

Châtelperron: Theoretical Agendas, Archaeological Facts, and Diversionary Smoke-Screens

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COMMENTARY ON ZILHÃO ET AL. (PALEOANTHROPOLOGY 2008: 1–42)

When we published our response to the earlier (2006) attack by João Zilhão and his colleagues on the Châtelperron stratigraphy in the *Proceedings of the National Academy of Sciences* in January 2007, we never expected that they would take this conclusive refutation of their extraordinary “excavation backdirt” model quietly. And of course we were right. In our earlier paper we presented a series of about eight separate arguments bearing on the “backdirt” interpretation for Henri Delporte’s repeatedly published (1952, 1955, 1957, 1964, 1999) stratigraphy at Châtelperron, any one of which, in our view, was sufficient to make the entire backdirt model both untenable and, frankly, bordering on the archaeologically bizarre. Nevertheless they have now returned with a further series of arguments which (insofar as we can understand them) are even more extraordinary than those presented—and subsequently refuted by us—in their earlier paper.

It must be recognized at the outset that the present paper is, explicitly and transparently, an attempt to defend, and to scientifically “salvage,” three theoretical viewpoints which Zilhão and d’Errico had already published in a number of earlier papers, before our own critique of their recent “backdirt” model for the stratigraphy reported by Delporte at Châtelperron was published (in *PNAS*) in 2007—namely:

1. That there cannot be any “interstratifications” between Châtelperronian and Aurignacian occupations, either at Châtelperron, or any other sites;
2. This of course follows directly and inexorably from their long-standing belief that there was no significant period of coexistence between Neanderthal and modern human populations in western Europe, and therefore, that any suggestion of mutual interaction, influence, or “acculturation” between late Neanderthal and early modern human populations cannot be contemplated; and,
3. That we must now accept that the appearance of a range of distinctively “modern” (Upper Palaeolithic-like) features among the final Neanderthal (i.e., Châtelperronian) populations in France

must be taken as a clear proof that the late Neanderthal communities in western Europe independently and spontaneously “invented” a range of distinctively “modern” cultural features both substantially prior to, and without any influence from, the new, intrusive populations of anatomically and behaviorally modern humans who are known to have been expanding rapidly across Europe during the period between ca. 43,000 and 35,000 bp (in uncalibrated radiocarbon terms; all radiocarbon dates cited below are given in uncalibrated terms).

All of these pre-existing beliefs and theoretical viewpoints can be traced and documented clearly in a succession of papers by Zilhão and d’Errico spanning a period of ten years between 1998 and the present day (d’Errico et al. 1998; Zilhão and d’Errico 1999, 2003; d’Errico 2003; Zilhão 2001, 2006 etc.). Seen in these terms, the passionate defence of their arguments for a lack of evidence for interstratification between Châtelperronian and Aurignacian levels at Châtelperron, and their remarkable range of arguments for their own “excavation backdirt” model for Delporte’s reported stratigraphy at Châtelperron—which are now re-presented in the present article—must be seen as an entirely expected and predictable attempt to salvage their long-standing views on the whole of the now notorious “Châtelperronian acculturation” issue, which has raged through the paleoanthropological literature over the past decade. The extraordinary range of new arguments presented in the present paper is, in our view, inexplicable and incomprehensible, unless viewed within this historical perspective.

So what exactly *are* these new arguments, presented at great length and, in our view, almost incomprehensible complexity, in the present article? We believe that the issues they raise are in fact vastly more simple than they would have us believe, and can be dealt with at much shorter length than in their own extended narrative. As briefly as possible, we will summarize and comment on their arguments as follows.

THE IMPOSSIBILITY OF THE “EXCAVATION BACKDIRT” MODEL

First, the basic and fundamental premise of their “excavation backdirt” model for Delporte’s (1952, 1955, 1957, 1964, 1999) repeatedly published stratigraphy at Châtelperron is that Delporte somehow totally failed to recognize the inevitable contrasts between fully *in situ* archaeological occupation horizons and associated geological deposits formed on the site around 40,000 years ago and the unstructured, unstratified “backdirt” of 19th century excavations carried out on the site only 80 years before the time of his own excavations. From everything that has been published so far it must by now be clear that this suggestion is effectively bordering on the preposterous. Delporte’s publications make it clear that in all four years of his excavations (between 1951 and 1954) he was effectively obsessed with the goal of identifying any surviving remnants of the original, intact deposits which had escaped the 19th century excavations, and dug extensively into both the central parts of the site and the cave interior in order to locate such deposits—as shown clearly in his 1955 and 1957 site plans (Figure 1; see also Zilhão et al. Figure 6, this volume) and, of course, as anyone embarking on the re-excavation of a previously excavated site would inevitably do. As we commented in our last paper, even an amateur excavator should be capable of recognizing major stratigraphic and depositional distinctions of this kind, at least in contexts where the two sets of deposits are immediately juxtaposed. To impute this level of incompetence to an archaeologist who, immediately after his work at Châtelperron (in 1955) went on to conduct some of the most respected and sophisticated excavations at a range of other major Middle and Upper Palaeolithic cave and rock shelter sites in France (Le Facteur, La Rochette, La Ferassie, Brassempouy: cf. Movius 1998: 308) is to us totally incomprehensible (see Delporte’s Competence section below). Put differently, we have here a situation where six archaeologists who have never put a spade or trowel into the Châtelperron site are claiming to be able to make—*post facto*—a more informed and authoritative judgement on the differentiation between immediately adjacent *in situ* and excavation infill deposits on the site, than that made by a respected prehistorian who conducted five successive seasons of personal excavations on the site. We offer no further comment on this extraordinary claim.

ILLUSORY STRATIGRAPHY?

A second fundamental premise of their “backdirt” interpretation is that Delporte in some way deluded himself into observing and recording a complex and detailed sequence of at least five separate “sub-horizontal” archaeological horizons within his main excavated area (in the so-called “*palier sud*” trench). Each was marked by a distinct horizon of heavily “reddened” sediments, which contained the great bulk of the archaeological and faunal material, and each was separated by intervening paler and archaeologically almost sterile zones—all extending continuously over a distance of at least two meters (from east to west) and clearly recorded in both the lateral and transverse sections

of his trench (Figure 2). This stratigraphic pattern was described explicitly and repeatedly in all of Delporte’s publications on the site, extending from his initial report in *Gallia* published in 1955 down to his final summary of the site published in 1999 (Delporte 1955: 81, 1957: 456-7; 1964: 11; Delporte et al. 1999: 8-10). What form of professional self-delusion—or alternatively outright scientific dishonesty (see below)—can one visualize to account for this perceived, clear-cut stratigraphy in what Zilhão et al. now interpret as a succession of totally unstructured and unstratified 19th century excavation backdirt deposits? Leaving aside Delporte’s own “competence” and “authority” in this context, Delporte reports (1952: 1, 1955: 84) that he was “guided and aided” in the excavations by the regional Director of Antiquities and two Professors of Archaeology (R. Nougier and Malvesin-Fabre), all of whom were presumably similarly deluded into observing this “phantom” stratigraphy in a succession of totally unstructured backdirt deposits.

19TH CENTURY EXCAVATION LIMITS

Third, an entirely new, and even more bizarre, allegation in the present article is that while Delporte did indeed clearly recognize and carefully plot a relatively sharply defined interface between *in situ* and 19th century backdirt deposits during his excavations (as Zilhão et al. clearly accept at numerous points in their article and associated illustrations) he somehow—amazingly—*reversed* the true significance of this clear depositional and stratigraphic interface, and proceeded to interpret the intact, *in situ* deposits as 19th century excavation infill material, and to interpret the immediately adjacent “backdirt” deposits as highly stratified, *in situ* occupation deposits. This is stated explicitly and repeatedly in their article, in the following terms:

“Mellars et al.’s reasoning, however, assumes that the lines drawn by Delporte to identify the area affected by the 19th century excavations represent the **western**, not the **eastern** limits of that area. Delporte (1957: 456), in fact, leaves no doubt that the line described in the caption to his plan as “*limites des fouilles Bailleau*”can only be interpreted as the outer boundary of the area lying between that line and the cave entrance. Delporte’s work was carried out **north** and **south**, not **east** of the area excavated in the 19th century.” (p. 11: emphases as in their text).

Here we must frankly confess that after numerous close and repeated readings of Zilhão et al.’s lengthy discussion of the site stratigraphy, we still find their discussion and attempted justification of their extraordinary allegation so convoluted, verbose, and confusing that we are virtually at a loss to understand it. It reads to us like a classic example of attempting to “control by confusion” or perhaps “blinding by [pseudo] science”—even with the aid of the inordinate number (thirty-three!) of illustrations provided. We must confess that we find many of these illustrations (e.g., Figures 8, 11, 16, 21) so elaborate, complex, and at times self-contradictory (see below) that they do little to clarify their models. Personal discussions with the senior author

1954 site plan

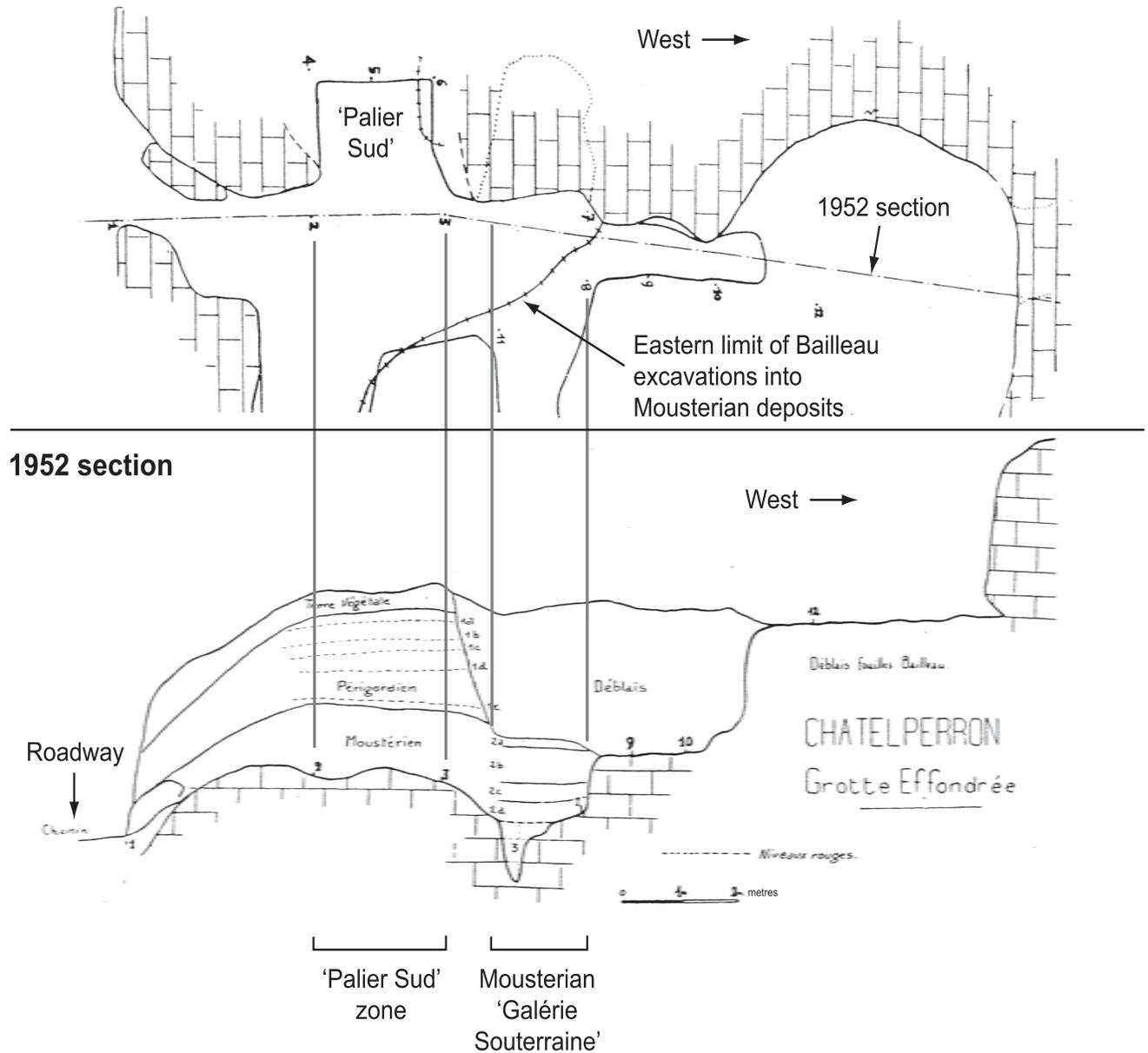


Figure 1. Comparison between Delporte's 1955 site plan (upper) and the section recorded along the southern face of his long, east-west trench excavated in 1952 (lower), reproduced from Delporte 1955, Figures 1 and 2. Note the perfect correspondence between the two drawings in relation to the positions of the different topographic features, and the location of his own and Bailleau's excavations. Note also how the limits of Bailleau's backdirt ("déblais") recorded in his 1952 sections slopes diagonally upwards from right to left (i.e., west to east) and is clearly banked up against the *in situ* deposits in Delporte's palier sud trench. (See also Zilhão Figures 6, 8, 13).

