Reply to comment on "U-Th dating of carbonate crusts reveals Neandertal origin of Iberian cave art".

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Introduction

Slimak *et al*.[1] present a technical comment motivated by a perceived inconsistency between our oldest dates for parietal art[2], i.e. those >65 ka old, and the wider archaeological record. The authors challenge our oldest results by raising methodological questions relating to: 1) open-system behavior, 2) non-radiogenic ^{230}Th in source water, and 3) detrital contamination corrections. While these topics have formed the focus of discussion in previous publications[3,4] and were thoroughly assessed in our paper, we show here that their criticisms have no scientific basis and that their archaeological arguments are circular or unfounded.

Archaeological criticisms

Slimak *et al*.[1] see an early origin for parietal art as problematic for two reasons: 1) a supposed lack of parietal art after the >65 ka examples in our publication, i.e. for the succeeding ~25 ka until the Upper Paleolithic, and 2) an absence of portable art from contemporaneous Middle Paleolithic archaeology.

Slimak *et al.*'s supposed ~25 ka hiatus results, quite simply, from their misunderstanding of the logic of working with minimum ages; contrary to their claim, our results in no way can be taken to imply the existence of such a hiatus. We present a minimum age for painting in Ardales of 45.9 ka (ARD16) and a minimum-maximum pair of 32.1 and 63.7 ka (ARD08, 09 and 06). Either of these painting episodes could fall in Slimak *et al.*'s 'hiatus,' as could the El Castillo red disk dated to before 40.8 ka ago[5]. Indeed, bar those >65 ka results questioned by Slimak *et al.*, any of the hundreds of minimum ages we have obtained is consistent with an age in the 40–65 ka interval for the stratigraphically associated paintings. On present evidence, the most likely scenario is that parietal art was first created prior to 65 ka, and this tradition continued, perhaps episodically, throughout the remainder of the Paleolithic.

It is likely that further work and additional maximum and minimum ages would better characterize the temporal and geographic extent of Neandertal painting — namely in France, which hosts over half of all caves containing Palaeolithic art. However, on 24.10.2013, subsequent to having been informed that we had obtained results in excess of 74 ka for painted speleothems from the Grotte des Merveilles (Rocamadour), the French Commission Nationale des Monuments Historiques (CNMH) issued a moratorium forbidding any additional sampling of calcite for cave art dating purposes anywhere in the country[4]. Two of the authors of Slimak *et al*. were/are members of the CNMH and therefore must be aware of this obstacle; one which led to denial of our request for permission to re-sample at the Grotte des Merveilles in order to obtain corroborating evidence, and prevented us from adding such early results to our database.

With regards to portable art, note that all the parietal art dated to >65 ka is non-figurative: i.e. lines, hand stencils, and pigment shapes. If these motifs fully represent the earliest phase of parietal art and no figurative art was being made at that time, then equivalent portable art, if it exists, will be of a very different nature than for the Upper Paleolithic. Some potential candidates do exist in the European Middle Paleolithic, in the form of linear and in some cases clearly structured forms[6,7]. Moreover, pigment-stained shells such as those found in Spain prior to 115 ka[8] are non-figurative, portable, symbolic objects.

Even if we discount these, an absence of portable art is irrelevant to the existence of parietal art. The contemporaneity of the impressive portable figurines of the European Upper Paleolithic and figurative parietal representations is specific to that context, and not necessarily relevant to earlier phases of art, or indeed to coeval or later prehistoric and ethnographic art in other parts of the world. Why would we expect Neandertals to behave symbolically and culturally the same as modern human groups at least 25 ka later? This is not even the case for earlier modern humans in Africa where there is a gap of at least 50 ka between evidence for symbolic objects and the earliest figurative paintings[9]. But we would not, on these grounds, question the evidence of pigment processing and non-figurative art at Blombos Cave prior to 75 ka BP[10]. It should not be assumed that the two necessarily go together. Nor should authenticity require an uninterrupted chronological sequence of similar material from the time of first appearance onwards, as in the case of the much longer hiatus in the evidence for personal ornaments in post-Still Bay southern Africa[11]. Clearly the inconsistency with the wider archaeological record resides in Slimak *et al*.'s argument, not with the Neandertal authorship of art implied by our results.

