Earth Surf. Dynam. Discuss., 2, C208–C213, 2014
www.earth-surf-dynam-discuss.net/2/C208/2014/

Earth Surface Dynamics Discussions

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

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Received and published: 22 July 2014

This paper presents a new statistical method based on Bayesian statistics to estimate burial dose distribution at single-grain level of partially bleached deposits using multiple-grain single aliquot De distribution and applies the method to 46 fluvial samples from the Rhine delta to test the hypothesis that bleaching of fluvial deposits occurs mostly during fluvial transport, with additional bleaching occurred to sediments deposited close to mean water level of relevant river reach. The statistical method involves two Bayesian models for burial dose estimation. The first model estimate single-grain sensitivity distribution using multiple-grain sensitivity distribution data obtained from De measurements by assuming that the single-grain sensitivity distribution follows gamma distributions. The second model takes the output of the first model as input, in

addition to inputs of assumed distributions of burial dose, proportion of well-bleached grains and residual doses in a sample to simulate the measured multiple-grain De distribution. The relatively high proportion of well-bleached grains in the 46 tested samples exported by the Bayesian model leads to the conclusion that bleaching of fluvial deposits occurs mostly during fluvial transport, which is in line with data showing the control of fluvial transport distance on residual OSL. Correlation of a bleaching parameter calculated using the proportion of well-bleached grains for the 46 fluvial samples with parameters related to depositional environments of these samples are used to argue for additional bleaching of fluvial deposits associated with post-depositional reworking for samples deposited at elevation close to mean water level of relevant river reach. The major contribution of the paper is to start developing of the Bayesian methods for burial dose estimation of insufficiently bleached deposits, which is extremely valuable to OSL dating community. However, I have a major concern on defining the parameter quantifying the proportion of well-bleached grains in the model. In addition, as a developing model, sensitivity of the model to many uncertainties associated with the model makes the bleaching conclusion based on the application of the model wobbling. Furthermore, it would be preferred for the paper to present formulations of the Bayesian statistics for readers to follow the details of the method. Finally, my feeling is that the discussion on the relationship between deposition depth and bleaching has room for significant improvement. Extension of these concerns is as following.

Major points: 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, the insufficiently 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.

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. Second, 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.

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.

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.

Minor points: 1. The paper used both MG sensitivity distribution and MG De distribution. But in many places, it is not clear enough whether the 'MG distribution' in the paper refers to sensitivity or De. Please clarify that wherever possible.

2. It may be better to introduce parameters a and b in Section 2.3.1 for readers to follow that a and b are model output of the Bayesian analysis in this section, although this is pointed out in figure captions.

3. The values of parameter σbSG should be given in the method section, but not the end of the discussion.

4. P2: Line 10: ''. . . energy of sunlight. . . ". The writing here is a little bit lazy to me. Bleaching is a product of light energy, intensity and time while bleaching efficiency is not related to time. This sentence is quite misleading for people not familiar with the luminescence process.

5. P2: Line 21: 'a' should be added before 'primary source'.

6. P2: Line 22: The first sentence can be understood in two ways. A. To distinguish De representing well bleached grains from those representing grains with a residual. B. To distinguish dose build up during latest burial from dose remnant at individual grain/aliquot level. The paper seems to lean toward the second interpretation, but

please clarify.

7. P3: Line 6: what dose "natural variation" mean? What are "identical samples"?

8. P3: Line 12: "more samples" compared with what?

9. P4, line 8: "grainsize" should be "grain size"

10. P5, line 9: decreasing De?

11. P5, line 20. Should it be MG sensitivity distribution?

12. P6, line 10, MG De distribution or sensitivity distribution?

13. P7, line 12: what same rejection criteria?

14. P7, line 25: This may be an issue of wording or my understanding on the procedure to estimate posterior. Why are the parameters corrected to better approximate the target posterior? Was not the posterior the objects of the Bayesian analysis? If so, the target posterior is unknown and how to evaluate that the corrected parameters better approximate the target posterior? My feeling is that this should be likelihood, which is the measured MG De distribution. Is that correct?

15. P8, line 13, MG De distribution, but not MG de distribution.

16. P8, line 18, missing one bracket.

17. P10, line 8, Are the p and σ parameters used to generate figure 5 the mean of the posterior estimate? The posteriors are all probability distributions. Please clarify.

18. P10, line 28-29. The correlation coefficient between logσ and logitp is bolded and interpreted as statistically significant correlation (caption of Table 2), different from the statement in the paper.

19. P11, lines 12-16: it would be a much better argument for the paper to show the grain-size distribution data after sieving.

20. P11, line 25: clustering of the very well bleached samples?

21. P11, line 28-29: transported in the photonic part of the water column does not mean shallower in stratigraphy.

22. Table 1: true value should be 'model input' as there is no true value.

23. Figure 2: should not the peak of the distribution be 1 because the y-axis is normalized to the peak of the distribution?

24. Figure 3 why the prior of parameter b is multiple-mode? Is that real?

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