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Earth Surface Dynamics Discussions

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Interactive comment

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

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The manuscript address a very interesting topic, which is the use of OSL data as a proxy for environmental processes. With a range of samples from the Mississippi delta, any inferences on geomorphic processes made from the OSL data will be highly beneficial to geoscience, and relevant to society. The authors use a measure of the residual dose in these samples and observe a dependence on grain size that contrasts with previous studies. They also observe a relationship with the sample age, which implies a change in the transport or sourcing of the sediment over time. These relationships make for an enlightening discussion, but given the complexity of the topic, I have some reservations about the validity of the results.

There are a number of challenges involved in research of this type, which make it very difficult to draw firm conclusions. The OSL measurements are used to define a 'response' variable, which is the residual dose in this case. The data is then correlated against some potential 'predictor' variables, to try to ascertain which, if any, are driving the changes in the residual dose. However, the predictor variables are also correlated themselves- e.g. the age and distance seaward, so it is not at all obvious which, if any, would be driving the observed changes.

A further complication comes from the definition of residual dose, which is estimated from a statistic of the equivalent dose distributions. This measure is open to error/bias, because it is estimated from imperfect models. For example, the dispersion in the dose distributions is affected by the number of grains in the aliquot; without accounting for this, a spurious correlation of bleaching on grain size might be observed. The authors recognise this effect, and use a measure of bleaching that seeks to account for the differences in aliquot size- the sigma_b parameter of the minimum age model.

However, there are other reasons that the residual dose statistic might change, even if actual residual dose remains constant. If the aliquot size increases, then the number of well-bleached aliquots is reduced (the 'p' parameter of the minimum age model). We would wish that model performance does not depend on 'p', but it is very likely that is does in some way – my guess is that the results would get more erratic when p is small, with a bias introduced to the burial dose estimate. Another question is in the performance of the models as the burial dose increases: is it possible that the accuracy of burial-dose estimate depends on the actual burial dose? I guess that it would, because at low doses the precision of Des is correlated with their central estimate, and there is an order-of-magnitude difference in the burial doses across the samples being considered.

The observed dependence of residual dose on the sample age, and distance downstream, could both be due to a dependence on the burial dose– which could plausibly arise as an artefact of the data analysis. Before reaching for geomorphic explanations, some effort should be put into checking the validity of the results, and ruling out more mundane explanations for the trends. I can think of a few ways this might be done..

–A simulation of the process. Simulate a poorly bleached distribution, and record how the residual dose statistic depends on the burial dose. Is there a dependency on the number of grains, or the sensitivity distribution? What conditions would be necessary to induce a dependence on De?

–Experimental simulation. Create artificial poorly-bleached populations by giving different beta doses to different aliquots. Mix the aliquots in relevant proportions, measure De and calculate the bleaching statistic. Is there trend with 'burial' dose?

–Apply the method on an unrelated dataset. This is probably the easiest test. Take similar data from elsewhere –the Rhine-Meuse for example– and repeat the analysis. If you see a similar trend with De/age, then it is probably an artefact.

An addition along these lines would greatly strengthen the manuscript, by permitting more confidence in the validity of the conclusions. In addition, I would hope to see some recognition of the limitations of the methods used: a description of the key assumptions in the methods section, and discussion section that reflects on the validity of the results, given the assumptions and limitations.

other points

Title – may need a softer title that reflects the caveats above. 'Seeking enlightenment of sediment pathways. . .' ?

192 and elsewhere. - There is a difference in method used for modern and palaeo data. Is this necessary? Could not the unlogged mam3 be used for the moderen data?

195 - there needs to be a reasonable explanation of the method being used to evaluate the residual dose. This should be a good paragraph, enough for an informed reader

to understand the approach without looking up your earlier paper. It is also important that the key assumptions and limitations of the method are described; in the discussion section, the results should be interpreted with regard to these limitations.

219 – 'channel depth'.. or rather, the sampling depth within the channel?

255 – The residual dose is defined after subtracting the 1-sigma uncertainty. This seems odd, and I couldn't find an explanation in the previous paper.

265 and fig. 5 – the relationship with grain size looks impressive, but there are other possible explanations for the relationship. For instance, it seems the smaller grain sizes relate exclusively to modern sediments, while the larger grain sizes relate to holocene deposits (this is the problem of correlated variables again). Then there is the question of how well the statistic performs when the non-bleaching parameters change- number of grains, or the sensitivity distribution.

section 4.6 – I found this section a bit confusing. The objective is to compare the bleaching of sand and silt fractions of the same sample, is it not? But data is plotted as ages, not doses (or residuals), and using different age models for the different fractions. There are very few samples, and eventually it is suggested that they are well bleached anyway. It might be best to omit this section entirely, as it doesn't seem to add anything to the paper.

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Discussion paper