(João Zilhão: 22 November 2007) also have failed to clarify this. Nevertheless, the explicit conclusion of their analysis is that Delporte consistently failed to differentiate between a series of *in situ*, intact archaeological and geological deposits and the unstructured, unstratified infill of the 19th century excavations, throughout the whole of his excavated areas. Despite identifying a clear contact zone between these two sets of deposits over a large area of the site (as Zilhão et al. clearly accept), they maintain that Delporte

consistently mis-interpreted the 19th century infill material as representing highly stratified occupation deposits, and interpreted the original, *in situ* deposits, as 19th century excavation backdirt. As noted above, the degree of archaeological incompetence, observational blindness, or outright dishonesty implied in Delporte's observation and recording of the actual stratigraphy of the deposits virtually defies belief.

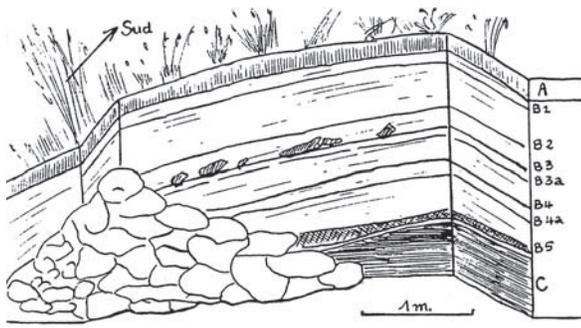


Figure 2. Delporte's isometric sections of the deposits in the "pallier sud" trench, as recorded at the end of his excavations in 1954. Note that there is an error (foreshortening) in the drawn length of the meter scale. All of the other plans and sections of the site (Figures 1 and 3) confirm that the east-west width of the trench was c. 2.0 meters. Reproduced from Delporte 1957: Figure 2; see also Delporte et al. 1999: p. 11.

The fundamental confusion in Zilhão et al.'s latest interpretation clearly derives from their misunderstanding of the "limites des fouilles Bailleau" as recorded on Delporte's main (1955 and 1957) published plan of his own and Bailleau's excavations (see Figure 1; Zilhão et al. Figure 6, this volume). Delporte is at pains to emphasize (1955: 79–81) that this line representing the "limits" of Bailleau's excavations was recorded not at the top of the full sequence of deposits excavated by Delporte but specifically at the base of the Upper Palaeolithic (i.e., Châtelperronian) sequence, that is, at the surface of the underlying Mousterian deposits (Delporte's Level C). Zilhão et al. accept (p. 38, this volume) that the very different color of the Mousterian deposits would have made the limits of Bailleau's excavations into these deposits easy to define, and makes their own strange confusion on this score incomprehensible. As illustrated in Figure 3, it would have been a simple matter of inspection on Delporte's part to see whether Bailleau's intrusion into the Mousterian deposits lay to the east or west of the recorded interface zone, readily apparent in both the recorded sections through the deposits and the sharp contrasts in colour between the *in situ* Mousterian deposits (yellow) and the infilled (darker) backdirt deposits, when seen in plan. Interestingly (and curiously) the lower right-hand section depicted in Zilhão et al.'s Figure 21 (this volume) illustrates this point perfectly.

Delporte's plan is recording, in fact, that the only areas in which Bailleau excavated significantly into the basal Mousterian levels lay to the north and west of his 19th century excavation "limits" — that is, mainly within the interior part of the cave, towards the north and west of the main sequence of Châtelperronian deposits, within Bailleau's so-called "foyer" area which he excavated (over an area of around 24m²) in the central parts of the site (see Figure 3; Zilhão et al. Figures 6 and 16, this volume). This would make perfect sense in archaeological terms, since Delporte clearly records that, in his excavations, the basal Mousterian deposits were relatively thin and very poor in artifacts and fauna in the central and eastern parts of the site, and

only became much thicker and very much richer (at least in terms of faunal remains) further to the west, towards the cave interior. Delporte himself could only locate a small area of these intact, Mousterian deposits within the cave interior (in what he referred to as the "Galérie souterraine" along the southern wall of the cave: see Figure 1) but he did indeed record that these deposits contained not only three Mousterian cordiform bifaces and several other tools (Delporte 1955: 81–84), but above all a very rich assemblage of faunal remains, amounting to at least 260 identifiable specimens (as recorded in Jean Bouchud's analysis of the fauna recovered during Delporte's excavations)—very much richer than the total recorded for the whole of the overlying Châtelperronian levels in the sequence (Bouchud 1957, 1961). It would therefore make perfect sense that Bailleau should have concentrated his excavations into the basal Mousterian deposits primarily within the areas inside the cave (presumably to recover the maximum number of faunal specimens), exactly as recorded by Delporte's plot of the eastern limits of Bailleau's excavations into the basal Mousterian levels, in his three site plans (see Figures 1 and 4).

A further proof of all this is that when Delporte *did* excavate extensively into the deposits inside the cave interior in the search for intact occupation deposits (as the published plans of his excavations clearly reveal) he could only locate undisturbed Mousterian deposits within the one small area of the "Galérie souterraine", referred to above, along the southern wall of the cave (Figure 1 lower; Zilhão et al. Figure 31, this volume). Clearly, in Delporte's view, most of the remaining Mousterian deposits within the cave interior had already been dug out during Bailleau's excavations, exactly as Delporte's drawn plan of the Bailleau "excavation limits" (see Figures 1 and 4) clearly indicates. Since Bailleau is said to have completed his excavations in the central "foyers" zone of the site by the end of 1869 (Zilhão et al., p. 14, this volume) one can only assume that during the two remaining years of his excavations (in 1870 and 1871) he was indeed excavating extensively into the cave interior, as Zilhão et al. (p. 14, this volume) appear to admit. Interestingly, they themselves argue (p. 36) that "Even if he had indeed explored them [i.e., the Mousterian deposits in the cave interior] significantly, his chances of finding any artifacts...would have been very small"—so one can hardly take the relative scarcity of Mousterian artifacts in the surviving museum collections as an argument against "extensive excavations" by Bailleau into the Mousterian levels in the cave interior. Finally, one might add that the direction of the shading attached to Delporte's line of Bailleau's excavation "limits" as recorded in his later (ca. 1964) site plan (see Figure 4) leaves no doubt that he did indeed regard the deposits lying to the west of this line as representing the areas already excavated by Bailleau, and not those to the east of the line, as Zilhão et al. now attempt to argue. When seen in these terms, Zilhão et al.'s own reconstruction of the total extent of Bailleau's excavations on the site (as shown in their Figures 8, 16, and 21) becomes entirely untenable.

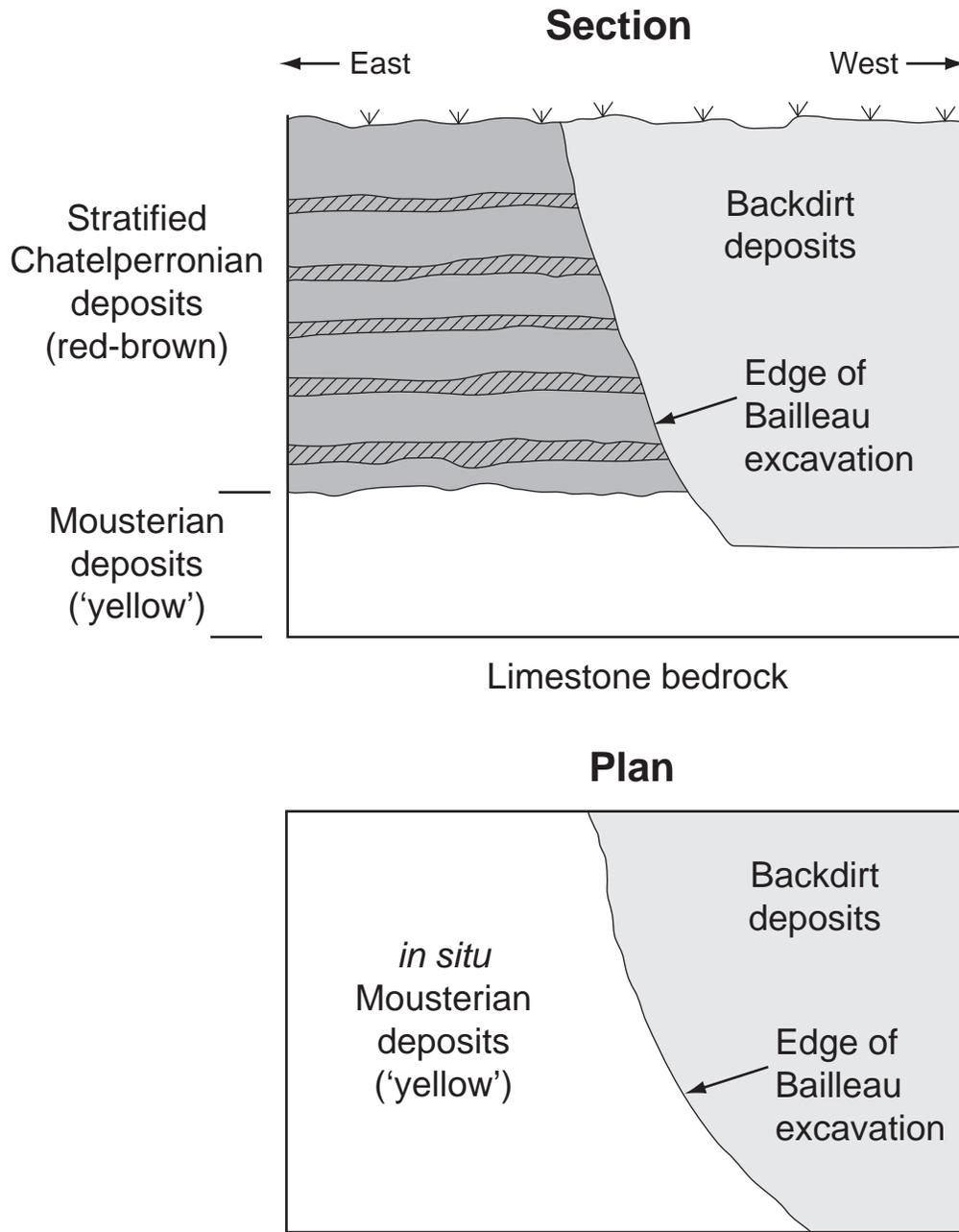


Figure 3. Diagrams to illustrate how the “limits” of Bailleau’s excavations into the basal Mousterian deposits at Châtelperron would have been readily visible to Delporte, in both his recorded sections and in the clear horizontal contrasts between the intact (“yellow”) Mousterian deposits and intrusive (darker) “backdirt” deposits visible at the surface of the Mousterian deposits at the base of his trenches. In both the stratigraphic sections and the horizontal plans of the deposits it would have been totally explicit and inescapable to Delporte that the area of Bailleau’s excavation into the Mousterian deposits (subsequently infilled with excavation backdirt material) lay to the west and not to the east of his recorded interface zone. Note that the lower right-hand section illustrated in Zilhão et al.’s Figure 21 (this volume) depicts this situation perfectly!

