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Discussions

Interactive comment on "Does deposition depth control the OSL bleaching of fluvial sediment?" *by* A. C. Cunningham et al.

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We greatly appreciate the thoughtful comments of the two referees, and would particularly like to thank Zhixiong Shen for his transparant and open review. Both referees have comprehended our work and indicate their appreciation of it. Both stress the importance of the further development of Bayesian models for interpreting dose distributions of poorly-bleached deposits, and we value their encouragements.

The referees correctly identify the main weaknesses of our manuscript, and we agree with most of their criticism. However, we feel it would be unrealistic to address and solve all these issues in this manuscript. For this reason we propose the following approach: We will improve and clarify those parts of the MS text which were not clear to the reviewers.

We will modify the MS text to clarify shortcomings of the procedure, and/or further tests that should be carried out in the future. Further development and testing of the proposed method will be conducted in future research by us or by others. One of the advantages of the open review process of ESURFD is that other researchers can benefit from the reviewer comments; this will enable and encourage the luminescence dating community to collaboratively face the challenges to develop improved methods and procedures.

Below, we have grouped the reviewers' major points by theme and indicate our response and planned modifications to the MS.

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Rev1: 1. The major concern with the model is related to parameter p, the proportion of well-bleached grains. Unfortunately, the paper is not clear how this parameter is defined exactly. My understanding is that each grain has a burial dose in the model. In addition, a portion of insufficiently bleached grains (1-p) have remnant doses drawn from the right half of a normal distribution with a mean equal of 0. If this is correct, then sufficiently bleached grains have a quite high probability to obtain a close to 0 remnant dose, which means that quite some grains assigned by the model as insufficiently bleached are technically speaking well-bleached because the residual is close to 0. Therefore, model output of parameter p seems has no real meaning in terms of quantifying bleaching of the sample. If this is true, the conclusion about bleaching based parameter p would not be valid.

There is a valid issue here- if the De values are closely grouped, is that because of perfect bleaching, or are aliquots poorly bleached to a small extent? Due to uncertainties on the estimates, there is a limit to how well we can resolve this. This problem is equally true of the minimum age model, where it is restricted by a lower bound on sigma. In our model we can similarly put a lower bound on the sigma prior, but we have additional information from the sensitivity distribution; and the alternatives can be explored by the Markov Chains. Where there is uncertainty, this will be reflected in the posterior distributions. So while this issue doesn't disappear, our model is better at dealing with it than any other. We will modify the MS text to explain limitations of the model, and the improvement of the model compared to existing alternatives.

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Rev2: A) The authors provide a "[. . .] new, Bayesian age model [. . .]" (P578, line 3), meaning a methodological, statistical contribution. Thus a more detailed mathematical descrip- tion seems appropriate; at least for the supplement. Along with this, I may encourage the authors to provide more technical details on the computation (e.g., software code . . . Matlab, R, Mathematica?). Just from the text it is hard (impossible within an appro- priate time range) to reproduce all single calculation steps.

We will carefully re-assess whether any relevant information is missing, and add such information to the revised MS. The maths is actually straightforward, it is the coding that is tricky. We avoided superfluous equations because most things can be looked up easily. For example, we make use of the gamma distribution: the maths can be found on wikipedia, but you don't need to specify it when using an inbuilt function in R/Matlab etc.

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Rev1: 2. Another concern related to the model is the lack of sensitivity test of the Bayesian model to various prior inputs and assumptions. First, both Bayesian models developed in the paper assumes that the number of grains on each multiple-grain aliquot does not change as long as the masker size and grain size of grains remains constant. However, this assumption must not be valid as even at single-grain dating. As the luminescence community is still facing uncertainty associated with the number of grains in each hole, as pointed out in the paper, the uncertainty on the number of grains on each specified sized multiple grain aliquot should be quite significant. Therefore, the paper should include a sensitivity test of the model on this uncertainty.

