On the new dates for Gorham's Cave and the late survival of Iberian Neanderthals

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Keywords

Gibraltar, radiocarbon, Neanderthals, Middle Palaeolithic, modern humans

Abstract

On the basis of radiocarbon dates recently obtained for a trench in the back part of Gorham's Cave, Gibraltar, it has been claimed that Neanderthals survived in the region until at least 28,000 and probably as late as 24,000 radiocarbon years ago (Finlayson et al 2006). The stratigraphic and archaeological context of these results, however, does not warrant such an interpretation, because of the microscopic nature of the dated samples, the wide scatter in the dates obtained, and the lack of any correlation between age and stratigraphic depth. An Early Upper Palaeolithic occupation of the site was documented by Waechter's 1950s excavations (Waechter 1951), and the younger among the new series of results are likely to relate to such an occupation. We conclude that the most parsimonious reading of the evidence is that of a Middle Palaeolithic occupation of Gorham's until, but not beyond, ca 32–30,000 radiocarbon years ago.

Radiocarbon chronology plays a critical role in current debates over the nature of Neanderthal extinction, modern human expansion, and whether there was any interaction between the two populations. Only by the measurement of stratigraphically valid samples bearing clear relations to diagnostic osteological or archaeological remains can we ascertain a chronometrical relationship between those events and the samples used in attempts at dating them. Even when we can demonstrate this, our efforts are severely hampered by the unreliability of chronometric accuracy at present (measurements do not reflect real ages and may be surprisingly wide off the mark), and by relatively coarse precision (large measurement errors). Together, these create a fuzzy dataset which requires interpretation and, hence, is open to over-interpretation and error (eg, Pettitt 2005).

Here, we respond to a recent publication by Finlayson et al (2006) in which it is suggested on the grounds of radiocarbon dates for level IV of the back part of Gorham's Cave, Gibraltar, that Neanderthals persisted in the region as late as 28,000 radiocarbon years ago, and possibly much later. Such conclusions, if robust, have significant implications for the biogeography of Neanderthal extinction, particularly in the light of specific models such as the 'Ebro Frontier' (Zilhão 1993, 2000, 2006). As discussed by Delson and Harvati (2006), those conclusions also bear on the interpretation of the Lagar Velho child skeleton (Duarte et al 1999; Zilhão & Trinkaus 2002) as evidence for admixture between Neanderthals and modern humans at the time of contact.

In the following, radiocarbon dates are expressed as kyr BP. Current comparisons of radiocarbon and other chronometric data such as the CALPAL intercomparison (Weninger & Jöris 2005) suggest that, in this time range, radiocarbon may underestimate the true age of measured samples by as much as five millennia. However, a broad relative order of events – which lies at the heart of the issues raised by Finlayson et al – should not be affected by this underestimation. In order, therefore, to avoid dealing with an additional layer of complexity, we will refer only to uncalibrated radiocarbon dates.

1 Issues of logic

Finlayson et al present new radiocarbon dates on charcoal samples from excavations to the rear of Gorham's Cave, suggesting that 'taken together, all the dates show that Neanderthals occupied the site until 28 kyr BP and possibly as recently as 24 kyr BP. The evidence in support of the 24 kyr BP date is more limited than for 28 kyr BP, which is taken as the latest well-supported occupation date'. (p 1) Finlayson et al clearly believe that their data demonstrate reliable Neanderthal occupation of the cave as late as 28 kyr BP, but this statement implies that the six results younger than 28 kyr BP are in some way unreliable. The grounds upon which the 24 kyr BP date is thought to have 'more limited' support are not mentioned; in particular, no argument is made that the different levels of confidence attached to the results relate to intrinsic properties of the samples (such as size, species composition, stratigraphic position, pre-treatment chemistry, etc).

Consequently, from a logical point of view, the justification of Finlayson et al's assessment of their chronological data could only come from criteria of external consistency. Put another way, their downplay of the significance of the six younger measurements and full acceptance of the remainder would only be reasonable on the basis of an argument along the lines of 'there must be a problem with the younger dates that we cannot explain, but the fact that the others replicate similar dates at other sites in the region leads us to believe they are truly associated with the Middle Palaeolithic assemblage in the same deposits'. They argue, however, that Gorham's is the only site in Iberia where a prolongation of the Middle Palaeolithic beyond 30 kyr BP is documented and, hence, pre-empt the use of external consistency criteria to support their interpretation.

