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 will change that 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 will clarify these points 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 will rewrite this part to make it clear.

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 will revise the introduction and focus 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. We will correct this in our revision.

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

We agree. We include this to make comparisons later in the discussion, *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 will take 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.

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 will revise this sentence to make it clear

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

We will correct 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. We will clarify this in our revision

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.

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 will clarify this in the revision

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.