results for the Middle Paleolithic of the Geissenklösterle come from samples in "uppermost Middle Paleolithic horizon IV", which is separated from "lowermost Aurignacian horizon III" by "largely sterile geological layer 17". Moreover, Conard et al. (this volume) are emphatic in describing a sharp discontinuity "between geological horizons GH 15, the main unit at the base of AH III, and the underlying archeologically nearly sterile layer GH 17", and they suggest that "the unconformity may well result from solifluction or gelifluction between the depositions of the two units". Given the overall low sedimentation rates characterizing the sequence, this sterile level must correspond to several millennia, if we extrapolate from its thickness of ca.40 cm and from a rate of 4 to 8 cm/millennium derived from the dates for the Aurignacian sequence. Therefore, the beginning of the deposition of the lowermost geological unit included in archeological horizon III is separated from the end of the deposition of the Middle Paleolithic by the time interval corresponding to the occurrence of two major geologi-

cal processes: the accumulation of GH17, possibly taking as long as three millennia, if not more, and the subsequent erosion of the extant sequence of deposits by solifluction or gelifluction, a process whose duration cannot be estimated. In any case, it is clear that, if the Middle Paleolithic dates to ca.41-42 000 calendar years ago (i.e., ca.38-39 000 radiocarbon years BP), as implied by Conard and Bolus's (2003) explanation of the ca.32 kyr BP anomalous results, the lowermost Aurignacian cannot be earlier than ca.38-39 000 calendar years ago (i.e., ca.35-36 000 radiocarbon years BP). That such is likely to be the case, regardless of which explanation for the site's anomalous Middle Paleolithic results is preferred, is demonstrated below.

No precise and universally accepted calibration curve exists at present covering the time interval of concern in this controversy but, for the period immediately post-dating the  $^{14}\text{C}$  production peak of ca.41-42 000 calendar years ago proposed by Beck et al. (2001), Köln Radiocarbon Laboratory's CalPal (Jöris and Weninger, 1998, 2000, 2003) is the best calibration software currently available. When this software is used to calibrate the 33 dates accepted by Conard and Bolus for the Aurignacian of the Geissenklösterle (Fig. 10), the pattern apparent in Fig. 9 does not change. The time intervals corresponding to each result are larger, and the intersections more important, but the underlying pattern is the same; on the basis of present evidence, one is forced to conclude that the primary cause for the scatter in the dates cannot possibly reside in the fluctuations of atmospheric  $^{14}\text{C}$  between ca.30 and ca.40 kyr BP. Undetected chemical contamination and excavation error certainly contribute to a certain extent to this pattern but, given the data from the lithic refitting work, post-depositional vertical displacement must be, as we argued before, a major factor.

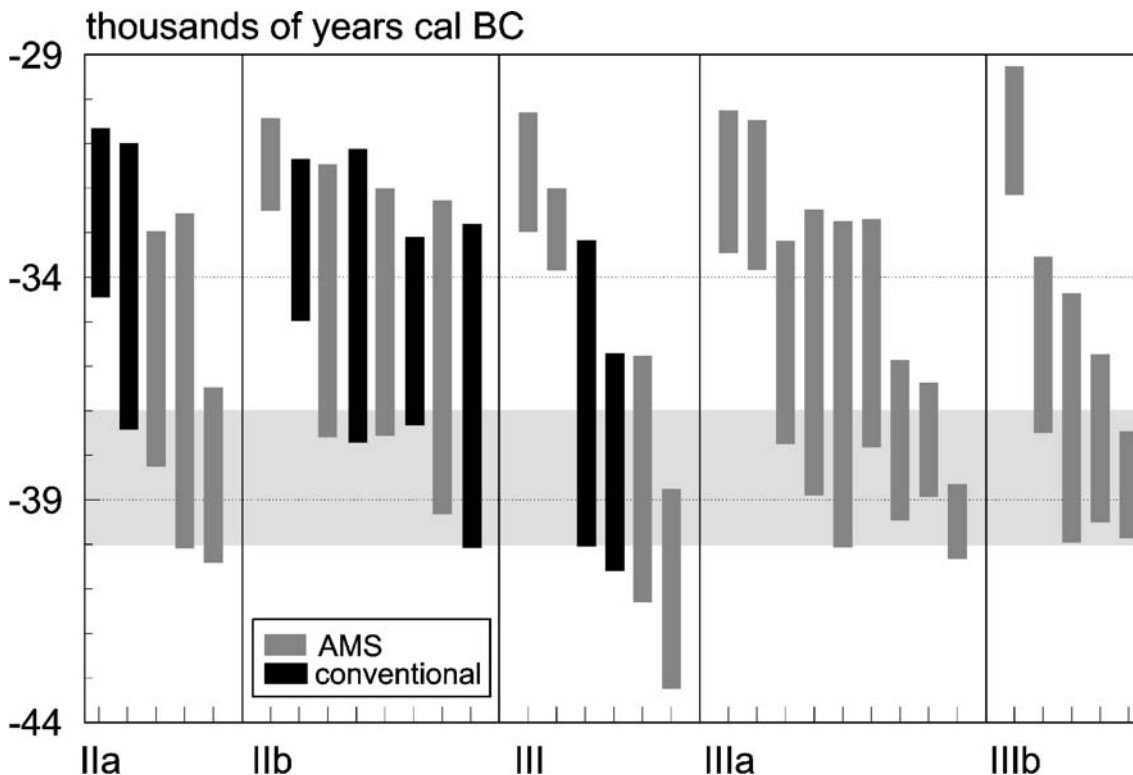


FIG. 10 –  $^{14}\text{C}$  dates accepted by Conard and Bolus for the Aurignacian levels of Geissenklösterle calibrated (at 2 sigma) with Köln Laboratory's CalPal software. In this and subsequent graphs using calibrated dates, the grey band represents the time interval between 37 and 40 ka cal BC, the 95% confidence interval of the  $38.5 \pm 0.75$  ka cal BC ( $36.5 \pm 0.5$  ka BP) age for the emergence of the Aurignacian proposed by Zilhão and d'Errico (1999).

Thus, contra Conard and Bolus (2003), we maintain that the “researchers in Tübingen” who, “for years”, “sought to explain the odd patterns in the radiocarbon dates from archaeological sites, including Geissenklösterle, by invoking taphonomic arguments about the mixing and reworking of finds” were completely right. In fact, the magnitude of the problem is further illustrated, for southwest Germany, by the radiocarbon data provided by the same authors (p. 339-340) for the other site with samples from modern excavations, the Höhle Fels. Level IIc, Gravettian, yielded two AMS dates on bone of ca.29 kyr BP; immediately underlying levels II d and II e, already Aurignacian, yielded three AMS dates on bone of ca.29.5-30.5 kyr BP; below, level III yielded another three AMS dates on bone of ca.30-31 kyr BP, and level IV two AMS dates on bone of ca.31-33 kyr BP; all, therefore, internally consistent and in stratigraphic order. However, one AMS charcoal date for level III of ca.28 kyr BP and another AMS charcoal date for level IV of ca.29 kyr BP are in disagreement with the sequence. As in the Geissenklösterle, no simple explanation exists for these results and it is clear that none will be found unless the vertical displacement of items is duly accounted for as part of the problem.