Thus, if their measurements are all reliable, then the conclusion should be that Neanderthals occupied the site until around 23, not 28 kyr BP. On the other hand, if some dates are unreliable (and no independent criteria are given to assess which are and which are not) then external consistency dictates that the new results from Gorham's can only be invoked in support of the current consensus view (Zilhão 2006), that of a regional survival of Middle Palaeolithic Neanderthals until 30–32, not 28 or 24 kyr BP. We view this logical inconsistency as a fundamental flaw in the argument of Finlayson et al, and we believe, and intend to show in the following discussion, that their interpretation is also at odds with the empirical data.

2 Dating charcoal: are Finlayson et al's measurements accurate?

A broad chronology for the Middle to Upper Palaeolithic transition at Gorham's was published by Pettitt and Bailey (2000), using samples derived from the excavation directed by CB Stringer and RNE Barton (cf also Barton et al 1999). They concluded that the end of the Middle Palaeolithic at the site could be set at 30–32 kyr BP, but Finlayson et al *(*ibid) throw doubt on the reliability of this chronology as '…all excavations and soundings had been made in the external part of the cave' and 'problems of contamination of radiocarbon samples from wet, unprotected, exterior, parts of caves have recently been brought to light'.

In fact, the trench from which Pettitt and Bailey's samples came, while not as deep inside the cave as the trench from where Finlayson et al are reporting their data, can in no way be described as 'exterior'; it lies beyond a narrowing of the cave walls quite some distance from the cave's drip line and, in fact, only some 15 m towards the cave mouth from Finlayson et al's own excavation area (figure 1). Since no information is given that would substantiate a contrast between the two trenches in terms of depositional activity or post-depositional chemistry, we cannot rule out that similar regimes were occurring in both areas and, therefore, that any perceived (but as yet undemonstrated) problems are not equally valid for both sets of samples. Whatever the case, if Finlayson et al think that the carbon in the samples published by Pettitt and Bailey was to some extent and in some way intrusive into the samples, and thus not a reliable indicator of their age, they should at least provide testable hypotheses about the chemistry, or the mechanics of the intrusion. One should certainly be very wary of making unsubstantiated statements about the reliability of samples, particularly when the levels of carbon remaining are high, as they were in the samples reported by Pettitt and Bailey, which, being charcoal, are virtually pure carbon.

Finlayson et al also inform us that their samples 'were identified as individual pieces of charcoal under a microscope before being dated', but no taxonomic identification of the combusted wood is provided, nor is there, among the abundant supplementary information supplied, any picture of the dated samples corroborating that diagnosis. Perhaps the samples were too small to warrant species identification, as one could infer from the fact that a microscope was needed to ascertain their 'individuality' (species identification is