U-Th dating of carbonates associated with cave art

Slimak *et al*.[1] claim that U-Th dating of carbonate crusts associated with cave art is both a "disputable approach" and "a recent development". Whilst we appreciate that many readers will not be familiar with the details of the dating technique, their argument does exaggerate somewhat the 'novelty' of dating carbonates by U-Th techniques – which has been performed more widely by the geochemical community for over half a century. In relation to cave art, the approach has been used for over a decade, and the intricacies of such an application, including considerations of open-system behavior and detrital Th correction, have been thoroughly discussed in recent peer-reviewed articles[3,4]. It is therefore worth stressing that the dating presented by Hoffmann *et al*.[2] has a firm scientific basis, and one indeed that other laboratories are employing[12].

Regarding open-system behavior, as Slimak *et al*. acknowledge, we employ a sequential sampling methodology to test for such an occurrence. When dates for sub-samples are in correct stratigraphic order (i.e. from younger to older systematically from the 'outside' of a crust inwards towards the pigment), it is possible to have confidence that the carbonate has remained a closed-system. In an open-system, it is highly unlikely that the chronological order of sub-samples would be preserved. We have published multiple sequences of three or more sub-samples with ages in the expected stratigraphic order, including examples from all three caves under consideration, and can therefore demonstrate that open-system behavior is not an issue here.

Concerning non-radiogenic ^{230}Th entering the carbonates from the source water, we have dated speleothem samples from all three sites to the very recent past, i.e. ~1 ka (e.g. see PAS35a and c[2], and Fig. 2). This is entirely inconsistent with the hypothesis of high ^{230}Th drip water; dates as young as ~1 ka cannot be obtained by U-Th if the drip water has a high ^{230}Th content.

Our approach for detrital Th corrections is discussed in detail in our paper's Supplementary Information[2]. Slimak *et al*. use the $(^{232}Th/^{234}U)_A$, i.e. $^{232}Th/^{234}U$ activity ratio, to assess the degree of detrital contamination, and state that a ratio of ~0.005 is widely considered to be "the upper limit for highly reliable ages". All carbonate samples will be contaminated by detrital Th to some degree, and a threshold of reliability based on $(^{232}Th/^{234}U)_A$ is entirely arbitrary. Whilst we acknowledge that samples with higher degrees of detrital Th need to be corrected for, this is a standard procedure in U-Th geochemistry, and by applying appropriate corrections the calculated ages can still be considered reliable even in cases where samples are characterized by $(^{232}Th/^{234}U)_A$ >0.005. The authors provide a figure showing corrected U-Th age versus measured $(^{232}Th/^{234}U)_A$ for some of the data presented by Hoffmann *et al*.[2], and suggest a positive correlation between the two to support claims that the older ages, i.e. those of the order of ~65 ka, are not reliable. The data presented in their figure is highly selective and hence dangerously misleading, and when the datasets are considered in their entirety (see below) it is obvious that the implied positive correlation does not exist. The 'relationship' shown by Slimak *et al*. is, in fact, irrelevant and has no implications other than demonstrating that higher degrees of detritus result in higher uncertainties.

It is true that La Pasiega PAS34c has large uncertainties due to the detrital Th correction; this was discussed at length in the Supplementary Information of our publication. There, we demonstrated that the chosen correction factor is appropriate by looking at the $(^{234}U/^{238}U)_A$, which is also affected by the detrital correction. A higher detrital $(^{238}U/^{232}Th)_A$ yields an initial $(^{234}U/^{238}U)_A$ for PAS34c inconsistent with all other samples from this cave. The uncertainties of the correction we used are fully propagated, and whilst the PAS34c result is less precise than the other dates presented, within the propagated uncertainties it is robust. Furthermore, the reliability of U-Th dating of crusts in La Pasiega in general has been demonstrated by dating many crusts with multiple sub-samples[2,3]. Even if the authors mistakenly want to reject the PAS34c sample, the PAS34a and PAS34b samples provide a minimum age of 53.0 ka, which must be associated with pre-Upper Paleolithic activity.

