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Interactive Comment

Interactive comment on "Short-term variability in the sedimentary BIT index of Lake Challa, East Africa over the past 2200 years: validating the precipitation proxy" *by* L. K. Buckles et al.

Anonymous Referee #2

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The aim of this study is to test the BIT index (ratio describing the proportion of branched GDGTs, of soil origin, versus isoprenoids GDGTs, of aquatic origin) as a proxy of precipitation in tropical Africa. Buckles et al. used data from a sediment trap, soils, and lake sediments combined to climate data to evaluate the BIT index in the Lake Challa area (Kenya/Tanzania). They found that brGDGTs were also produced in the lake water column and that the BIT index in Lake Challa sediments reflected the crenarchaeol abundance, rather than brGDGT abundance thus complicating the original interpretation of this proxy. Here, Buckles et al. proposed that pulses of Thaumarchaeota production during the driest and windiest years mostly control the BIT index in a lake system where allochthonous sedimentation is dwarfed by autochthonous sedimenta-

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tion. I found their interpretation realistic for the modern/recent lake sediments but it is also possible that the proposed mechanism varied for the older sediments (cf. the 25,000 yr record). For example, the high BIT index (of ∼1) during the early Holocene may also be related to the increase of brGDGTs derived from soils (this period was significantly wetter compared to the present-day conditions). The data presented in this study are highly valuable, particularly since data derived from such a long-term monitoring program are very rare but remain necessary to understand the sedimentation processes in lake systems. Overall I found this paper interesting to read and ultimately worth for publication at Climate of the Past after substantial adjustments. Like the authors, I think that the BIT index remains a potential good proxy for paleohydrology, although the monitoring data presented here suggests that the behavior of the GDGTs and the exact meaning of this proxy remain still elusive in small lake systems. I disagree with the authors that their study 'validates' the use of the BIT index in such environments since the new mechanism they promote (i.e. high in situ brGDGT production combined to a production of crenarchaeol triggered by precipitation in the lake's catchment) to explain this proxy strongly differ with the initial one (i.e. soil versus aquatic origin of the GDGTs). Moreover, they do not provide a way to evaluate which mechanisms (soil-derived brGDGTs versus in situ production of brGDGTs) can control the BIT index in the sediments. This would be necessary for an unambiguous interpretation of the sedimentary BIT index. For example, when looking at different time periods, both mechanisms could operate in the same lake system and their impact on the BIT index cannot be considered as identical (until proven). The authors should provide here a more balanced discussion and importantly they should also provide more ways to help future understanding of this proxy. Below are other important points that, in my opinion, need to be fully clarified prior to publication.

Point (1). The modern data: settling particles.

Most of the sediment trap data presented by the authors derived from Sinninghe Damsté et al. (2009) and Buckles et al. (2014). The authors mentioned here that they “report additional results for GDGTs I to IV . . . present in these samples”. It is not clear reading this manuscript what is really new and what is derived from the former studies on this site. This should be better defined. The authors should emphasize more their discussion on the new findings. I wonder if the GDGTs data (and their indexes) from the sediment trap are contemporaneous with the weather events presented for comparison (temperature, precipitation. . .). Here are some open questions that should be discussed more in detail in this manuscript: - What is the estimated residence time of the GDGTs in the both the Lake Challa water column and its watershed? - What is the velocity of settling particles within the water column of the Lake Challa? Does this velocity remain constant during a seasonal cycle? Looking at the Wolff et al. (2014) data, it seems that there is a systematic lag of ∼2-4 months between the deposition of Ti and the preceding main peak of precipitation. Does that also apply to the GDGTs?

Point (2). The paleo-record: comparison of the BIT index and varve thickness during the last 2000 yr.

