

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

Anonymous Referee #2

Received and published: 29 August 2014

General comments

The authors present a new age model for D_e distributions affected by insufficient bleaching, using previously published OSL multi grain coarse grain quartz data sets from the Rhine river system. Trying to identify the most bleached part of the D_e distribution they apply Bayesian statistics.

To overcome the limitation of working with multi grain data, the authors compute D_e distributions based on single-grain parameters obtained from independent single-grain data sets that were further compared with the existing multi grain D_e distributions to find those parameters that match best. An interesting approach. With this, data evaluation on a single grain level seems possible without measuring single grains. Finally, they use Bayesian statistics to identify the most bleached part of the D_e distributions. Based

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on their results, they propose furthermore that bleaching occurs likely during transport rather than upon deposition.

I appreciate this work as further valuable contribution accounting for the problem of insufficient bleaching by incorporating possibilities of computerised statistical analysis. With this contribution the authors additionally continue their work on fluvial deposits and modelling De distributions.

One main problem of such contribution is the question on the rigour/impact and the definition of the variables that were used for the calculation. Though undoubtedly important, it would be unfair to raise up a too detailed discussion here, as this may never end and may never give any credit on the valuable work the authors have done. Nevertheless, once a new approach, age model, is introduced the authors should provide as much information/details as possible (use the supplement) to make their contribution rigour and to make sure that no room left for technical/methodological speculations.

My major concerns are:

(A) The authors provide a “[. . .] new, Bayesian age model [. . .]” (P578, line 3), meaning a methodological, statistical contribution. Thus a more detailed mathematical description 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 appropriate time range) to reproduce all single calculation steps.

(B) My other concern might appear as the opposite: I miss some kind of flow- (easy to understand) chart/graphic that accounts for the single steps that have been done and a more detailed listing/explanation of used variables. I'm sure that the text will highly benefit from such figure and information.

(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

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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?

Specific comments

- I'm wondering whether the title of the contribution fits well to the content of this contribution. First the authors present a new age model. Second, the fact that "the bleached samples are found close to the mean water level" is an interesting, but not the main, aspect. I may suggest to rephrase the title like: "Re-evaluating burial doses and bleaching parameters of fluvial deposits using Bayesian computational statistics"

- What kind of software did you use for your calculation?

- What a about the water content? I saw in the cited literature that it varies considerably from sample to sample. Of course, this values are included in the total dose rate values, however, in contrast to the nuclide concentration values, which based on measurements, the water content is just a rough guess, but it have an significant influence on the remanent dose.

P577, line: 7–8: Maybe I did not get you, but why should an old deposit be responsible for poor bleaching? The bleaching should be independent of any "previous" age. I presume you mean that the sample deposit consists of both: Transported and not transported material. I may suggest to to rephrase.

P577, line 22: What do you precisely mean with 'identical' samples? If the samples are identical one might state this is similar measuring subsamples (aliquots) from one sample.

P580, line 5: Please be a little bit more specific or provide further informations: For example: How does the dose rate affects the bleaching(!) of the samples? Please explain your judgements.

Table 2: How did you calculate the correlation matrix, or better, how do you justify the

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calculation of the correlation coefficients? You mixed up variables of different levels. For example: 'Site' of nominal type, 'Depositional environment' is (best assumption) of ordinal type, 'grain size' of metric type.

P584, line 25: Where this summary statistics can be found? Table/figure?

P587, line 7: Yes, but this is not the real problem with that variables. See previous comment on Table 2.

P588, paragraph 4.2: Interesting, but so far I cannot see the link. This paragraph is within the discussion, but you did not discuss anything here.

P589, line 18: Yes, but for this contribution it might become the most important aspects. You have to validate your approach.

Technical comments

P576, line 13: Change to “found”

P577, line 15: “Schielein and Lomax (2013)”; reference is missing

P578, line 12: Please add this information (reference) in the table in the supplement (additional column), so that the reader is able to trace the origin of your data set

P578, line 22: Please add reference for the used conversion factors, so far they are different for both publications

P578, line 26: Change to “180–212 μm , 18–250 μm . . .”

P580, line 6: Change to “180–212 μm , 180–250 μm or . . .”

P582, line 15: Check: Heer, A., Adamiec, G., Moska, P., 2012. How many grains are there on a single aliquot?. Ancient TL 30, 9–16. If you already considered, please add reference.

P583, line 26: Change to “Minimum . . . “

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P586, line 6: For your study yes, but nowadays also MG measurements are possible with stimulation in the green.

P586, line 8: The authors might consider to refer to the earlier publication: Singarayer, J.S., Bailey, R.M., 2003. Further investigations of the quartz optically stimulated luminescence components using linear modulation. Radiation Measurements 37, 451–458. doi:DOI: 10.1016/S1350-4487(03)00062-3

Figure 1: Please add latitude and longitude values in the map (much more important than the north arrow)

Figure 6B: Change to “Dose rate”

Figure 6F: Change to “Depositional environment”

Figure 7a and b: “depth” (small letter)

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

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