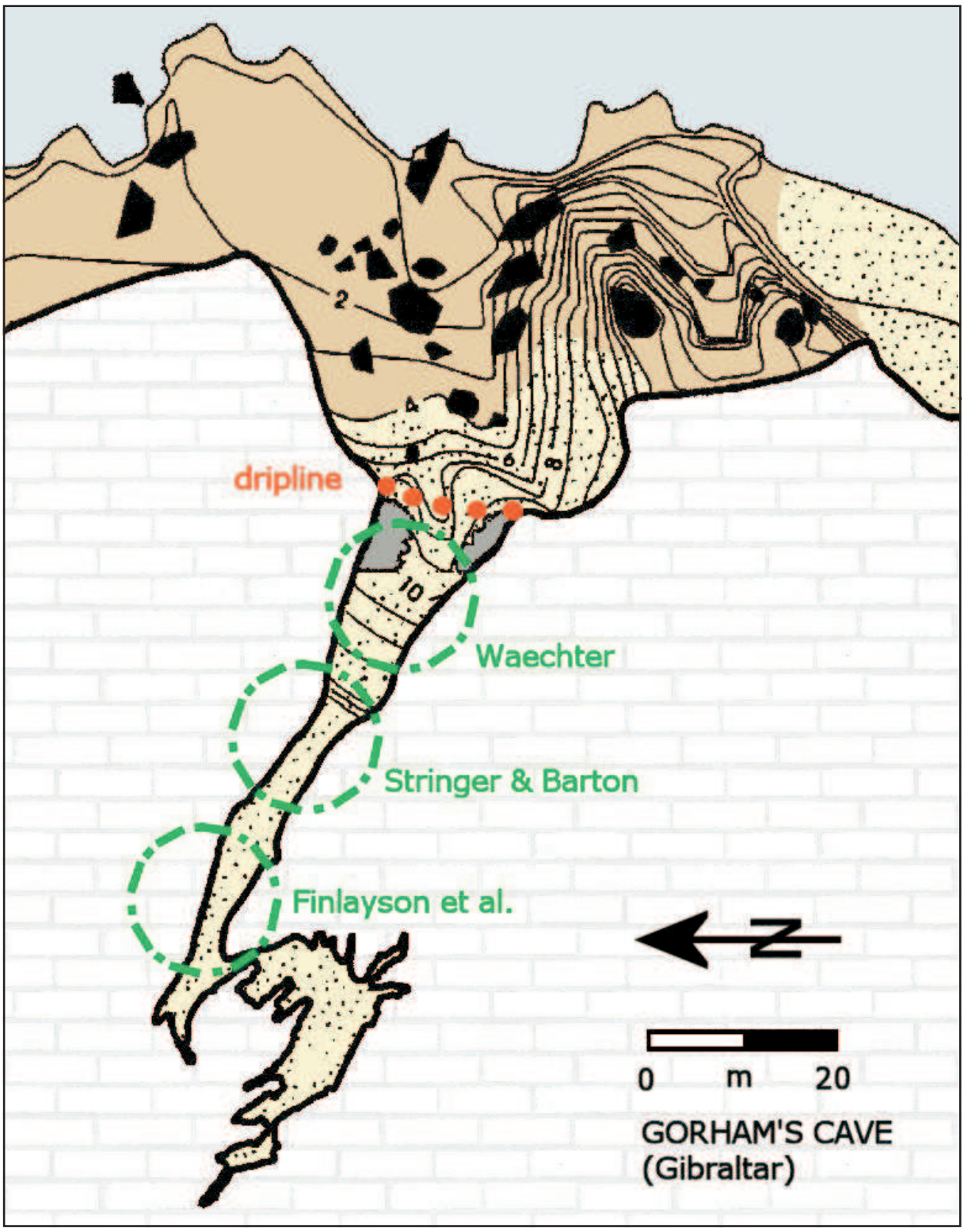

Figure 1 Plan of Gorham's Cave (modified after the GIEX 1995 topography of JJ Mateos, JM Galafate & A Santiago), indicating approximate position of the excavations whence came the dating samples. The dotted line indicates the modern (and probably Pleistocene) dripline

possible for charcoal particles as small as 2 mm, and you do not need a microscope to ascertain the 'individuality' of a chunk of charcoal that size). If the dated samples were charcoal of microscopic size, then the possibility cannot be excluded that, despite the lab's 'full treatment', the wide scatter in the reported dates relates to incomplete decontamination of those samples that yielded the younger results. In fact, it is well-known that, when very small charcoal samples close to the limit of applicability of radiocarbon are used, complete decontamination may be hard to achieve (cf Turney et al 2001; Fortea 2002; Pettitt & Bahn 2003); moreover,

no chemical signature of potential problems may be available to help in the interpretation (unlike when dating bone collagen). We would certainly disagree that the miniscule samples used by Finlayson et al are necessarily more reliable than the macroscopically identifiable samples classified to genus reported by Pettitt and Bailey (see Gale & Carruthers 2000 for identifications).

In this regard, we must note that Finlayson et al fail to mention that a sample from the same level IV in the same back part of the cave had previously been dated to 32,330 ± 390 BP (OxA-10230) (Pettitt in Bronk Ramsey et al 2002). This sample was a cone scale of pine; its size and nature make it in our opinion a reliable radiocarbon sample, and it is significant that it is statistically identical (ie, overlapping at two sigma) to five out of the new 22 results for level IV (samples 13, 25, 26, 27, 30). This result adds to the cluster of statistically identical measurements in the 30–34 kyr BP range available for the late Middle Palaeolithic of Gorham's and is in agreement with the broad chronology proposed by Pettitt and Bailey (2000).

