Review Summary:

In this paper, the authors present a novel approach to modelling depth profiles of 10Be using linear regression coupled with Monte Carlo sampling. This actually consists of two inversion approaches that consider mass-loss from the surface from different perspectives, erosion rate or net erosion, with the latter also including muon production with an approximation based on a Maclaurin series expansion of the classical depth profile equation. The authors test their model on datasets previously modelled by other published methods and demonstrate a reasonable replication of those results.

Generally, I think the linearization approach is very interesting, because it does simplify the age analysis from depth profiles into linear systems that are easier to manipulate. However, I do have some reservations about how the approximations might translate into biased probability density functions, and with some of the statements made regarding muon production. I also think more technical detail is needed on the model functionality and parameter specifics that include how sampling distributions are chosen. Additionally, there are several instances in the manuscript where statements are made that should be supported with citations. That said, the approach seems to have merit and I would be happy to recommend for publication provided the authors address the comments below.

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.

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 cross-over depth.

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

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

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.

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?

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

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?

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

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.

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?

Line 135: what corresponding probability density functions are used?

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.

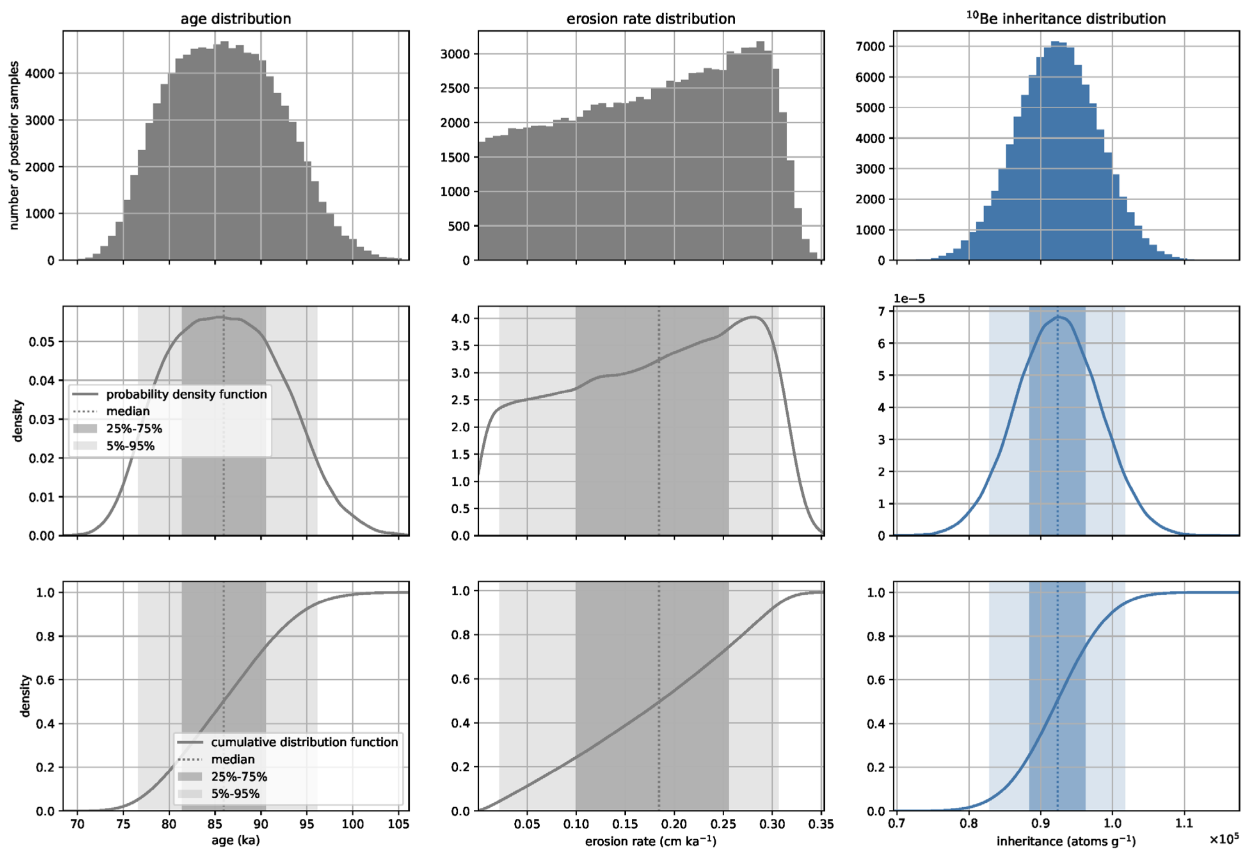
Table 3: In the Hidy et al. 2010 model of Lees Ferry, muons are not approximated with a two-term exponential (it uses a 5-term approximation like Schaller et al. 2002 and is internally optimized for the sample site and specific depth range). Also, the erosion rate range used was 0-0.4 cm/kyr. These differences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

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

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.

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.



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.

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

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.

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.

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.