The age of fossil StW573 ('Little Foot'): Reply to comments by Stratford et al. (2017)

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We reply to comments by Stratford et al.[1] on our article[2] 'The age of fossil StW573 ('Little Foot'): An alternative interpretation of $^{26}Al/^{10}Be$ burial data', in which we revisit the burial age reported by Granger et al.[3] for the sediments encasing the fossil and the data on which this was based.

The history of the discovery of fossil StW573 and the subsequent controversy concerning its age is well documented.[3-10] The burial isochron age of 3.67±0.16 Ma, obtained by Granger et al.[3] via regression of ^{26}Al and ^{10}Be concentrations in quartz samples taken from the deposit encasing the fossil, has been broadly accepted as the final settlement of the debate.

As Stratford et al.[1] emphasise, the potential importance of the fossil in the timeline of hominin evolution is huge. It is for that reason that we re-examined the data in order to find if this reported burial age truly represents the only possible interpretation of these data for yielding the age of the fossil. We found that one aspect of the data, not considered by Granger et al.[3], prohibited an age older than 2.8 Ma for the fossil, and in order to resolve the paradox we proposed a two-stage burial scenario, with the sediment material encasing StW573 having been derived from a higher level cave chamber, rather than directly from the surface. Concluding, we state that, as this scenario 'can reconcile the indicated 2.8 Ma maximum age for the fossil with the much older isochron date, it deserves serious consideration'[2].

The discussion has now been re-opened. In their comment, Stratford et al.[1] conclude that our analysis is 'purely hypothetical and based on unjustifiable assumptions rather than observations and measurements', and state further that 'all data and observations are consistent with a single episode of deposition contemporaneous with StW573 at 3.67±0.17 (sic) Ma'.

They reach this conclusion based on 11 points characterised as 'a series of assumptions that are unjustified and based on demonstrably incorrect interpretations of the cave structure and stratigraphy'[1]. Here we address these points, which were fortunately numbered so that the reader is spared any repetition.

Point 1: This issue is semantic. In geochronology, a 'lower' and 'upper' age limit is commonly understood to mean the minimum and maximum age for a possible age range. There is thus no contradiction with Clarke[7] as the authors claim.

Point 2: This is a substantive argument which will be considered in the discussion below.

Point 3: The detailed microstratigraphic work referred to by the authors is limited to a small portion of Member 2 in the Silberberg Grotto and restricted to a distal part of the debris cone, removed from the point where sediment entered the Grotto. If this sediment entry point had opened up, or been modified as a result of a collapse or a shift in the sediment passageways higher in the chamber, proximal effects like collapse blocks, may only be seen in the immediate vicinity of a modified entry point. No detailed stratigraphic work exists for most of the Silberberg Grotto that contradicts such a scenario. The authors also state here that 'the whole depth of Member 2….is stratified consistently and conformably – indicative of a long and progressive accumulation'. Note that Granger et al.'s[3] samples 3 and 9, taken at a vertical distance of between 2.8 m and 3.3 m in the deposit, have the same burial age well within their uncertainty limits of ca 0.8 Ma and 0.2 Ma, respectively. For a theoretical isochron to be valid, the samples included in the regression must have been deposited over a (geologically speaking) short period of time, consistent with rapid changes in the sedimentary regime. The statement is further in direct contradiction to their statement on 'a single episode of deposition' quoted above.

Point 4: It appears to be assumed here that all secondary deposits would have to be similar to each other. However, secondary deposits will vary as a function of many parameters including cave geometry, sedimentation rates, proximity to sediment entry points and provenance. In addition we want to restate that we do not envisage that the StW573 skeleton was redeposited over a great distance, but that instead the animal fell into the Silberberg Grotto at the time the sediments in the Grotto accumulated; in our view the sediments surrounding the fossil were redeposited, not the fossil itself.