*The most likely age of horizon III*

Figures 9 and 10 also make it clear that even the earliest of the radiocarbon dates obtained for the Aurignacian of the Geissenklösterle, when their 95% confidence intervals are duly considered, intersect the ca.36.5 kyr BP interval that we had postulated as the lower limit for the age of the earliest Aurignacian in Europe (and this is even more clear when calibrated results are used); a single exception exists, the 40 200±1600 BP OxA-4595 result. Therefore, and given that reasonable explanations can be provided for this outlier (see below), the first thing to note is that the new data reported by Conard and Bolus (2003) do not affect the major conclusion of our 1999 review of the evidence. However, those new data are extremely useful in that they allow us to approach the issue of the real age of Geissenklösterle’s horizon III with a lot more precision. In fact, the current data set suggests that we were wrong on two counts when we suggested that the occupation of the site represented by horizon III, although probably not Aurignacian, could be earlier than ca.37 kyr BP and possibly contemporary with the Châtelperronian. The bulk of that occupation is Aurignacian indeed, but it is also more recent than ca.37 kyr BP and well within the time range of the classical Aurignacian of southwestern Europe.

All the dates reported by Conard and Bolus (2003, p. 335) for the Geissenklösterle are from samples that meet stringent conditions. Some showed clear anthropogenic modifications including impact fractures and cut marks, or were obtained directly from bone artifacts; with the exception of one date on red deer antler, all were made on well preserved bone, and in every case the yield of collagen was significantly high to produce a reliable result. Moreover, the authors consider that (1) when errors in collecting and processing samples are excluded, conventional and AMS dates are comparable, and the same is true of dates on carefully prepared samples of bone, antler and charcoal, and (2) that there is no reason to believe that the previous handling of the specimens or contamination with calcium carbonate led to anomalous ages. On the basis of these criteria, the authors accept as valid and reliable 33 dates for the Aurignacian levels of Geissenklösterle. In order to make the argument more clear, we will accept their judgment, particularly since the main chemical factor which we believe biases to a certain extent this issue — the small, but systematic difference between bone dates and charcoal dates (see also Jöris et al., 2001) — has no impact where the Aurignacian of Geissenklösterle is concerned, because all the results so far

obtained for it are on bone. In the light of this, we must point out that Conard and Bolus's (2003) line of reasoning, as clearly expressed by the following statement, suffers from a fundamental logical inconsistency:

“Six  $^{14}\text{C}$  dates from three accelerator and one conventional lab fall in the range between 36-40 ka BP. (...) These early dates [our underlining] are roughly consistent with the mean age of  $40.2 \pm 1.5$  ka BP based on Richter et al.'s (2000) six thermoluminescence dates on burnt flints from horizon III. These TL dates range between 38.3 and 44.7 ka and have standard deviations between 2.1 and 5.6 ka. Based on the taphonomic and archeological arguments mentioned above, we find no basis, at present, to reject these six radiocarbon dates.” (p. 353)

It is clear from this that the six results in question form the empirical foundation for the claims concerning a beginning of the Aurignacian at Geissenklösterle ca.40 kyr BP. The problem, however, is that one of those results comes from horizon II, and that the authors accept another 15 results for horizon III. So, the issue here is not that these six results should not be rejected. The issue here, as pointed out by Liolios and Teyssandier (this volume), is why do Conard and Bolus exclude from further consideration those other 15 results; if all 20 samples and measurements are equally reliable, why is the chronology of horizon III based on only five of them, i.e., one quarter of the evidence? Why should one quarter of the evidence carry more weight than three quarters?

When all the evidence is considered (Figs. 9 and 10), the picture that emerges is quite different from that presented by Conard and Bolus (2003). If we accept that there are only two occupations and that all 13 results from horizon II and all 20 results for horizon III are equally valid, it seems reasonable, regardless of which is the preferred explanation for the observed scatter, to consider that the age of those two occupations must be given by the average of all the valid results obtained for each. If so, then horizon II would date to 32 488 BP, and horizon III to 33 825 BP. Put another way, the beginning of the Aurignacian of the Geissenklösterle would have to be placed ca.34 kyr BP, not ca.40 kyr BP, as Conard and Bolus contend.

Another way, and perhaps a more realistic one, to look at the evidence, is to assume that the site is a palimpsest of multiple occupations and that the spread of the results is primarily a reflection of this fact. This could still be reconciled with Conard and Bolus's view of only two “levels” by postulating that, of the two modes of cave occupation, differing more in function than in technology, which Liolios and Teyssandier (this volume) recognized, the first of the many different occupations recorded in horizons III and II would all have featured the “earlier Aurignacian” mode, whereas subsequent ones would all have featured the “later Aurignacian” one. The earliest of the dates for horizon III, therefore, would mark the first appearance of that “earlier Aurignacian” mode of site occupation in the Geissenklösterle sequence.

Once we admit that we are dealing with a palimpsest, however, we also have to admit that other agents of bone accumulation besides humans may be responsible for the faunal assemblage contained in the deposits, as Conard and Bolus do acknowledge. And because, with most radiocarbon samples, we are not directly dating human activity itself but items presumably associated with it, it makes sense to consider that only those bones which are anthropically-modified (transformed into tools, cut-marked, impacted, or with breaks in fresh) are unquestionable evidence of human presence at the site, as argued by Richter et al. (2000). In Figs. 11 and 12, we plotted all the results for the Geissenklösterle derived from such samples in decreasing order of stratigraphic provenance. It appears from these plots that:

1) the beginning of the Upper Paleolithic occupation of the site documented in horizon III cannot be forced back beyond ca.36.5 kyr BP (i.e., ca.38.6 kyr cal BC);

2) only two of the results are clearly inverted relative to the stratigraphy and may well represent vertically displaced items, but the picture that emerges (particularly when calibrated results are used) is one of broad positive correlation between stratigraphic depth and age (which is not the case when all samples, including those that may represent natural components of the deposit, are used, as in Figs. 9 and 10);

3) the other ten results cluster into two chronological groups, one between 29 and 32 kyr BP (30 and 34 kyr cal BC) and another between 33 and 37 kyr BP (35 and 40 kyr cal BC); this pattern does not change if the two inverted results are replaced in their presumed original position and incorporated in the comparison.

