

## ***Interactive comment on “Fluvial sediment pathways enlightened by OSL bleaching of river sediments and deltaic deposits” by Elizabeth Chamberlain and Jakob Wallinga***

### **Kreutzer (Referee)**

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Dear Elizabeth,

Dear Jakob,

Thank you for this exciting and unconventional application of luminescence techniques in Earth Sciences. Your manuscript takes up the challenge to apply luminescence (dating) methods to trace Earth surface processes through signal bleaching trends and mechanisms. As a natural laboratory, you have selected the Mississippi Delta, from where you have reanalysed 49 samples; a dynamic environment and a sufficiently

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large sampling size. Given the title of your manuscript, it fits very well into the scope of ESurf, and I am convinced that the topic will foster exciting discussions. The text reads smooth, and the figures are almost well prepared. Nevertheless, given that I was asked to critically review your article, below I will raise some points that, in my view, needs to be addressed before the manuscript can be considered for publication. I will first start with some general remarks, followed by some more detailed comments. Although I am a little bit too late already, maybe the discussion gets extended, and we can have a real debate here.

### **General remarks**

1. To start with, in the invitation email by the editor, I was asked to draw particular attention to the methodological aspects of your study. Three weeks ago, I printed your manuscript and took it with me into the field where I had no internet access. Unfortunately, I realised too late that I could not correctly review your paper without having access to your article Chamberlain & Wallinga (2018). So far it concerns me, you refer far too often to your article, and your manuscript does not (yet) stand for itself. At some point, I was wondering why you did not merge the two articles. Nevertheless, if the other work (Chamberlain & Wallinga, 2018) is that important, the reader has a right to understand what you have done in there. Your approach is not a standard method, and hence you should adequately describe it. If you feel that it does not fit into your manuscript, you should at least provide sufficient details as a supplement. In particular, since the other article is not open-access and not everyone may have access to it.
2. The first sentence was a surprise. You start with “OSL dating [...]”. Somehow it does not fit to the title; you submitted your work to ESurf, not to QG. In focus should be your implications for Earth-surface science (and its potential), but not

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the review of work done in the past by the luminescence dating community. I found in line 75 to 80 a suitable paragraph that I suggest to take as a start. Means, I suggest that you amend the introduction of your manuscript. I don't mean that you have to rewrite it, but a focus shift would considerably improve the attractiveness of the manuscript by starting with 'sediment pathways'

Furthermore, please try to avoid reference dropping. If you talk about significant 'methodological advances' in line with Huntley et al. (1985) and Murray & Wintle (2000), I cannot see Cunningham & Wallinga (2010) or Cunningham & Wallinga (2012) (even not Galbraith et al. 1999) here. I sincerely do not want to diminish the work they have done, but references should be justified and not selectively favour articles in which one of the authors of the here bespoke manuscript is a co-author.

3. What I do miss in your manuscript is a proper hypothesis you can test. Maybe you have something in lines 60 to 63, but it is not very obvious. If you reshape your introduction, please try to provide a leading hypothesis you can follow-up and test throughout (please do not hesitate to correct me if I have accidentally overlooked your hypothesis in your text).
4. You did already a good job walking the reader through all the results, but still, sometimes I felt abandoned, and I had to look back to the title and try to connect, e.g., the discussion, with the title. The mix of grain size classes, statistical approaches and depositional features makes it not always easy to stay on track. Moreover, I guess the problem is that with the presented data basically every interpretation you want to favour is possible. Locally different transport histories, as you have written it to justify the need for a large dataset, allow different interpretations. Anyway, again, I guess you did a good job, but maybe you can add a subsection (part of the discussion) that honestly identifies, in brief, the limits of your study.

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5. The conclusion feels a little bit off from the rest of the text. I suggest that you rephrase the conclusion to re-connect it with the rest of the manuscript.
6. I suggest modifying the title a little bit: "OSL signal" or "luminescence signal" and the word 'Mississippi' should be part of the title.

## 1 Detailed comments

### 1.1 Main text

1. Line 42–49: Perhaps this falls a little bit too short; the effectiveness is also a question of the time domain. You may want to add another sentence clarifying how the cited references have addressed this question.
2. Line 139: Please define 'small aliquots' in brackets.
3. Line 145: According to Rex Galbraith (personal. comm.) it should be termed 'early light' and not 'early background'. I tend to agree, since you subtract signal + background from the signal and not only background.
4. Line 146: Please exchange the reference by Ballarini et al (2007); except I did overlook something here (?)
5. Line 171: What do you mean with 'full details'? I did check you manuscript and the supplement, but 'full details' are somehow missing.
6. Line 178: Please make an estimate of the grain number range.
7. Line 182: 'bin files', please change to 'BIN/BINX-files' and explain what do you mean. I know it, but I doubt that a lot of readers of ESurf know it as well.

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8. Line 184: It is a little bit difficult to evaluate from the data given whether your early-light subtraction is justified at all (please see comments on the supplement where I asked to provide (some) raw data).
9. Line 185: Please check the reference, it should be Ballarini et al (2007) (or both, you know it better)
10. Line 189: I would not use the term 'age modelling' for calculating a 'mean  $\pm$  standard error'. Besides, the reader needs a leap of faith and trust that everything that you have done here is justified. Please add additional details (in the supplement if required and if better placed).
11. Line 191: Please specify the type of standard error. If it is the standard error of the mean, it should be written. If not, you can leave it as it is.
12. Line 197-198: I finally double checked your article after could access it. What I do not understand is the estimation of  $\sigma_b$ . Your first step is the application of the CAM to obtain  $\sigma_b$  values for each sample; then you treat the distribution of the obtained overdispersion values with the bootMAM. However, where do you get your  $\sigma_b$  from that you feed into the CAM? Depending on what you put in here, you get everything that you want out from the CAM. Maybe you can clarify this point.
13. Line 210-213: Please add a few more information on how you calculated your ages. Since you have the values, please add them to the table in the supplement. Did you use, e.g., *DRAC* to re-calculate your ages or some other software? Please provide a proper reference to make your data analysis conclusive.
14. Line 221: Please add corresponding sample numbers in brackets for the residual dose.
15. Line 222: What you report here appears to be the (unweighted) mean  $\pm$  the standard deviation; please clarify.

