1. I really don’t think it is necessary to explain well-known concepts such as the Newton-Raphson method (Section 3.1) and Monte Carlo error propagation (Section 3.3) in such minute detail. Thus, I think that Equations 4-6 and Figure 1 can be safely removed from the paper.

We have not made any changes here. While we respect the editor’s request to shorten the paper where practicable, we know through experience (as argued from the initial responses) that there are practitioners and users of the (U-Th)/He community for whom these topics are not second nature and would benefit from the inclusion of these topics. We therefore strongly feel that to make this topic as accessible as possible, these background portions of the paper are important to retain.

2. Equations 7, 8, 19 and a11 are wrong. The covariances should not be squared.

This is an embarrassing mistake primarily caused by the lack of a generally accepted symbol for variance (i.e. σ_a would typically be the standard deviation of *a*, while σ_{ab}^2, rather than σ_{ab}, would be the covariance of *a* and *b*). We appreciate that prof. Vermeesch caught it before the paper went to press. We have inspected the code and fortunately, the correlated errors are calculated correctly by HeCalc. Rather than changing the equations, we have corrected the definitions (now correctly identifying σ_{ab}^2 as the covariance rather than describing σ_{ab} as the covariance). This notation is in alignment with McLean et al., 2011 and will hopefully be most familiar to the general geochronology community.

3. I was disappointed that the revision retains the ad-hoc definition of skewness (or “skew”). Not only is this definition confusing, but it is also unnecessary. As I mentioned in my first review, the skewed error distributions likely fit a simple lognormal distribution. If this is the case, then the skewness would disappear after taking logs, and the analytical uncertainty could be adequately captured with a single number, namely the standard error of the log. The absolute standard error of a logarithmic quantity corresponds to the relative standard error of its linear equivalent. Had the authors carried out their linear error propagation in log space, then this would have avoided the need for Monte Carlo simulations. Since two thirds of the paper is dedicated to explaining the difference between the linear and Monte Carlo uncertainty estimates, this means that two thirds of the paper is essentially unnecessary. This includes Figure 3, 4, 5, C2 and the entire appendices D and E. Taking logs would also avoid the issue with negative dates that is mentioned in Appendix B.

To avoid the confusion that the reviewer points out, we have added in the formal skewness values to the text, table, and plots.

On the topic of linear vs log-space equations, prof. Vermeesch acknowledged previously that the Monte Carlo approach achieves the same results, and is sufficiently fast that no real efficiency is sacrificed. On the other hand, we know through experience and anecdote that many labs are using linear (not log) approximations for their uncertainty propagation. The reason for the inclusion of linear equations here is to 1) formalize the process for those already using these approximations, and 2) provide a comparison point for those labs to decide for themselves whether the age equation non-linearity causes sufficient error to move away from this approximation. In the CU TRaIL, where dates >1 Ga are measured, this is a relevant factor. In prof. Vermeesch's lab, where such ages are not observed, any version of uncertainty propagation is probably fine to a very good approximation.

Other comments:

1. Section 1 claims that the uncertainty of the alpha-retention factor Ft is "increasingly well-constrained". This is only the case for the measurement of the grain dimensions. Unfortunately, there has been little or no progress in quantifying the larger effect of compositional zoning (Hourigan et al., 2005). This effect is, essentially, unknowable.
It is untrue that the effects of compositional zoning are unknowable. Compositional zonation is quantifiable via published LA-ICP-MS mapping (Farley et al., 2011) and drilling (Johnstone et al., 2013) methods. We now cite these papers in the manuscript. Whether it is worthwhile to routinely acquire such data for typical studies is another question.

2. Section 3.3. typo "6odelling"
Corrected.

3. Section 3 cites Efron and Tibshirani (1986) as a reference for Monte Carlo error propagation. However, this paper discusses bootstrapping, which is a different procedure than the one described in the manuscript.
This is a holdover from a much older version of the paper. This reference has been removed.

4. Table 1. Typo: "convariance"
Corrected.

5. Section 4.2 defines a 95% Monte Carlo confidence interval as the difference between the 2.275 and 97.725 percentiles. However, 97.725 - 2.275 = 95.45 ≠ 95!
This is another good catch; we had simply converted from a 2-sigma equivalent. This is now corrected in both the paper and in the code.

6. The denominators of Equations a1-a8 are all the same. It would save lots of ink if they were replaced by a separate parameter. I suggest 'd' for 'denominator'.
In the interest of shortening the paper, we have removed these equations entirely as they are duplicates of equations in the main text. The denominator of the remaining equations for direct quantification have been updated with the summation notation used in the main text.

7. Figure C1: the three columns of this plot are, essentially, the same (with the first and third being exactly the same). This reflects the fact that U, Th, He and Ft are simple multiplicative factors in the age equation. So the sensitivity of the age is, essentially, the same for them. I don't think that this figure is very informative. I suggest that it be removed.
We appreciate that this figure isn't very informative for Prof Vermeesch given his statistical expertise, but we feel that it will be informative for our target audience of method practitioners who have less statistical background. Just as in the first round of edits, we mainly wish to convey an intuitive sense of which components of uncertainty are most important for uncertainty in the

(U-Th)/He date. In a paper focused on explaining uncertainty sources and how they propagate, this figure should be included.

8. Figure C3 is misleading. As explained in its caption, it exaggerates the non-linearity of the (U-Th)/He age equation by decreasing the uranium decay constant. In other words: the non-linearity is a straw man problem. It only affects >1Ga samples, which are never found on Earth anyway. Consequently, I think that Appendix C can be removed as well.

Ages >1 Ga absolutely are found, and measured, on Earth. They are routinely observed in CU TRaIL. For that reason, we feel strongly that appendix C should be retained. However, the detailed text explaining the rollover phenomenon is likely to be review for those with advanced statistical knowledge and uninteresting to others. We have therefore removed the final paragraph and figure of appendix C in the interest of shortening the paper.