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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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Establishing independent chronologies is a major challenge when investigating long terrestrial palaeoclimate records. In this context, the present study is of major importance as a variety of different, partly relatively new luminescence dating methods are applied to one of the longest terrestrial palaeoclimate archives available - Lake El′gygytgyn. Due to the outstanding nature of the record, the urgent need for an independent chronology at this site, and the novelty and importance of the results presented here, the present paper really deserves publication. However, there are several

shortcomings in the present version of the manuscript that required major revisions before the article should be accepted. One major issue I see is that the authors do not clearly define the aims of their study. In the Abstract it is stated that "This study tests the sediment ... the deposition history [of the core, derived from other methods]". At the end of the Introduction you say that "The objective of this study was to test different approaches of luminescence dating..."; this are opposing statements. Either you test the methods or the age model, but you should not test both at the same time. Based on your decision of what is the aim of the article, you will have to re-write part of the Abstract and the entire Introduction (see below). Structure of the article The Title is long and intricate. I suggest a short and more handy alternative such as "Potential of different luminescence methods for dating Middle Pleistocene sediments from Lake E′gygytgyn, Russia" The beginning of the Abstract is also a bit lengthy. The first two sentences provide information that is not essential for this paper. I suggest omitting. The Introduction does not provide a good positioning of your study in the research field. It requires complete re-writing. The first paragraph should highlight the importance of the research field (I suggest you focus on the need for independent age control for cross-checking age models of long lacustrine archives) followed by a concise summary of literature in this field (luminescence dating of lake sediments). [It might be appropriate to add a separate section where you summarise the principles of luminescence dating and the problems involved in more detail. There, you could also add present text parts introducing the different methods used in this study]. In the Introduction, you should then position your paper and define the research questions you are addressing, followed by an outline of this article. It might be a good idea to move all information on the site to a separate section (e.g., Regional setting). I regard it as mandatory that the two previous studies dating sediments from the lake (Forman et al. 2007; Juschus et al. 2007) are discussed in detail in an early part of the article. Both studies present a number of results important if not essential for this paper, which are almost ignored (i.e. discussion and solutions for the water content problem). I suggest you add a section heading "Methodology" with the subheading "Sample preparation",

"Dose rate determination", and "De determination". Please do not mix methodological aspects with results. Your present Section 4. is highly chaotic. Please move all technical aspects into the Methodology section. Also, please move all literature review of the different methods into one section together with the general introduction to luminescence dating. This could be placed, for example, after the general Introduction. Detailed comments 4781, line 18ff: The statement that "Only very few studies have focused in luminescence dating of lake sediments. . ." is a bit misleading. There has been actually quite some research starting with Kronborg (1983, PACT 9) and Berger (1988, QSR; 1990, J. Geophys. Res.) and some dozend afterwards. I agree it is not very much but also not "ver few" (implying <10). The following leaves the impression that luminescence dating of lake sediment is highly challenging but none of the papers I am aware of (ca. 30) reports any major problems. The greatest challenge I see is sediment moisture but I have seen very little evidence for radioactive disequilibrium so far. Turbidites are usually identified when logging cores and can hence easily be avoided. 4784, line 24f: Were exactley 157 g measured at both labs? 4784, 1ff: Please add literature explicity showing disequlibrium in water-lain sediments. All siginificant disequilibrium I am aware of is related to either the presence of organic matter or carbonates. Do you have any of this in your sediments? Please also present an example showing that the effect of disequlibirum is substantial (cf. Preusser and Degering 2007, QI). 4784, 5ff: You imply that the gamma spec in Dresden is not sensitive in the high energy range, which is simply not true. In contrast to the gamma spec in Cologne, this machine is ADDITIONALLY sensitve in the low energy range (correctly saying the bachground level is lower). Since you are concerned about disequilibrium, you should have measured all samples in Dresen as your machine cannot detect this. 4784, 12ff: Please add reference showing that radium is mobile in lake sediments. You don′t observe a "decrease" but "lower values" (it could be an increase of 238U/234Th). What are "early isotopes"? You should decide wheather or not the disequilibrium is significant or not. Please carry out some calculation demonstrating the effect of disequlibrium on your samples. We should not assume if it is "massive" or not. 4786, 8:

". . .did not improve the data set and was hence rejected." Please be a bit more specific. You are in this line using the proper writing of my name but I am ever since publishing using my pseudonym "Preusser". 4790, 22ff: Your statement ". . .polymineral fine grains are not suitable for the standard SAR-IRSL50 dating protocol" is not supported by published evidence (Forman et al. 2007, Juschus et al. 2007). It is based on the dose recovery tests for different preheat temperatures, which are using relatively low temperatures compared to previous studies. In fact, this is not a proper preheat test that would support the statement that insufficient preheat will deliver only minimum estimates. Your plateaus are falling and not rising! I consider your statements regarding this approach as not being sufficiently supported by data. 4796, 21ff: I think that partial bleaching is not a likely explaination for the observed overestimation as the sediment input is (mainly?) aeolian. This is shown by the fact that you could extract quartz from the sediment – but the bedrock in the surroundings of the crater lake is basaltic and does not bear quartz. I have also worked on sediments from the direct surroundings of the lake and these have no quartz and terrible IRSL properties (e.g. fading rates between 5-10 g).

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