Reply to comments

Dear editor and reviewers,

I have the pleasure of sending you the revised manuscript entitled “Combined linear regression and Monte Carlo approach to modelling exposure age depth profiles” authored by Yiran Wang and Michael E. Oskin to be considered for publication as a research article in Geochronology.

Fully considering the concerns of the editor and the reviewers, we have made some major revision of this manuscript.

- We have rewritten the introduction, focusing our motivation on expanding the application of linear regression approach, and on its advantages: easy to invert, and provides a direct way to explore the trade-offs between erosion and age.

- We have included a new section, “Simulated depth profiles” (section 3.1), which demonstrates the performance of our approach under various circumstances. We included scenarios such as low inheritance, large denudation, and deep profiles, to address the concerns raised by the editor and reviewers.

- We have deleted demonstration of the erosion/denudation-rate approach for the case examples to shorten the manuscript, while kept demonstration of the more accurate denudation-length approach (section 3.2). We have also added a demonstration of using the denudation-rate approach to explore the denudation-rate vs exposure age relationship for the Beida River site (L263-268).

- We have made other minor revisions, including using site specific muon production rates for our models; details are in our point-by-point replies.

We appreciate your consideration of our manuscript, and we look forward to receiving further comments.

Yiran Wang

Reply to the editor, Dr. Pieter Vermeesch:

1. *Linear fitting with muons.*

We agree with the reviewers that it was inappropriate to claim that our method does not require any prior knowledge: erosion depth or rate must be estimated. What we meant to convey is that linear regression inverts for exposure age and inheritance directly, without needing to define a pre-set boundary or initial prior distribution for these. The erosion rate (or eroded thickness) is a required input for our inverse modelling approach.

Our motivation for introducing this approach is, first, to expand the application of linear regression to model exposure-age depth profiles. Prior published linear regression methods ignore muons and do not adequately address erosion. Our approach incorporates muogenic production, making it possible to estimate ages for sites with steady rates or a defined amount of erosion. As demonstrated in the discussion section, this method can provide exposure age estimations with high precision for profiles with less than 2 times attenuation length erosion, suggesting it may be applied for most surface exposure dating scenarios. Our second motivation is that 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. In addition, as an added benefit of our inverse model approach, the effective age Te provides a straightforward way to explore the trade-off between erosion rate and exposure age.

We have incorporated these ideas into our revised manuscript, and completely rewrote the introduction.

By comparing results from our least squares linear regression with a Bayesian approach using Metropolis-Hastings sampling (Section 3.1 of the revised manuscript), we find the age estimations from linear regression have precision and accuracy comparable to results from Bayesian approach, under various circumstances. These examples support our claim that the linear regression approach is robust.

2. *Monte Carlo error estimation*

The editor's description of the procedure of our inversion and Monte Carlo simulation approach is correct. We would like to modify the procedure as follows:

1. Generate all the input parameters (10Be concentration, sample depth, eroded thickness, production rate, attenuation length, etc.) from pre-defined distributions.
2. Fit the data with eq. 10 to get Ten and inheritance.
3. Calculate the exposure age use Ten from step 2 and parameters generated from step 1 using eq. 11.
4. Repeat step 1-3 for many times, sampling the underlying distributions of each parameter, to produce a distribution of results.

In our inversion, we treat the erosion as an input of the model, therefore the uncertainty related to the erosion can be treated the same way as other parameters. We treat the erosion as a known value instead of an inversion output for several reasons. First, it simplifies the inversion process, so that one can inverse linearly in 2D space, and the Te value resulted from the first step could be used to explore the trade-off between erosion rate and exposure age. Second, though theoretically it is possible to find a unique set of solutions for erosion rate and exposure age for a certain depth profile, in practice, the sample uncertainties always exceed the resolution that required to define that unique solution. This means including erosion as an unknown is unlikely to increase the utility of the estimation results. As mentioned earlier, we are considering including comparisons of our least squares linear inversion and Bayesian inversion with pseudo depth profiles to demonstrate that a simple Monte Carlo sampling with two-step estimation can provide sufficiently accurate results.

In the revision, we modified our sampling steps (L140-145) to make it clearer. We also emphasised in the introduction (L33-34) about treating erosion as a model input.

3. *Negative Inheritance*

Negative inheritance estimations can be prevented from inversion process, and we have incorporated that into our earlier published work (e.g. Wang et al., 2020). However, we realized in the writing of this manuscript that those negative results, though physically impossible, are necessary for mathematical reasons.