Point 5: In Figure 1 of Stratford et al.[1], the corrected position of the collapsed block shows the walkway ending in the middle of it. As can be seen in our Figure 2b, the walkway abuts on the south side of it. However, the precise position of the collapsed block is less important than the fact that it unequivocally documents the previous existence of a cave chamber above the eastern end of the present Silberberg Grotto (shown in yellow in our Figure 2a). This suggests any opening in the roof of the Silberberg Grotto was connected to a higher level chamber in the cave, and at the time of deposition of Member 2 was unlikely to connect directly to the surface. We further emphasise again that, following the erosion rate of about 5 m/Ma reported by Granger et al.[3], the land surface at Sterkfontein should have been about 14 m higher at 2.8 Ma than it is today, and thus there was room for a cave chamber above the Silberberg Grotto at that time. The suggestion of a previous upper cave above Silberberg Grotto is thus not entirely speculative, as Stratford et al.[1] claim. The fact that the collapse blocks occur in Member 4 to reflect roof collapse of an upper chamber, has no bearing on how such collapse would have affected sedimentation processes in the deeper Silberberg Grotto, other than the fact that sudden changes in the sedimentary regime in the cave did occur. The point we are making is twofold: an upper chamber probably existed above the Silberberg Grotto, and the sediment passageway connecting the Silberberg Grotto with an upper chamber could have changed its geometry over time. We did not, and do not, explicitly posit that this upper cave was separate from the one that contained Member 4 (now in the open excavation), but from outcrop observations this cannot be excluded either.

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Point 6: Whilst Bruxelles et al.[11] do not interpret the sediments surrounding StW573 as being debris flows, we point out that their descriptions of the sediments are entirely consistent with debris flows. The fact that the body was mummified does not exclude this interpretation. The dried out body may have been enveloped by the debris flow, in a manner similar to that described for *A. sediba* at the Malapa site (Dirks et al.[12]).

Point 7: This comment is puzzling. The chert fragments must indeed have been derived from a few metres below surface, as indicated by their aggregate ^{10}Be and ^{26}Al concentrations. That is all we know for certain. We do not see an essential difference between our wording and that of Granger et al.[3] Stratford et al.'s[1] assertion here that they have to come from the same chamber is unfounded.

Point 8: Here the authors criticise our reference to the shelf stones as evidence that the Silberberg Grotto was once flooded. The work cited in this instance (and with this interpretation) was Clarke[13] who reported shelf stones attached to stubby stalactites. Thus the authors indirectly criticise the work of one of their number.

Point 9: Of the boreholes drilled around the Sterkfontein open excavation,[14] BH1 is immediately south of the east end of the Silberberg Grotto, and shows calcified clastic cave sediments and calcite speleothems from the surface down to a depth of 16 m. BH5, drilled immediately north of the west end of the Grotto, shows dolomite down to 7.5 m and then cave sediments down to 21.5 m. The flowstone at the base of the cave sediments in BH1 was dated at 2.80 ± 0.28 Ma by U-Pb (logging and age from Pickering and Kramers[9]). Thus it is not true that M4 and M5 in the open excavation represent the only higher level cave fills in the vicinity of Silberberg Grotto. The argument that the lack of *Australopithecus africanus* fossils in Member 2 indicates that therefore it cannot be derived from more hominin-rich reworked sediments, now potentially included in Member 4, would assume detailed knowledge of the timing and provenance of the Member 2 sediment. This type of information is not available.

Point 10: On 26 February 2009, one of us (J.K.) visited StW573 in the company of Ron Clarke and the late Tim Partridge. On that occasion the flowstones around the fossil (F2, F3 and F4) were examined and J.K. pointed out that these flowstones would most likely turn out to be fracture fillings once the fossil was excavated and the outcrop clear. This was subsequently confirmed by Bruxelles et al.[11] (therefore the statement that we 'admit' the intrusive character of these flowstones is strange and inconsistent with the actual facts). However, despite having both visited the locality several times, we have seen no evidence to convince us that the lowest flowstone, F1, is not a stratigraphic one. Also, Bruxelles et al.[11] hardly describe this flowstone and present no such evidence. In our discussion of the palaeomagnetic results of Herries and Shaw[10] we clearly mention both possibilities for F1 and discuss the consequences.

Point 11: The most reliable currently available age range for Member 3 of Makapansgat is 2.58–2.85 Ma (Herries et al.[15]), not 3 Ma as quoted[1]. We admit that this age is older than 2.5 Ma and apologise for the error. This range is nevertheless much younger than 3.67 Ma.

Thus of the 'assumptions that are unjustified and based on demonstrably incorrect interpretations of the cave structure and stratigraphy'[1], Points 1 and 3–11, most of which are marginal to our arguments, have been answered and can be put aside. We now turn to the core issues, which are our analysis of data on two chert fragments and questions around the isochron itself, referring to Point 2 as well as Stratford et al.'s[1] conclusion.

