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**Response to reviewer comments of ESurf-2019-21: “Inferring the timing of abandonment of aggraded alluvial surfaces dated with cosmogenic nuclides”**

Dear Dr Malatesta,

Thank you for your detailed and constructive review of our manuscript. We are pleased that you like the essence of our work, and we have responded to each of your comments in detail below. We have incorporated many of your suggested changes and we feel the manuscript is now much improved.

Yours sincerely,

A handwritten signature in black ink that reads 'Mitch D'Arcy'.

Dr Mitch D'Arcy  
(on behalf of all authors)

## Reviewer 1: Dr Luca Malatesta

Dear Editor, I have read the latest a manuscript by D'Arcy and colleagues with pleasure. They offer a new probabilistic approach to identify the likeliest age of abandonment of an alluvial surface based on series of exposure-dated samples at its surface. They build a power law that predicts the likeliest amount of time elapsed between the youngest surface age and the effective fluvial incision based on the distribution of surface ages assuming their uniform distribution during the period of activity of said surface. It is a useful contribution that can be applied widely and is definitely worthy of publication in ESurf! In my opinion, the manuscript is ready for publication pending minor clarifying modifications. The article is very well written and easy to read. I would however encourage the authors to consider modifying their probabilistic approach and follow an explicit derivation of their probability power law without requiring the use of "artificial data" for empirical fitting.

We thank Dr Malatesta for his helpful review. We are pleased that he likes our work, and we respond to his comments below.

Below I briefly describe an alternative approach for the probability law and I provide line by line comments on the text.

Probability The approach using synthetic data has the advantage of mimicking a field situation with  $n$  dated boulders out of a larger number. However, it seems to me that using an explicit approach would be much more advantageous. There is no need to graphically fit the powerlaw and deal with the associated error margins, the term "artificial data" can be avoided altogether, and the theoretical framework would be reinforced. Further it would become a more flexible platform, for example to introduce non-uniform distribution of surface ages. I have asked Quentin Berger, probabilist at Paris-Sorbonne, for some help as to how the explicit derivation can be made. I include a document that summarises his explanation hereby. The derivation would replace section 4.1 and provide a definitive and clean solution for this approach. I think it would improve the impact of the manuscript. That being said, it is not a necessary modification and the manuscript stands on its legs as is. It is for the authors to decide whether they want to follow an explicit approach or not.

We agree that it's worthwhile to consider an analytical solution, and we're grateful that Dr Malatesta and Dr Berger have taken the time to derive these suggested equations. Nonetheless, there are several reasons why these equations cannot replace the approach we take using artificial data.

First, let us quickly summarize how we understand the derivation in the document. Essentially, the probability is evaluated that a particular sample is older than the time of abandonment (set to zero in the document) plus a specified duration  $\tau$  (eq. 4). This period  $\tau$  is identified with the time difference between the youngest sample and the time of surface abandonment (which is also called  $\tau$  in our manuscript). Because we assume uniform distribution of sampled boulders with equal likelihood of sampling, the probability is given by  $1 - \tau/T$ , where  $T$  is the length of surface activity. Next, it is required that all samples fulfil this criterion, and consequently, the probability is raised to the power of  $n$ , where  $n$  is total number of samples (eq. 5). Finally, assuming  $\tau \ll T$ , the equation is expanded to first order, using a Taylor series, noting that the result is equal to a Taylor expansion of an exponential function to first order.

There are a number of points that can be made in response.

Firstly, the suggested equations are incomplete. A comparison of eq. 5 in the derivation and eq. 2-3 in our manuscript shows that our equations yield more detail, for example a prefactor to the exponential term (Eq. 2) and the dependence of the exponent on the specified percentile (Eq. 3). Further details of

our results, for example the relationship between  $T$  and the spread of sample ages (Eq. 6) are not dealt with. Therefore, the derivation may provide a first step, but further steps still need to be worked out.

Secondly, we do not think that the derivation actually reproduces what we are simulating with the artificial-data approach. The youngest sample age in our approach is not older than  $\tau$ , but determines  $\tau$  (i.e., we require a sample with age  $\tau$ ). Thus, the determination of the probability is not correct (Eq. 4 in the derivation). We have since developed some ideas of how to correct the equations, but this is far from giving a usable or publishable result. Whether the exponential approximation that arises from the derivation is coincidental or whether there is a relationship to our approach is not yet clear to us.

Thirdly, even if a complete analytical solution is possible (which it might not be), it seems likely that a numerical solution or artificial data are necessary to provide other elements of a workable approach. An example is the estimation of  $T$ , where only the span  $a_{max} - a_{min}$  can ever be measured empirically (i.e., Fig. 6 and section 4.2). There are actually several advantages to choosing an artificial-data approach, which we mention at the end of section 2. One additional reason that is not mentioned in the manuscript is that the artificial data may be easier to understand for researchers who do not have a rigorous mathematical background. Our equations are correct, the error margins of our parameterisation are very small (see Fig. 5), and importantly, our equations do not require approximations such as  $\tau/T \ll 1$  such as in the suggested derivation, which Fig. 4 shows is not realistic.

We are very open-minded about the possibility of developing a full analytical solution in the future, but this is a complicated problem to solve and the suggested equations only provide a starting point. For these reasons, we believe it is advantageous to continue with our approach using artificial data. We decided to not include analytical derivations in the present paper.

Line by line

p. 2 L. 31. “These approaches risk circular or inaccurate interpretations.” Can you elaborate or give a few examples of these risks?