In this context, Zilhão et al.’s allegations (in their Figure 16, this volume) that “in the basal Châtelperronian, bedrock would have outcropped immediately to the west of Bailleau’s excavation limits,” and their reference to “bedrock at the surface of the Mousterian” are both incomprehensible and self-contradictory. How could Delporte have identified the “limits” of Bailleau’s excavations into the

basal Mousterian levels (as they clearly accept and reiterate at many points) if bedrock outcropped at the “surface of the Mousterian deposits”? Indeed, how could there be any Mousterian deposits at all *within* the basal “bedrock” formation? Even if Delporte’s long east-west trench encountered bedrock (or the rock wall of the cave) towards the western end of the trench at a level coinciding roughly

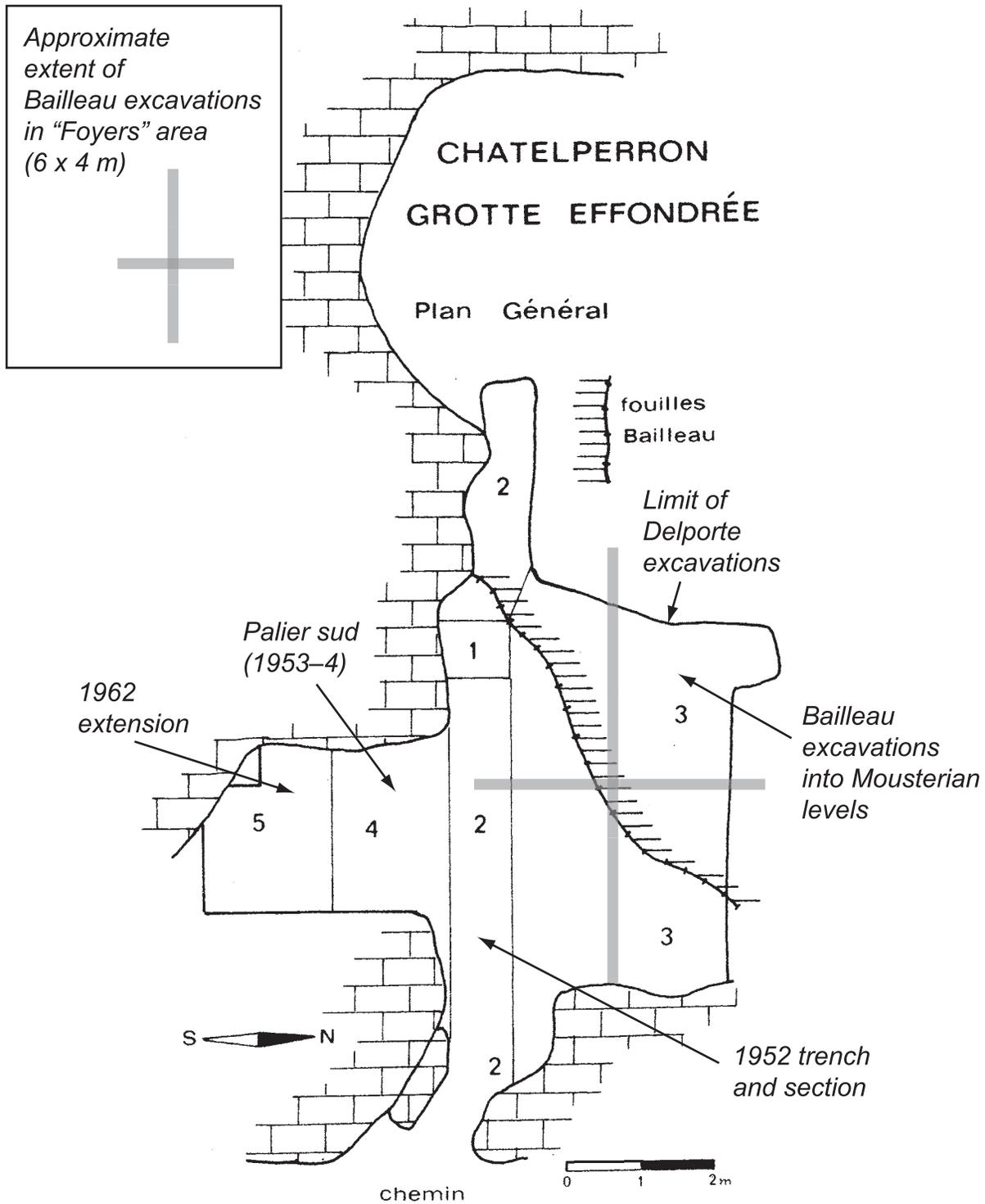


Figure 4. Delporte's ca. 1964 plan of the Châtel Perron ("Grotte Effondrée") excavations in the archives of the Musée d'Archéologie Nationale. Our own annotations on the plan are indicated in italic script. The probable (approximate) extent and location of Bailleau's (ca. 6 m x 4 m) excavations in the central "foyers" zone is indicated by the crossed, grey-tone lines. Compare this with the reconstruction proposed by Zilhão et al. in their Figures 16 and 21 (this volume).

with the surface of the adjacent Mousterian deposits (see Figure 1; Zilhão et al. Figure 6, this volume), there must have been intact Mousterian deposits directly to the west and north of this line (i.e., within the northern and western

areas of the adjacent cave interior) in order for Delporte to record the "limits" of Bailleau's excavations into the Mousterian deposits at all.

The clear implication of all this is that Bailleau did *not*

excavate substantially below the base of the Upper Palaeolithic levels within the southern and eastern parts of the site, since (as Delporte himself clearly recorded) the basal Mousterian levels in these areas were both relatively thin and contained very few Mousterian artifacts and (apparently) faunal remains. In these areas it would seem that Bailleau chose to confine his excavations to the extremely rich overlying Châtelperronian levels within his main central “foyer” zone, and to leave any seemingly sterile Mousterian deposits at the base of the sequence largely untouched—as again is clearly reflected in Delporte’s drawn section of the long, 1952, east-west trench through the deposits (see Figure 1; Zilhão et al. Figure 6, this volume). As noted above, Bailleau’s excavations into the Mousterian levels in the site were clearly concentrated mainly toward the western and northern parts of the immediately adjacent cave interior.

As a final explicit confirmation of this interpretation one can refer again to the long section recorded by Delporte (1955: Figure 2) along the main southern face of his long east-west trench excavated in 1952 (see Figure 1). This shows clearly that the deposits which Delporte interpreted as 19th century infill deposits from Bailleau’s excavations sloped diagonally upwards from west to east, where they are clearly banked up against (and therefore stratigraphically overlie) the deposits which Delporte interpreted as fully intact, *in situ* Châtelperronian deposits, and which he subsequently excavated as his main “palier sud” trench (see Figure 1). This slope is indeed clearly indicated in all of Zilhão et al.’s own reproductions of the site stratigraphy (in their Figures 6, 8 and 13). To reverse the significance of this depositional interface—as Zilhão et al. now propose—would require the stratigraphic impossibility that the “infill” deposits sloped diagonally *beneath* the *in situ* deposits, to account for the clearly diagonal slope recorded in Delporte’s drawn section of this interface. Indeed, Delporte emphasized in his text (1955: 79–81) that the “limits” of Bailleau’s excavations (and associated backdirt) encroached laterally outwards from the level of the Mousterian deposits into the overlying Châtelperronian deposits—exactly as Delporte records in his long drawn section. All of this is totally in contradiction to Zilhão et al.’s interpretation that Delporte somehow reversed the true stratigraphic situation, by regarding the deposits to the east of this excavation interface as *in situ* deposits and those to the west as 19th century excavation infill. The alternative, of course, would be to suggest that Delporte effectively “faked” all of his stratigraphic records—including the character and slope of the recorded interface between 19th century backdirt and the adjacent *in situ* deposits—seemingly as part of a coordinated campaign to bolster his own preconceived interpretation of the site’s stratigraphy (see Delporte’s Competence section below).

BAILLEAU’S “FOYERS”

Fourth, Zilhão et al.’s assertion (pp. 12–15, and Figure 16, this volume) that Bailleau could not have excavated the area of the rich “foyer” deposits in the central part of the site over an area of ca. 6m x 4m *without* penetrating into the area of Delporte’s “palier sud” is equally incomprehensible.

In fact, Delporte’s own plans (1955: Figure 1; 1957: Figure 1; ca 1964: unnumbered: see our Figures 1 and 4) show that an area of almost exactly this size existed in the central part of the site (in front of the main cave entrance) in which Bailleau quite clearly carried out the bulk of his excavations into the so-called “foyer” deposits (see Figure 3). To have extended his excavations into the area of Delporte’s palier sud would have involved pursuing the excavations into a small peripheral zone (described by Zilhão et al. themselves [p. 24, Figure 21 caption, this volume] as a small “recess in the southern wall of the cave”) located between two major rock outcrops, exactly as described and illustrated in Delporte’s plan. Extending into this zone would have increased the total area of Bailleau’s excavations by at most ca. 4m², compared to the area of around 24 m² which he had already excavated in the main, central (“foyers”) parts of the site, and which produced the extremely rich concentration of Châtelperronian artifacts and associated faunal remains described in his published accounts (conveniently reproduced in Figures 14 and 15 of Zilhão et al.’s paper, this volume). Incidentally, Zilhão et al.’s assertion (p. 12, this volume) that unexcavated deposits in the palier sud zone would have constituted a “baulk” preventing the removal of excavation backdirt from Bailleau’s excavations into the adjacent roadway is incomprehensible (except in terms of their own mistaken interpretation of Bailleau’s excavation “limits”) since they themselves point out (p. 12) that the exit from the “foyers” zone into the roadway was constricted by two major bedrock outcrops, separated by a space of less than one meter at the extreme eastern end of the site (see Figures 1 and 3). In fact, an unpublished section in Delporte’s 1964 site report (see Zilhão et al. Figure 7b and Figure 31, this volume) strongly suggests that Bailleau’s backdirt from his 1869 excavations in the “foyers” part of the site was initially dumped *inside* the cave interior—to a depth of almost two meters—before some of these deposits were subsequently transferred (presumably into the trenches already opened within the foyers area) in order to pursue his excavations into the Mousterian levels within the cave interior, in 1870 and 1871. The two other alternative locations for the 1869 backdirt dumps depicted in their Figure 13 are, as they admit, effectively inconceivable.

THICKNESS OF CHÂTELPERRONEAN DEPOSITS

Fifth, a final conclusive refutation of the whole of the Zilhão et al. “excavation backdirt” model has now emerged from Zilhão et al.’s own analysis of the original published accounts of Bailleau’s excavations at the site, presented (unobtrusively) in footnotes 10 and 12 of their paper, and reproduced *verbatim* in their Figures 14 and 15. These report unequivocally that in Bailleau’s excavations within the central “foyers” zone, the Upper Paleolithic (i.e., Châtelperronian) levels were found to extend over a total depth of “over one meter,” and that the richest part of this sequence—containing a series of clearly structured hearths at several different levels—occupied a depth of ca. 75cm. Zilhão et al. appear to have missed the fact that these depths correspond almost

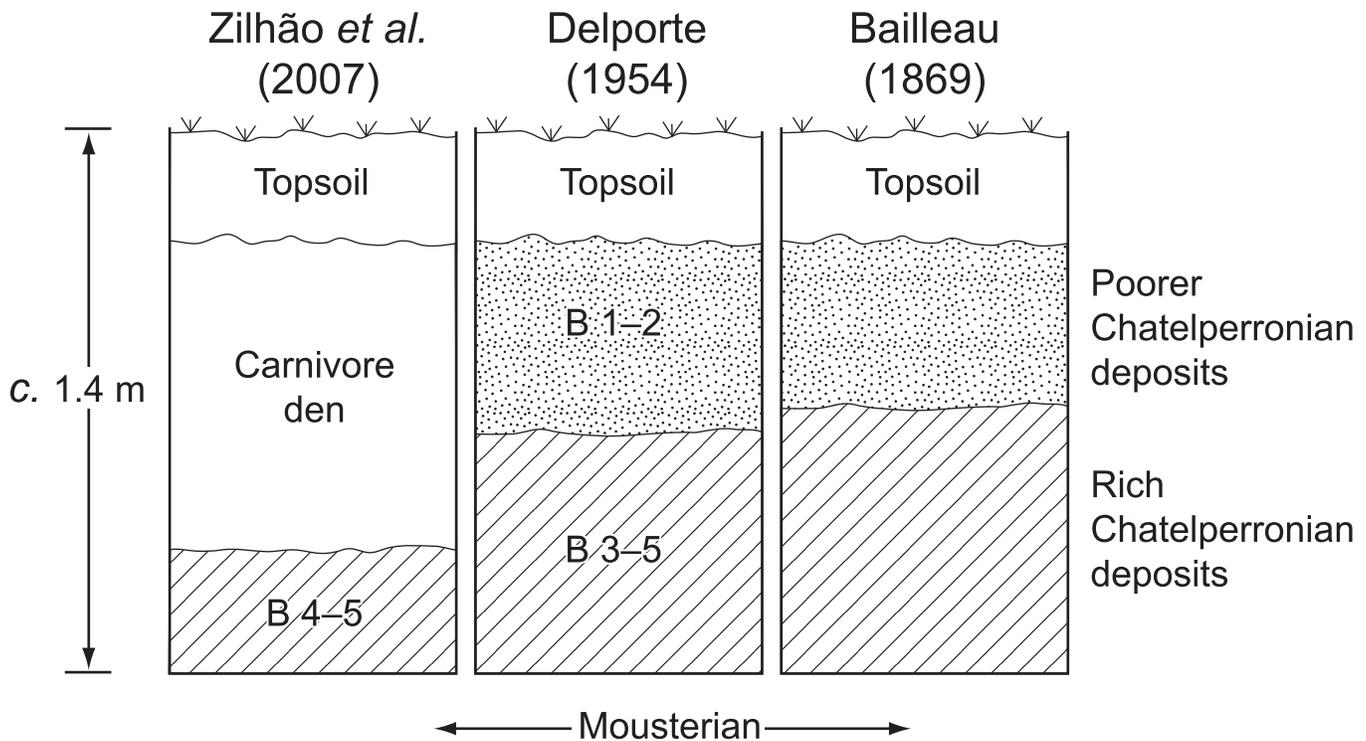


Figure 5. Diagrammatic comparison between the stratigraphic sections recorded in the nineteenth century excavations by Bailleau (in his “foyers” zone: right), that recorded by Delporte in his “palier sud” trench (center), and the new interpretation of the stratigraphy proposed by Zilhão et al. (left). Note how Delporte’s recording and interpretation of the stratigraphy coincides almost exactly with that described by Bailleau, while the new interpretation by Zilhão et al. implies a sudden conflation in the thickness of the Châtelperronian occupation levels from ca. 1m in Bailleau’s excavations to ca. 35cm in the immediately adjacent areas of Delporte’s excavations, directly to the south.

