Comments: Line 19: I don't want to be a stickler on terminology, but I do recommend using TCN (terrestrial cosmogenic nuclide) vs. CN to separate this class of dating explicitly from extra-terrestrial applications.

Thank you for the suggestion. We will change the terminology in our revision

Line 25-27: I completely agree with the difficulty. However, it should be mentioned that in some cases both can be ascertained. One would need the data resolution necessary to characterize the muon crossover depth.

Thank you for the suggestion, we will add this to the revision.

Line 37-38: It would seem this sentence should have a reference. To what linear inversion techniques are the authors referring?

The linear inversion technique was introduced by Anderson et al., 1996. We will add the reference to the sentence.

Lines 40-42: v1.2 of the Hidy et al. 2010 calculator (released in 2012) is also Bayesian (see Mercader et al. 2012)

We apologize for this mistake; we will correct it in our revision.

Lines 42-44: What is meant by stating that the available methods require prior knowledge of surface age and inheritance? Those are both free model parameters in the models I am familiar with. I think they may mean that some of those models require users to specify parameter boundaries—which should always be done arbitrarily large to avoid constraining the model. But that is not what is communicated here. What is communicated is that those models require some independent knowledge of those parameter values, which isn't the case.

We realise the claim of not requiring any prior knowledge in the introduction is inappropriate. What we meant is linear regression method inverse for exposure age and inheritance directly, without any pre-set boundaries. We agree that theoretically the boundaries can be set arbitrarily large to avoid constraining the model, while in practice, we find a certain degree of prior knowledge is useful to save model running time, or to avoid parameters been trapped in unrealistic solution space.

As an inverse approach, the least squares linear regression approach directly solves for age and inheritance, while treating the erosion rate/eroded thickness as an input instead of an output of the model. This characteristic makes it a convenient tool for exposure age estimation. It can be used with Monte Carlo sampling to explore the full distribution of possible ages and inheritance from the variation of input parameters (including erosion). Linear regression is also useful as a starting point for forward (e.g. Bayesian) models. Inverse-modelled age and inheritance may thus help researchers to tune the boundary values for the forward models to get better simulation results. Therefore, instead of replacing forward methods, we will argue that our approach complements forward methods.

Line 47: How is the minimum prior knowledge different between the linearization approach and others? Linear regression is functionally simpler, yes, but are the authors implying that their model has a reduction in degrees of freedom? What is the basis for this?

The degrees of freedom are the same for both methods.

Line 65: Why use r vs. the more commonly used lowercase epsilon to represent erosion rate?

We will change it to ε instead.

Lines 89-92: On one hand, yes muon production at the surface is small relative to spallogenic production, on the other hand it becomes increasingly important with depth. So, what does this mean for depth profiles where samples near the surface can't be obtained and muon production is far more important? This is a common issue, so should be addressed. Also, why ignore that 2%? Wouldn't it be a slightly better approximation to lump that 2% in with the nucleons and then treat it as simple exponential to linearize for the approximation?

We agree. We address the effect of muons in our discussion section. The erosion rate approach, excluding muons, is useful for exploring some of the trade-offs between erosion rate and age (using Te, eq. 7). We will rearrange our manuscript to make this point clear.

Line 102: Agree. Also, there are lots of reference options that might be added here that support the benefit of constraining total eroded thickness.

We will include references here in the revision.

Line 115: Should this reference actually be Braucher et al. (2009)? Also, this raises an interesting question…does the applied muon approximation approach offer at least the possibility of constraining a unique solution for age and erosion rate, or does that vanish with this approximation? This could be tested with a carefully composed pseudo-profile that characterizes the muon cross depth. I'd be more convinced of the acceptability of the approximation if the authors could show this. I'm still a bit concerned that there might be an issue here with deep profiles.

We apologize for the mistake; we will correct it in our revision.

Whether calculated using erosion rate or eroded thickness, our least-squares approach requires external information (or assumption) for the erosion rate/thickness, therefore cannot be used to calculate a unique solution for age and erosion rate.

We will include an example in the discussion to show how our approach behaves with deep profiles.

Line 127-130: What about uncertainty in density? I realize that this is basically an uncertainty in depth (assuming the authors are accounting for mass-depth), but it is unclear exactly how uncertainty in mass depth is applied as it can include both a random (individual samples) and

systematic component (effective depth shifting of all samples). Also, how does uncertainty in inheritance factor in at this stage?