When uncalibrated results are used, a single result, ca.36.5 ka BP, seems to represent a separate moment of occupation inside this second cluster, where all other dates fall in the interval between 33 and 35 ka BP. However, when calendar ages are used, that earlier result cannot be distinguished from the others. One must also bear in mind that this apparently earlier result comes from a sample of roe deer bone. This species is so rare in the Aurignacian of southwest Germany that it is not even listed in the inventory of the region's fauna provided by Niven (this volume), but Münzel (personal communication) confirms that a few roe deer bones were indeed found in the Middle Paleolithic and lower Aurignacian levels of the Geissenklösterle in the 1999 field season. Since Niven (2001, this volume) and Münzel (1997, 1999; Münzel et al., in press) describe the Aurignacian use of Vogelherd and Geissenklösterle as focused on the exploitation of mammoth, horse and reindeer, the introduction in the deposits of the dated roe deer bone cannot be related to that use; roe deer is a temperate forest species not compatible with the steppe-tundra environments implied by the mammoth-horse-reindeer association. Thus, the dated roe deer bone confirms that the deposits making up Geissenklösterle's horizon III are indeed a palimpsest which not only contains different occupations by humans, cave bear, and other carnivores, but also accumulated over an extended period of time comprising climatic oscillations of opposite signal.

It is possible that the temperate environment occupation of the Geissenklösterle represented by that roe deer bone was also by Aurignacian people, even if the bulk of the site's Aurignacian deposits accumulated somewhat later and in a different climatic setting. But it cannot be excluded that the roe deer in question relates instead to a sporadic human incursion into the site (Middle Paleolithic? Transitional? truly Proto-Aurignacian?), one that may not have left behind the diagnostic remnants of lithic or osseous technology necessary for its corresponding archeological individualization and cultural assignment. In fact, once we accept to work under the assumptions of palimpsest models of site formation process, the hypothesis of episodic occupations leaving behind the bone remains of meat processing or meat consumption associated with undiagnostic artifacts must indeed enter the picture. In this regard, the extremely well preserved level EE15 of the Lagar Velho rockshelter (Zilhão and Trinkaus, 2002), featuring 100% refitting of hundreds of items within a few centimeters of thickness of deposit, associated with intact patterns of horizontal organization of the evidence (hearths, differential accumulation of animal body parts, lithic concentrations defining drop zones and toss zones), is an example that immediately comes to mind. In fact, on the basis of its lithic component

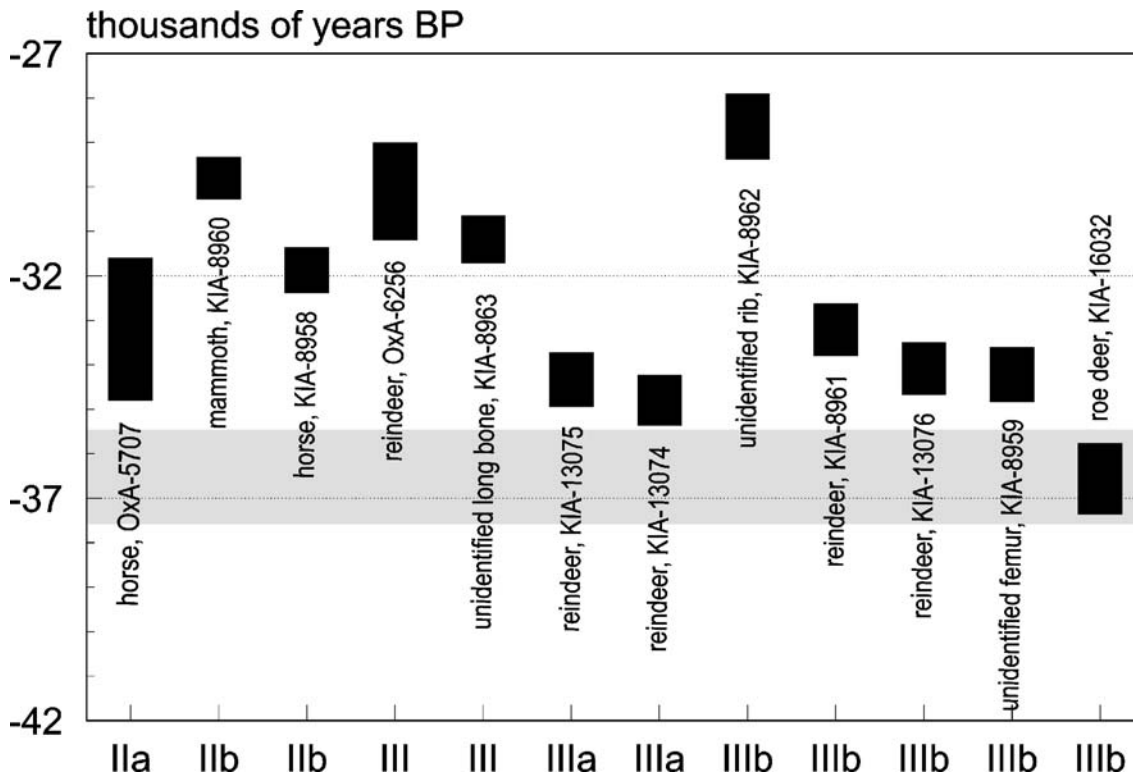


FIG. 11 – <sup>14</sup>C dates (at 2 sigma) on anthropically-modified bones accepted by Conard and Bolus for the Aurignacian levels of Geissenklösterle.

