

## Replies to the reviewers; Reviewer #1 and #2

and interactive comment

1. Reviewer #1: "On a large scale, the interpretation of hydrological changes in the catchment of the basin appears convincing (...) On shorter time scales, the interpretation appears less convincing (...) due to the large errors in the age models, containing various outliers and scattering of results with deviations of several hundreds to even thousands of years from the mean values applied to generation of the age scale." and similar point made by Reviewer #2: "(...) small changes in XRF-based K or Cl measurements (...) are not consistent across the three cores, and therefore seem to be no more than proxy noise."

Reply: We absolutely agree with the reviewer but understand this comment rather as a note than as a criticism. The signal-to-noise ratio of paleoclimate records always decreases from lower to higher frequencies. This is due to the general redness of the spectrum of paleoclimate records in combination with the appearance of noise from various sources, including those addressed quite right by the reviewer. However, we have modified the text by using more cautionary words where we interpreted the high-frequency variations.

2. Reviewer #1: "(...) the element distributions at the different sites show correspondence with respect to the long-term changes but significant differences and inconsistencies concerning millennial and centennial time scales. The reasons for the contrasting response of different parts of the lake basin to abrupt climate change remain unresolved. (...) As a consequence, this study contains quite some over-interpretation, particularly concerning the nature of the more abrupt events and their connection with North Atlantic climate events." and similar point made by Reviewer #2: "I agree with the other Reviewer that the millennial and centennial-scale interpretations in this manuscript are suspect."

Reply: Again, we agree with this reviewer. For us, however, this is no surprise as it is a common effect within large sedimentary basins. It is true that we have the processes that lead to these differences, not yet fully understood. In fact, we have proposed several projects to decipher the processes that lead to the recording of the climate in the Chew Bahir basin and first test runs are already in progress. Unfortunately, none of these proposals has yet been funded and we are currently working on their revision. However, we have modified the text to clarify that the interpretation of our records, in particular on short time scales, remains preliminary and hypothetical until the proposed work to study the recording process of climate has been done.

3. Reviewer #1: "(...) the results of the new manuscript do not significantly add to the already existing knowledge of the study by Foerster et al. 2012. This paper presents data on core CB-01-2009, which is part of the data again shown in the new manuscript." and similar comment made by Reviewer #2: "This paper appears to be an extension of the work published in Quaternary International in 2012, in which XRF data from core CB-01 was presented."

Reply: The paper by Foerster et al. 2012 was published in a special volume of the Collaborative Research Center (CRC) 806, based on the analysis of a single core and analyzed as part of V. Foersters doctoral project. The paper submitted to *Climate of the Past* is based on a much larger data set, including the records of three cores and a much more sophisticated age model based on a combined tuning and interpolation approach, together with numerous additional radiocarbon ages. The major contribution of this new work based on three cores has been outlined by this reviewer quite nicely: it helps distinguishing between local vs. regional effects on sediment composition, as well as it helps separating long-term variations (which are regarded as significant) from short-term variations. The latter need to be more critically evaluated due to the possible

presence of noise. From our point of view, this is important for our understanding of the response of the Chew Bahir basin to climate change. On the other hand, the data presented in this paper is still the result of a pilot study for the upcoming deep-drilling within the ICDP HSPDP project, which will certainly allow us to provide a much more complete picture of climate change and environmental response in southern Ethiopia.

4. Reviewer #1: "(...) comparisons with stable isotope data from the Iberian margin and with nitrogen isotope data from the Indian Ocean (as shown in Fig. 7) appear not really useful since they address different processes. The main climatic conclusions clearly lack a significant improvement of the understanding of the underlying climatic and regional depositional processes."

Reply: We fully agree with Reviewer #1 in this point and apologise for the mistake that has been made here. We have removed the record from the Iberian margin from Figure 7 now Figure 6.

5. Reviewer #1: "I do not see a strong relation of your data and manuscript to the evolution or migration of modern humans since this is only superficially addressed in the manuscript."

Reply: The reviewer is absolutely right in this point. The topic of human migration and evolution was included in the title as most of the funding comes from two projects on that topic, namely the CRC 806 "Our way to Europe" and the "Hominin sites and paleolakes drilling project". While writing the manuscript we realized that we do not yet have much to say about the topic and this was not the aim of this work and therefore we have changed the title in the revised manuscript.

6. Reviewer #1: "(...) No quantitative information on the distribution of organism remains is shown. (...) In addition, the ecological significance of the different organisms groups (e.g. diatoms) is not properly discussed and underpinned with references. For example, on page 994, lines 10-15, you do not cite any literature with respect to the inferred ecological preferences of certain diatom taxa."

Reply: In principle we agree. However, as said in the methods section, we only found small numbers of diatoms, their occurrence restricted to a few horizons with little diversity and therefore we cannot say much about the environmental conditions based on algae assemblages. We have, however, added Gasse (1986) as the reference we have used to interpret the diatoms.

7. Reviewer #2: "The authors state that the termination of the AHP is gradual, but it appears to me quite obviously that the data suggest that it is abrupt, especially in core CB-03. Even in core CB-01, the transition occurs faster than might be expected from orbital forcing (cf. Fig. 6). The mechanism that the authors invoke to explain the transition involves an interplay of seasonal insolation curves, yet insolation can in no way can explain the abruptness; some sort of feedback mechanism must be involved. The authors need to clarify and address this important issue.", similar to the comment by Y. Garcin: "The data presented by Foerster et al. suggest that defining the AHP termination based on their records – in terms of a gradual versus abrupt process – will remain ambiguous, unless a better chronological framework is developed across the sedimentary records that reflect this climate transition."