In sum, if there is reason to disregard any of the results so far obtained for Gorham's on technical grounds, the prime suspects are the younger among the new ones from the back of the cave, not those previously published for the trench closer to the entrance.

3 Sediments, stratigraphic mobility, and reliability of the dataset

Pettitt and Bailey (2000:158) drew attention to the fact that, given the near absence of bone, almost all available samples for radiocarbon dating were small lumps of charcoal. These authors suggested that the data indicated a *terminus ante quem* for the Gorham's Cave Mousterian of around 31 kyr BP, but they also noted the considerable age range of the results obtained, which '…clearly indicates stratigraphic mobility, as was suspected through stratigraphic observation. This is not surprising given that all of the samples [measured] are small, isolated lumps of charcoal which are prone to stratigraphic mobility; these samples were dictated not by choice, but by the lack of any other more suitable samples'. It is worth noting, in any case, that the three measured samples taken from remnant hearths (which one might regard, therefore, as not having moved vertically) yielded stratigraphically consistent results: 32,280 ± 420 BP (OxA-7857) for Context 16, which contained Mousterian artefacts; 28,680 ± 240 BP and 25,680 ± 280 BP (OxA-7792 and OxA-6997, the first on charcoal, the second on burnt bone), both from within Context 7, which lacked any Mousterian archaeology and may be correlated with the Upper Palaeolithic deposits excavated by Waechter in the early 1950s (eg, Waechter 1951).

Finlayson et al describe their level IV samples as coming for the most part from a 'hearth' that is in fact best described elsewhere in their paper as a recurrently used combustion area (see also below). Figure 2 plots the existing radiocarbon measurements that are available from this 'hearth' and from the equivalent stratigraphic depth in adjacent squares (ie, with the exclusion of Finlayson et al's samples 9 to 13, which came from higher up in level IV); the overall depth represented is approximately 0.5 m. We use the two sigma age ranges, and plot the results in an order following, from left to right, their depth and horizontal coordinates as given in figure 1c of Finlayson et al. It can be seen that, at two sigma, charcoal seems to have been combusted at or near the cave between approximately 33 and 23 kyr BP, ie, over a period of some 10,000 radiocarbon years. Note, however, that six out of 17 measurements in the combustion area and environs are clearly statistically older than 30,000 BP, and the range of another three overlaps that chronological horizon. The other eight samples reflect combustion of wood after 30 kyr BP, with half of them being statistically much younger than 28 kyr BP.

Finlayson et al acknowledge the high degree of dispersal, but explain it as a result of 'repeated use', causing 'localized alterations' due to 'for example trampling and cleaning' (p 1). It is no less likely, however, that this pattern relates instead to the fact that the stratigraphic sequence is more problematic than they imply. Their geochemical information indicates that level IV is indeed distinct from the overlying level III (which contains diagnostic later Upper Palaeolithic material, Solutrean and Magdalenian), but stratigraphic distinctiveness does not preclude the kind of post-depositional vertical movement of soil and charcoal particles that was inferred from the trench closer to the entrance whence came the samples published by Pettitt and Bailey. Worms are worms, roots are roots, and both can move samples through the most distinctive of sediments. It seems unquestionable, in any case, that 'trampling and cleaning' of hearths made before 28 kyr BP by Mousterian people living at the site until 28 kyr

