Dear Andreas, Dear Elizabeth, Dear Jakob,

First, thank you for the carefully revised manuscript. The authors' response is detailed and covers almost all points raised. I am ok with most of the provided answers and changes. However, after carefully reading the provided answers along with the 2nd review and revised manuscript, I realised that the entire manuscript comes with a significant weakness:

In its current shape, the manuscript is somehow extended testing of the method presented in Chamberlain et al. (in press). In one regard this is consequent, and here I do agree with the authors' response, one single paper would have been too long. So this paper is a logical step, yet, you consider your work as "explorative research", without "clear expectations beforehand". It makes it tricky to evaluate the study design. Still without a clear hypothesis to test, the given answers remain ambiguous and the manuscript cannot really decide whether it wants to decipher surface processes or surface dynamics (environmental process focus), or it wants to hand in an extended application test (methodological focus) of the method by Chamberlain et al. (in press).

In their response to the 2nd reviewer, the authors deny the need for a simulation that could test the method by arguing that this would complicate and lengthen the manuscript. Contrary, I believe that a properly developed hypothesis regarding surface dynamics paired with a simulation of the method (that can be compared) would dramatically strengthen the manuscript and make it more rigour.

Similar, the manuscript still starts with "Luminescence dating ... " (before "Optical dating ...)", the interesting part, 'sediment pathways' can be found line 87–94. Given the journal scope, it is starting at the wrong end of the stick.

I did not give much thought beforehand as I suggested to the authors to ship some BIN/BINXfile(s) along with the manuscript allowing readers and reviewers to play with the method parameters. The more surprising was the answer: "Reasons are that the data is not sole property of the authors, and that follow- on publications are planned using part of the same dataset. Publishing the data at this point would jeopardize these publications." I understand that the authors used raw data from published work from five different studies, in three out of this five studies the first author is also the first author, so data property should not be an issue. Besides, I would not expect that the authors reveal their entire dataset (for understandable reasons), but share, for example, one single BIN/BINX-file from one sample of their choice. These data alone is of no value to others, except for verifying the here used method. I cannot see how future publications would become jeopardised. Please consider again to add a file of your choice enabling a proper cross-check of your results.

Since my comments above probably read too negative, I should emphasise (again) that I am in favour of the article and the work done here. Still I think that the manuscript could be stronger and my comments aiming at this goal.

1 Detailed remarks

1.1 Response to authors' comments

1. I acknowledge that you have reworked the introduction, but the first paragraph feels still misplaced given the target audience of the journal. The primary focus should be the process, and then you highlight the benefits of luminescence dating the method. On the

other hand, I don't want to interfere too much with a generally well-written text. Means, my recommendations still stands, the decision is yours.

2. "It is the standard deviation of the Des divided by the square root of n.":

Please share this information also with the reader by making it part of the main text, i.e. it should read "standard error of the mean" where applicable.

3. "We do not feed a sigma_b to the CAM. We feed a sigma_b of [0 0] to the bootMAM, when modeling the overdispersion of the dataset. This is now described in section 3.2.."

In other words, your initial sigma_b for the CAM is 0. Since you did not share information on the software you used to calculate the CAM (self-written Excel sheet, *MatLab*?), I can only speculate that your CAM calculation does not allow any sigma_b as input to account for any additional overdispersion that may also be observed for well-bleached samples (e.g., Cunningham & Wallinga, 2012). This is not important, as long you make it clear in the text and add some words on how do you justify this assumption. Your answer should cover the case that people want to start with a (justified and not extreme) pre-assumption on sigma_b (e.g., based on information on dose rate heterogeneity). If your approach is robust, it should allow such values without altering the overall outcome of your study.

- 4. "We used a conventional excel sheet with standard inputs to determine dose rates; we do not think this detail is needed in the manuscript.".The readers have a right to know how you did calculate your results (you have the supplement if you don't like to have it mentioned in the main text).
- 5. "...calculated by dividing the paleodose of each sample by its dose rate, and propagating uncertainties in quadrature."
 What did you do then with the systematic uncertainties?
- 6. "In our mind, both remnant dose and remnant age are of importance. The remnant dose upon deposition is the most direct measurement of the degree of bleaching, and thus relevant when investigation the dependency of bleaching on depositional context or sample properties. Yet, when dating fluvial samples, one is concerned about potential age overestimation due to incomplete bleaching. Therefore the 'remnant age', as inferred from the remnant dose in combination with the sample dose rate, is also relevant."

I guess your answer means that you did not check for the impact for micro-dosimetry effects, right?