exactly to those recorded by Delporte (1955: 81, 1957: 456–7, 1964: 11; Delporte et al. 1999: 11–12) for the whole of the stratified sequence of Châtelperronian levels (his Levels B1 to B5) within his excavations in the palier sud zone—i.e., according to Delporte’s accounts and drawn sections, a total of ca. 110cm for the entire B1–B5 sequence, and a depth of ca. 65–70cm for the richest Châtelperronian deposits in Levels B3–B5 (Figure 5). Zilhão et al. accept, and emphasize at several points in their text, that Delporte’s excavations in the palier sud were located (according to their own reconstructions: Figure 16) immediately adjacent to Bailleau’s excavations in the central “foyers” zone. All of this accords perfectly with Delporte’s conclusion that the whole of his stratified B1–B5 Châtelperronian succession in the palier sud represented the direct, lateral equivalent of the long sequence of similar Châtelperronian levels excavated by Bailleau in his main foyers zone. What these observations directly contradict is Zilhão et al.’s own interpretation that only the deposits at the base of the Delporte’s palier sud deposits (i.e., his Levels B4 and B5, which he recorded as occupying a total depth of only ca. 35cm) represent the stratigraphic equivalent of Bailleau’s entire Châtelperronian sequence, while the whole of the upper levels of Delporte’s section (his Levels B1–B3—occupying a further 70cm of

deposits) represent entirely *post* Châtelperronian deposits, consisting entirely of the backdirt of Bailleau’s excavations. Since they fully accept that Delporte’s excavations in the palier sud lay immediately adjacent to Bailleau’s excavations in his main foyer zone (see their Figure 16), this massive discrepancy in the thickness of the Châtelperronian deposits in the two immediately adjacent excavations remains totally unexplained—and clearly inexplicable in terms of their current “backdirt” model. The only escape we can see from this obvious contradiction would be for Zilhão et al. to revert to their initial (d’Errico et al. 2006) suggestion that the entire sequence of B1–B5 deposits in Delporte’s palier sud represents the unstratified, unstructured backdirt of Bailleau’s excavation infill—a suggestion which they now accept (p. 38—despite some convoluted equivocations) is highly unlikely. Quite apart from all the other categorical arguments against this model that we have already spelled out in detail, this interpretation would of course leave totally unexplained the sharp separation between the sequence of three radiocarbon dates we secured for Delporte’s Level B5 (40,650±600 to 39,150±600 bp), on the one hand, and the seven dates for the overlying Levels B1–B3 (36,340±320 to 34,550±500 bp), on the other (Figure 6; see also Zilhão et al. Table 4, this volume). In statistical terms, the probability

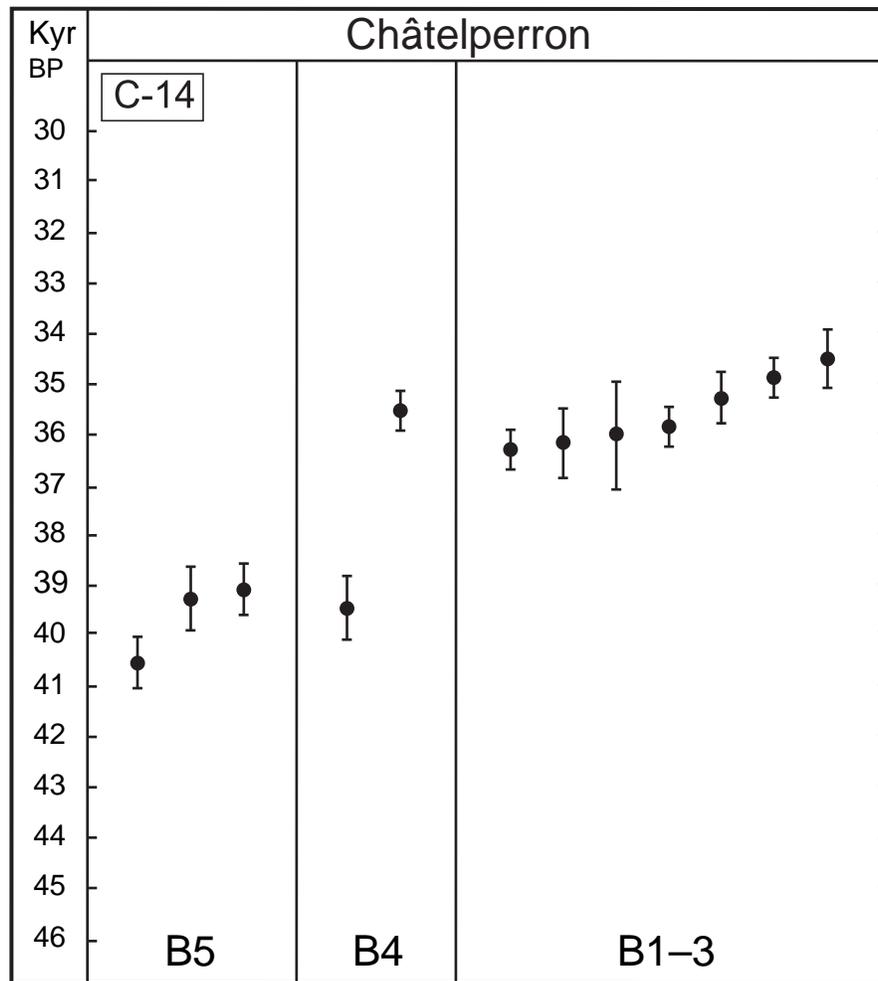


Figure 6. Radiocarbon dates secured on bone samples from Levels B5, B4, and combined Levels B1–B3 from Delporte’s excavations. The dates were produced by the Oxford Radiocarbon Accelerator Laboratory, using the newly-developed “ultrafiltration” techniques of sample preparation. The dates clearly imply a depositional hiatus, spanned by the two dates for Level B4, during which the Aurignacian occupation probably took place. The date of >53,900 bp secured for an obviously displaced bone sample in Levels B1–3 has been omitted. See Gravina et al. 2005; Mellars et al. 2007. For details of individual dates, see Zilhão et al. (this volume) Table 4.

of this situation arising entirely by chance in a totally unstratified sequence of backdirt deposits is of the order of one in 250. Indeed, in an accumulation of backdirt deposits one might expect the date sequence to be reversed! Clearly, all of this provides a further explicit and categorical refutation of the whole of their current “backdirt” model for Delporte’s documented stratigraphy at Châtelperron. We await with interest the convoluted arguments which will no doubt be advanced to explain this otherwise inexplicable contradiction in Zilhão et al.’s reinterpretation of the Châtelperron stratigraphy.

WEST FACE OF THE “PALIER SUD”

Sixth, the one point where we can have slightly more sympathy with Zilhão et al.’s comments relates to the very small area of Bailleau’s presumed excavations recorded in Delporte’s site plan at the extreme western end of his palier sud (Figure 7), and evidently reflected in the photograph of this western face of the palier sud trench, which we dis-

cussed in our earlier article (see our Figure 8 and Zilhão et al. Figure 9, this volume). The point to emphasize here is that although Delporte clearly identified this as a zone of heavily disturbed deposits, evidently reflecting an earlier episode of excavation in this part of the site, there is no reason whatever to assume that this necessarily occurred during the 19th century excavations by Bailleau, and could well represent a later episode of intrusion (perhaps by clandestine “flint hunters”) at any time prior to Delporte’s work on the site in the 1950s. The main point to recognize is that this clearly *was* a zone of disturbed sediments, as Delporte distinctly recorded, and as reflected in the available photograph of this western end of the palier sud trench reproduced in our Figure 8 and Figure 9 of Zilhão et al.’s article, which we discussed in detail in our earlier paper. As discussed further in the following section, we believe it is highly unlikely that Delporte would have pursued any further excavations into any surviving remnants of this western face of the palier sud during his later, enigmatic “1962”

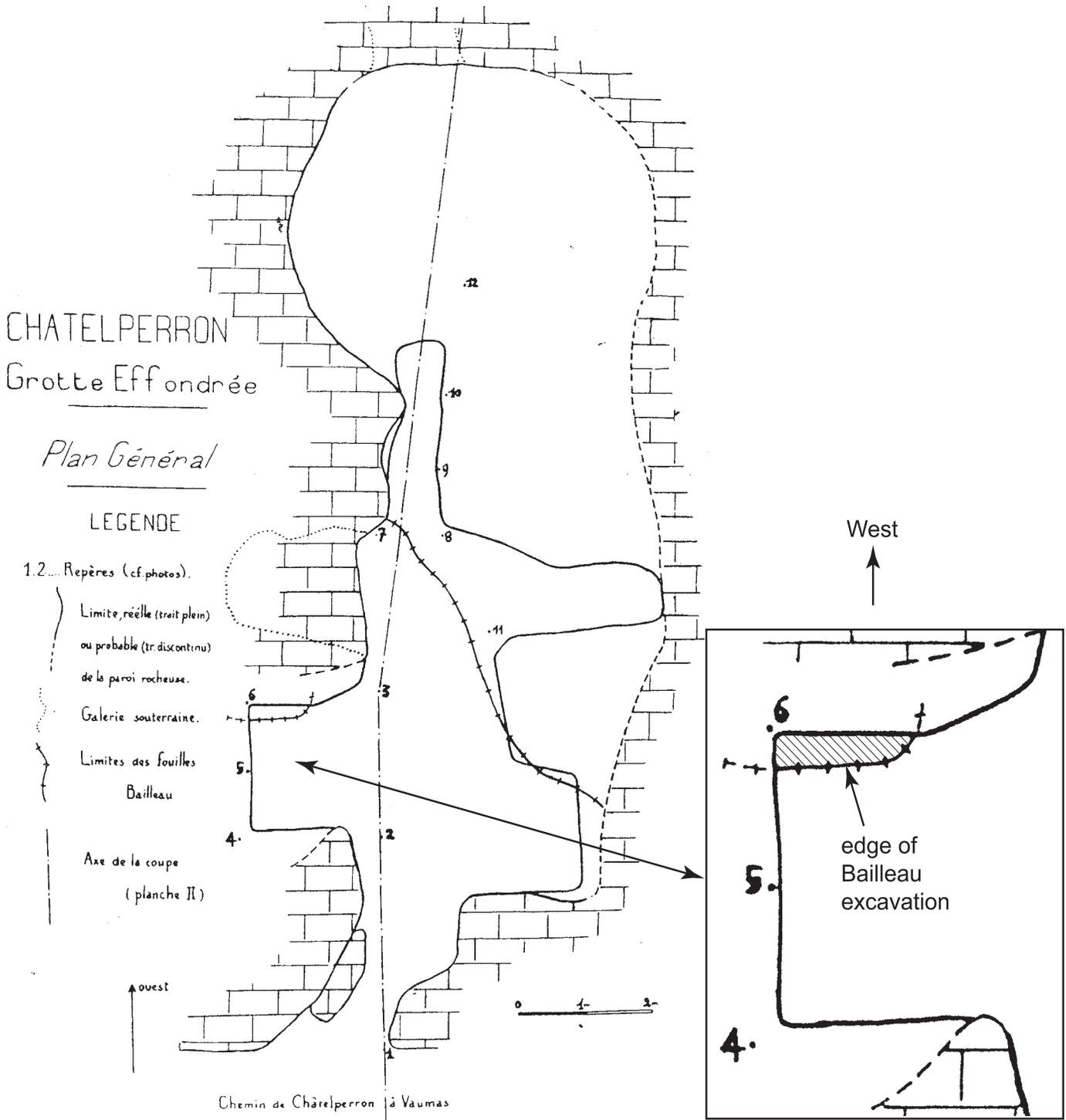


Figure 7. Plan of Delporte's 1951–1954 excavations at Châtelperron, showing (inset) the zone of clearly disturbed, "infill" sediments recorded along the western face of the palier sud trench (see also photograph of this section in our Figure 8 and in Zilhão et al. Figure 9, this volume). Whether this disturbance was caused during the 19th century excavations by Bailleau, or at some other time prior to 1951, remains uncertain. Reproduced from Mellars et al. 2007: Figure 1.

excavations, since (as noted above) he had already recognized this as representing an area of heavily disturbed, presumably "backfill", sediments during the course of his 1953–54 excavations.