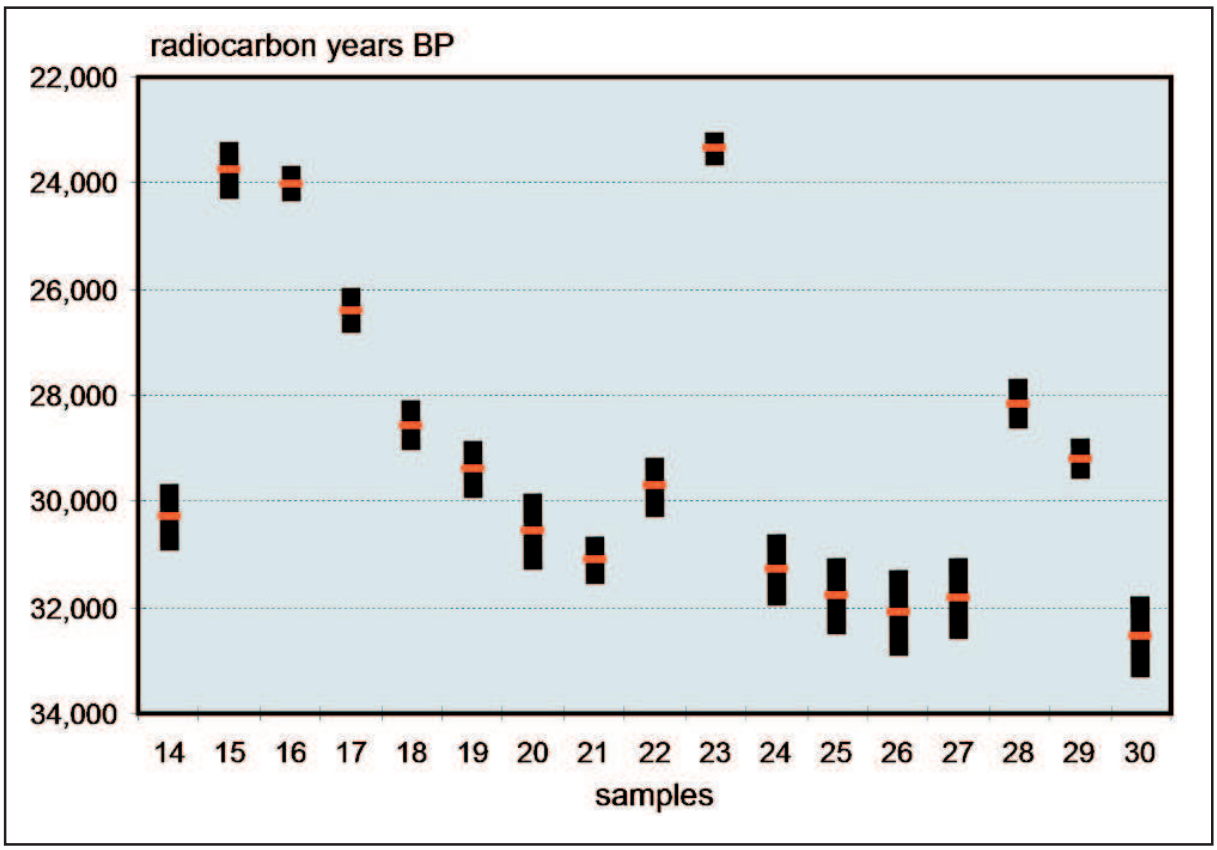

Figure 2 Two sigma age ranges of 17 samples for level IV (after Finlayson et al 2006), from the combustion area or at similar stratigraphic depth. The samples are plotted from left to right as they are recorded on the schematic stratigraphic section of Finlayson et al's Figure 1c

BP cannot explain why the remains of such hearths contain charcoal from wood that was not to be burnt until 26–23 kyr BP.

In this context, the fact that three of Finlayson et al's samples (16, 17 and 20) 'came from *in situ* Mousterian superimposed hearths' providing 'a stratigraphic sequence from 24,010 ± 320 to 30,560 ± 720 yr BP' can hardly be taken as proof of stratigraphic integrity. In any case, none of these samples (the measured age of which was, from top to bottom, ca 24.0, ca 26.4 and ca 30.6 kyr BP) yielded a 28 kyr BP result, so we do not see how this 'sequence' can be used to support the notion that evidence for Neanderthal persistence at the site is secure until 28 kyr BP and less secure beyond that. If the evidence lies in the repeated use, then the Middle Palaeolithic would have continued until 24 or 26, not 28 kyr BP. If, on the other hand, the 28 kyr time horizon is an 'average', how was the average calculated? In fact, the average of those three results is 27, not 28 kyr BP, while the average of all of Finlayson et al's 22 dates for level IV is 29, not 28 kyr BP. Moreover, we note that, at two sigma, samples 9, 11, 15, 16, 17 and 23 form two non-overlapping clusters, fully separated from the other 16; excluding these six younger results, the average is 30.4 kyr BP. We see in this marked temporal clustering of dates from the same area and elevation a strong indication that the Middle Palaeolithic use of the combustion feature had already ceased by ca 30 kyr BP, and conclude that the possibility must be considered that the scatter of significantly younger results is due, at least in part, to post-depositional displacement.