For example, suppose we have a sample group with a true age of 100 kyr and a true inheritance of 0 atoms/g. Because of the uncertainties within the sample measurements, ideally, we want to have the estimated distribution of the exposure age to be centered around 100 kyr, with approximately half of the estimation older than 100 kyr, the other half younger than 100 kyr. The estimations younger than 100 kyr correlate to some positive inheritance, while those older than 100 kyr correlate to negative inheritance. The overall distributions of the inheritance should be centered around 0, meaning that approximately half of the estimated inheritance should be negative. If we require non-negative inheritance during the inversion, it will end up with deviated estimation results: the inheritance would center around a positive value, while the central age would be younger than 100 kyr.

To demonstrate this, we tested the two methods (least squares linear inversion described in our manuscript and a Metropolis-Hastings Monte Carlo Bayesian inversion) using pseudo depth profiles with true inheritance equals to zero and 5000 atoms/g. We find that not permitting negative inheritance during inversion leads to significant underestimation of the exposure age for both approaches. For cases with low but not zero (5000 atoms/g), we find the underestimation still occurs, but not as significant as the zero-inheritance case. (Section 3.1.2).

4. Scaling methods

Thank you for the suggestion. We updated muon production rate used in the pseudo profiles and in the Beida River case. We also deleted the sentences that indicate fixed muon production rate in our revised manuscript.

Reply to the 1st reviewer:

Despite being in a field I really appreciate, I have some difficulties to judge what is the value added by this paper. This is probably by the fact that too many assumptions or to be more precise too many shortcuts are used to simplify the main equation governing the cosmogenic production equation as a function of denudation rate and time. Some of these shortcuts are dangerous and some other can be avoided with the used of numerical calculations. I will thus ask for a revision of this paper

In the entire paper I suggest changing erosion by denudation that is more appropriate for cosmo.

Thank you for the suggestion, we have changed erosion by denudation in our revision.

At the end of abstract you mention "compared to the error from omitting muogenic production…" I totally agree so, why do you present a linearization that ignores muons contributions?

The formative paper using linear regression to interpret depth profiles omits muogenic production (Anderson et al., 1996) so it is important to at least comment on the error that arises from their omission. We also find the erosion rate approach (omitting muons) is helpful in demonstrating the trade-offs between denudation and exposure age. We clarified these points in the abstract (L14-15) and the introduction (L41-44) in our revision.

Line 35-40: despite muons contributions are small at surface compared to the neutron one, ignoring their contributions and considering only neutrons will yield to multiple time/denudation pairs that can model a depth distribution.

We agree with the reviewer. We have completely rewritten the introduction to address the concerns of the editor and reviewers.

Line 44: If you want to be totally objective you should live all parameters free and in a second step consider the solutions that can match the field observations. If you constrain at the first step your unknowns, time or denudation you may miss the real solutions.

We realise the claim of not requiring any prior knowledge in the introduction is inappropriate. What we meant is linear regression method doesn't require prior knowledge of the exposure age and inheritance, while the erosion rate (or eroded thickness) is a required prior knowledge for a single isotope depth profile method. We have completely rewritten the introduction and focused the motivation of our study to expanding the use of the linear inversion method in exposure-age dating.

Line 55 Legend of Figure 1: you should update the muon contributions; since Braucher 2003, these contribution have been updated (Braucher 2011,2013, Balco 2008, 2017). More it has been also shown that Heisinger muons contributions were too high. You should correct them in your matlab code and in the Hidy one.

Thank you for the suggestion. In the revision, we use the SLHL production rate based on Martin et al., 2017 and Balco 2017.

Line 90-91: again do not omit muon contributions! In a high denudation environment, their contributions are far from being negligible.

We agree. We find the erosion rate approach (omitting muons) is helpful in demonstrating the trade-offs between denudation and exposure age. We clarified these points in the introduction (L41-44) in our revision and demonstrate the application in the case examples (section 3.2, L263-269). In the discussion, we also explored the error related to omit muon (section 4.2.2)

Line 67: I think Nishiizumi, 2007 is not appropriate as in this paper he proposed a half-life of 1.36±0.07 Ma.

Yes, we took this reference off from our revision.

Line 100 and following paragraph: I think this is not the right approach. First I will have a look to the distribution as a function of depth (in g/cm2) to see within the first two meters what is the value of the "slope" of the exponential decrease. Lower than 250 g/cm2 will traduce a contribution mainly due to neutrons with moderate denudation rate. If higher muon contributions are more important due to higher denudation rate or can be due to a recent rejuvenation of the profile making deep samples to be now closer to surface. In this latter case, running an inversion model with density as free parameter will probably propose high values for density >3 g/cm3 making clear that the profile has been perturbed. This can be the case when loess covers are rapidly eroded by wind deflation, so fast that the cosmo production cannot be at equilibrium.