Zilhão et al.'s remaining observations on this photo-

graph, where they refer to the potential disturbance caused by the roots of a large tree which penetrated into the uppermost part of the section in the southern face of the trench, seem to us totally irrelevant to the present arguments, although (as they point out: p. 11) it could conceivably have



Figure 8. Photograph of the south and west faces of Delporte’s palier sud excavation. Note how the western face (on the right) appears much looser and more heavily disturbed by roots than the adjacent southern face (on the left). Delporte recorded that the deposits in the southern face were in situ, but the western face contained the infill of an earlier excavation, by Bailleau or otherwise (see Figure 7). Reproduced from Mellars et al. 2007, Figure 1. Compare this with the more selective view of the same photograph reproduced in Zilhão et al. (this volume) Figure 9.

led to some localized disturbance of the deposits in the uppermost (B1–B3) Châtelperronian levels at this particular point in the section, and thereby to some possible intrusion of later (post-Châtelperronian) artifacts into the underlying levels (see below).

Finally, we can see no reason whatever why the section recorded by Delporte along the northern face of what he subsequently excavated as the “palier sud” (i.e., the section recorded in the central part of his long, east-west section along the southern face of his 1952 trench: see Figure 1 lower) should not be closely similar to that subsequently recorded in the southern face of the palier sud trench (see Figure 2), located only two meters further to the south—and according to Delporte’s sections (1955: Figure 2; our Figure 2), showing exactly the same pattern of five “sub-horizontal” and highly stratified Châtelperronian levels as that recorded in the rest of the palier sud trench. Once again, Zilhão et al.’s reasoning and arguments in this context are totally incomprehensible to us—and totally in conflict with Delporte’s own recording of the site stratigraphy in both his 1952 excavations (see Stratigraphic Distribution of Aurignacian Artifacts section below) and the subsequent excavations in the palier sud trench.

Incidentally, Zilhão et al.’s claim (p.11, and Figure 11

caption, this volume) that there is a major conflict between Delporte’s recording of these stratigraphic sections and his published site plans is without foundation (as shown in our Figure 1)—although there is admittedly an error in the drawn length of the meter scale on Delporte’s hand-drawn “isometric” sections of the palier sud excavation in his 1957 paper (see Figure 2). But to employ this small graphical error to undermine the whole of Delporte’s detailed, meticulous, and totally internally consistent records of the site plans and sections must be seen, we believe, as a further desperate, attempt to underpin their extraordinary attack on Delporte’s observations at Châtelperron. Everything in Delporte’s publications and associated illustrations confirms that the east-west length of the palier sud excavation was indeed 2.0 meters, as his three separate site plans of the excavations (Delporte 1955: Figure 1, 1957: Figure 1, and 1964: unnumbered plan) clearly record (see our Figures 1 and 4).

DELPORTE’S 1962 EXCAVATIONS AT CHÂTELPERRON

We come now to the question of the enigmatic 1962 excavations at Châtelperron. Curiously, these excavations re-

ceived only a fleeting reference in the earlier (2006) paper by Zilhão et al., and represent an effectively new element in their analysis of the site. Even more surprisingly, there is only one passing reference to these 1962 excavations in any of Delporte's own publications, in the form of the inclusion of "1962" in the list of dates on which excavations were conducted in the palier sud area, on page 10 of Delporte's otherwise detailed 1964 report. Nor is there any reference to this work in the booklet co-authored by Delporte on the site published in 1999 (Delporte et al. 1999). As Zilhão et al. point out (p. 7), 1962 is "the only year for which we lack published information.

Nevertheless there can be no doubt from the passing reference to "1962" in Delporte's 1964 report that some work in the region of the palier sud area was indeed carried out in 1962, and there are a number of artifacts in the Museum collections which are marked (either on the flints themselves or on associated paper labels) as "Ch. 62." In the sparse museum archives relating to Delporte's excavations there is also a single, uncaptioned plan of the site, which indicates an area (labeled area "5") which extends as a ca. 2x2 meter square immediately to the south of the palier sud area, previously excavated by Delporte in 1953 and 1954 (see Figure 4). Despite the lack of any caption or explanation of this isolated plan, we see no reason to doubt that this does indeed represent the area excavated by Delporte in 1962, as a direct, lateral extension of his earlier excavations in the palier sud in 1953 and 1954. We assume that this new area was excavated by Delporte at the time when he was preparing his (ca. 1964) unpublished "thesis" on the site (cf. Bouchud 1961), which still provides by far the most detailed and complete account of Delporte's work at Châtelperron as a whole.

In the absence of any published details of the 1962 excavations, neither we nor Zilhão et al. can do more than speculate as to exactly what Delporte encountered in this area. The rationale for Delporte's work in this particular area in 1962 seems self evident. As noted above, the area ("area 5") in question lies immediately to the south of the main "palier sud" excavations in 1953/1954, in which he recorded the highly detailed stratigraphy of at least five clearly defined, clearly separated, "sub-horizontal" and heavily "reddened" occupation levels, which contained the great bulk of the artifacts and associated faunal remains in his excavations. His published sections of the palier sud (see Figure 2; Delporte 1957: Figure 2; 1999: p.11) indicate that these stratified occupation deposits were fully represented in the southern face of the palier sud at the end of his excavations in 1954. It is entirely reasonable therefore that Delporte should have gone back to this two-meter wide (east-west) section in 1962, in an attempt to recover more archaeological material (both artifacts and faunal remains) from the full sequence of Châtelperronian deposits already exposed in this section.

Zilhão et al.'s attempt to analyse and reconstruct what Delporte found in this 1962 extension of the palier sud is once again so convoluted that we defy any readers of *PaleoAnthropology* to clearly understand their analysis. Combin-

ing what we can understand from their presentation with further attempted clarification provided by João Zilhão himself (pers. comm., 22 November 2007) however it is clear that they envisage two alternative possibilities for the 1962 excavations (Figure 9). Either the whole of the 2x2 meter area consisted of unstratified infill of Bailleau's excavations (Figure 9B)—i.e., the area of the 1962 excavations lay entirely within the area of Bailleau's original excavations. Or, alternatively, that in the course of excavating two meters southward from the southern edge of the palier sud trench, Delporte crossed a major interface from what Zilhão et al. still resolutely regard as 19th century "backdirt" deposits within the palier sud zone itself, into essentially intact, undisturbed deposits which lay beyond the southern limits of Bailleau's excavations and the associated excavation infill (Figure 9C). Their Figure 21 implies that they favor the latter alternative. This interpretation would once again imply that during Delporte's 1962 excavations he crossed a major depositional boundary between 19th century excavation backdirt and fully *in situ* deposits, without apparently recognizing the existence of such a major and positionally conspicuous interface (see Figure 3). Given the six years of excavation experience that Delporte had acquired through his "sophisticated and methodical" excavations (Movius 1998: 308) at the Abri du Facteur site between his 1955 and 1962 excavations at Châtelperron, such a failure to recognize a clear stratigraphic and depositional interface seems to us, once again, extraordinary. Incidentally, the stratigraphy depicted in Zilhão et al.'s Figure 21 (lower right), this volume, involves yet another contradiction in their models. Whereas their stratigraphic reconstruction indicates "Châtelperronian" deposits extending throughout the whole of Levels B1 to B5 in this section, their textual discussion of the same section and the associated radiocarbon dates (pp. 19–26) asserts that Levels B1–3 in the 1962 extension of the palier sud trench consisted almost entirely of a "carnivore den" accumulation, formed *after* the deposition of the Châtelperronian levels, and probably contemporaneous with the Aurignacian occupation either in the site itself or in the same region.

The cornerstone of Zilhão et al.'s new interpretation of the 1962 excavations is based explicitly on the fact that most of the artifacts in the existing museum collections which are specifically marked as "Ch 62" are also marked with the layer designations of either "B1–3" or "B4–5" and not (with a few exceptions) with specific B1–B5 layer designations. They take this as a definitive indication that Delporte was not able to recognize clearly all of the individual stratigraphic "reddened" levels which he had distinguished in his earlier (1953–54) excavations in the adjacent palier sud area, and note that one of the pieces which is specifically marked as "Ch. 62 / B1–3" (a typical Aurignacian end scraper made on exotic flint) is referred to in his 1964 report (p. 56: footnote) as coming from an area where the individual levels "are not well differentiated" (but which he suggested could well be derived from close to the contact zone with the underlying Levels B4–5). None of this of course is in the least surprising in Delporte's own terms, since it is entirely

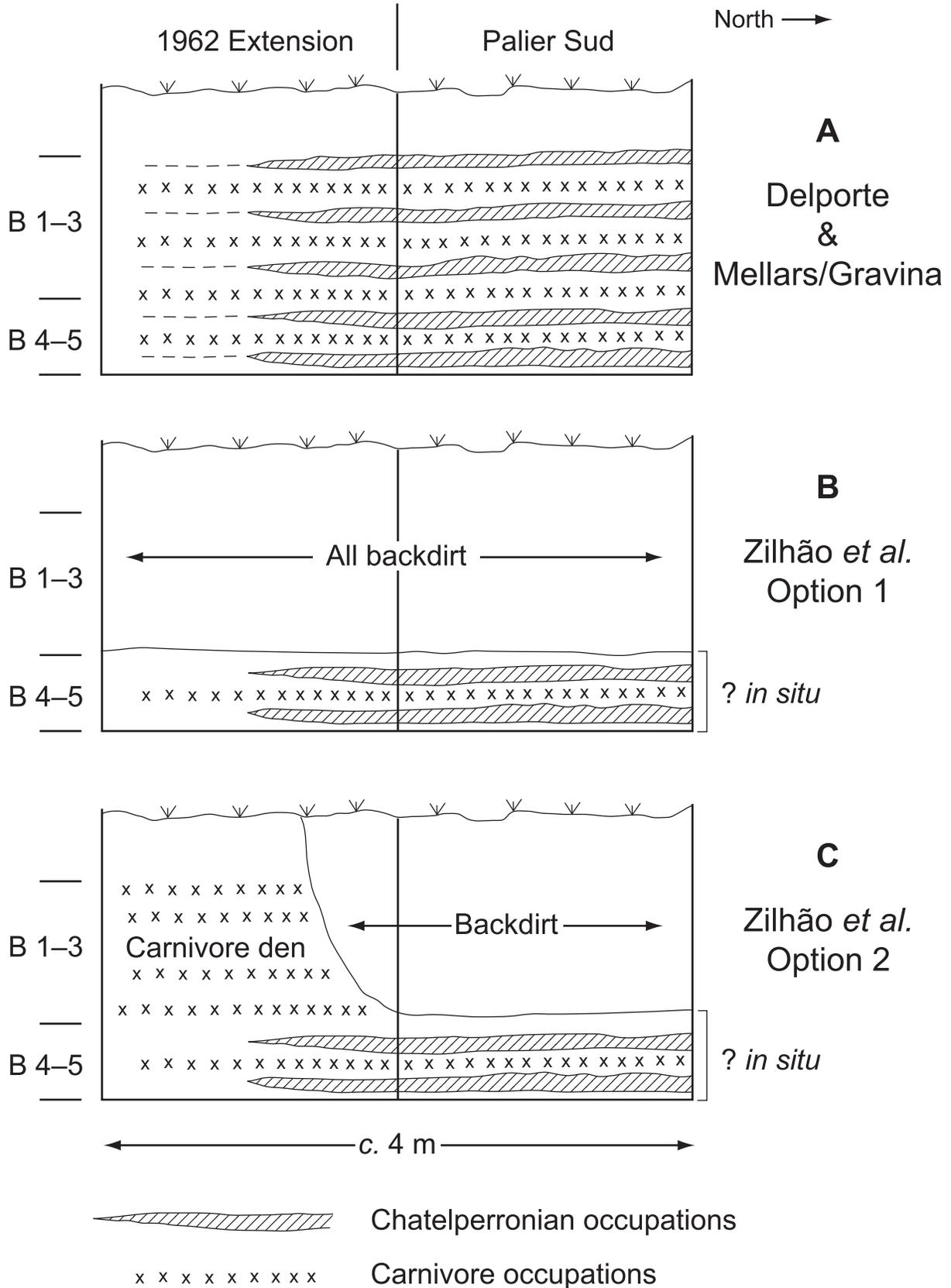


Figure 9. Three alternative models for the stratigraphy in the 1953–54 excavations by Delporte in the palier sud trench, and in the 1962 southern extension to this trench. For discussion of the alternative interpretations, see text. See also Zilhão *et al.* Figure 21, this volume.

reasonable to assume that as Delporte pursued his excavations into areas up to two meters beyond the southern limits of his 1953–54 excavations, the individual occupation levels would indeed have become thinner, much poorer in occupation material, and less intensely reddened (and accordingly more difficult to identify and separate) than in the areas closer to the central parts of the main occupation zone, further to the north (see Figure 9A). This, of course, is a common, if not universal, feature as archaeological occupation deposits in cave and rock shelter sites are pursued into the marginal and peripheral parts of the main occupation zones.

