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Interactive comment on "Inferring the timing of abandonment of aggraded alluvial surfaces dated with cosmogenic nuclides" *by* Mitch K. D'Arcy et al.

Anonymous Referee #2

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In this manuscript, D'Arcy et al. propose a new probabilistic approach to constrain the timing of alluvial surface abandonment using cosmogenic radionuclide dating of surface boulders. Using randomly sampled surfaces ages from a hypothetical alluvial surface, a distribution of surfaces ages are obtained where both the number of samples and age of the depositional surface are varied. The discrepancy between the ages sampled and the true timing of surface abandonment are then determined. The relationships drawn from this analysis are then also applied to an independently dated alluvial fan system in Mexico to infer the timing of surface abandonment. The manuscript is motivated by better constraining the timing of surface abandonment; the authors suggest that this may be a more useful constraint than an average surface age

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Discussion paper

which is unlikely to relate to any particular forcing or event of interest. In contrast, the timing of abandonment will likely reflect changes in climate, base level change, tectonic forcing or major drainage reorganization. I enjoyed reading this manuscript – it addresses a well thought-out set of questions, is very well written and I believe is a valuable contribution to the field. I would recommend the manuscript for publication pending a few very minor clarifications.

I have a general query about boulders and age distributions and their representation. In the conceptual model, it is assumed that boulders are evenly distributed across the surface and that there is a uniform probability distribution of selectable ages. This is mentioned in the experimental assumptions too (section 3.3). I am curious as to how much these two assumptions are likely to modify the modelling results, and whether these assumptions are actually more likely to be the norm in reality. Is this by any chance something that has been examined or tested? The fact that many alluvial surfaces are not characterized by large numbers of large boulders does indeed suggest that their delivery downstream of their source areas may be temporally clustered and correspond to very large events – this may not be relevant given that it is only the youngest ages which matter here. In general, the authors do a thorough job of highlighting the assumptions and limitations of their approaches.

Section 3.1 – The first time I read this section I was a little confused – it felt like the second paragraph was more observations made from the data rather than a description of methods (line 20-25 in particular). Perhaps some re-phrasing or reordering of material may help with the flow of this section.

Fig. 1B – Could you add a y-axis on the kernel density plot?

P4. L4-11. Again, I had to read this paragraph a couple of times over to work out what was a 'true' timing and a 'real' surface – some confusion on what you have modelled and what is a 'real' example. You also do not mention/introduce that you apply the modelling to a case study in either section 1 or 2. Instead, it does feel like it pops

slightly out of the blue during the paper discussion. Perhaps integrate this into the end of section 1 where you outline what you are going to present with respect to the artificial data and generation of probabilistic equations.

P9. L13 – I don't think it is unreasonable to say that an average age does not/should not correlate with an external forcing.

P.9 L 21. This is probably more for my own curiosity. For the variables you have modelled, you state that 6 to 7 ages are sufficient to characterize the timing of abandonment when T = 30 kyr. You also touch on this in section 5.3 but was wondering if you could just clarify/expand. In your artificial case, the period of surface activity is defined. What if you turn up at a new field site without any indication of how old/period of time each surface has been active for? How many samples are needed/adequate to estimate the timing of surface abandonment to a high degree of probability? Perhaps some idea of the periodicity of forcing mechanism needs to be known (if climatically driven) – but then the argument becomes somewhat circular! Or should we just grab as many samples as we can and state the uncertainty/probability?

P9. L25 – There is no Fig. 5D.

P11. L9 – Should this be Figure 8D?

P12. L12 – If displacement can only occur after surface abandonment, do you have any constraint on a minimum age of displacement onset? Could this estimated time-averaged slip still only be a minimum rate? If so, perhaps state somewhere.

P12. L 28-30 This is a really good point – I also feel that this shouldn't just be in a limitations section! Deriving an average surface age would certainly be biased by burial of older material but by focusing on the timing of abandonment this bias is removed. Perhaps bring this up earlier in the manuscript as an additional strength of this method.

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