4 Charcoal and artefacts: problems of archaeological association and significance

Problems of stratigraphic integrity and chemical contamination aside, there is also a broader methodological issue here, which relates to the preconceptions of archaeologists when treating their chronometric data; put another way, how reliable are the measurements on the charcoal samples of Finlayson et al as indicators of specific human groups?

Lacking diagnostic Early Upper Palaeolithic (EUP) archaeology in their trench, Finlayson et al erroneously assume that their results are unproblematically indicative of Neanderthal presence. However, all the nine dates associated with the Late Upper Palaeolithic (LUP) assemblages in overlying level III are younger

than 19 kyr BP. Therefore, following Finlayson et al's implied reasoning that absence of evidence is evidence of absence, one could also argue that no occupation of the cave by Upper Palaeolithic people occurred before that time, and, consequently, that a local Neanderthal persistence until as late as 19 kyr BP remained conceivable. Why do Finlayson et al refrain from suggesting that Gorham's Neanderthals may well have been around until Solutrean times? We assume they believe this to be unlikely but, as we have argued above, so is the notion that measurements younger than 30 kyr BP reflect Neanderthals. Just because there is no clear indication of Aurignacian or Gravettian index fossils in the material from the ongoing excavations at Gorham's (which are remarkably limited in extent, given the size of the cave) does not mean that the younger dates do not reflect either these assemblages or the presence of modern humans in this period on Gibraltar.

In this regard, a crucial fact that Finlayson et al fail to address is that the material in layers D, E and F from Waechter's excavations in the entrance area of the cave documents a clear and well-dated EUP occupation. This EUP yielded materials that could not be specifically assigned to a given technocomplex (Aurignacian or Gravettian), but they are unquestionably of an Upper Palaeolithic nature, and, as mentioned above, are associated with dates, some of which from combustion features, in the 28–30 kyr BP range. So, if one were to accept Finlayson et al's view, in this period the cave entrance would have been occupied by Upper Palaeolithic moderns, whereas the back area would have been occupied by Middle Palaeolithic Neanderthals. But how would the Neanderthals access that back part of the cave without going through the Upper Palaeolithic population encamped at the entrance? Or must we infer that the back part of Gorham's contained a self-sustaining, fully troglodytic population of Neanderthals surviving in isolation from the exterior world for two to eight millennia? These questions are obviously rhetorical, but we feel that asking them still helps in illuminating the fact that, in our view, Finlayson et al have not fully considered the implications of their interpretation of the chronometric data.

Finlayson et al's functional interpretation of the combustion area also raises a number of questions, ones that may help in exploring alternative ways of looking at the dating evidence. They describe this area as 'a favoured location that was visited repeatedly over many thousands of years', and where 'hearths were made in the same location many times', taking advantage of a situation 'unique within the cave system', 'where natural light penetrates deep into the cave and where a high ceiling permits ventilation of smoke'. But how do we then explain that the measurements significantly decline in numbers after 30 kyr BP? And why are none apparently older than 34 kyr BP? Is not a parsimonious reading of the entire data set that Neanderthals sporadically used the site between 34 and 30 kyr BP? Furthermore, if this use of the cave was sporadic, how representative might it be as to Neanderthal presence and absence in the region as a whole? And, more to the point, why was such a repeated use restricted to the Middle Palaeolithic only? Given the documented EUP occupation of Gorham's entrance, the possibility must be discussed that the scatter in the dates for level IV, with many going into the range of that occupation, indicates that these EUP people also lit fires in this same favoured location. If Middle Palaeolithic Neanderthals were able to perceive its advantages, why wouldn't they have been perceived by EUP moderns too? Or, to ask another rhetorical question, must we infer that EUP moderns failed to realise those advantages because they were cognitively handicapped by comparison with Middle Palaeolithic Neanderthals?

The reason why Finlayson et al did not consider the possibility that the charcoal in level IV relates to activity in the back part of the cave by both Middle Palaeolithic and EUP people, or EUP activity further towards the cave mouth from which small charcoal samples were blown or trampled towards the rear of the cave, probably lies in the fact that the level only contains lithic materials diagnostic of the Middle Palaeolithic. However, they report that the total number of artefacts recovered is 103, which, for an area of 20 m² and over a thickness of deposit of ca 1 m, means an exceedingly low artefact density (5/m³). This suggests utilisations of this space implying little artefact use and discard, and is consistent with the notion of a sporadic and intermittent use of the site. If so, then the persistence of such a pattern of utilisation into EUP times might well explain why no lithics diagnostic of either the Aurignacian or the Gravettian were recovered.

