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Climate of the Past Discussions

Interactive comment on "A distal 145 ka sediment record of Nile discharge and East African monsoon variability" *by* W. Ehrmann et al.

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We acknowledge the comments by the reviewer, and especially for bringing up the suggestions for improving the age model. The comments helped substantially to improve our manuscript. Below we respond to all comments raised by the reviewer.

Main comment:

I read with interest the manuscript by Ehrmann and co-workers "A Distal 145 ka Sediment Record of Nile Discharge and East African Monsoon Variability" submitted to Climate of the Past Discussion. The authors use clay mineral data to reconstruct the history of the Nile River discharge over the last 145,000 years. The manuscript is well written and presents interesting new results. The chronology of the core could and should be improved. This would allow better evaluation of the relationship between proxy records from core SL110 and orbital and sub-orbital climate variability recorded in time series of orbital insolation and Greenland/North Atlantic. The discussion is very long, I suggest shortening it (where/if possible) and using sub-sections to make it easier to follow. Once strengthened by a more robust age model, I believe the conclusions drawn by this study could be of interest to the readership of Climate of the Past.

Response:

Thank you for the generally very positive vote.

The chronology will be changed for the revised manuscript (see below). As suggested, we will introduce sub-sections in the discussion chapter of the revised manuscript. However, because reviewer #2 asks for more detailed discussion in several cases, a substantial shortening will not be possible.

Chronology:

I do not think that establishing the age model for an eastern Mediterranean core, such as SL110, by correlating the G. ruber δ18O from this core to the LR04 benthic δ18O stack (Lisiecki & Raymo, 2005 – Paleoceanography) is appropriate. First, G. ruber is a surface-dwelling foraminifer while the LR04 stack is based on a compilation of benthic (bottom water dwelling) foraminifer δ18O records. Second, and most importantly, differently from the LR04 stack, the G. ruber δ18O record for core SL110 reflects surface hydrographic (temperature, monsoon-related freshwater, excess evaporation) conditions in a marginal basin (superimposed upon and partly controlled by a global ice volume signal). The LR04 stack also reflects global ice volume conditions but it is also strongly influenced by global deep ocean temperature variability. There is, in my opinion, an alternative and better-suited approach to construct an age model for core SL110. It does not require generating new data but it does require using a tuning target other than the LR04 benthic δ18O stack. The authors could correlate the SL110 G. ruber δ18O to the LC21 G. ruber or to Soreq Cave δ18O (see Grant et al., 2012 – Nature). Likewise core SL110, core LC21 is also from the eastern Mediterranean. The LC21 chronology was constructed by using 14C dating for the last 40 kyr and by correlating its G. ruber δ18O to Soreq Cave speleothem δ18O. Hennekam (2015 – PhD thesis Utrecht University) also used a similar approach for constructing the chronology of core MS21 that is located close to core SL110 presented in this study. I therefore encourage the authors to revise their age model by using the LC21 and/or Soreq Cave δ18O records as tuning target.

Response:

We followed the recommendation of the reviewer and modified our age model. As suggested, we tuned our δ18O record to the LC21 record (Grant et al., 2012), which has been tuned to the absolute-dated Soreq cave record. The new age model for SL110 exhibits some changes, mainly a shift ages within the MIS5 interval (and thus timing of S3, S4 and S5) to slightly older ages. These changes, however, did not require a general reinterpretation of our results and thus our main palaeoenvironmental conclusions did not change. For consistency reasons, we will also update the age model for GeoTü SL143 shown in Fig. 3 for the revised manuscript.

Comment:

I think the records in Figures 2 and 3 should be shown versus age rather than versus depth. This would make clear where the hiatus sits in core SL110 and the virtual lack of sediments recording sapropel S5 in this core.

Response:

The data will be presented versus age in the revised manuscript, as also requested by reviewer #2.

Other Points:

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Line 32-33, page 3:

during periods of precession minima/insolation maxima (i.e. periods of sapropel deposition) the “freshwater surplus” in the eastern Mediterranean was not only controlled by the Nile River run-off but also by other river/wadi systems in Northern Africa. This could be made clearer here.

Response:

We will change this in the revised manuscript.

Lines 23-25, page 7:

information on where the data will be made available could be moved to the acknowledgements.

Response:

We will change this in the revised manuscript

Lines 13-17, page 9:

sedimentation rates should be shown in Figure 3. This would help the reader follow the discussion. Also, to substantiate the connection between sedimentation rates, silt/clay ratios, and sea level it would be good to show a sea level curve in one of the figures. If the authors follow my recommendation to use the LC21/Soreq chronology for core SLSL110, then the sea level curve presented by Grant et al. (2012 – Nature) could be used.

Response:

We will add the sedimentation rates to Fig. 3 in the revised manuscript. We prefer not to add the sea-level curve in form of another panel to Fig. 3 or Fig. 4 because the details of this record do not provide crucial information for the discussion and interpretation of our results. We also prefer to keep the figures as simple as possible in order to focus

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on the most relevant data series.

Lines 6-10, page 12:

the enhanced productivity during sapropel deposition was unlikely taking place at the sea surface but in the subsurface (e.g., Rohling & Gieskes, 1989 – Paleoceanography; Sachs & Repeta, 1999 – Science; Grelaud et al., 2012 – Paleoceanography). This statement should be revised.

Response:

We will change this in the revised manuscript and add the references.

Lines 33-34, page 16:

the authors should be expand on the concept of a lack of vegetation feedback over Northern Africa during glacial times.

Response:

Vegetation feedbacks were obviously less expressed during glacial conditions, particularly during the last glacial maximum. This is linked to the generally more arid boundary conditions during the last glacial in tropical and northern Africa (Gasse, 2000) and resulting sparse vegetation cover, particularly in the deflation areas of the Sahara (Tjallingii et al., 2008). We will add this discussion to our revised manuscript.

Additional references, not cited in our manuscript

Gasse, F.: Hydrological changes in the African tropics since the Last Glacial Maximum, Quaternary Science Reviews, 19, 189–211, 2000.

Grant, K. M., Rohling, E. J., Bar-Matthews, M., Ayalon, A., Medina-Elizalde, M., Bronk Ramsey, C., Satow, C., and Roberts, A. P.: Rapid coupling between ice volume and polar temperature over the past 150,000 years, Nature, 491, 744–747, 2012.

Tjallingii, R., et al.: Coherent high- and low-latitude control of the northwest African

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hydrological balance, Nature Geoscience, 1, 670–675, 2008.

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Interactive Comment