Clim. Past Discuss., 8, C2488–C2493, 2012
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Climate of the Past Discussions

Interactive comment on "Testing the potential of OSL, TT-OSL, IRSL and post-IR IRSL luminescence dating on a Middle Pleistocene sediment record of Lake El'gygytgyn, Russia" *by* A. Zander and A. Hilgers

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Received and published: 22 November 2012

As already pointed out in the previous comments by Preusser and Lowick, establishing a reliable, independent, numerical chronology for the sediment record of Lake El'gygytgyn is essential. Therefore I would also like to stress the importance of the results presented in the paper by Zander & Hilgers, which clearly deserve publication. However, I also agree with Preusser and Lowick, that there are some questions which need clarification before final publication of the manuscript.

In its current state, the manuscript consists of six main chapters and I have organized my comments according to this structure:

1. The introductory chapter mainly gives an overview about possible problems related to the application of luminescence dating techniques in general and problems related to luminescence dating of lacustrine sediments in particular. This is followed by a very brief introduction of the research area. The chapter is concluded by a statement of the main objective of the study: "...to test different approaches of luminescence dating ... and provide complemental information on the core stratigraphy." (page 4782, lines 27-29). This statement contradicts the main objective as provided in the abstract: "This study tests the paleomagnetic and proxy data-based Mid- to Late-Pleistocene sediment deposition history ..." The analyses and results presented in the paper fit the first objective much better than the latter, which needs to be clarified in the abstract and throughout the introduction. The introduction is also lacking an overview about the results of previous dating studies dealing with samples from Lake El'gygytgyn (Forman et al., 2007; Juschus et al., 2007).

With regard to the research area, I would suggest to introduce a more detailed, individual chapter. Apart from the general setting, I think it is important to describe the sampled sediments and the relevant depositional processes in more detail, because this may help to better understand the results observed in the luminescence measurements:

- All luminescence measurements were conducted using the fine grain fraction of 4-11 μm – grains of that size may be transported as suspended load in air and/or water. Is it possible to identify the primary type of transport of the sediments before they were deposited in the lake? If the primary source is Aeolian material and far distance transport, bleaching (cf. chapters 4.4 and 5) and poor sensitivity should ideally not be an issue. However, even if the primary sediment source is from a fluvial environment, the bleaching conditions in the lake itself are closely linked to the lake circulation. The circulation in the ice free summer months in a monomictic lake should prevent the

suspended load from being deposited and may enable the signal to be reset even in the lake environment itself.

- Today, you describe Lake El'gygytgyn to be a lake with a very small catchment area and low sediment input from the catchment – is there any indication that that may have changed over time and therefore altered the input of fluvial material? How did climate change during the lake's history influence possible sediment input (cf. Melles et al., 2007, page 95)?

- Is the mineralogy/ geochemistry of the catchment area significantly different from the mineralogy/ geochemistry of the sediments deposited in the lake? If so, this may indicate dominant Aeolian, far distance transport.

- The analysis of reference samples from the inflow areas of the recent rivers and the comparison of the luminescence properties of the suspended load deposited in a fluvial environment vs. the lake sediments would be of great help to solve the previously outlined issues. However, if I understand it correctly from your concluding remarks (page 4796, lines 23-25), such samples are currently not available.

Nevertheless, I suggest adding information on the depositional environment and the main relevant depositional processes to a new chapter "Research area". This also holds true for processes possibly causing significant post depositional mixing of sediments (one of your samples is from a turbidite layer) after deposition in the lake. Please add information about the basic characteristics of the sediment layers the samples were taken from.

2. Concerning the determination of the water content, I would suggest estimating the saturation water content for the samples if that is still possible. Doing so would at least help to evaluate the results from the fictive water content calculations (tables 3 and 6).

3. Please decide whether you regard the detected radionuclide disequilibria as being significant or not. In the caption of Figure S1 you use the following definition: "If the

decay series is in equilibrium the activities of all daughters agree within 2-sigma errors with the activity of the mother." (Please also use 2-sigma error bars in Figure S1.) According to that definition, all your samples are in equilibrium. In the main text however, you still assume an impact on your dose rate and age calculations. In that case you should try to quantify that impact. A number of models to correct for the effects of radionuclide disequilibria have been put forward (e.g. Degering & Krbetschek, 2007; Guibert et al., 2009). If you decide to regard the disequilibria as not significant, please back up that decision by giving references.

3.1 Are the fictive water content values you used within a realistic framework of the saturation water content (cf. comment 2) of the sampled material (cf. comment 1 - sample characteristics)?

4. In chapter 4 methodology and results sometimes get mixed up. If you decide to stick to the suggestion of Preusser and create a new "methods" chapter, all information concerning the measurement setup and the applied protocols could be moved there. If you would like to maintain the current structure, I would suggest introducing subchapters, e.g. "4.1 SAR-OSL on fine grain quartz, 4.1.1 measurement setup (which should include instrumentation and protocols), 4.1.2 Results (from SAR-OSL), 4.1.3 Discussion (of SAR-OSL results). Maintaining this structure would deserve the introduction of an overall discussion before the conclusions chapter as well. I think both structures (with or without a general "methods" section) are possible.

4.1 Early background subtraction did not improve the dataset (page 4786, lines 5-9). Please be more specific: Which effects did you observe when using early background subtraction? Did you have to reject more aliquots, because they did not match the quality criteria? A short remark: In line 25 on the same page you refer to figure 5, but I assume you mean figure 6 instead. I order to back up your findings concerning the erroneous sensitivity correction above saturation, resulting in the linear part of the fine grained quartz growth curve, I would strongly recommend comparative measurements of samples below the saturation limit of fine grained quartz in the specific setting of Lake

El'gygytgyn (<400Gy, equivalent to about 200ka, page 4786, lines 25-28), in order to show that within its limits, the fine grained quartz from Lake El'gygytgyn does actually function as a reliable dosimeter.

4.3 Please be more specific about the modifications of the SAR-IRSL50 protocol (page 4789, lines 24-25). I think a more thorough discussion is needed to clarify why the SAR-IRSL50 worked for Juschus et al. (2007) and Foreman et al. (2007) and why it cannot be successfully applied in this study.

4.4 Concerning the successful dose recovery test after six month storage I strongly support the comment of Lowick. Was the time span of six months randomly chosen? I think it would be worth trying to investigate the minimal time span of storage resulting in a successful dose recovery and accordingly in a probably more reliable De.

5. The apparent age overestimation of samples from the age range between 200 and 300 ka deserves a more thorough discussion with respect to the depositional environment (cf. comment 1). If you consider insufficient bleaching as a possible reason, please discuss the conditions that may have caused this effect to occur. Is there possibly a correlation between sediment type/ sediment structure (a more detailed sample description is essential here, cf. comment 1) and the occurrence of the age overestimation? As a second possible reason you again consider radionuclide disequilibria – if you think they may have a significant influence on the dating results I recommend considering to correct for these effects (cf. comment 3).

6. The conclusion that insufficient bleaching may have resulted in age overestimation needs to be backed up by a thorough discussion, as already pointed out in previous paragraphs.

After revision of the points outlined above, I strongly support the publication of the manuscript in Climate of the Past.

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Interactive comment on Clim. Past Discuss., 8, 4779, 2012.