Reply: The abruptness of the termination of the AHP is a hotly debated topic since more than a decade. The representatives of the most extreme hypotheses are Peter deMenocal (abrupt, see deMenocal, QSR 2000, based on dust records) with collaborator Jess Tierney and Stefan Kröpelin (gradual, see Kuper and Kröpelin, Science 2006; Kröpelin et al., Science 2008; based on <sup>14</sup>C-dated paleoenvironmental reconstructions and archeological findings). Since these publications, modelers including the handling editor of this paper, Martin Claussen, have contributed much to the discussion, where Claussen is on the "abrupt" side of the discussion (see Brovkin and Claussen, Science 2008, based on a model). A recent seminar, convened by Edouard Bard

together with Peter deMenocal at the College de France held on the 16th May 2014, gives a comprehensive overview of the current discussion including presentations by the contributors to the discussion, some of which might have also reviewed this manuscript. The "abrupt" side of the discussion claims a strong biophysical feedback on climate, following the "green Sahara model" of Claussen, while the others do not see such a strong feedback. To date, there is no clear measure of the abruptness nor is there a common sense on the reference, with respect to the defining of an abrupt vs. gradual change of climate. Jess Tierney, very well aware of the problem, has nicely addressed this problem in her paper with Peter deMenocal ("Abrupt shifts ...", Science 2013), providing probability distributions of the timing of the transitions, taking into consideration the uncertainties of the age models. As reference, most modelers, also including Tierney and deMenocal (2013), use forcing as the relevant reference. Compared to orbital forcing most climate variations on that time scale seem abrupt, due to the involvement of various feedbacks in the climate system. We have decided not to participate in the discussion of abruptness because we believe that it is more semantic in nature. Therefore, we included a brief statement on the topic in the introduction and later avoid the terms abrupt and gradual while interpreting our records.

8. Reviewer #1: "The figures appear much too busy."

Reply: We followed the advise of this reviewer and tried to reduce the complexity of Figures 1 and 4. Figure 4 was simplified to a table (Table 3 now).

9. Reviewer #2: "Finally, the writing style of this manuscript seems casual for a scientific paper."

Reply: We thank reviewer #2 for this comment and tried our best to improve the English. We kindly ask you and the reader to please keep in mind that only a part of the scientific community are native English speakers and especially watching the tone and style can be quite tricky sometimes if English was acquired as a second or third language and that appropriateness can easily be a matter of age and cultural background.

10. Reviewer #2: "CI appears to be interpreted as something that derives from an allochthonous source, in response to an increase in chemical weathering. From which rocks does the CI derive? Is it possible that some of the CI reflects changing lake salinity?"

Reply: We absolutely agree with Reviewer #2 in this point. This is the most plausible interpretation of the CI contents of the sediment. However, as said before, this is the topic of follow-up projects, which will, if funded, give us a much better picture of all the proxy-generating processes in the basin, as part of the much larger HSPDP in near future.

11. Reviewer #2: "I'm not quite convinced that insolation during October and November could be responsible for the persistence of the AHP, nor that the lakes respond only to JJA insolation. It certainly cannot explain why the termination of the AHP was abrupt in some locations and not gradual. First of all, there is some evidence that lake levels declined abruptly at 5 ka (e.g., Lake Turkana; Garcin et al. 2012), so it is not true that lakes only respond to JJA. Secondly, the increase in ON insolation is accompanied by a decline in JJA; so if the climate responds straightforwardly then a gradual decline should still be observed. A feedback mechanism is needed to explain an abrupt transition, whether that be vegetation, oceanic feedbacks, or perhaps dust."

Reply: Studies by Junginger and Trauth (2013), Costas et al. (2014) and Junginger et al. (2014) for example have shown that the precession over the past 15,000 years and the associated insolation change from a JJA maximum in the northern hemisphere (NH) to a SON maximum at the equator has influenced the location and moisture amount of two precipitation sources (Congo Air Boundary and ITCZ) during the peak times of the AHP and its termination. We agree, that feedback

mechanisms (such as that caused by a dense vegetation cover) could have played a significant role in the character and especially the timing of the termination of the AHP. The abrupt termination of the AHP as described for Lake Turkana, however, could have been caused by the cut off from two extra water sources. One coming from the Chew Bahir basin, that was fed by numerous lake basins far up the Ethiopian Plateau and the other source coming from the south via the Suguta Valley, which has received water from numerous lake basins high up the East African Plateau, as Junginger et al. (2014) suggest. With those lakes shrinking towards the end of the AHP, Lake Turkana has lost probably two thirds of the amount of extra water that was provided during the AHP via these basins and abrupt changes may thus be explicable. Several cooperating projects are currently in progress and aim at supporting this hypothesis with data. This could be an explanation why an abrupt transition of the AHP is reflected in data from Lake Turkana while lake basins between the equator and 7-10°N declined gradually, following the insolation and thus the decrease in moisture in SON via the control of the ITCZ.

12. Reviewer #2: "In the Gulf of Aden leaf wax  $\delta D$  record (Tierney & deMenocal, 2013) as well as the Lake Tanganyika leaf wax record (Tierney et al., 2008) there is evidence for more humid conditions during this precessional cycle, and it is also clear that sedimentation rates increase at Chew Bahir, which would seem to suggest wetter conditions in spite of the XRF data."

Reply: We do not understand the comment made by Reviewer #2. Our data are in full agreement with the leaf wax  $\delta D$  record by Jess Tierney and Peter deMenocal, as it shows an increase in moisture during the last precessional cycle, in the course of higher sedimentation rates. Is Reviewer #2, which is probably one of the authors, simply asking us to cite her/his work?