The authors spent a large part of their manuscript to discuss the modern data, while a smaller part of it is devoted to the discussion of the paleo-record. The balance between the two parts could be improved. Figure 7 shows the direct comparison of the BIT index and varve thickness during the last 2000 yr. The authors used 5-point and 7-point running average for the BIT index and varve thickness data, respectively, which were sampled with a different resolution. Instead, I would advise them to resample the varve thickness data using the exact sampling resolution as for the BIT index and to show a time series of varve thickness (with the mean and standard deviation for each sample interval) that is directly comparable with the BIT index data. Then, the same running averages could also be overlaid above the two records. Correlation plots with significance level would be also valuable. Does the correlation vary in time? The authors suggested that “the BIT index should not be used as a precipitation proxy on the interannual timescale. Rather, one data point per decade seems sensible, with a five-point moving average (Fig. 7b) providing a robust reconstruction of longer-term

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dry/wet trends." To validate their statement, they should calculate the correlation (and demonstrate that it is significant or not) between the BIT index and the varve data at different timescale (using for example a moving average or a band-pass filter): according to their statement, correlation should increase towards lower frequency variability of the BIT index. Buckles et al. compared their BIT index data with the total varve thickness. However, Wolff et al. (2011, 2014) demonstrated that "the total varve thickness is controlled by the thickness of the light layer... varve thickness mainly reflects the quantity of diatom frustules deposited during the dry season and in particular during April to September". Wolff et al. (2014) noted that "varve thickness can be used as a proxy to reconstruct paleo wind variations during the dry season." Thus the total varve thickness is not a direct proxy of precipitation. Instead, the small and organic dark varve layers in the sediments record monsoon precipitation (unfortunately biased by additional in situ lake precipitation products): "The darker layers represent the two rainy seasons (November to December and March to May) and the brief intervening dry season with amorphous organic matter derived from phytoplankton and calcite precipitation..." (Wolff et al. 2014). The authors should then also compare the BIT index data with the dark varve thickness data and provide a correlation plot as for the total varve thickness data. It would be important to know which varve thickness data (light or dark) provide the best correlation with the BIT index through time.

Other points:

- Although I am totally confident with the GDGTs data produced by the authors and the calculated BIT index for the different substrates presented in this study, I am not very confident with the precipitation data they used for comparison. Buckles et al. (2014) first showed the precipitation data and indicated that it derived from a governmental agricultural station "immediately north of Lake Challa". In the current paper Buckles et al. showed the same data but discriminated the data from "Challa" and "Taveta". The time series looks weird, it is not seasonal but erratic, showing for example only one significant month of precipitation for 2007 with ca. 650 mm (extreme precipitation amount for March), and almost not a single drop of water during the rest of this year. What is even stranger is that other authors who also worked on the same material from the same sediment trap used other precipitation data for their comparisons: the record of Voi located 100 km to the East (Wolff et al., 2014). The Voi station is also not ideal since it is located too far away from the study site and may not have recorded some major events at Challa. Since the precipitation record from Voi show less gaps of precipitation (i.e. prolonged period of no precipitation) and since the mean annual precipitation is significantly lower in Voi compared to Taveta according to Sinninghe Damsté et al. (2009; Fig. 3f), the data presented here seem obviously incomplete/biased. According to the data of Voi (Wolff et al., 2014), there is no "long drought that stretched from May 2007 to February 2008 due to failure of the short rains in 2007" as Buckles et al. asserted. If the precipitation data derived from local ground-based stations have issues, the authors may check the remote sensing product for alternative solutions. For example, the data from the Tropical Rainfall Measuring Mission (TRMM) are easily available, and provide 3 hourly rainfall amounts with a spatial resolution of 5x5 km2 from 1997 to 2015. I am not arguing that a more robust precipitation record would provide a complete mechanism for interpreting the complex BIT signal but this will certainly help the data interpretations.

- The nomenclatures of the GDGTs are not straightforward and will confuse many of the readers who are non-specialists. Referring to GDGT-0 for GDGT-I or to GDGT-1 for GDGT-II does not facilitate the understanding of complex ratios of molecules. It would be wise to state that this dual nomenclature exists at the beginning of this manuscript and to stick to one or the other all over the manuscript.

- I also found highly relevant and valuable the comments provided by the first reviewer.

Interactive comment on Clim. Past Discuss., 11, 1177, 2015.