Yes, we have clarified the text. We changed “*approaches*” to “*assumptions*”, because this sentence is referring directly to the previous sentence where we open this point with two specific examples, including citations. We added an additional citation to Macklin et al. (2002). We now explain in the text that these examples (1) assume that abandonment coincides with palaeoclimate events, and then conclude that climate controls aggradation/incision cycles (risking circularity); and (2) assume that the youngest sampled age approximates abandonment, which our analyses show will often not be the case (risking inaccuracy).

p. 3 L. 13-15. I suggest to indicate that these ages are arbitrarily selected to produce the scenarios. The reader (or at least I) might think that they are lucky draws from random rounds and that you are already talking about experiment results. It’s a small detail but it would help focusing on the examples you are building.

Done, this is a good suggestion.

p. 4 l. 4. “In this study, we use artificial data [...]” At this point it can be unclear whether you use artificial data on virtual surfaces or if you populate a real “geomorphic surface” with artificial data. I suggest to maybe include the purpose of the approach here already: e.g. “we use artificial data to simulate the characteristics of surveyed surfaces” (which you bring up only later at the end of the paragraph on l. 9-10.) This entire paragraph is actually paramount as it frames the use of “artificial data” for the first time. I suggest to carefully edit it such that the combination/coexistence of artificial data and field sites is clear.

At this point in the text, Many readers will be asking themselves “ok i understand the problem and motivation but how is that useful for my field site?”.

These are good points. We have edited the paragraph to make our approach and its utility clearer, and elaborate on how our synthetic data approach can inform real field studies.

p.4 l. 27. Missing coma after “T”

Corrected.

p. 4 l. 27-28. I suggest to indicate the uniform distribution of the ages here already. The readers might be wondering about it.

We agree that it’s important to point out the uniform distribution of selectable ages, but we think a better place to discuss the assumptions of our approach is section 3.3 ‘Experimental assumptions’ (now at p.6, l.12), once the reader is familiar with our overall approach involving artificial data. We now address this particular assumption explicitly in sections 3.2, 3.3, 5.1, and 5.3, which we think is in good context.

p. 5 l. 13.  $\tau = a_{\min} - t_{\text{aban}}$  is an important relation, I’d suggest to give it a full equation line.

Good idea, we have done this. We put the equation at the start of section 3.1 and re-numbered the other equations accordingly.

p. 7. The lines of equations lack punctuation.

We’re not sure what punctuation is missing from the equations. If our article is accepted then we are of course happy for it to be formatted following ESurf style guidelines.

p. 7 l. 6. “then  $\tau = 12$  kyr for  $P = 0.95$ .” I’d suggest to paraphrase the end of the sentence in plain english for clarity.

Done.

p. 7 l. 11-15. the parameter  $k$  has a negative value. It should be mentioned here (important for what happens when  $n$  grows to infinite). Potentially even better  $\hat{A}^T$  and  $\checkmark$  I believe in accordance with the convention for such parameters  $\hat{A}^T$  give  $k$  a positive  $\checkmark$  value with an explicit negative sign in the equation.

We now explicitly point out that  $k$  has a negative value (p.7, l.34).

p. 8 l. 21: section 4.3 is very good and will be very useful.

Thanks!

p. 8. l. 24: using the parameters values listed above I assume? It might be worth specifying it.

Yes, the parameters (and equations) we derive from our artificial data are universal. Rather than specifying this here, we have added a line, “*Equations 2 through 6 are thus calibrated using our artificial data...*”) to the end of section 4.2 above (p. 9, l. 11-11), to make this clear.

p. 9 l. 11-17. this paragraph reads a little like conclusion material. I am not sure it is necessary.

We disagree. This is the opening paragraph of the Discussion and we think it should briefly outline the key implications of our work, namely that abandonment ages will often be more informative than average surface ages, and that our probabilistic approach provides a new way of constraining abandonment. Given that readers thinking about their own field sites will likely jump to this subsection (5.1, “Implications for surface dating”), we think that a very brief discussion of the key points is helpful.

p. 9 l. 23. “significantly” probably needs defining since you provide a quantity of “one order of magnitude” thereafter.

We changed “*significantly*” to “*substantially*”, as this sentence is only supposed to be a qualitative statement.

p. 9, l. 25. There is no figure 5d.

This was a typo, we have corrected it to Fig. 5b. Thanks for spotting it!

p. 12 l. 10 Without much context, I don’t see why that would be a “conundrum”.

We changed “*This conundrum could be partly resolved...*” to “*More realistic values can be obtained...*” (p.13, l.19).

p. 13 l. 2-4. I am not sure that this characterisation is fair to previous work, many authors showed the importance of timing abandonment and not mean ages. The standout “finding” of the present manuscript is to propose a simple and efficient method to get there using incomplete datasets. It’s an important step.

We think this is referring to p. 14, l. 2-4, rather than p. 13 (which does not refer to previous work). The majority of studies that date surfaces such as alluvial fans do simplistically represent the surface with an average age (whether a mean, mode, or the peak of a frequency distribution) and rarely attempt to infer the subsequent age of abandonment (although we do explicitly acknowledge several examples in the Introduction). We’re certainly not claiming to be the first to consider abandonment, but we do feel it is fair to conclude in our paper that “*the timing of surface abandonment may provide more informative and more precise interpretations than taking an average of measured surface ages*”, because one of the novel contributions of our work is quantify the precision with which abandonment can be inferred.

For example, Fig. 4 demonstrates that for desirable probabilities, the timing of abandonment can indeed be pinned down more precisely than the period of surface formation,  $T$ . Similarly, the example application to the younger Q4 surface on the Baja California fans (Fig. 8, left) shows that surface abandonment likely overlaps with the Younger Dryas, offering a more precise and informative interpretation than the average surface age (for reasons we elaborate on in section 5.2.1). Both of these results demonstrate the value of inferring abandonment, and the potential precision with which this can be accomplished, in a new way that hasn’t been demonstrated before.