In Point 2, Stratford et al.[1] find no fundamental fault in our direct calculation of the underground production rate of ^{10}Be and ^{26}Al, but criticise the fact that no uncertainty limits were given, and state (1) that we used a constant erosion rate, (2) that we fixed the ^{26}Al/^{10}Be production rate at 8.1, (3) that depth, density and erosion rates in the past are not known with sufficient confidence and (4) that production rates at depth cannot be calculated exactly, citing Balco[16], an excellent review that became available online in February 2017, when our paper was in press. They further state that their isochron regression solves directly for underground production rates, implying that our estimates were unnecessary.

The methods and parameters we used are documented in our paper. The key parameters for the production rates, i.e. probability factors and effective cross sections, were taken from Balco et al.[17], where they are given without error limits, making error propagation difficult. We did not fix the ^{26}Al/^{10}Be production rate, but calculated ^{26}Al and ^{10}Be separately. We used both a zero erosion model and one with an erosion rate of 5 m/Ma (as found by Granger et al.[3]). The depth of StW573 below the present day surface (23 m) was taken from Clarke[13] although Partridge et al.[6] give 25 m, which would yield lower production rates. The density is discussed in terms of likely porosity. Obviously there are uncertainties, but we have chosen to err on the conservative side.

Our finding of lower in-situ production rates than those derived from Granger et al.'s[3] isochron regression is not based on the direct calculation alone. A second approach is based on one of the samples analysed by Granger et al.[3] – MC2A – which was considered 'reworked' and not included in the isochron regression. We found that the combination of ^{26}Al and ^{10}Be concentrations in this sample are impossible if the in-situ production rates yielded by the isochron regression are assumed.[2] As we describe[2], the upper limits of production rates for which the ^{26}Al and ^{10}Be concentrations in MC2A are realistic coincide with the range of directly calculated values. The fact that Stratford et al.[1] do not comment on this indicates that they accept it.

We also point out that two of the samples included in the isochron regression of Granger et al.[3] are composites: 'STM2 dark', consisting of material from ST1, 2, 8 and 9, and 'STM2 light', consisting of material from ST1 and 2, selected according to the presence or absence of Fe-Mn oxide staining. Using data from composite samples in a regression can be compared to homogenising a sample to show it is homogeneous. As a result of this practice, the uncertainty limits given for the isochron regression of Granger et al.[3] as well as the underground production rates derived from it are underestimated by an unknown amount. Our lower values for the in-situ produced ^{26}Al and ^{10}Be are likely to fit comfortably within the real uncertainty limits from the isochron regression and are thus not in conflict with these.

Notwithstanding the above caveat, we did not deconstruct the isochron as Stratford et al.[1] claim. We merely queried the interpretation of it by Granger et al.[3] There are two questions here: (1) how long has the sediment material (on average) been underground and (2) how old is the sedimentary deposit containing StW573? The isochron addresses the first question, and our approach the second one. This approach is quite robust as long as the analytical results and their given precision are accurate, which we have no reason to doubt. The fact that the apparent maximum age for the deposit is derived from one sample only can be compared to a situation in which the maximum age for a sedimentary unit is given by the youngest detrital zircon found in it, which is nothing new.

Our proposal of previous burial of the sediment material in an upper cave chamber was made to reconcile the apparent contradiction between the 3.67 ± 0.16 Ma isochron date and our 2.8 Ma maximum age for the deposit. We explored whether such previous burial (over a long and variable period of time) could still produce the observed isochron, and concluded that this was possible. This exploration elicited no criticism from Stratford et al.[1] The inverse order of the three burial ages of Partridge et al.[6] with stratigraphy (although within error), noted also by Herries and Shaw[10], can also be understood as a result of previous burial.

We conclude that none of the points raised by Stratford et al.[1] invalidates our data analysis and the two-staged burial hypothesis. We have proposed a way in which this two-staged scenario can be tested, namely by doing more analyses on originally surface-derived samples to test for burial age heterogeneity outside the now available better uncertainty limits. Although this has not been responded to by Stratford et al.[1], it is a viable way forward. The discussion on the age of StW573, 'Little Foot', should not be dismissed. While this discussion may appear somewhat arcane at the moment, the age of this remarkable and complete *Australopithecus* fossil will become extremely important in the timeline of hominin evolution once its complete description has been published.

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