As documented by numerous examples at different cave sites in Western Europe, including Iberia (cf Bahn & Vertut 1997), one EUP activity that would potentially leave a lot behind in the way of charcoal but little or

nothing in the way of diagnostic artefacts is parietal art. And, although this is not discussed by Finlayson et al, it just happens that the walls of this back part of Gorham's are indeed decorated with Upper Palaeolithic motifs (Balbín et al 2001). These authors tentatively refer them to the Early Magdalenian on stylistic grounds, but they do not exclude that they could be of a later or earlier age. So, the art could well be from the EUP, and it is in any case completely legitimate to speculate that earlier phases of decoration could have existed that were since lost or remain undetected even if that identified does date to the Early Magdalenian.

In sum, the fact that EUP activity is recorded in the cave entrance makes it reasonable to suggest that the dates ≤30 kyr BP reported by Finlayson et al for the combustion area in level IV of the back part of Gorham's may be related to such activity. Thus, use of this area could relate to both Middle Palaeolithic peripheral habitation (leaving a few lithics behind) and sporadic (perhaps artistic) EUP incursions (leaving no lithics behind, or only non-diagnostic ones, such as unretouched flakes) or other EUP activity in other areas of the cave which may, or may not, have left tangible archaeology.

5 Interpretation: the long-term contemporaneity scenario

Finlayson et al argue that 'the transition from the Middle to the Upper Palaeolithic was not, in southern Iberia at least, a sudden rupture but instead took the form of a long and diffuse spatio-temporal mosaic involving populations at low density'. This is the same kind of scenario put forward by Mellars and others since the late 1980s (eg, Mellars 2004, 2005) to explain, ten millennia earlier, the putative long-term contemporaneity between Châtelperronian Neanderthals and Aurignacian moderns in France. However, as a consequence of research carried out over the last decade (eg, Zilhão et al 2006), Mellars' position, for the most part based on illusory patterns of Châtelperronian/Aurignacian interstratification produced by excavation error, faulty intra-site correlation, and poor dating, has become increasingly difficult to sustain. Finlayson et al resurrect the scenario with even less empirical support.

On a theoretical level, we also note that the notion of 'limited contact' may be inconsistent with the notion that populations of both Neanderthals and modern humans were 'thinly scattered across the region'. It is obvious and extensively discussed in the relevant literature that, in order for the reproduction of human societies to be viable, the lower the population densities are, the wider the alliance and exchange networks (including exchange of mates) have to be. For one such network of some 500 people (the minimum required for long-term survival), and at a low population density of, say, 0.01 persons/km², you need 50,000 km²; if you are based in Gibraltar, that amounts to a strip of land extending 150 km east, west and north, ie, eastward, fully overlapping with a similar network centred in Málaga where, in the scenario of Finlayson et al, based on evidence from the cave site of Bajondillo, modern humans would already be living since ca 32 kyr BP. So, their scenario amounts to suggest that, over many millennia, Neanderthals and modern humans lived in total sympatry, with extensively overlapping mating networks, but with virtually no contact and exchange of genes and culture. Unlike Mellars, however, Finlayson et al do not argue for the existence of major cognitive differences that would have represented an effective barrier to interbreeding and cultural interaction. Their scenario, therefore, is one where, despite sympatry, two fully symbolic species of humans, differing in ecology but with similar capabilities, would have not interacted, biologically and culturally, for many millennia. In our opinion, such a scenario stretches the bounds of credulity.

Commenting on the new dates for Gorham's, Delson and Harvati (2006) add another strand to Finlayson et al's long-term contemporaneity argument by stating that 'until now, one of the main objections to the acceptance of [the Lagar Velho child] as a possible hybrid … has been its chronology', because 'the specimen dates to several millennia after the Neanderthals were thought to have disappeared' and, hence, is 'much too recent to be a hybrid'. They further add that 'this criticism would no longer hold if Finlayson and colleagues' youngest dates [indicating a Neanderthal survival until 24 kyr BP] could be accepted'. In reality, the interpretation of the Lagar Velho child (Duarte et al 1999; Zilhão & Trinkaus 2002) was based on the assumption that Neanderthals had survived in Iberia until 30 kyr BP, and certainly no later than 28 kyr BP, and that the mosaic of Neanderthal and modern features apparent in its anatomy reflected extensive admixture at the time of contact, many millennia before the child had been born. This interpretation stands irrespective of whether Finlayson et al's (2006) claims

are accepted or rejected.