Rev1: 3. I am a little bit surprised that the paper does not compare the new Bayesian age data with published ages of the 46 samples. Many of the published ages have been extensively discussed and cross-checked with independent dating techniques. Therefore, the comparison will offer a test of the model with know-age samples.

Rev2: (C) My last major comment was also raised by the first referee: A comparison using the new approach with the so far published age results is essentially missing. The authors are aware of that, but they should ask them self: Why readers should apply this approach without evidence on substantial (not to say significant) improvements on the reliability of the age results?

In developing the Bayesian approach to age models, we agree that a sensitivity analysis and age-comparison should be performed and we will stress the need and importance of this in the MS. We feel that adding such a sensitivity analysis to the present paper would be unrealistically demanding. Taking the first point above, our model provides an explicit formulation of the number of grains in the aliquots and the influence on the De distribution. This is a major advance, and it would be interesting to know how fluctuating the assumed or actual number of grains changes the model outcome. But that is a paper in itself, and it cannot be done with data we use here because we don't know the specific aliquot size (a point we discuss in section 4.)

Moving on, we are expected to compare the ages with the already published MAM3 results and to independent ages. But of course the MAM3 is also sensitive to the actual and assumed number of grains in the aliquots, it's just not explicitly stated; it would first need a sensitivity analysis of the type demanded above, which has not been done even 15 years after publication. A comparison with independent ages would be beneficial, but can't be done with this dataset; some of these samples have limiting ages from historical maps, but very few have meaningful precision. Again, this is a question for a different paper. In the revised MS, we will explain why a comparison with known age control is not presented.

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Rev1: the prior of burial dose distribution at single-grain level is assumed uniform in the paper. However, it is well-known that such a uniform distribution is unrealistic. It would be great for the paper to present a sensitivity test of the model to this parameter.

There is a misunderstanding here. The single-grain dose distribution is assumed to be normal (well-bleached) plus half-normal (poorly bleached). The uniform prior is applied to the parameter gamma, which is the mean burial dose.

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Rev1: 4. The paper concludes that reworking by wind or water is the process that may lead to additional bleaching of fluvial deposits while the potential of reworking is related to the vertical position of deposit relative to local mean water level in the channel. I have a few concerns with this conclusion. First, the paper linked present-day sample depth to 2001 average water level in the channel. However, the age range of the samples spans the last 800 years when significant engineering and vertical aggradation of the embanked floodplain have taken place. The paper should address the possibility of water level change in the channel and potential sample depth change associated with compaction during the last 800 years. Second, as the thesis of the paper is on depositional depth dependence of bleaching related to reworking, the discussion should be extended to discuss how depositional depth relates to factors affecting reworking of deposits. For example, mobility of relatively freshly deposited sediments by wind depends on factors such as wind regime, moisture content, and vegetation coverage. How may these factors be related to depositional depth? Because the dataset to support the paper's conclusion (5 samples (?) out of 64) is relatively small, additional discussion on the genetic relationship between mobility of freshly deposits sediments and bleaching is helpful to consolidate the conclusion. By considering elevation alone, it is easy to assume that deposits of higher elevation may be more susceptible to reworking, which is certainly not the case shown in Fig. 6A.

These issues are worthy of discussion, and fruitful discussion will depend on getting robust datasets. We present our 'thesis' as a hypothesis, including a question mark in the title. This is because we don't feel our results are conclusive, given the uncertainties in the data, and given the method used (data-mining). We speculate on whether the results have any meaning, because they are intriguing. However, there is little point in allowing the discussion to wander far without more robust data. We will add this reasoning to the MS, to explain to the readers why we limit our discussion on this issue.

In light of this discussion, we agree with Reviewer 2 that the title should be changed to reflect the broader subject of the manuscript, rather than the uncertain conclusion; we will implement their suggestion in the re-submission.

Interactive comment on Earth Surf. Dynam. Discuss., 2, 575, 2014.