We didn't mention uncertainty in density, but we have considered it in our approach. We will add this to the revision. The uncertainty in inheritance is the outcome of the simulation.

Line 135: what corresponding probability density functions are used?

Either uniform or normal distributions. We will clarify this in our revision.

Line 137-139: how are the probability density functions calculated from the simulation results? Are the results weighted somehow, or is this a histogram vs. a pdf? It appears to be a histogram.

We currently only present the results in histograms. We will clarify that in our revision. We are also considering adding estimation of the pdf into our codes.

Table 3: In the Hidy et al. 2010 model of Lees Ferry, muons are not approximated with a two-term exponential (it uses a 5-term approximation like Schaller et al. 2002 and is internally optimized for the sample site and specific depth range). Also, the erosion rate range used was 0-0.4 cm/kyr. These differences should be noted.

Thank you for pointing this out, we will clarify that in our revision.

Line 216: Where does the 0-0.32 cm/kyr erosion rate estimate come from?

We find that using the full range of erosion rate (0 to 0.4 cm/kyr) results in an age range that is too old and inconsistent with the total erosion of 0 to 30 cm. This is because the upper bound on total erosion limits the range of acceptable results in the Hidy et al. (2010) model. Because our least-squares approach uses either erosion rate or total eroded thickness, but not both, we chose to set a narrower limit on the erosion rate, from 0-0.32 cm/kyr, so that the ages and total eroded thickness were consistent with that cited in the original paper. Using our eroded thickness fitting approach also avoids this problem. We will clarify this point in the revised paper.

Line 261-263: Not allowing negative inheritance actually changes the best-fit, or the peak in the distribution? I see how this would, and philosophically should, change the shape of the full distribution, but it shouldn't have an impact on the best fit—otherwise what makes it best? I guess it might because these are not probability density functions being generated, but histograms. So, doing this might actually be OK in the context of their modeling approach, but I'm hesitant to agree since I am unsure how all those allowable solutions with negative inheritance might introduce artefacts in other solution spaces.

We will be specific that the negative inheritance effect is important for our approach. We will also include a comparison of inversion results using pseudo profiles with and without negative inheritance in our revision.

Line 272-273: In the originally published Hidy et al (2010) Lees Ferry result, generous uncertainties in 10Be half-life (5%) and muon production (10%; probably still realistic considering Balco 2017) were applied, so it would be useful to know what uncertainties were applied here for comparison. This could also explain some of the differences in results between those histograms and these. Also, out of curiosity, I reran the original Lees Ferry dataset using the Bayesian version of the Hidy et al. (2010) model that generates actual probability density functions vs. the histograms of the original—basically by weighting all MC generated profiles (including solutions outside 95%) by the chi-squared likelihood function. Note that this is very different from what was presented in Hidy et al. (2010), but it is the version that has been adopted since 2012 so probably what should be used for a results comparison. With version 1.2, the results for age at 95% confidence are 76.6 – 96.1 ka (see figure below), with the probability weighting significantly tightening the distribution.

The version 1.2 we had gave us a resulting age range of 74.5 – 98.7 ka (we used the predefined Lees Ferry settings that came along with the program). We are not sure the source of this discrepancy. We are happy to use the new age range you present here, and we will consider the differences in parameter value and parameter uncertainties in our revision.

Lines 299-302: Yes, this can't be overstated! Also, there are numerous references out there that support the importance of soil processes to interpreting TCN profiles.

We will include references in our revision.

Lines 310-314: This is an interesting exercise, but are there approaches that ignore radioactive decay? Strange if there is.

We will remove this section from the revision.

Lines 330-332: Generally, I agree with this, but there are instances where dating highly eroded surfaces are useful when one is more interested in soil age vs. deposition age.

We agree. We should be specific that we do not recommend our approach to date surfaces with large eroded thicknesses.

Lines 368-370: True, this isn't really a revelation though and is why many depth profiles end up reported with zero-erosion rate minimum ages when constraints on surface erosion can't be justified.

We agree. However, we feel this is important to state and will keep this sentence as written.

Lines 399-401: I disagree with this statement. While it may be true for this modeling approach, it is incorrect to infer for all inversion models that may apply different statistical methods for reporting solutions.

We will be specific that the negative inheritance effect is important for our approach.