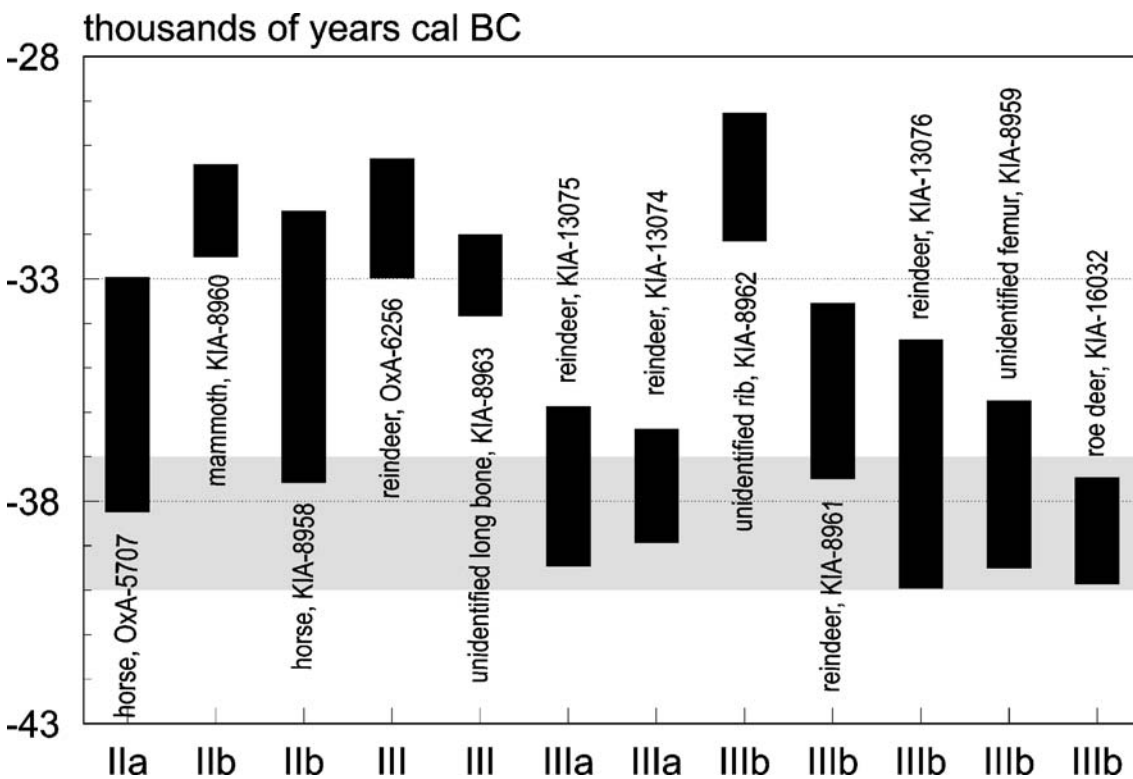


FIG. 12 – <sup>14</sup>C dates on anthropically-modified bones accepted by Conard and Bolus for the Aurignacian levels of Geissenklösterle calibrated (at 2 sigma) with Köln Laboratory's CalPal software.

alone, this level might well be deemed Middle or even Lower Paleolithic, whereas its extensive <sup>14</sup>C dating and position in a well dated stratigraphic sequence show that it is of late Gravettian age.

*Conclusions on the chronology of the Geissenklösterle*

At the end of this review, we are forced to conclude that the radiocarbon dates for the Aurignacian of the Geissenklösterle only provide unmistakable evidence for two distinct periods of human occupation: one, diagnosed on the basis of samples in association with the “later Aurignacian” mode of cave occupation, ca.29-32 kyr BP; the other, diagnosed on the basis of samples in association with the “earlier Aurignacian” mode of cave occupation, ca.33-35 kyr BP. It is also possible (and in fact quite likely, given the distribution of the dates inside the two intervals) that these two “periods” in fact subsume several different moments of occupation, and, with more dates on anthropically-modified bones, and with a more sophisticated statistical analysis of the results obtained, that their individualization may well be achieved eventually. All the available evidence suggests, however, that the earliest of those moments of occupation that unquestionably can be associated with the “earlier Aurignacian” dates to ca.33-35 kyr BP. This time interval corresponds to the global climatic deterioration known as the Heinrich 4 event, dated in a number of marine cores to between ca.35.3 and 33.9 kyr BP (Elliot et al., 2002; Auffret et al., 2002). Correlation of AMS dated Aurignacian sites from France and northern Iberia with the Dansgaard-Oeschger climatic variability has recently shown (d’Errico and Sánchez Goñi, 2003) that most have AMS dates falling in the time span of this climatic event. The assemblages found therein are characterized by a technology similar to that observed at Geissenklösterle and similarly associated with cold faunas.

These conclusions conform well to the fact that the Geissenklösterle’s “earlier Aurignacian” in no way can be assimilated to the “Proto-Aurignacian” rich in Dufour bladelets for which dates in the range of ca.35-37 kyr BP have been obtained. As argued by Liolios and Teyssandier (this volume), AHII and AHIII are quite similar from the technological point of view, both have close parallels in the classical Aurignacian of southwestern France, and the differences between the two assemblages “may well be due to functional and economic factors, hinging on distinct subsistence-related on-site activities”, accounting for “the similarity between the operative concepts identified for the lithic and organic productions” as well as for “the differences in the frequency of tool-types and in the completeness of reduction sequences”. That classical Aurignacian of southwestern France is dated to ca.33-35 ka BP and Liolios and Teyssandier conclude that AHIII, considering the spread of dates for it (between ca.33 and ca.40 ka BP), “could therefore arguably be relatively close in time to classical early Aurignacian assemblages”.

Our view also conforms well with recent revisions of the French Aurignacian (Bordes, 2002a, this volume). In fact, the technological features of the earlier Aurignacian in AHIII of the Geissenklösterle virtually replicate those of the Aurignacian in level 7 of Roc de Combe and in levels G-I of Le Piage. This technological system corresponds to the “classical” Aurignacian I, characterized by the use of carinated “burins” and carinated “scrapers” to produce mid-sized bladelet blanks with a curved profile and which for the most part remain unretouched (as is the case in horizon III of the Geissenklösterle). It post-dates and, at Isturitz and Le Piage, stratigraphically overlies the earliest Aurignacian of the Aquitaine basin, dated at Isturitz to ca.35-37 kyr BP and characterized by the use of prismatic cores and nucleiform burins to produce large-sized bladelet blanks with a straight profile and which

for the most part are intensively retouched into Dufour bladelets of the Dufour subtype. It should come as no surprise, therefore, that the Aurignacian in horizon III of the Geissenklösterle in all likelihood dates to a later time interval, ca.33-35 kyr BP, not to ca.35-37 kyr BP, much less to ca.40 kyr BP or more, as claimed by Conard and Bolus.

The notion that the Aurignacian of the Swabian Alb begins that early may at best rest on one, and only one, of the 20 results for horizon III of the Geissenklösterle, the OxA-4595 date ( $40\,200 \pm 1600$  BP) on a horse femur. In fact, if we look at the results on an individual basis, we verify that there are only three (all on bones with no evidence of anthropic modification) with mid-points in excess of 37 kyr BP: OxA-5163 ( $37\,300 \pm 1800$  BP); ETH-8267 ( $37\,800 \pm 1050$  BP); and OxA-4595 ( $40\,200 \pm 1600$  BP). The standard deviations of the first two are such that they are in fact within the chronological horizon of ca.36.5 kyr BP postulated by Zilhão and d'Errico (1999) for the earliest Aurignacian (Figs. 9-10). And, in our view, the third result is more likely to reflect the presence of carnivores at the site prior to the Aurignacian, at the time the package of sediments making up horizon III began to accumulate, during the major hiatus in human occupation between the Middle Paleolithic in horizon IV and the ca.36.5 kyr BP "roe deer incursion". We feel that this interpretation is at least a more parsimonious reading of the AMS  $^{14}\text{C}$  evidence than that proposed by Conard and Bolus, which implies giving more weight to one sample (5% of the evidence) than to the remaining nineteen (95% of the evidence).