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16. Line 223-224: '[...] plus other methods described in [...]' is a little bit vague; please detail.
17. Line 238: Given the uncertainties, I am not sure whether you can make such a statement; also the number of aliquots (max. 4 per sample) is not very large.
18. Line 241–242: Now it gets a little bit tricky. The way you have written, it implies that you can determine the degree of bleaching also for the silt fraction, but you have only an averaged value. I am convinced that you are aware of it, so please make it clear to the reader.
19. Line 251: Why do you believe that the Pleistocene deposits have suffered from a limited light exposure?
20. Line 296: Sorry, I am a little bit lost, I thought for the silt fraction you used only the 'mean  $\pm$  standard error' (cf. line 191), now you state that you have used the CAM(?). Please clarify.
21. Line 282: 'improvement with time', please clarify
22. Line 299: Minor detail, please use 'ka' or 'a' instead of 'years' as you does in the figures/.
23. Line 300–309: Readers without a background in luminescence dating will have difficulties to follow you through. I understand that you cannot provide more details, but what does, e.g., 'strong luminescence signals' means to you and why it is important? Details are somehow missing here, but then you talk about a 110 °C TL peak (presumably measured in the UV wavelength range) without further information what did you expect. I suggest that you rephrase this paragraph. I am not even sure whether you need it in the main text. It would be better placed in the supplement.

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24. Line 308: Why do you talk now about an 'age overestimation; before you did not talk about ages.
25. Line 310–315: In light of what you have written in Sec. 3.2 I don't quite get why you apply the CAM on the silt samples. I'm sure I am overlooking something, maybe you can help me out here.
26. Line 383: I do agree, the complexity of the dataset and the mixture of grain sizes, statistical (grain size linked) methods and depositional feature makes is extremely hard to follow your conclusions. However, I cannot come-up with a better idea for the moment.
27. Line 388: It appears that you somehow mix 'luminescence signal bleaching' with dose and luminescence ages; you should separate the three. Otherwise one may believe that you can draw a similar conclusion from all the three. For example, why you did not focus solely on a (normalised) luminescence signal itself? With this, you would get rid of difficulties regarding your interpretation. For instance, a residual dose of 2 Gy for one sample may indicate the same degree of bleaching as a 3 Gy residual dose for another sample if the dose rate differs. The (total) dose rates (your Table S1) (without accounting for micro-dosimetric effects); did you check for the impact?
28. Line 400: "OSL bleaching", should better read "Luminescence signal bleaching". I know it is also your title, but it does not sound right. Another suggestion: "OSL signal bleaching".
29. Line 400–404: I do agree with this bullet point but have difficulties to understand why do you believe that this is a direct outcome of your study. I suggest to rephrase this conclusion, please also clarify why your finding only concerns 'sand'.
30. Line 406: 'previous findings': Please add references.

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31. Line 410: What do you mean with 'there are many unknowns with regard to drivers of luminescence signal bleaching'? The 'drivers' of luminescence signal bleaching is the exposure to light. Please rephrase.

## 1.2 Figures

1. Figure 4: Check: 'Paleodose' is underlined red?
2. Figure 7: Please check red line below 'bootMAM'

## 1.3 Supplement

Maybe you can supplement your manuscript with some BIN/BINX-files that would enable the readers (and the reviewers) to better assess what you have done and play with the dataset and the presented method. I guess it will be exciting to test the impact of different parameter settings. If you don't want to provide access to all data, a selection of a representative dataset would be just fine; as long as your method can be tested independently.

1. Please add page numbers to the supplement.
2. Figure S1: Please add a proper reference for your data source (e.g., URL + access date).
3. Table S1:
  - (a) After I had been carefully looking at your table, I realised that I probably do not understand the reasoning for the quoted minimum residual dose. You basically use the residual dose and subtract the quoted standard error and

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term it 'minimum'. I would call it 'residual dose' - 'SE(residual dose)', but still, I don't know what you intend to imply, in particular if the value is negative. I suggest to remove this column.

- (b) You have to admit that 'n = 4' or even 'n = 2' is not really a sampling size we should rely on. Not even for 'fine grain'. Do you see any chance to measure a few aliquots more?
  - (c) Please round values to meaningful digits.
  - (d) Your note under the table: I suggest just 'mean  $\pm$  standard error' not 'central age', in particular since you list dose values, not ages.
1. Figure S2: Please add few more information in the figure caption; just "Typical TL response for PV I-4 silt" makes it a little bit hard to see any value in this figure.
  2. Figure S3: Please remove the red line under 'bootMAM'
  3. Figure S5: Please add the data source, either to the figure caption or to the map itself and please replace the map by a version with a better resolution; it looks blurred. What means 'NAVD 88 (m)'? The inset is a little bit uninformative (probably readers more familiar with your research area have fewer problems).

Sebastian Kreutzer | Bordeaux, 2018-12-08

## References

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