The approach taken by Slimak *et al*. to use the three results on PAS34 to derive an isochron and use this result to argue for both a higher correction factor and a younger minimum age is scientifically dubious for two reasons. First, deriving an isochron from three points is not scientifically sound; a minimum of five data points would instead be needed. Secondly, the assumption that the crust formed within a short time and that all three samples are of the same age is not supported. Previous results[2,3] demonstrate that this type of formation usually forms very slowly or episodically. Thus, the pseudo-isochron results presented by Slimak *et al*. are again highly misleading. To demonstrate this we provide an example for a flowstone section from Ragged Staff Cave, Gibraltar, dated at mm resolution by U-Th (Fig. 1a). The dates reveal a 6 ka long hiatus at 40 mm. The sample just before the hiatus has a higher degree of detrital contamination than the following samples. However, the suite of dating results before and after 40 mm clearly confirms the stop in growth. If we follow the same approach as Slimak *et al*. and use only three data points, one just before the hiatus and two thereafter, to derive an 'isochron' (i.e. assuming all are of similar age and the difference in detritus is the reason for the age difference) then we obtain an age of 36 ± 3 ka and a detrital $(^{238}U/^{232}Th)_A$ of 5.7 ± 0.5 (Fig. 1b). The age is clearly wrong for the sample before the hiatus, and the very high detrital correction factor is largely a result of the faulty assumption that the samples are coeval. The result is biased by the pair of younger samples which, coincidentally, also have smaller detrital contamination, exactly as is the case for PAS34. Unless Slimak *et al*. can demonstrate PAS34a-c are contemporary, their isochron approach is inappropriate.

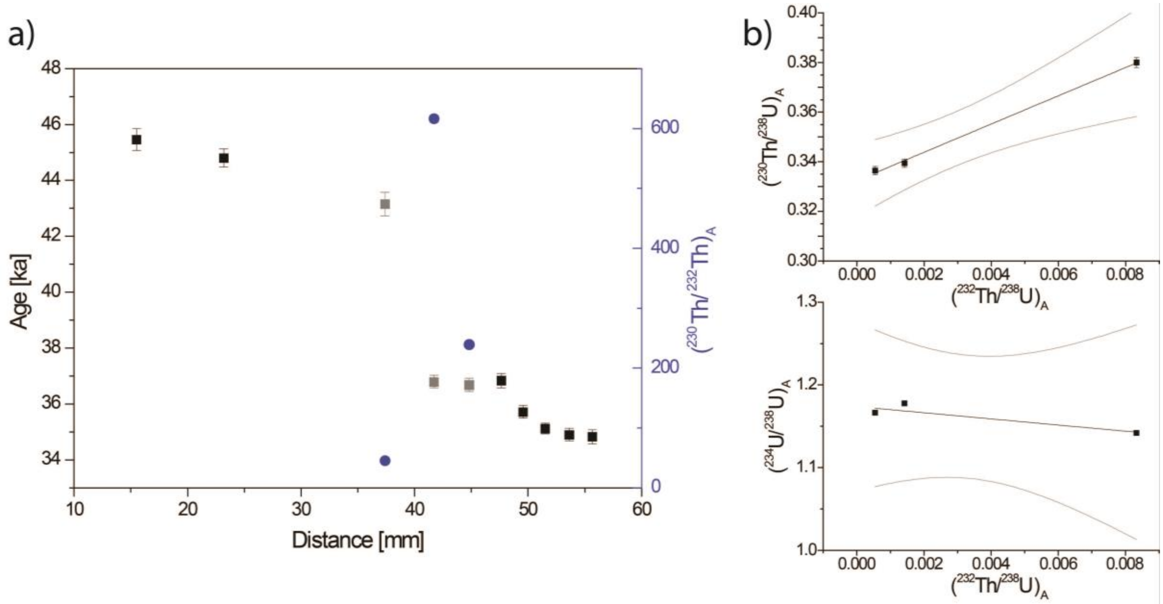

Figure 1: a) U-Th dating results across a 5 cm section of a flowstone. The results clearly reveal a 6 ka long growth stop at 40 mm. Dating results before and after the growth stop are supported by the full dataset. The three grey data points are used to obtain an Osmond type pseudo-isochron, as done for PAS34a-c by Slimak *et al*., and shown in b).