### How recent are the "Transitional" industries?

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Given the arguments presented above for El Castillo, l'Arbreda and Geissenklösterle, the case for an Aurignacian earlier than ca.36.5 kyr BP must rest entirely on level 3 of the Austrian site of Willendorf II. As Haesaerts and Teyssandier (this volume) point out, however, the small size of the assemblage makes an unambiguous cultural assignment difficult at present. One must also bear in mind that all three dates for that level currently available are on charcoal (Svoboda, this volume), whereas the ca.36.5 kyr BP age for the earliest Aurignacian suggested in our 1999 review of the evidence was to a large extent based on the pattern displayed by the dates on bone samples; and, as mentioned above, a systematic difference seems to exist between dates obtained on these materials (Zilhão and d'Errico, 1999; Jöris et al., 2001). So, even if Willendorf II's level 3 is accepted as indeed related to the Aurignacian, the fact that two of the dates for it are in the 38-39 kyr BP range does not necessarily refute the validity of the ca.36.5 kyr BP limit, particularly if the corresponding 95% confidence intervals are duly considered. In fact, as Figs. 13 and 14 clearly illustrate, those dates intersect the two sigma interval of the limit we postulated, and are fully inside it when calibrated results are used. Moreover, they compare well with the results obtained on similar samples of charcoal that we had discussed in our 1999 review of the evidence: those from the early Aurignacian levels of La Viña and Isturitz, as well as those from the remnant deposits where the Aurignacian of Romaní was once contained.

### *The age of the Bohunician*

On the other hand, a date of ca.34 kyr BP also exists for Willendorf II's level 3, which brings to mind the coexistence of two clusters of charcoal dates in connection with the Bohunician occupation documented in the upper paleosol of Stránská skála (Svoboda, this volume):

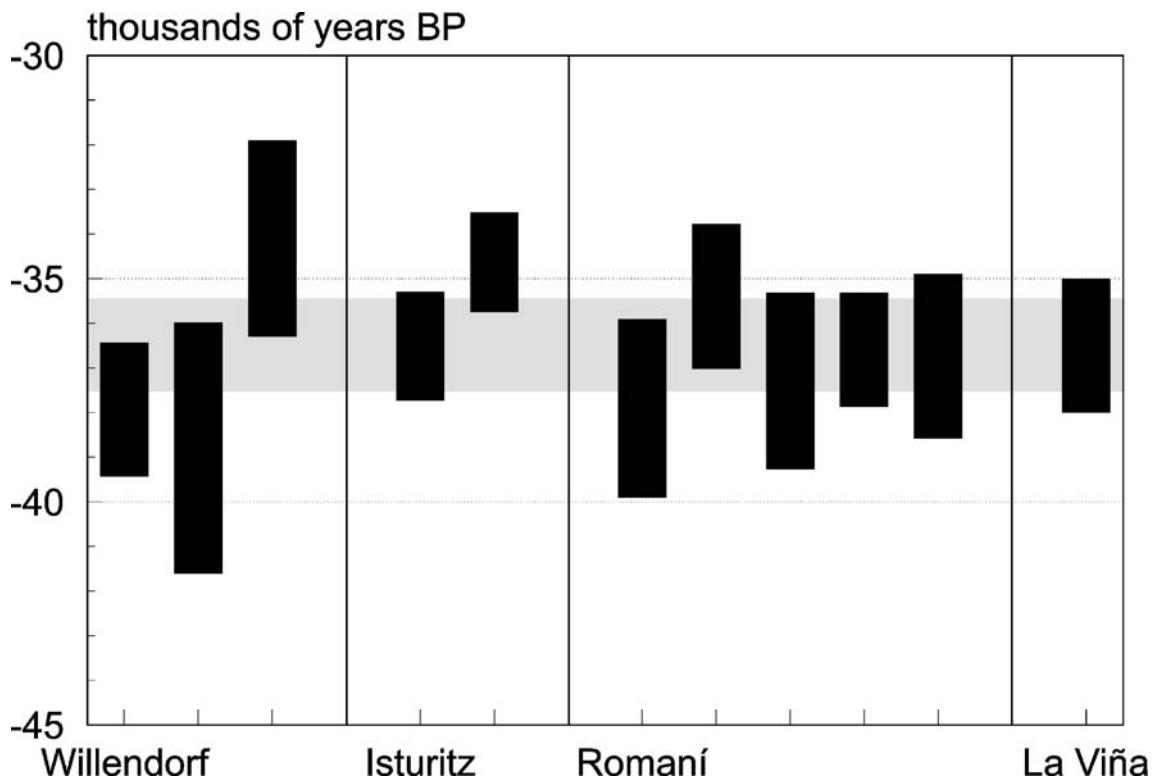


FIG. 13 – <sup>14</sup>C dates (at 2 sigma) on charcoal for the earliest Aurignacian levels of Willendorf II, Isturitz, Romaní and La Viña.

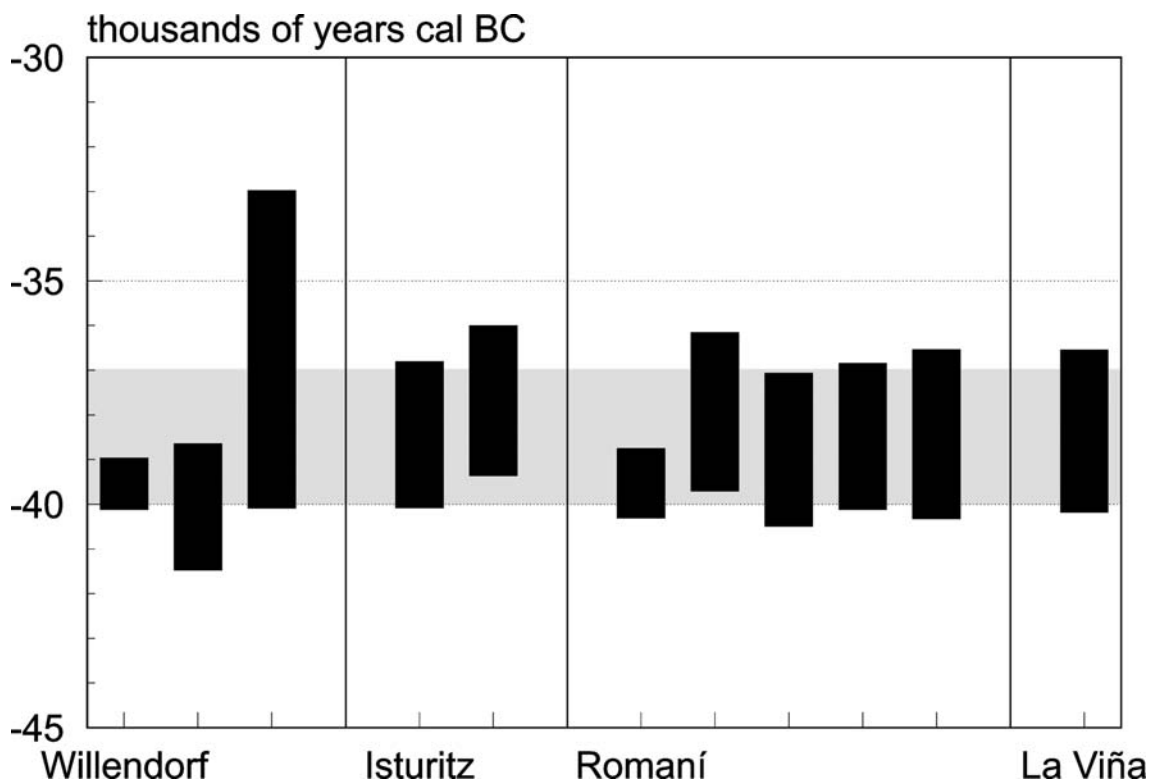


FIG. 14 – <sup>14</sup>C dates on charcoal for the earliest Aurignacian levels of Willendorf II, Isturitz, Romaní and La Viña calibrated (at 2 sigma) with Köln Laboratory's CalPal software.