What is entirely clear and inescapable from Delporte's marking of the artifacts recovered from his 1962 excavations, however, is that he himself clearly believed these pieces to come from levels which were stratigraphically continuous with, and equivalent to, the upper (B1–B3) and lower (B4–B5) Châtelperronian levels recorded in his earlier excavations in the immediately adjacent palier sud. As noted above, there is no reason to believe that he crossed a major interface between two entirely separate sets of deposits in his 1962 excavations —i.e., from the full sequence of B1–5 levels recorded in his earlier palier sud excavations (which of course Delporte himself regarded as entirely *in situ* deposits, but which Zilhão et al. now interpret as 19th century backdirt) into a totally different sequence of geological and stratigraphic levels in the areas immediately (i.e., within a maximum of two meters) to the south (see Figure 9; Zilhão et al. Figure 21, this volume). Certainly, if he did, he failed to recognize this, or to report it in either his 1964 or 1999 site reports, despite his extensive and “meticulous” excavation experience acquired at the Abri du Facteur site in the intervening period.

All of this evidence fails to provide any shred of confirmation for Zilhão et al.'s new claim (pp. 19–26, this volume) that the samples of bones which we dated from the aggregated levels “B1–3” of Delporte's 1962 excavations derive from an entirely separate sequence of deposits, which Zilhão et al. now interpret as a largely if not entirely *post* Châtelperronian “carnivore den” accumulation (pp. 1, 21, and 23, Figure 21 caption, this volume; see our Figure 9), formed on top of the Châtelperronian deposits over the period between ca. 36,500 and 34,500 bp (as clearly reflected in the measured radiocarbon dates). They report from their own analyses (Table 3) that around 16 percent of the 154 faunal fragments attributed to Levels B1–3 in the 1962 excavations show evidence (according to their criteria) of “carnivore modification.” This of course still leaves around 84 percent of the bone fragments from these levels without any detectable traces of carnivore action. This stands in sharp contrast to the frequency of ca. 66 percent of clearly carnivore modified bones they record in the demonstrably carnivore-accumulated bone assemblage from Level B5a of Delporte's excavations in the adjacent palier sud zone (p. 23–24; Table 3). Even if these figures are accepted at face value, they need imply nothing more than that there were occasional episodes of carnivore activity on the site *between* the separate (probably brief) episodes of Châtelperronian

occupation in Levels B1 to B3, as we noted in our earlier (2005, 2007) papers (see Figure 9A). While they maintain that none of these bone fragments shows any clear indications of human butchery marks, they accept that at least two of the bones do show clear signs of burning (Table 3). And in their own analyses of the 77 lithic artifacts recovered from the combined Levels B1–3 in the 1962 excavations, they accept that the great majority of these (with only two clear exceptions) are clearly Châtelperronian pieces, including at least eight clearly ‘backed’ specimens, and four other pieces which they regard as “diagnostically” Châtelperronian (see their Table 2). Their attempted explanation of this, apparently, is that the total 2x2 meter area of the 1962 excavations could have included either some part of the surviving remnants of the hypothetical 19th century “backdirt” deposits in the adjacent palier sud (see Figure 9C; Zilhão et al. Figure 21, this volume), or, alternatively, that these Châtelperronian artifacts were in some way derived from the underlying (or adjacent) *in situ* Châtelperronian levels, by the highly energetic activities of the carnivores themselves. While both of these are no doubt hypothetically conceivable scenarios, the former suggestion would imply (as noted above) that Delporte totally failed to recognize a blatant depositional interface between *in situ* and backdirt deposits—and would of course rest entirely on their extraordinary battery of now discredited arguments that the whole of the B1–B3 sequence in the palier sud was indeed an unstructured backdirt formation, as opposed to a highly stratified sequence of *in situ* occupation deposits, as Delporte himself had clearly observed and uncompromisingly reported in all of his publications on the site. Both of these hypotheses would appear to be pushing special pleading to its limits, in a way which is totally unsubstantiated by any of the available excavation records for the site.

RADIOCARBON DATING

The simple, direct upshot of all the above is that there is no reason whatever to doubt that the deposits which Delporte excavated in 1962 with the combined designations of Levels “B1–3” represent a direct, lateral extension of the deposits which he had excavated in his earlier excavations in the immediately adjacent palier sud as the individual Levels B1 to B3, even if (for the reasons already explained) these thin, individual levels could not be isolated and separated so easily in this highly marginal and peripheral part of the site as in the more central and intensively occupied areas directly to the north (see Figure 9A). On this basis there is no reason whatever to doubt that the sequence of seven dates we secured on individual bone samples from the combined Levels B1–3 in the 1962 excavations (see Figure 6; Zilhão et al. Table 4, this volume) relate directly to the same stratigraphic sequence as that recorded in Delporte's preceding excavations in the adjacent palier sud. The question of whether or not some of these bones could derive from occasional episodes of carnivore activity in the site is effectively irrelevant to the chronological arguments, if these bones relate *stratigraphically* to the sequence of later Châtelper-

ronian levels recorded in Delporte's earlier excavations in the immediately adjacent areas. Of course, one could argue that in the absence of specific layer designations, all of the dated samples could derive—by some strange coincidence—from just one limited part of the B1–3 sequence, but the long span of time covered by the seven dates (ranging from $36,340 \pm 320$ to $34,550 \pm 500$ bp) makes this highly unlikely (see Figure 6). In addition, the fact that one of the bones specifically marked as coming from Layer "B4" was dated to $35,540 \pm 280$ bp makes it highly unlikely that all of the dated samples from the combined Levels B1–3 derive entirely from the *uppermost* part of the depositional sequence, potentially overlying the main sequence of B1–B3 levels recorded in the palier sud, and documented by Delporte as containing three separate levels of explicitly Châtelperronian material. Clearly, the only way that Zilhão et al. can escape the implications of these dates is by falling back on their by now untenable position that the entire sequence of material in Delporte's excavation in the palier sud—or at least that in Levels B1–B3—represents the unstructured backdirt of the 19th century excavations. The reasons why that hypothesis is now entirely untenable have been set out clearly above and in our earlier (2007) *PNAS* article. In short, the entire sequence of later Châtelperronian occupation levels at Châtelperron must date broadly between ca. 36,500 and (at least) 35,000 bp (making due allowance for the documented standard errors on the dated samples: Zilhão et al. Table 4, this volume)—a pattern which is entirely consistent with the available radiocarbon dates for other Châtelperronian sites in western and central France (see Gravina et al. 2005, Figure 4; Zilhão et al. Table 4, Figure 20, this volume). At present, Châtelperron stands as the best dated (and longest) sequence of Châtelperronian levels so far recorded in France.

In this context, the whole structure of Zilhão et al.'s arguments about the "serendipity" of the internal consistency of our ^{14}C dates from the B1–3 levels must be seen as a further example of special pleading. Even if we can readily accept that one or two of the dated samples from the B1–3 levels could derive from carnivore as opposed to human activity (as noted above), the central fact remains that not one of the dated bones from these levels produced a date in the range from 38–40,000 bp, that one would inevitably predict if the material excavated from these B1–3 levels in 1962 contained any substantial amount of either excavation backdirt material, or material derived from the earlier Châtelperronian levels by the actions of the carnivores themselves. Zilhão et al. themselves have pointed out (Tables 3 and 4, this volume) that these B1–3 levels in the 1962 excavation contained over 70 clearly Châtelperronian artifacts, and at least two specimens of clearly burned bones. So where *are* the dates in the 38–40,000 year range that we would inevitably expect for any bone samples derived (along with the 77 Châtelperronian artifacts and two burned bones) from the underlying or adjacent Châtelperronian levels? Only by invoking a high degree of statistical "serendipity" (in other words, statistical improbability) can they attempt to accommodate this striking consistency in

the radiocarbon dates for Levels B1–3. Almost by definition, arguments of this kind fall squarely within the "special pleading" category.

Zilhão et al.'s discussion of why some of the pieces from the 1962 excavations carry specific layer designations such as "B4" and "B5" is equally unconvincing. Their suggestion that these pieces *must* derive not from the 1962 southern extension of the palier sud trench but from 1962 excavations into the surviving remnants of the original *western* face of the palier sud has already been discussed in the preceding section. This made it clear that it was highly unlikely that Delporte would have devoted much effort to excavating what he had already clearly recognized as representing the infill of an earlier excavation episode (by Bailleau or otherwise) at the extreme western end of this section (see Figure 7). In our view it is much more likely that the pieces individually marked as "B4" and "B5" came from areas immediately to the south of the existing southern face of the palier sud trench, in a part of the section (i.e., the basal levels) in which Delporte believed he could still recognize some clear, continuing traces of the basal B4 and B5 levels. The other pieces, recorded simply as "B4–5", would presumably come from other parts of the trench, further away from the earlier palier sud excavations, where these two specific levels had become more difficult to clearly separate (see Figure 9A). But to make the leap from these few pieces marked individually as "B4" or "B5" to the assumption that they *must* derive from the (obviously disturbed) western face of the palier sud excavations seems to us, once again, entirely unjustified, from any of the evidence cited by Zilhão et al.

STRATIGRAPHIC DISTRIBUTION OF AURIGNACIAN ARTIFACTS

Zilhão et al.'s discussion and reinterpretation of the stratigraphic distribution of the range of unmistakably Aurignacian artifacts in the Châtelperron deposits brings with it a further range of confusion and rhetorical red herrings. That Delporte himself believed that there was strong concentration of these Aurignacian pieces either within or immediately adjacent to Level B4 of his recorded stratigraphy is not in doubt, and is repeated at several points in his publications (despite Zilhão et al.'s claims to the contrary) in the following terms:

"We must however note a tendency for the "concentration" of Aurignacian influence in level B4 (blade and leptolithic indices, carinate scrapers, end scrapers on Aurignacian blades, perforated teeth); perhaps the "Pointes d'Aurignac" found by Bailleau came from this level B4." (Delporte 1957: 472)

"As far as grattoirs are concerned [we see] a clear Aurignacian influence, which is apparent particularly in levels B4 and B4a." (Delporte 1957: 469)

"However, the convergence of the total indications confirms the idea that even if the Aurignacian does not form an independent level, its influence seem to be localized

in the parts of the deposits adjacent to level B4" (Delporte 1964: 57-8)

Zilhão et al.'s response to these assertions which notes that Delporte qualified his observations by suggesting that the presence of Aurignacian pieces in Level B4 could conceivably reflect some form of "influence" or "exchange" from adjacent Aurignacian populations, rather than an actual episode of Aurignacian occupation on the site, is, of course, a total red herring in the context of the present discussion. Whether the Aurignacian pieces in question were brought to the site by Aurignacian groups themselves, or alternatively by some kind of exchange or cultural "influences" between the local Châtelperronian and contemporaneous Aurignacian groups in some adjacent region, the implication would still be inescapable that the Châtelperronian occupants at Châtelperron were living contemporaneously with adjacent Aurignacian communities, from at least the time of Level B4 of Delporte's sequence, and potentially throughout the remaining stages of the Châtelperronian occupation in the overlying Levels B1–B3. Neither of these scenarios of course would offer much comfort to Zilhão et al.'s long-standing belief that there was no significant overlap, or mutual interaction, between Châtelperronian (i.e., presumed Neanderthal) and Aurignacian populations within this extreme, north-western zone of Europe (d'Errico et al. 1998; Zilhão and d'Errico 1999, 2003; d'Errico 2003; Zilhão 2001, 2006, etc.).

They offer two possible escape routes from this dilemma. First, they propose that the entire "stratigraphy" of these later (B1–B3) occupation levels is in fact an unstratified 19th century backdirt deposit, with no chronological significance whatever. And second, they attempt to bolster this model by suggesting that the actual distribution of unequivocally Aurignacian pieces within the deposits excavated by Delporte was in fact distributed throughout the *whole* of his recorded B1–B5 succession, without any demonstrable concentration within or closely adjacent to his Level B4.