Of more importance than an arbitrary boundary of measured $(^{238}U/^{232}Th)_A$ for 'reliable' U-Th ages, is the sensitivity to the applied correction of the resulting corrected age. To demonstrate the insensitivity of our oldest dates to detrital correction, Fig. 2 shows corrected ages and $(^{232}Th/^{234}U)_A$ for all the published Ardales data[2] alongside unpublished data for other speleothems found in association with art in this cave. Corrected ages are shown using two detrital $(^{238}U/^{232}Th)_A$ values: 0.8 ± 0.4, i.e. bulk earth, the preferred value for this site, and an elevated value of 5 ± 2.5. Firstly, it is clear that there is no positive correlation between age and $(^{232}Th/^{234}U)_A$. Secondly, the shift in the two sets of corrected ages is not significant. This is because the samples from this site are, in fact, all relatively clean. Finally, the measured $(^{238}U/^{232}Th)_A$ of a sample constrains the maximum possible detrital $(^{238}U/^{232}Th)_A$. i.e. the (unrealistic) scenario in which all the uranium in a sample would be derived from detritus. Following the widely accepted assumption that the ^{238}U decay chain in a detrital component will be in secular equilibrium, the detrital $(^{238}U/^{232}Th)_A$ also cannot be higher than the measured $(^{234}U/^{232}Th)_A$ and $(^{230}Th/^{232}Th)_A$ of the sample. Based on the measured activity ratios of samples from Ardales, detrital $(^{238}U/^{232}Th)_A$ values of ≥5 produce unresolvable dates for an increasing number of samples, and therefore represent an absolute maximum value. Even with this unrealistic value of 5, ARD13b still gives a minimum age of 59.0 ka, significantly earlier than the dates in the order of ~47 ka preferred by Slimak *et al*. A highly unrealistic detrital $(^{238}U/^{232}Th)_A$ value of ≥11 (assuming a 50% uncertainty) is required before the corrected age of this sample is in the order of ~47 ka. When applying detrital Th corrections to relatively clean samples such as those from Ardales, using the bulk earth $(^{238}U/^{232}Th)_A$ with a conservative error is adequate, and our applied detrital corrections are robust. There is no scientific basis on which to discount the minimum age of 65.5 ka calculated for ARD13b.