one ca.37-38 kyr BP, the other ca.34-36 kyr BP. Since these charcoal dates are indirect, not direct evidence of human activity, and since these discrepancies may well relate to the complex formation process of the loess paleosols of central Europe, caution should be the rule when interpreting them in terms of cultural process. It is legitimate to suggest that the evidence from Willendorf II and Stránská skála indicates that the Aurignacian begins ca.39 kyr BP or that the Bohunician lasts until ca.34 kyr BP, as implied by Haesaerts and Teyssandier (this volume) and by Svoboda (this volume). But the same evidence can be used with equal, if not more legitimacy to argue that only the ca.34-36 kyr BP dates for these sites relate to the Aurignacian and that, at least at Stránská skála, only the earlier ones relate to the Bohunician.

Where Stránská skála is concerned, this is all the more so since the recent dates for the Bohunician come from the upper paleosol, where the Bohunician and Aurignacian levels are in direct contact. Svoboda notes that “the upper paleosol is preserved more or less *in situ*, but affected at certain places by cryoturbation, moving in material from below”. Commenting on the overlap between the series of dates for each paleosol (41-37 kyr BP, for the lower; 38.5-30 kyr BP for the upper) he also states that “I would see this overlap as a result of the coarse-grained stratigraphic value of the paleosols in loess, where the visible layers may, in fact, include several smaller-scale oscillations”. The archeological corollary of these observations is that the charcoal contained in the upper soil cannot be directly related to the two human occupations documented therein unless it comes from well defined hearth features, as is the case with *locus* IIIf sample AA-41 472, which provided a result of 29 020±440 BP. On the other hand, several samples from different *loci* of the site provided ages of ca.37-38 kyr BP for the Bohunician level in the upper soil that are very close to the age of the Bohunician level in the lower soil. In light of the geological observations reported by Svoboda, and if the real age of the Aurignacian from Stránská skála is indeed in the vicinity of 30 kyr BP, as indicated by the hearth date, it is quite possible that those of the results in the 30-32 and 34-37 kyr BP intervals obtained, respectively, for the Aurignacian and the Bohunician levels of the upper paleosol, in fact reflect varying degrees of admixture between charcoal of different ages, some of which may even be of non-anthropogenic origin. In spite of their large standard deviations, the dates obtained for the eponymous site of Bohunice (Svoboda, this volume) further strengthen the hypothesis that the Bohunician entirely predates ca.37 kyr BP and is fully contemporaneous with the Châtelperronian of southwestern Europe.

Given the evidence on the chronology of the earliest Aurignacian, the relevance of the above conclusion becomes immediately apparent if we bear in mind that the hypothesis that such “transitional” technocomplexes as the Châtelperronian are the product of “acculturation” could only continue to stand if it were shown that the emergence of the latter post-dated ca.36.5 kyr BP. Even if the Bohunician did last until ca.34.5 kyr BP, there is no doubt, however, that its earliest manifestations significantly pre-date ca.36.5 kyr BP, and the same is true of the Szeletian (Svoboda, this volume). Regardless of how long they lasted, they had already been around for several millennia before the Aurignacian; their emergence, therefore, cannot have been triggered by contact with the arrival of modern humans into these regions, at least not if one works under the assumption that the earliest Aurignacian represents the archeological proxy of such an arrival.

### *More on Arcy*

A variant of the acculturation argument is that, even if these transitional technocomplexes emerged before the Aurignacian, their most salient “Upper Paleolithic”-like features (ornaments, bone tools) are comparatively much later. Where the Châtelperronian is

concerned, this is the line of reasoning followed by Floss (this volume), who, quoting David et al. (2001), contends that “the new dates for the Châtelperronian [of the Grotte du Renne] confirm those previously obtained (...) and suggest its age lies between ca.34 000 and ca. 32 000 BP”. Based on this, Floss goes on to state that “the Châtelperronian of Arcy is not earlier than the Aurignacian of Solutré, a site situated only 150 km to the southeast”, and that “the Swabian Aurignacian begins even earlier than the Arcy Châtelperronian”.

In our view, the new chronometric results for the Grotte du Renne have only served to confirm that serious contamination problems affect the samples, as was to be expected given that, after the collapse of the cave’s roof during the Gravettian, the site stayed in the open, and an important forest soil developed on top of the previously accumulated deposits, significantly altering them, throughout at least the entire Holocene. The impact this factor must have had on the rejuvenated results obtained for the Grotte du Renne sequence is made apparent by the systematic differences between samples collected in exterior and interior areas of the same levels that have been reported from the cave sites of Fumane, Italy (Broglio and Improta, 1994-95), and Sesselfels, Germany (Richter, 2002). In Fumane, dates for basal Aurignacian level A2 collected in the porch range between 32 100±500 BP (UtC-2047) and 32 800±400 BP (UtC-2051), whereas those collected inside the cave range between 34 200/+900/-1000 BP (UtC-2690) and 36 800/+1200/-1400 BP (UtC-2688). In Sesselfels, dates for the sequence of Micoquian levels G1-G4a/5 range, from top to bottom, between 30 770/+250/-240 BP (GrN-20305) and 36 030/+1180/-1030 BP (GrN-20312) for samples collected in the porch; for samples collected inside the cave, however, the range is between 39 950/+970/-870 BP (GrN-20302) and 47 860/+960/-860 BP (GrN-20314). These examples show how likely it is that the dates for the Grotte du Renne, all from samples collected in porch deposits, similarly underestimate the true age of its Châtelperronian occupations. Thus, the only significant progress made in our understanding of the Grotte du Renne’s sequence since 1999 is (1) the further demonstration of both the stratigraphic integrity of the Châtelperronian levels and the originality of the bone technology contained therein (d’Errico et al., in press and this volume), and (2) Bon’s (2002; Bon and Bodu, 2002) and Bordes’s (2002b) suggestion that the technological and typological features of Aurignacian level VII indicate strong affinities with the “Proto-Aurignacian” of Isturitz, l’Arbreda and Fumane. The latter being dated at ca.35-37 kyr BP, it follows by implication that, if such a techno-typological diagnosis is accepted, the entire Châtelperronian sequence of the Grotte du Renne must be earlier than any Aurignacian site known.