6 Conclusion

Finlayson et al claim that a series of 22 new radiocarbon dates for level IV from an area excavated at the back of Gorham's cave provide evidence for the survival of a Neanderthal population in Gibraltar until 28 kyr BP and, with less certainty, until 24 kyr BP. They then derive a scenario of long-term contemporaneity between these late Neanderthals and early modern human populations that would have been present in the region since 32 kyr BP.

Their claims are based on several assumptions about the nature of their data which are critical to the success of their argument. First, they assume that all measurements are correct. Secondly, they assume that level IV is intact and uncontaminated by anything intrusive from level III above, but they do not demonstrate how they have arrived at this confidence; given the remarkably small size of their measured samples, we would be very surprised if such stratigraphic mobility had not occurred (and it is irrelevant here that level IV is geochemically distinct from level III). Thirdly, they assume that level IV contains a pure Middle Palaeolithic assemblage. Fourthly, they assume that all measured age ranges must relate to this assemblage, which by this argument would have accumulated until around 23 kyr BP.

All things considered, these assumptions are unsupported; the majority of available results place the Middle Palaeolithic in level IV in the 30–34 kyr BP range, ie, in the same range obtained for the area closer to the entrance excavated in the late 1990s, and the more parsimonious reading of the new evidence remains that the charcoal in that level relates to the use of the back part of the cave by both Middle Palaeolithic Neanderthals and EUP moderns. Finlayson et al need to falsify this hypothesis before their view of a survival of the Mousterian at the site to or beyond 28 kyr BP can be accepted for discussion. In any case, it is at least undeniable that the cultural and biological affinities of the samples dated to between 30 and 19 kyr BP remain ambiguous and that, in order to resolve the ambiguities, further research is required (including not only more excavation but also analyses of lithic taphonomy, namely the testing via refitting of different hypotheses concerning stratigraphic integrity and intrasite relations).

Additional dating work is also clearly in order. For instance, the hypothesis raised above that the scatter in the dates could relate to incomplete decontamination of the samples that yielded the younger ages, due to their microscopic size, would be easy to test by dating associated cut-marked or otherwise humanly modified bones. Whether charred or uncharred (and thus whether or not directly associated with the hearths), such bones would in of and themselves constitute direct evidence of human activity at the site, and the dates obtained for them would likewise represent direct estimates of the chronology of such activity. We appreciate that charred bone is a difficult sample material to date as burning usually destroys carbon, but a cumulative use of available indicators of unambiguous human activity can only be a positive step. If the (unburnt) bone samples (measured, for example, using the Oxford Radiocarbon Accelerator Unit's new ultrafiltration stage of pre-treatment; Higham et al 2006) only gave results in the 30–32 kyr BP range, one would be forced to conclude that incomplete decontamination of some of the samples must indeed explain the scatter in the charcoal dates. Conversely, if the bone dates were to replicate the range obtained with charcoal, then it would be clear that level IV of this part of the cave did record use as a combustion area at scattered times throughout some ten millennia. One would then have to discuss whether such multiple uses were at all 'continuous', and whether the dates obtained for the different episodes of use also reflected a EUP activity component, instead of being indeed exclusively related to Middle Palaeolithic people, as assumed by Finlayson et al.

Finlayson et al's argument for a late persistence of Neanderthals in the Gibraltar region illustrates well the numerous problems in supporting statements of this nature. Prehistorians are at the mercy of notoriously inaccurate and imprecise dates, and it does us no favour to brush aside problems with sample selection, sample mobility, potential for chronometric error, simplistic extension of problems encountered elsewhere with the assumption that they are relevant for the situation at hand, lack of consideration of age ranges and of exactly what measured samples reflect in the way of human activity. Dating specialists are working hard to overcome the problems with chronometry, but there will always be limits to the interpretation of the better 'dates' they eventually produce. Such an interpretation remains primarily a task for the prehistorian, and one where, in our opinion, the field should validate for discussion only those scenarios that are supported by a rigorous taphonomic critique of the data sets.

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