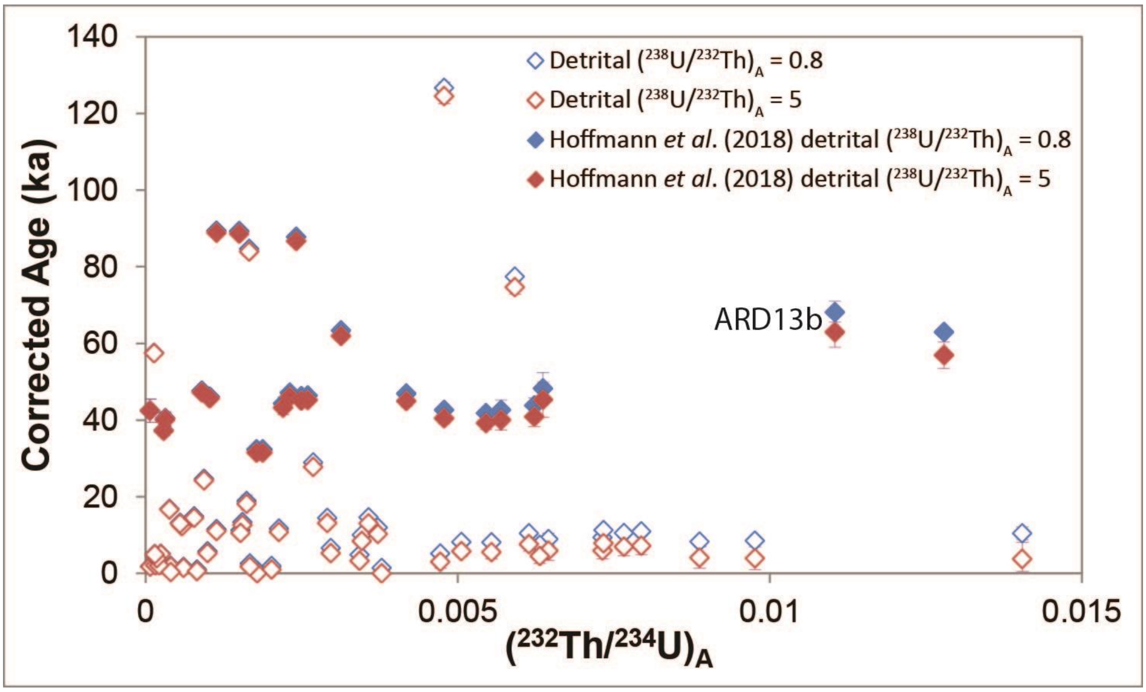

Figure 2: Corrected ages and $(^{232}Th/^{234}U)_A$ for carbonate samples associated with art (i.e. as maximum or minimum ages) in Ardales cave. Closed symbols reflect published data[2], open symbols are unpublished data.

Samples from Maltravieso are characterized by higher detrital Th, and it is for this reason that extra effort was made to characterize the detrital component directly. Because samples were taken sequentially on carbonate deposits of complex geometry, it was not possible to collect coeval samples allowing isochron methodologies to be utilized, and the level of insoluble residues was insufficient to enable direct analysis of the detrital fraction itself. Therefore, sediment from the cave was collected and analyzed as a proxy for the detrital fraction of the samples. A speleothem column was also sampled and a series of six growth layers dated to provide a control for this sediment-derived correction (which uses a detrital $(^{238}U/^{232}Th)_A$ of 3.3 ± 0.2). This is fully explained in the Supplementary Information of Hoffmann *et al*.[2], and as Slimak *et al*. accept, this is "a much better approximation for the Maltravieso site than the bulk earth ratio".

Fig. 3 shows the corrected ages versus $(^{232}Th/^{234}U)_A$ for all the Maltravieso data from Hoffmann *et al*.[2]. Corrected ages are shown using two detrital $(^{238}U/^{232}Th)_A$ values: 3.3 ± 0.2, i.e. the sediment-derived correction value preferred for this site, and an elevated value of 4 ± 2. Detrital $(^{238}U/^{232}Th)_A$ of ≥4 are not possible as beyond this limit values exceed the equivalent measured $(^{230}Th/^{232}Th)_A$ of an increasing number of samples. Irrespective of this, and as can be seen in Fig. 3, shifting the detrital $(^{232}Th/^{234}U)_A$ to this high value has very little effect on the corrected ages, for example giving a minimum age of 64.9 ka (instead of 66.7 ka) for MAL13a. Whilst the sample does contain a notable detrital component, it is not so high that the detrital correction has a significant impact on its corrected age, and certainly not to the extent where we would change our conclusion about Neandertal authorship of the art.