Having dealt exhaustively with the whole of the "excavation backdirt" issue in both this and our earlier paper, we will focus now on the recorded distribution of the undisputed Aurignacian pieces within the deposits, as documented by Delporte himself. Here it can be said at the outset that if Delporte's own attribution of these pieces to specific levels is accepted as he presented them, the strong concentration of these pieces within or immediately adjacent to his Level B4 is beyond question. As we have documented elsewhere (2005), at least 8 of the 14 specifically Aurignacian or Aurignacian-like pieces were attributed directly by Delporte to Level B4, while two other pieces were attributed by him either to the base of the immediately overlying Levels B1–B3, or to the top of the underlying Level B5. In addition, two small Dufour bladelets (discussed below) were recovered from the superficial, topsoil horizon, clearly overlying the Châtelperronian levels. In terms of his own attributions, at most two other possible Aurignacian pieces were recovered from other parts of the sequence—a small fragment

of an Aurignacian endscraper from Level B2, and a possible carinate scraper made on a blade, also from Level B2—both of which we discuss further below. Expressed in statistical terms, the probability of this vertical distribution arising entirely by chance in a sequence of totally unstratified backdirt deposits is of the order of one in 13,000 (C. Brookes, personal communication).

Zilhão et al. now attempt to contest this stratigraphic distribution as recorded by Delporte in two ways—for which our responses are as follows:

1. They argue that the small fragment of an apparently Aurignacian end scraper which they identified in their reanalysis of the Museum collections as deriving from Level B2 of Delporte's stratigraphy, is indeed an unquestionable Aurignacian piece—a point which we had previously contested, in view of the small size of the fragment. They now provide the new information that this piece is manufactured from demonstrably exotic flint, which we both accept is unlikely to be part of the Châtelperronian lithic repertoire. Given this new information, we are now happy to accept the probability that this is indeed an Aurignacian piece, attributed by Delporte to his Level B2.
2. Their other arguments depend entirely on the fact that, according to their own analyses and documentation of the material, four of the pieces which Delporte himself diagnosed as specifically Aurignacian and attributed to Level B4 of his stratigraphy were recovered by Delporte during his 1952 excavations from his long, east-west trench, which lay immediately to the north of his subsequent (1953 and 1954) excavations in the palier sud (see Figures 1 and 3). But their questioning of the stratigraphic provenience of these pieces of course hinges entirely on their own (as opposed to Delporte's) belief that the *whole* of the deposits encountered in this 1952 trench represented the unstratified backdirt of Bailleau's excavations, and contained no *in situ* Châtelperronian deposits whatever (p. 15, this volume). Their reasoning here clearly is identical to that used when they dismissed the *whole* of Delporte's reported stratigraphy in the immediately contiguous palier sud trench, for which any further comments on our part would be both repetitive and superfluous. The clear and inescapable implication of Delporte's own recording of these four pieces in question (together with a large number of other, explicitly Châtelperronian artifacts to which Delporte also attributed specific layer designations in his 1952 excavations) is that Delporte *did* believe he could reliably recognize precisely the same intact stratigraphic levels in at least certain parts of the 1952 trench as those which he subsequently identified and excavated in his 1953–54 excavations in the immediately adjacent palier sud. This point is made explicitly by Delporte in his 1955

(p.79) and 1964 (p. 10) excavation reports, and is further explicitly confirmed by that fact that when he recorded the main, northern face of this long 1952 trench, he clearly indicated the same sequence of at least five distinct stratigraphic levels within the Châtelperronian deposits (which at that time he referred to as Levels “1a to 1e”) as those subsequently employed in his ensuing palier sud excavations (see Figure 1; Delporte 1955: Figure 2; Zilhão et al. Figure 6, this volume). Indeed, this sequence had already been recorded in Delporte’s first-ever publication on the site written in November 1952—immediately after his excavation of the long east-west trench, and before he commenced his subsequent excavations into the palier sud trench in 1953 (Delporte 1952). Because he recorded these levels directly along the northern face of what he subsequently excavated as the palier sud, it is entirely reasonable to assume that it was the discovery of these clearly stratified levels at this point in his long 1952 section which prompted him to extend his excavations directly into the immediately adjacent palier sud zone during the following (1953) season of excavations. Put differently, if the deposits within the palier sud zone were indeed fully stratified Châtelperronian occupation levels (as Delporte resolutely believed, and we have confirmed) would it not be a virtual miracle if the northern limits of these *in situ* deposits coincided precisely with the southern face of his long 1952 section? None of this provides any reason whatever to doubt that when Delporte attributed specific layer designations to finds in his 1952 excavations, he firmly believed these to come from intact, *in situ* Châtelperronian occupation levels precisely equivalent to those recorded in the subsequent excavations in the immediately adjacent palier sud. There is absolutely nothing whatever in Delporte’s published excavation records to cast doubt on this conclusion. Only by invoking their battery of now untenable arguments surrounding the entire “backdirt” issue, by relying on their own demonstrably fallacious (i.e., reversed) interpretation of the spatial “limits” of Bailleau’s excavations as recorded by Delporte, and by imputing a further massive degree of either archaeological incompetence or deliberate scientific dishonesty on Delporte’s part can their new interpretation of the “1952” discoveries at Châtelperron (with their clearly marked layer designations) be salvaged. Further comment on all these observations would now, we believe, be superfluous.

Once these points have been recognized we are left with the conclusion that the great majority of the explicitly Aurignacian pieces recovered by Delporte at Châtelperron were indeed concentrated within, or immediately ad-

acent to, Layer B4 of his stratigraphy, as Delporte consistently maintained in all his publications between 1957 and 1999. How many distinctively Aurignacian artifacts were reliably recovered from any *other* levels of the site still remains to us an open question. Zilhão et al. may now (with the new information provided on the raw material) have demonstrated this for the small fragment of an Aurignacian end-scraper recovered from Delporte’s Level B2 in his palier sud excavations (assuming of course that this is not intrusive from a later episode of Aurignacian occupation on the site directly overlying the Châtelperronian levels, possibly due to the action of tree roots, as discussed above) and there may be one other carinated end scraper (as Delporte noted) also from his Level B2. As Delporte himself emphasized (1964: 56), the fine Aurignacian endscraper (Zilhão et al. Figure 18, this volume) recovered during the 1962 excavations from his aggregated Levels B1–3 came from an area where the individual levels were “not well differentiated”, and which he suggested could well come from close to the contact zone with the underlying Level B4. (Incidentally, why should he be so honest about the uncertainty of the provenience of this piece, if he was attempting to misrepresent so many other aspects of his excavation records: see below). The 1962 find of a Dufour bladelet attributed to “level B4-5” could similarly derive from at, or close to, the contact with Level B4. And while Zilhão et al. (predictably) still maintain that one of the two perforated teeth recovered from Level B4 could conceivably be a Châtelperronian product, the fact remains that *both* of these perforated teeth were unequivocally attributed by Delporte to Level B4—at the very least an interesting stratigraphic “coincidence”—especially if the whole of Delporte’s excavated sequence is assumed to represent an entirely unstratified 19th century backdirt formation. The two highly typical Dufour bladelets recovered from the superficial topsoil deposits (Delporte’s layer A) probably (as we have already suggested: 2007: 3661) derive from an entirely separate episode of *later* Aurignacian occupation on the site, directly overlying the long underlying succession of Châtelperronian levels (see Zilhão et al. Figure 27, this volume).

But even if we accept that one or two distinctively Aurignacian pieces *were* securely stratified within the sequence of later Châtelperronian levels (i.e., in the levels directly overlying the main concentration of these pieces within Level B4) what implications should we draw from this observation? Two obvious alternatives spring to mind. The first is that these pieces could indeed be derived from the underlying Level B4, either by the energetic activities of carnivores within the site, or, equally conceivably, by some similar form of disturbance into the underlying levels by the later Châtelperronian occupants themselves (for example, by the excavation of hut or hearth structures, as documented in the similar Châtelperronian levels at Arcy-sur-Cure, and as strongly suggested by Bailleau’s discovery of a concentration of large mammoth tusks within the zone of his “foyers”, at Châtelperron itself: Delporte 1957: 454). The second obvious alternative is that these later Aurignacian pieces could derive from sporadic visits to the

site by itinerant, adjacent Aurignacian groups during the formation of the later (B1–B3) Châtelperronian levels—or conceivably from continuing “exchanges” of occasional artifacts between the two populations. In short, the sporadic occurrence of one or two Aurignacian artifacts during the later stages of Châtelperronian occupation at Châtelperron could be easily (and very plausibly) explained by a number of different potential mechanisms—once the existence of a clear “concentration” of Aurignacian pieces has been recognized (as Delporte consistently maintained) within the underlying Level B4. The fact that Delporte was so cautious about pressing the hypothesis of a separate Aurignacian “occupation” (as opposed to “influence”) at the site in his 1964 publication (in contrast to his more confident belief in a separate “interstratified” lens of Aurignacian occupation in his 1999 publication: p. 33) probably reflects simply his recognition that at that time (ca. 1964) such a suggestion would have been regarded as positively heretical by his French Paleolithic contemporaries (and of course his 1964 report was apparently written as a thesis—presumably to be examined by senior colleagues). It is entirely predictable that as a relatively young archaeologist at the start of his professional career he would have been cautious—if not positively alarmed—about making such claims. Indeed, granted the climate of French Paleolithic opinion at that time, it says much for his professional honesty and integrity that he reported the apparent interstratification of Châtelperronian and Aurignacian material at Châtelperron at all. Why on earth would he choose to fabricate this contentious scenario, unless he had observed it—inescapably—with his own eyes?

OTHER ISSUES

The remainder of the arguments on the character of the lithic material can be dealt with more briefly. We are pleased (and relieved) to see that Zilhão et al. now accept (p. 27, this volume) that the supposedly diagnostic “Aurignacian II” Dufour bladelet recovered from Delporte’s Level B4 has no chronological significance, in the context of the present discussion. We still regard it as entirely likely that the small fragment of a “possibly” Solutrian, pressure-flaked piece reported from Level B2 could easily have penetrated through a depth of ca. 30–40 cm of deposits from the post-Châtelperronian surface, through the actions of tree roots or other well documented taphonomic processes (as Zilhão et al. themselves clearly acknowledge at other points in their papers)—and we now see that Zilhão et al. suggest (p. 27, this volume) that this could in fact be a small fragment of an invasively edge-retouched Aurignacian blade. We still find the abundance of fully retouched flint tools and large identifiable faunal remains (including at least 29 identifiable specimens of bovids, horse, and rhinoceros) highly surprising in a hypothetically excavation “backdirt” deposit—especially since (as we pointed out [2007: 3660]) the recorded frequency of retouched pieces is actually significantly higher in the hypothetical “backdirt” deposits in Levels B1–3 than in the putative *in situ* deposits in the un-

derlying Levels B4–5 (i.e., ca. 35% in Levels B1–3, as compared to ca. 30% in Levels B4–5). All this still leaves entirely open the question of exactly what the nineteenth century excavators were hoping to recover from their excavations, if not retouched stone tools and large, identifiable animal remains. Indeed, Zilhão et al.’s entire argument that the backdirt of Bailleau’s excavation still retained large numbers of retouched tools (as recovered by Delporte from the supposedly backdirt deposits in Layers B1–3 of his palier sud trench) raises yet a further major problem for their interpretation. If—according to their own model—almost the whole of Delporte’s excavations into the site were carried out within the areas of Bailleau’s backdirt, why did Delporte only record the discovery of retouched tools within (or closely adjacent to) the area of his palier sud zone, and not throughout the whole of his extensive 1951–1954 excavations into the other areas of Bailleau’s backdirt in the other parts of the site? And we can only characterize the convoluted arguments advanced to explain the dramatic (sevenfold) increase in “surface weathered” pieces between the *in situ* (B4–5) Châtelperronian levels and those derived from the same levels in the form of hypothetical excavation backdirt (in Levels B1–3) (Zilhão et al. Figure 22, this volume), as a further example of special pleading, however these figures are manipulated.

Zilhão et al.’s suggestion (p. 34; Figure 30, this volume) that the occurrence of two refitted fragments of a Châtelperron point recovered respectively from Level B3 and Level A of Delporte’s excavation implies that one of these pieces must have been “upwardly displaced” is, of course, unwarranted. What we suggested was that the fragment recovered from Level A (i.e., the topsoil horizon) was simply part of the scattered backdirt from Bailleau’s excavations in the adjacent parts of the site, as Delporte himself clearly assumed. In the case of the piece with supposedly marks of a metal tool clearly underlying a subsequent calcareous concretion (p. 27), we await a much clearer illustration of this piece than the one provided in their Figure 25, and/or a personal demonstration of this by one of the authors in the museum collections. Incidentally, how could they observe that the “metal tool” marks extend *beneath* the surface concretion, unless they have now removed the concretion from the piece in question, thus destroying the evidence they are now using to support their argument?