I do agree that it appears more appealing to translate your information into an age, which can be easily understood by readers not familiar with the method. Problematic is that by doing so, you make critical assumptions about the distribution of a variable you did not observe (dose rate). Contrary, looking only at the luminescence, i.e. something you indeed observe and normalise these values to its sensitivity, would give you information on the signal bleaching. The drawback is that you answer will be somehow binary (bleached, not bleached), so talking about a dose and translate this into an age sounds more precise, but your inference becomes much more complex and includes a lot of "ifs".

Nevertheless, I don't want to diminish your work, but you have to make your decision clear in the text to the reader. Means, your discussion and your conclusion should point out, in understandable words for non-specialists, that your data and your approach holds only for the assumptions you made (which should be repeated).

7. "Although we warmly support full access to data, this is unfortunately not possible for these data at this point in time. Reasons are that the data is not sole property of the authors, and that follow- on publications are planned using part of the same dataset. Publishing the data at this point would jeopardize these publications. Yet, the methods are fully documented in our papers, and can be applied to other large datasets. If problems occur, we would be happy to assist and support."

I did not ask for full access to **all** data. What I did ask has no value to others, except for the sole purpose of cross-checking the results and playing with the data. This should be in your interest. Besides, I presume that other people likely want to apply your method in the future. Having a reference would allow them to spot their own mistakes and it makes your paper stronger.

1.2 Main text

- 1. Line 202: Change to 'BIN/BINX-files'
- 2. Line 196–198: Please add the grain size you refer to in brackets. "Sand" and "silt" covers a large range, but you use a few different grain ranges, it should be clear all the time to which you are referring to. In particular, sometimes you use "fine silt" instead of 'silt", but given your data, I guess you always refer to $4-20 \,\mu\text{m}$.
- 3. Line 249: "Remnant" ¿¿ "remnant"
- 4. Line 250–255: This paragraph presents good reasoning to explain why you prefer the residual dose instead of a residual age. However, micro-dosimetric effects remain still unaccounted.
- 5. Line 274: Replace "estimated" by "approximated" (with this you remain consistent with your arguments given just a few lines above). You should check this throughout and more clear that any "age translation" is highly speculative and not necessarily supported by your data.
- 6. Line 330: "1 to" ¿¿ "1 Gy to"
- 7. Line 366: Too unspecific ("finer sand grains" and "coarser grains"). Please refer to the fractions you investigated.
- 8. Line 371–372: Please supplement this with brief information about the proportion of the sediment load moving in suspension and the sediment moving as bedload (approximated for your sampling). It is important to relate your findings to the actual transport process and its transport energy.
- 9. Line 376: The paper by Fuchs et al. (2005) does not help. Their study features the bleaching differences between quartz and feldspar. I think that this is not the same what you have done here.
- 10. Line 459: Please better refer to 'new' than 'established' here.
- 11. Line 464: You should rephrase the first point, to "Luminescence signal bleaching of sediments can be highly temporally and spatially variable".

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- 12. Line 467–468: Please reread these lines: You start with "dating purposes" and then add in brackets "overdispersion', and then you tell the reader, please do not sample sites you are not interested. This reads a little bit odd.
- 13. Line 471–473: In my opinion this a correct observation, but a wrong conclusion. The turbidity brings your particles in suspension and the suspension time determines your bleaching, currently, it reads as if you have a different process that controls your bleaching. Besides, if you bring this in the conclusion, you should also have it as an own paragraph in the discussion before (please see also my comments to the tidal influence below) and it should not come by surprise.
- 14. Line 523: I could not access the PhD thesis "Dating Deltas". I tried to get my hands on it via the usual university library networks (without success), and then I found an abstract in the "Tulane University Digital Library". Probably I wasn't trying hard enough, but maybe you can add a link, DOI or a least an ISBN? This would be somehow important since you refer in the manuscript to it when it comes to the suspended samples.

1.3 Figures

1. Figure 1: I've overlooked it the last time, please indicate the boundary of the entire delta. Similar important would be (an approximated) range of the influence of the tide since you argue finally with 'upwelling of turbid water'.

Sebastian Kreutzer — Montréal, 2019–01–30

References

Chamberlain, E.L., Wallinga, J., Shen, Z., 2018. Luminescence age modeling of variablybleached sediment: Model selection and input. Radiation Measurements 1–7. doi: 10.1016/j.radmeas.2018.06.00

Cunningham, A.C., Wallinga, J., 2012. Realizing the potential of fluvial archives using robust OSL chronologies 12, 98–106. doi:10.1016/j.quageo.2012.05.007 Fuchs, M., Straub, J., Zöller, L., 2005. Residual luminescence signals of recent river flood sediments: A comparison between quartz and feldspar of fine- and coarse-grain sediments. Ancient TL 23, 25–30.