Lucas et al. (this volume) also provide ample evidence that the Châtelperronian of Grotte XVI dates to ca.38 kyr BP or more. The three new dates obtained for Roc de Combe (Bordes, 2002b; Bordes et al., in prep.), from samples with a secure provenance in square K9, are fully consistent with this view. Not only do they prove that the site’s Châtelperronian levels are older than ca.40 kyr BP, their association with a detailed taphonomic study also help to explain the scatter in the results previously available and which we discussed in Zilhão and d’Errico (1999). The samples for the latter came from an area (squares G and H) which has been shown convincingly to lack stratigraphical integrity (Bordes, 2002b, this volume), in an outstanding example of how post-depositional disturbance and excavation error are more powerful explicators of anomalous radiocarbon results than undetected chemical contamination or the fluctuations in atmospheric <sup>14</sup>C content.

None of these Châtelperronian sites produced the variety of ornaments and bone tools seen at Arcy. This leads Floss (this volume) to view the Châtelperronian of the Grotte du Renne as entirely exceptional, and to construe it as a very late, epigonal manifestation of the technocomplex, one that would have undergone substantial transformation through the impact of Aurignacian moderns meanwhile established in the neighborhood: “the excep-

tional modernity of the Châtelperronian from Arcy could be explained by the proximity to, and the influences received from, nearby Aurignacian sites in, for example, southern Burgundy or the Swabian Jura". We will not repeat here the arguments presented before (d'Errico et al., 1998b) against such a view of the facts, but we must nonetheless point out the fundamental inconsistency underlying Floss's argument. The total number of Châtelperronian sites with some preservation of organics currently known is 65. The Grotte du Renne, therefore, stands for 1.7% of such sites. The total number of Aurignacian sites with some preservation of organics currently known may be estimated to lie in the vicinity of 230. Geissenklösterle, Vogelherd and Höhlenstein-Stadel are the only three Aurignacian sites with sculptured depictions of animals and humans, i.e., they are 1.3% of the total and stand for more or less the same percentage of the total relevant evidence as the Grotte du Renne. Put another way, where figurative mobiliary art is concerned, those three sites are as exceptional in the Aurignacian as, where ornaments and bone tools are concerned, is the Grotte du Renne in the Châtelperronian. In fact, the three German Aurignacian sites are even more of an exception because Châtelperronian ornaments and bone tools are in no way restricted to the Grotte du Renne; since they have been found at nine sites (d'Errico et al., 1998), i.e., 14% of the total, their occurrence is ten times more frequent in the Châtelperronian than the occurrence of mobiliary figurative art is in the Aurignacian!

If Floss's logic were to be applied to the German sites, they too would have to be considered anomalous and in need of a special explanation; for instance, that the items of sculpture might in fact be intrusive from the overlying Gravettian levels, given that figurines of humans and animals are common and widespread in western, central and eastern Europe only at that time. Instead, the figurative art of the Swabian Alb sites is taken as representative of Aurignacian modern human behavior in general, whereas the ornaments of the Châtelperronian Neandertals are taken as an unrepresentative behavioral oddity!

We have no doubt that the animal and human figures of the German Aurignacian are indeed Aurignacian and representative of modern human behavioral patterns, and, by the same token, we have no doubt that the ornaments from the Grotte du Renne are indeed Châtelperronian and representative of Neandertal behavioral patterns. We recognize the exceptionality of these productions in both the Châtelperronian and the Aurignacian, but we suggest that the explanation for it must lie elsewhere, in what they have in common and sets them apart from their contemporaries: both are the work of populations occupying what, during the respective time ranges, were the northernmost ranges of the territory available for human habitation in central and western Europe; in other words, German Aurignacians and northern Burgundy Neandertals were the "Inuit" of their times. Based on this, we contend, contra Floss, that an explanation for the extensive use of ivory and ornamentation in the arctic environments these people were the first to successfully exploit needs be no different from explanations used in Holocene archeology or in the ethnographic present to explain the equivalent contrast and variation in material culture encountered in any North-South transect of world societies.

That ornaments are widespread across vast expanses of Eurasia prior to the Aurignacian is in any case the inescapable conclusion that now must be drawn from the southeast Turkey and Lebanese evidence reported by Kuhn (this volume) and Stiner (this volume), and by the data from the lowermost cultural levels of Kostenki 14 (Markina gora) reported by Sinitsyn (this volume). We do not know the anatomical affinities of the people who successfully colonized northern Russia before ca.36 kyr BP and who, according to Svendsen et al. (this volume), had already reached the Arctic Circle at about the same time. Sinitsyn suggests they were anatomically modern, a view that is also accepted by Chabai (this volume), who further illustrates the complex cultural picture that existed in eastern Europe throughout this criti-

cal time period, and provides evidence that Neandertals were part of that picture until quite late, at least in the south. One must bear in mind, however, that Sinityn's suggestion is based on a single tooth, which is probably not sufficient to warrant the diagnosis.

### **Does the Aurignacian have an origin?**

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The data strongly suggest that the transformation of the archeological record of Eurasia in the period between ca.45 and ca.30 kyr BP, of which the appearance of ornaments is but the most visually striking aspect, is strongly correlated with a “go North” push. This push must have been the consequence of a demographic build-up made possible by the cultural and technological innovations developed by both Neandertals and moderns in the preceding period, once, after ca.60 kyr BP, the world started to come out of the adverse climatic conditions prevailing during OIS 4. It is now clear, however, that the patterns of material culture that most archeologists recognize as “the” Aurignacian are found only in the later half of that period, after ca.36.5 kyr BP. Therefore, the explanation of that transformation and the explanation of the Aurignacian must be taken as two different, albeit related problems.

The widespread geographic distribution of such idiosyncratic items as split-based bone points strongly suggests, *contra* Straus (this volume), that “the” Aurignacian is indeed more than simply a convenient short-hand for “the time of the transition”. Given the pattern of the physical anthropological record, it is quite possible, as discussed above, that the phenomenon is related to the spread of modern humans into Europe. Such a correlation would be strengthened if it could be shown that both processes had the same point of origin, which, given the physical anthropological evidence, would have to be somewhere in the Near East. Unfortunately, as we will argue below, the whole issue of Aurignacian origins is, in scientific terms, meaningless, and cannot be brought to bear on the issue of the emergence of modern humans in Europe.

Otte and Kozłowski (this volume) contend that the Bachokirian, or Pre-Aurignacian, as represented in levels 4/B-4/C of Temnata (dated to ca.38-39 kyr BP) and contemporary levels II/IV-II/III of Bacho Kiro, is an early, incipient form of the Aurignacian. According to these authors, at Temnata, “typical carinated endscrapers start to appear in level 4/A, but in levels 4/B and 4/C there are already nosed scrapers (Fig. 1)”. Such a presence would substantiate their argument about a gradual increase in the frequency of Aurignacian types from the bottom to the top of these sequences, but inspection of their Fig. 1 suggests otherwise. Carinated and thick-nosed items are indeed found in level 4/A of Temnata, but not before (at least, not in the illustrations supplied). This is also the case with the levels of Bacho Kiro (Kozłowski, 1982) that are earlier than ca.35 kyr BP, as we had already pointed out in our 1999 review of the evidence from these sites. Therefore, we maintain our opinion that the Aurignacian appears in the Balkans at about the same time as everywhere else in its central and western European range.