DELPORTE’S “COMPETENCE”

The penultimate section of Zilhão et al.’s paper entitled “Delporte’s Competence” is in many ways perhaps the most remarkable and disturbing part of their article, when seen in the context of their discussions in the earlier sections. Here they emphatically (p. 37) “disagree that challenging Delporte’s stratigraphic interpretation equates to passing a judgment of incompetence,” and assert that “honest error is an inherent and fundamental component of the scientific enterprise.”

Viewed at a general, abstract level, of course, both of these statements are self-evidently true. But when seen in

the context of their discussions in the preceding sections of their paper, both of these statements are not only inherently self-contradictory, but frankly bordering on the hypocritical. The extraordinary degree of archaeological incompetence implied by Zilhão et al.'s sustained interpretation of Delporte's reported stratigraphy at Châtelperron in terms of totally unstratified, unstructured "backdirt" deposits has already been discussed in the preceding sections of our response. Here we showed that Delporte's recording of at least five separate, "subhorizontal," heavily red-dened Châtelperronian occupation levels (separated by intervening semi-sterile deposits), extending in an essentially "regular" fashion over the whole of the 2x2 meter area of the palier sud excavations, and recorded independently in the two (north-south and east-west) drawn sections of the trench (see Figure 2) could only imply an astounding degree of either observational blindness and self delusion, or conscious misrepresentation of the stratigraphic realities of the deposits on his part—if we now accept that the whole of these deposits consisted of the unstructured, unstratified "backdirt" of earlier excavation trenches. Equally, if not more extraordinary, is Zilhão et al.'s assertion that while Delporte clearly recognized and recorded a well-defined interface between *in situ* and 19th century backfill deposits over a large area of his own excavations, he consistently misinterpreted (and reversed) the true significance of this depositional interface, by consistently misinterpreting the *in situ* deposits as 19th century backfill, and the backdirt deposits as fully intact, highly stratified occupation deposits. If these are not regarded as accusations of gross professional incompetence on Delporte's part, it is difficult to imagine what other accusations could qualify in these terms.

Regrettably, but all too conspicuously and inescapably, several of the statements made in an earlier section of their paper under the heading of "Drawing vs. Photos" can only be read as implying not merely professional incompetence, but outright and fully conscious scientific dishonesty and deliberate misrepresentation on Delporte's part. On page 11 we read:

"These inconsistencies suggest, as we argued before, that Delporte's 1957 drawing...is a *post facto* schematic rendition of the site's stratigraphy.....not the recording of any profile physically extant at any time during the excavation process....The drawing was never meant to be a literal representation of observed reality, only graphic representation of the excavator's idealized stratigraphic model."

On pages 15–17 of their paper, the accusation of deliberate misrepresentation in Delporte's recording and interpretation of his field observations becomes even more blatant:

"The discrepancy between Delporte's topographic observations and stratigraphic interpretations is too obvious for him not to have perceived it at some point. Having found Bailleau's excavation limits where he placed them

in the 1954 plan, Delporte should have realized that any deposits excavated in the central part of the site east of that limit had to be disturbed, and specifically that the "red levels" between points 2 and 3 of the longitudinal profile (i.e., the southern face of the 1952 trench.....) could not be *in situ* Châtelperronian levels as he first thought.... We believe that, despite the apparent consistency of his accounts, Delporte must have been well aware of the problems. Perhaps that is why he never published these excavations in any detail...."

These statements are, in fact, asserting that by at least the end of his excavations in the palier sud zone in 1954, Delporte had already clearly recognized that all of the deposits in this part of the site lay entirely within the area of Bailleau's earlier excavations, and must therefore inevitably represent not *in situ* occupation levels but entirely excavation backdirt deposits. If these allegations are correct, the fact that Delporte subsequently reported all of these deposits as a succession of truly *in situ*, highly stratified Châtelperronian occupation levels, in a succession of publications between 1955 and 1999 (i.e., spanning effectively the whole of his professional career) must surely rank as one of the most flagrant and sustained examples of conscious, deliberate deception in the history of archaeology. To qualify these conscious misrepresentations of the documented stratigraphy at Châtelperron as "honest errors of interpretation" on Delporte's part is not only self contradictory, but in our view a frankly hypocritical attempt to damn Delporte's reputation on the one hand, and then to maintain that they are merely "reinterpreting" his "honest" scientific observations on the other. We must allow readers to make their own evaluations of Zilhão et al.'s paper—and indeed their own professional performance—on this score. Our own experience after numerous close readings of Delporte's successive publications on the Châtelperron discoveries has been to develop increasing respect for the quality of both his original excavations at Châtelperron in the 1950s, his meticulous and astute recording of both the site stratigraphy and the recovered artifacts, and his acutely perceptive (and forward-looking) interpretation of the true significance of the finds—all of course entirely consistent with his subsequent record of meticulous, and highly respected, excavations and publications at a range of other, classic, French Paleolithic sites.

CONCLUSIONS

In conclusion, we find the present paper by Zilhão et al. professionally disturbing, in terms of current approaches to the disciplines of archaeology and paleoanthropology, on several different levels.

First, as we observed in the Introduction, their article is quite transparently and explicitly designed to defend a series of earlier theoretical viewpoints—in our view, explicit scientific "agendas"—which the two principal authors had formulated and repeatedly published almost ten years before the new information on the Châtelperron stratigraphy and associated radiocarbon dating evidence was published

(d'Errico et al. 1998; Zilhão and d'Errico 1999, 2003; d'Errico 2003; Zilhão 2001, 2006, etc.). Their article is, quite clearly, an attempt to salvage a pre-existing theoretical viewpoint, which has now been heavily undermined by new evidence which has just emerged on the scene in the form of explicit radiocarbon confirmation of Delporte's recorded stratigraphy at Châtelperron—as we see it, explicitly “post-hoc” attempted scientific justification of a now discredited scientific hypothesis. We would recall that they have previously mounted similar attacks on a range of other early Upper Paleolithic sites across Europe (e.g., das Geissenklösterle, Keilberg-Kirche, Willendorf, Bacho Kiro, and l'Arbreda) where the published stratigraphic and chronological evidence fails to conform with their own preconceived models (Zilhão and d'Errico 1999, 2003).

Second, we are concerned about the way in which the article is written. As we have commented several times above, even after many close readings of their text, and attempted verbal clarification by one of the authors, we still find the language and the convoluted, hyper-complex structure of their arguments so impenetrable that we challenge any readers of *PaleoAnthropology* to clearly understand them. This seems to us, as noted earlier, a classic attempt to “control by confusion”—i.e., to induce readers into an almost trance-like state of confusion and incomprehension, in the hope that they will meekly accept the extraordinary conclusions offered, without being able to follow or clearly disentangle the convoluted arguments involved. In short, we regard the whole of Zilhão et al.'s article as more of a diversionary smoke-screen to obscure or escape the central issues at stake than a serious and coherent scientific response to our own criticisms of their models.

Thirdly, and perhaps most disturbingly, we are dismayed by the lengths to which Zilhão et al. have gone to criticize not only Henri Delporte's basic competence as an excavator (even on a relatively simple issue such as the distinction between 40,000 years old, fully *in situ* occupation levels and relatively recent excavation backdirt deposits), but also his scientific and personal integrity in the ways in which he recorded and repeatedly published his results, over a 40-year period. As we discussed in the immediately preceding section, their latest exposé of Delporte's “competence” and “inconsistencies” can only imply that he systematically and consciously misrepresented his field observations at Châtelperron over the greater part of his professional career from 1955 until 1999. To impute this level of incompetence and deception to one of the most senior, experienced, and respected figures in the history of French Paleolithic studies seems to us almost beyond belief.

Finally, as we have discussed in our earlier papers, we are still at a loss to understand the evolutionary rationale which ultimately underlies the whole of Zilhão and d'Errico's attack not only on the Châtelperron stratigraphy, but also the whole of the rapidly accumulating evidence for a significant period of chronological overlap—and therefore potential interaction—between the final Neanderthal populations and the new, incoming populations of anatomically modern humans over large areas of Europe (Mel-

lars 2005, 2006a, 2006b, 2006c; Conard and Bolus 2003, etc.) As we explained earlier (2005, 2007), the point is both self-evident and logically inescapable that if new populations of anatomically and behaviorally modern humans did indeed disperse progressively across Europe over the period between ca. 43,000 and 35,000 (radiocarbon years) bp (as effectively all paleoanthropologists now accept), then *some* degree of chronological overlap and mutual interaction between the two populations is not merely plausible, but ultimately predictable and inevitable in theoretical and demographic terms. When seen in these terms, the debates over the “reliability” of the reported “interstratification” at Châtelperron are largely irrelevant and superfluous to the underlying theoretical agendas which have driven the whole of Zilhão and d'Errico's publications on the notorious Neanderthal “acculturation” issue over the past decade. Even if they refuse to believe that an episode of Neanderthal/modern human interstratification could conceivably exist at Châtelperron itself—despite all of the explicit evidence in support of this—then it is theoretically inevitable that some sites in Europe must inevitably contain successive, “interstratified” occupations by the final Neanderthal and earliest anatomically modern communities, even if (as Zilhão et al. now seem to accept: p. 39, this volume) the close superpositioning of these occupations in many sites may be difficult to separate in stratigraphic terms. We suspect that most of our colleagues are now tired of this endless exchange of rhetoric on the Châtelperron situation. They should perhaps comfort themselves with the thought that the details of this particular site have little if any significance for the broader issues of potential coexistence and “interaction” between Neanderthal and incoming modern populations, either in France or in any other regions in Europe. As we noted earlier (2007: 3662), the alternative is to believe that the Neanderthals effectively and almost spontaneously self-destructed the moment the first modern humans set foot in their territories—not perhaps the most economical demographic scenario in the current state of paleoanthropological research. We refer readers to our earlier papers (Mellars 1996, 2006a, 2006b, 2006c; Gravina et al. 2005; Mellars et al. 2007) for a fuller discussion of how these processes of coexistence and demographic interaction between the two populations could have taken place.

Zilhão et al. have, of course, been given the final word in this exchange, in their concluding “authors response.” We anticipate that this will take the form of a further outpouring of convoluted and hyper-complex arguments that will attempt both to side-step and to confuse the major issues of substance we have set out in our own discussion. Since we have no opportunity for a further direct response, we would simply ask colleagues to refer back to our own text wherever appropriate, to avoid a further attempt to disguise the important issues of Châtelperron under a further diversionary smoke-screen of convoluted rhetoric. The site—and Delporte's meticulous work on the site in particular—deserves better than this.

SUMMARY

The renewed attack by Zilhão et al. on the Châtelperron stratigraphy involves a further range of demonstrable errors and misunderstandings. It is inconceivable that Henri Delporte could have “invented” a sharply-defined stratigraphy of five separate, “sub-horizontal,” heavily reddened Châtelperronian occupation levels over an area of four square meters of the site. It also is inconceivable that he could have consistently mistaken 19th century excavation backdirt deposits for highly stratified, 40,000-year-old, *in situ* occupation levels, over the whole of his (extensive) excavated areas. Moreover, it is inconceivable that, having correctly identified a clear contact zone between the *in situ* and 19th century infill deposits over a distance of at least six meters (as Zilhão et al. accept) he could have systematically reversed the true significance of this interface, by consistently interpreting the backdirt deposits as highly stratified occupation levels, and the *in situ* deposits as 19th century backdirt. Many different aspects of the site records explicitly contradict this suggestion. The Zilhão et al. model entails the impossible implication that the thickness of the *in situ* Châtelperronian levels conflated dramatically from over one meter in the 19th century excavations to ca. 35cm in the areas of Delporte’s excavations, within two immediately adjacent parts of the site. Additionally, there is an undisputable concentration of diagnostically Aurignacian artifacts within, or immediately adjacent to, Level B4 of Delporte’s stratigraphy, which would entail a statistical improbability of around 1 in 13,000 in terms of the excavation backdirt model. The probability of the recorded sequence of 12 radiocarbon dates emerging from a sequence of totally unstratified backdirt deposits is similarly of the order of 1 in 250. Zilhão et al.’s analysis implies that Delporte repeatedly and deliberately misrepresented his field observations at Châtelperron over the whole of his professional career. And the underlying evolutionary rationale for their belief in a lack of any significant coexistence and chronological overlap between the final Neanderthal and the earliest, intrusive populations of anatomically modern humans throughout Europe is untenable on theoretical and demographic grounds. We see this latest defense of the “excavation backdirt” model for the reported stratigraphy at Châtelperron as a transparent attempt to salvage a pre-existing “anti-acculturation” theoretical agenda, which has now been repeatedly undermined by the most recent archaeological field evidence from France and several other regions of Europe. The sequence of 12 AMS dates we secured on samples from Delporte’s excavation provides the longest and most secure sequence of radiocarbon dates at present available for the French Châtelperronian.

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