Therefore I will let run the model with totally free parameters and then cut the Time/ denudation space by probable eroded thickness to reduce this space. By imposing since the beginning of the modelling a constrain as important as the eroded thickness may be dangerous to my point of view.

Thank you for the suggestion. In this paper, we are attempting to provide an approach for surfaces under constant erosion, therefore we exclude any abrupt change of the deposition/erosion environment from the model, except where such a change may be independently modelled and removed, as is the case for the loess cover at the Beida River site (L258).

Line 108: which muon contribution do you used as Tem? Fast or slow? Is this choice important?

We use both, as shown in eq. 9. We revised this sentence as "Using a series expansion, we rewrite the effective age related to muons (negative and fast), T_{em}" to make it clear (L104).

*Please change the * by × in the tables. Please use uniform values for concentraions (at/g or 105at/g)*

We corrected this in our revision.

Line 174: why this denudation rate of 0.3±0.05 cm/kyr ?

This rate is calculated based on the 40±10 cm denudation and the surface age estimated using the eroded thickness approach. In the revision, we have removed this part from the manuscript and only demonstrated the most accurate denudation-depth approach.

With this loess covered surface, probably the use of two nuclides will be better than one.

Thank you for bringing this important point up. The loess deposit is well-dated, young, and quite continuous. Therefore, the concentration before loess deposition can be easily modelled. Using two nuclides would offer an additional constraint, but this is beyond the scope of this manuscript.

Line 177: I am not convinced by the fact that you authorize inheritance to be negative. This is as you mentioned "non-physical". Therefore what will happen if you restrict the modelling to inheritance ⩾ to zero? Is the overall space of solution affected?

Yes, the overall space of solution will be affected if negative inheritance estimations are omitted. See reply to editor comment, above. We have also included a simulation in section 3.1.2 to show this effect, and included a more detailed discussion in L386-397.

Paragraph 4.2.5 : I agree but using variable production rates implies adding more uncertainties and this is not the fact in the actual calculators !

We agree with the reviewer. We change the sentence as "In fact, the production rate is time dependent because the strength of Earth's magnetic field varies with time (Balco, 2017; Desilets et al., 2006; Dunai, 2001; Lifton et al., 2005; Stone, 2000). Extending our model to account for temporally variable production rate is beyond the scope of present study." (L400-402)

If you think to revise this contribution you should try to add a second nuclides (26Al for example) and try to remodel the depth profile with two nuclides. Inheritance can thus be variable and this can probably be a great value to the modelling of depth profile.

Though a second nuclide would certainly add to the available constraints, this is beyond the scope of our manuscript, which is focussed on presenting a least-squares solution to single-nuclide measurements.

Reply to Dr. Alan Hidy

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 have changed 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.

We agree. We have completely rewritten our introduction to focus on expanding the linear regression approach to include erosion and muons.

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 have added 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 have generalized the referenced approached as forward-modeling approaches instead in our revision (L37).

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 argue that our approach complements forward methods.

We have incorporated these ideas into our revised manuscript, and completely rewrote the introduction.

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 have changed 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 have added this clarification in the revised introduction (L41-44).

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 have included references here in the revision (L99).

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. 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 also find this sentence may not be suitable for our manuscript, therefore we deleted it from our revision.

We also included an example in section 3.1.4 to show how our approach performs 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. The uncertainty in inheritance is the outcome of the simulation.

In the revision, we have added this to L135-137, L141.

Line 135: what corresponding probability density functions are used?

Either uniform or normal distributions. We clarified this in our revision (L141-142).

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 added pdf into our codes and updated the figures in our revision (Fig.8 and 9).

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 have added a footnote to the table to clarify this difference. We have also included the difference between muon approximation in the discussion section (4.1, L325-327). In our revision, we have deleted the erosion-rate approach part and only use the eroded-thickness approach (Section 3.2).

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.

In our revision, we have deleted the erosion-rate approach part and only use the eroded-thickness approach (Section 3.2).

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.

In the revision, we specifically stated that the negative inheritance effect is important for our approach (L394-395). We also demonstrate this effect with simulated depth profile examples in section 3.1.2.

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 (L295).

We find the difference of the two approaches may come from the following two sources: first, our examples with pseudo depth profiles demonstrated that different approaches always return different estimates; second, we use a two-term exponential for muogenic process instead of a 5-term approximation. We have included this in our revision (L323-327)

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 included references in our revision (L349-350).

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

We removed 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. In the revision, we used simulated depth profiles (section 3.1.3) and error analysis (4.2.2) to show that our model can still provide accurate enough estimations for highly eroded surfaces.

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. In the revision, we have rearranged and deleted this section.

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.

In the revision, we have specifically stated that the negative inheritance effect is important for our approach (L394-395).