Otte and Kozłowski also believe that Aurignacian-like assemblages exist in the Altai which could be even earlier than the Bachokirian. They suggest that this part of central Asia, and especially neighboring Afghanistan, might correspond to the region of origin of the technocomplex, which would have spread from there into the Near East and then Europe. During the process it would have become more and more “Aurignacian-like”, to the point that its “Aurignacian-ness” is best recognized at the westernmost end of the range, not at the point of origin.

There are two problems with this view, one empirical, the other logical. The first is that there are no dates for this putative Altai Aurignacian earlier than those available for Europe

and the Near East. The second is that no criteria are supplied for the recognition of the “Aurignacian-ness” of lithic assemblages in the absence of diagnostic Aurignacian items. In some passages of their contribution, Otte and Kozłowski seem to suggest a view of “becoming Aurignacian” that mistakes the processes of cultural change for those of biological evolution: initially, thick blanks are shaped by lamellar retouch and, over the millennia, these kinds of artifacts gradually evolve into true carinated scrapers-cores for the production of bladelets. However, the morphology of stone artifacts (be it intended or accidental), is imposed from the outside as a result of the manipulation of raw materials by people who are themselves, not the stones they knap, the subject of natural selection and evolutionary change. Carinated reduction schemes, therefore, do or do not exist, and a reduction scheme either is or is not carinated technology; there can be no such thing as a reduction scheme that is 25%-, 50%- or 75%-carinated technology. Put another way, there can be frequency shifts in the extent to which carinated reduction schemes are used, but not in the extent to which a specific reduction scheme is or is not carinated technology.

It is clear that a production system has to be invented and ameliorated, but anyone familiar with stone tools will realize that the amount of time involved in the process must be in the order of magnitude of the minutes, the days, or, at most, the weeks that an experienced knapper would need to perfect the technology, not the centuries or the millennia of duration of Otte and Kozłowski’s process of “constitution of the Aurignacian”. Moreover, these authors operate under the concept that the widespread geographical range of this technocomplex is an artifact of diffusion, not of convergence resulting from independent invention. In that case, gradualism in the distribution of carinated technologies would be expected in terms of the horizontal dimension of the process (i.e., in how fast was the neighbor-to-neighbor transfer of the know-how), not in terms of its vertical dimension (i.e., in how long it took for the Aurignacian to become Aurignacian).

If one works under these assumptions, then the whole problem of the origins of the Aurignacian is not a problem at all, simply because, given the lack of resolution of our time scales, it cannot be resolved. As the controversy on the significance of the series of radiocarbon dates for the Geissenklösterle illustrates so well, we can at best work with units of time whose length is in the range of one to five millennia. In contrast, the amount of time required for an individual to invent a system that works and for him, his fellow band-members and the rest of their relations to test it, to ameliorate it, and to spread it, is, at most, in the order of magnitude of a few generations. In the same amount of time, a technological innovation, because it is advantageous or simply because it is fashionable, can spread over the open exchange networks of hunter-gatherers across thousands of kilometers, as far as the network extends or as far as people stop finding it advantageous or fashionable. The extension covered by the diffusion process may be delimited by the fact that different environments (and, hence, different adaptations) are reached, by the fact that the innovation in question somehow does not fit the cultural system in place from a certain point onwards, or because some cultural, biological or physical geographical barrier forms an insurmountable obstacle, and hence, limit, to the process. Once the obstacle disappears, the innovation can extend into those regions, and if the obstacle lasted for a sufficient amount of time, its existence may be identifiable archeologically as a lag in the pattern of radiocarbon dates; this is, for instance, the underlying mechanics of the “Ebro frontier” model (Zilhão, 1993, 1997, 2000).

In such a theoretical context, it comes as no surprise that the Aurignacian appears in the archeological record all of a sudden and with no clearly identifiable predecessors (i.e., with no identifiable “origins”). The same thing happens with all other technocomplexes of the early Upper Paleolithic, the Solutrean being a good case in point. As extensively argued by one of

us (Zilhão, 1997), the scientific interest of the Solutrean (or, by the same token, of the Aurignacian) is not where exactly it originated but that, once it originated, wherever that was, it spread through an extensive geographical range in a way that, with available time scales, appears “instantaneous” to our eyes. Rather than a “problem”, this fact is instead a major source of information on the demographic and social properties of the human occupation network in place during that specific time interval and in that specific geographical range. We believe that mining such information is what should drive archeological research on the Aurignacian and the Transitional complexes in the first place, not the search for origins.

## Conclusion

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A further implication of the above is that the scope of origins-oriented research is by logical necessity crippled by the paradox that the more we will learn about the details of a given regional archeological record, the more we will be forced to reject that such a region contains the putative ancestor we are seeking to establish. That is why, if we follow the intellectual history of the discipline, whenever an origins issue is raised in a context where adequate chronometric frameworks are lacking, the most common trend is to look for the answer elsewhere; innovations are assumed to come from the outside and, most importantly, given the logic of the argument, from some place where the relevant archeological record is non-existent or poorly known. In the first half of the last century, from an Aquitanian perspective, the origin of the Aurignacian was sought in southeastern France, whence the search then moved on to the Near East, the Balkans, Anatolia and central Asia, farther and farther away, as our knowledge of the different regions increased.

This mechanism has turned out to be advantageous for the discipline, in that it has led archeologists to explore the riches of new places, and so this kind of “Red Queen race” (Carroll, 1872; Ridley, 1993) has continued unabated. In the case of the Aurignacian, however, there seems to be good reason to believe that the race has finally come to an end. Now that its origin is placed in Afghanistan, there is little chance that, in the foreseeable future, a local investigation of the hypothesis will force the search into even more remote places. Since accepting an Afghan solution for the conundrum of Aurignacian origins has the undeniable heuristic advantage of allowing research efforts to concentrate in what should be the interesting questions, we propose that the discipline should settle for it... and concentrate on what, based on the different papers brought together in this volume, would seem to be the top priorities for future research: achieving a better understanding of the Transitional complexes and the instances of late Middle Paleolithic survival, as well as establishing a precise and widely accepted calibration curve for radiocarbon dates in the critical time range of ca.30-45 kyr BP.

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<sup>1</sup> Otte and Kozłowski (this volume) write: “The question that has caused most of the debate, and that has been the primary motif of the “*nouvelle bataille aurignacienne* (we do not dare to say the ‘last *bataille aurignacienne*’ – the phrase that Zilhão and d’Errico used, 1999, p. 57)”. The implication seems to be that we went too far in presuming to have solved the issue with our 1999 paper. In fact, Otte and Kozłowski omit the question mark which, in the text they cite, is placed at the end of their quote. That subtitle in no way conveyed the meaning they imply, as is further revealed by the French version of our review, published in *L’Anthropologie* under the title of “*La nouvelle bataille aurignacienne*” (Zilhão and d’Errico, 2000). In fact, therefore, it turns out that the expression they prefer is originally our own too.

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