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Interactive comment on "Stepwise drying of Lake Turkana at the end of the African Humid Period: an example of forced regression modulated by solar activity?" *by* Alexis Nutz and Mathieu Schuster

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ANSWERS TO ANONYMOUS REVIEWER 1

We are grateful to reviewer 1 for his review and time he spent on the manuscript. In the following, we present answers to his comments. We first bring some remarks responding to the general consideration of reviewer 1. Second, we discuss the chronology issue that corresponds to the main concern of reviewer 1. Please not that all recommendations are now integrated. Third, we integrated recommendations proposed in the "Minor comments" section.

General appreciation

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Discussion paper

We would like to precise to reviewer 1 that we decided to withdraw our submission to a previous journal because reviewer 1 was already asking for more precise ages that are not possible to provide for several reasons. The main reason for this is because the accuracy of the dating methods (14C or OSL) does not allow working at such a short time scale of 10 to 100 years. Opposite affirmation would reveal an unfounded (almost foolish) confidence in such methods. Second, during our field trip in the area, we did not find any potential material usable for accurate and meaningful radiocarbon dates (we mean especially charcoals because mollusk shells would introduce more uncertainties considering the unknown reservoir effect). In addition, sampling for OSL would need important logistics in order to remove large amount of eolian deposits before reaching the material to date. This is not relevant especially for a method that does not provide the requested precision. Finally, we hope that reviewer 1 will consider those limits in his potential new review (please find our answer in the "Major comments" section below). Moreover, we would like to remind that the core of the paper is not to refine the age or the timing of the termination of the AHP in Lake Turkana, but rather to understand the drying trend, that up to now was considered as relatively linear in Lake Turkana as in other lakes of Africa. In our opinion, this information is essential regarding our necessity to understand how lakes respond to transition from wet to dry period in this part of the world. Nevertheless, we are grateful that reviewer 1 who acknowledges our learning as well as successive improvements of this paper as stated in its general appreciation "I reviewed two previous versions of this manuscript submitted originally to a different journal, and production of successive improvements to the manuscript has been a learning process on the part of the authors." Thanks again. We are now convinced that the presently submitted version of this paper to SE is the best ever.

Major comments

The major comment of reviewer 1 is "the lack of good chronology". For this paper, we rely on age-models that are published in international peer review journals. Since the aim of this paper is not to refine this age model (which appears difficult to do if

one considers the precision that can be reached by applicable methods), we did not focused our work on providing new ages which would have been at best in the same precision range as the already published ages. Reviewer 1 continues to think that more precise ages can be given for this study. In our opinion, significant more precise ages are not possible to get and the diverse calculations proposed by reviewer 1 are just pseudo-quantitative gestures, which first only propagate more and more errors and second complicate the manuscript. Worth noting, we however decided to follow the recommendation of reviewer 1 because it does not change something to the core of the manuscript. First, we rerun calibration (curve INTCAL13) of the 14C age of the beginning of the final forced regression. This age is from the sample SNU12-589 (Bloszies et al., 2015). A new calibration provides an age of 5.14 $\pm$ 0.18 ka cal BP (σ2). This is very close that date given by Bloszies et al. (2015) used in the previously submitted manuscript (fortunately it is). Concerning the age of termination of the final regression, we proposed to consider the age of 4.58 $\pm$ 0.25 yr BP (sample OSL23/1.30; Forman et al., 2014). This age is an OSL age. However, to follow up recommendation of reviewer 1, we converted this OSL age into a radiocarbon age. Based on 6 examples for which OSL and radiocarbon ages exist, we carried out statistic correlation between OSL ages and their radiocarbon equivalent ages (data Fig. 4; Forman et al., 2014). We obtained a correlation function (age(OSL)=0.98386063*age(14C(calibatred)); b(the intercept) has been forced to 0). This correlation gives an equivalent radiocarbon age of 4.65 $\pm$ 0.3 ka cal BP (4.13 $\pm$ 0.24 ka 14C BP) for the end of the final regression. Once again, this age is very close to the age proposed in the submitted manuscript. Finally, we considered the maximum potential time interval during which the final regression took place (4.57 to 3.90 ka 14C BP) and assigned it a mean age (4.23 $\pm$ 0.34 ka 14C BP). After calibration of this mean age, the probability curve suggests that there is a 44% of probability that the regression precisely occurred between 5.14 $\pm$ 0.18 and 4.65 $\pm$ 0.3 ka cal BP. This is slightly better than the 30-40% estimated by reviewer 1. This is now stated in the manuscript as recommended by reviewer 1 and the “Chronological framework” section has been extended to explain such processing. However, once

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Discussion paper

again we would like to remind that this paper provides firm evidences for a stepwise regression at the end of the AHP. In our knowledge, this is the first report of such dynamics during the final regression of the AHP. These are the facts that anyone can observe and this is the core part of the paper. We then only discuss a potential forcing, subsequently proposing a mechanism. We never argue that the role of this forcing and the veracity of the mechanism are firmly established. We ask to readers that the discussion be considered as a discussion and we are looking forward for alternative explanations based on other forcings for such a decadal to centennial repeated/cyclic lake level evolution that can impacts the general architecture of a delta.

Minor comments

Authors are grateful to reviewer 1 for his suggestions that improve the text. All minor comments proposed by reviewer 1 are now integrated.

Best regards Alexis Nutz and Mathieu Schuster

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/se-2016-95/se-2016-95-AC1-supplement.pdf

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