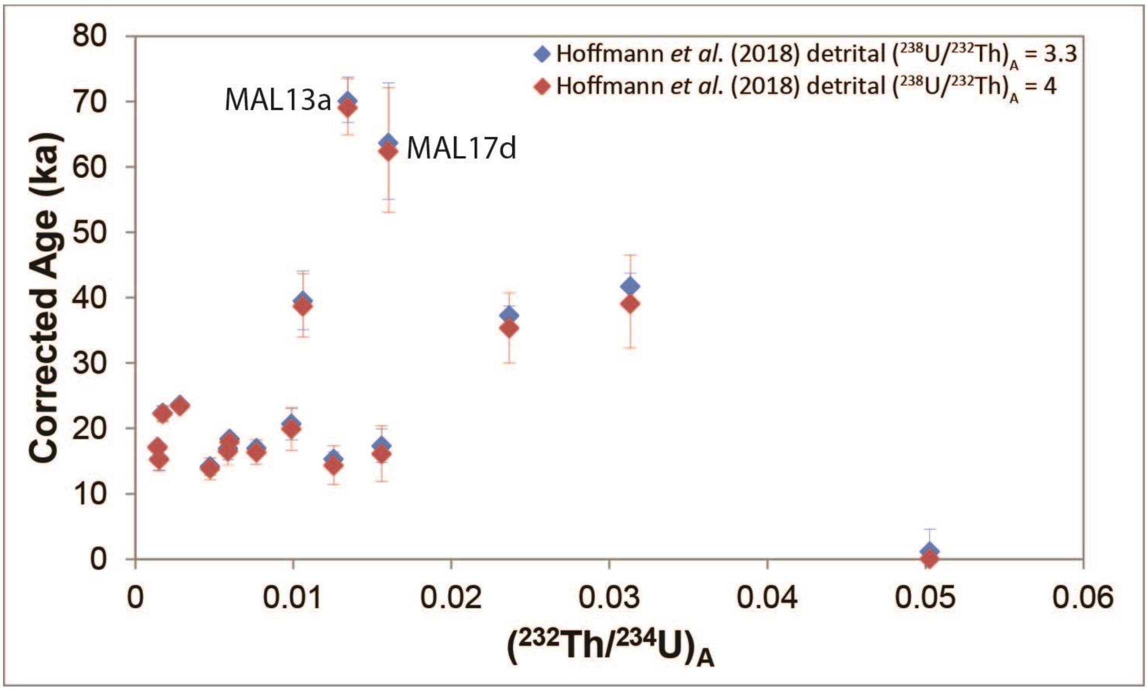

Figure 3: Corrected ages and $(^{232}Th/^{234}U)_A$ for carbonate samples associated with panel GS3b in Maltravieso cave[2].

Slimak *et al*. state that the range of dates from Maltravieso challenges the reliability of the date obtained for MAL13a. This is not true. Although all the samples were taken from relative proximity, the area is a complex combination of numerous individual deposits growing with multiple nuclei points and there is no reason why they should all be the same age. In fact, when all the dates are considered, a coherent picture of speleothem growth becomes apparent with three phases of growth at around ~20 ka, ~40 ka, and ~70 ka (Fig. 3). Slimak *et al*. also state that MAL13a represents “a single sample standing out of a larger group of samples”. Again, this is incorrect. MAL17d also supports an early origin for the hand stencil, providing a second Middle Paleolithic minimum age (63.6 +9.6 -8.4 ka). This is the oldest sub-sample in a sequence of four, thus providing a strong case for closed-system behavior. There is no scientific basis on which to discount the minimum age of 66.7 ka calculated for MAL13a.

Summary

The perceived inconsistency between the existence of early parietal art prior to ~65 ka and the absence of contemporaneous portable art in the Middle Paleolithic along with a supposed ~25 ka hiatus in the production of parietal art, is groundless. Furthermore, the methodological critiques concerning open-system behavior, source water derived ^{230}Th, and detrital Th corrections are not supported by the geochemical data. Slimak *et al*.[1] accept that they provide no evidence to prove our results are inaccurate, and indeed there is no scientific basis on which to discount the oldest of our dates.

Slimak *et al*. cap their comment with the insinuation that the red pigment found on the Ardales speleothems might be non-anthropogenic, something that no one has suggested until now. Since there is little iron in the Ardales limestone, and none visible in the cave’s drip water, how could such colorant material have accumulated by natural processes, and only at certain places, in certain periods, and often forming clearly patterned shapes? It seems more than a coincidence that despite a century of study, the anthropogenic origin of these color concentrations is being questioned only after there is scientific evidence that they were created by Neandertals.

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