Reply to Reviewer #2 (RC2)
Lucas Bittner et al.

Author comment on "A Holocene temperature (brGDGT) record from Garba Guracha, a high-altitude lake in Ethiopia" by Lucas Bittner et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-95-AC2, 2022

Reply to Reviewer #2 (RC2):

We are grateful for the time and feedback of Reviewer #2. His/Her valuable comments helped very much to improve the manuscript. In the following, we provide a point-by-point reply to all comments:

The paper by Bittner and colleagues proposes a novel mean annual air temperature (MAT) record from a high-altitude lake in Ethiopia. The record is obtained by analysis of the relative abundance of brGDGTs, bacterial membrane lipids, a proxy successfully used for more than a decade for continental paleoclimate reconstruction across the globe. In this paper, Bittner and colleagues first refines the brGDGT calibration to include specificities of northeastern African lakes before applying it to reconstruct temperature variability over the Holocene. The paper is well written and easy to follow and the research question addressed by the authors is relevant and of high interest. Indeed, the climate sensibility of the Horn of Africa has been less studied than other parts of (Eastern) Africa even though it’s at the northernmost limit of the zone impacted by the ICTZ fluctuations. Overall, I would recommend the paper for publication after moderate revisions. You will find below a list of my major and minor comments on the manuscript.

As a large part of the manuscript deals with the refinement of the temperature calibration, the authors should include a discussion on the origin of the brGDGT signal and what “temperature” the proxy is actually recording. Do they consider the brGDGTs to be mainly produced in the lake water column or in the catchment soils (notably in the alluvial swamp mentioned in the site description)? Can the source of brGDGTs change over time (in relation with the level of precipitations) and could this impact/bias the temperature reconstruction over the Holocene? In several instances in the discussion part, the authors seem to suggest that the lake physicochemical conditions may impact the proposed temperature record. Does that mean that they assume the brGDGTs to record the lake water temperature instead of the annual air temperature? These points need to be discussed and clarified by the authors in a revised version of the manuscript.

- We are grateful to Reviewer #2 for highlighting this topic, which we have not addressed enough. Generally, lake water temperature and mean air temperature have a good agreement for most temperate and tropical lakes on a global scale. brGDGTs produced in the lake water column or the lake sediments reflect lake water temperature. However, due to the good agreement of MAT and lake water temperature in tropical
lakes and the fact that temperatures used in the modern calibration datasets are reported in MAT and, at least in the Bale Mountains, derived from air temperature, we use MAT.

- Concerning the source identification of brGDGTs, we need to highlight that the producing communities of brGDGTs are not fully understood yet. Therefore, we can only use different indicators, proxies, and comparisons to predict the origin of most brGDGTs in the Garba Guracha sediments. The Garba Guracha is/was characterised by high aquatic productivity. Several analysed proxies used for organic matter source identification point to a predominantly aquatic production ($\delta^{13}$C, TOC/N, $P_{aq}$, sugar quantification ratios) (Bittner et al. 2020, 2021). Moreover, the composition of brGDGTs in soil samples and lake sediment samples in the Bale Mountain is not similar, indicating different producing communities. These findings are coherent with the results of Russell et al. (2018) that brGDGTs in lake sediments of eastern Africa are dominantly lake-derived. Therefore, we suggest that also most brGDGTs in the Garba Guracha sediment archive are of aquatic origin.

- To account for the valuable feedback of Reviewer #2 we added a figure to the supplements and a paragraph (line 250) to the manuscript focussing on the origin of brGDGTs: “In general, the sediments of the Garba Guracha are characterised by a high input of aquatic organic matter. Several analysed proxies used to identify the source of organic matter indicate a predominantly aquatic production ($\delta^{13}$C, TOC/N, $P_{aq}$, sugar quantification ratios) (Bittner et al., 2020, 2021). The composition of brGDGTs in the sediment of Lake Garba Gurahca is inconsistent with the soil samples in Bale Mountain, indicating different producing communities (Fig. S3). These findings are concurrent with the results of Russell et al. (2018) that brGDGTs in eastern African lake sediments are dominantly lake-derived. Therefore, we suggest that most brGDGTs in the Garba Guracha sediment archive are of aquatic origin.”

I also find the discussion of the Holocene temperature record in the section 5.3 to be too superficial. I am lacking a discussion of the similarities / differences between the record presented in this manuscript and the other East African records. What do we learn from this record about the connectivity between the climate in the Horn of Africa vs. in locations closer to the equator? Or about the west/east connectivity? Also, the amplitude of temperature change is very different between the different records presented in Fig. 8 but it is not discussed by the authors.

- We are thankful for the feedback. However, due to the RMSE of ~2.4°C, we do not think we can compare short-time similarities or differences in the eastern African records. We instead interpret the overall trends and have discussed and compared Garba Guracha to other records in particular time slices (deglaciation, AHP onset, AHP and AHP termination). For example, Sacred Lake and Lake Rutundu are only 12 km apart, showing similar trends in the Early to Mid Holocene, but their detailed temperature reconstructions are quite different. Under these circumstances, we do not see a straightforward interpretation concerning a north-south or west-east trend. Moreover, we do not consider this to be the scope of this manuscript.

- We added a small paragraph to broaden the discussion (line 369): “In contrast to precipitation reconstructions based on $\delta^2$H in East Africa (Garelick et al. 2021), the temperature records do not show a clear meridional, north-south temperature change, nor an east-west pattern. The reconstructed overall temperature ranges are coherent with the elevations of the lake archives. The amplitude of temperature change over the last 13 ka at Garba Guracha is ~6°C. Similar temperature changes have been reconstructed at other high-altitude sites (Lake Mahoma and Lake Rutundu) (Loomis et al. 2017; Garelick et al. 2022). Equatorial records at lower elevations yield lower temperature amplitudes (Lake Victoria and Sacred Lake) (Loomis et al. 2012; Berke et al. 2012), and higher temperature amplitudes are recorded in northern Lake Tana.”
(Loomis et al. 2015). Overall, temperature records in East Africa during the Holocene show similar trends and seem to be mainly influenced by local factors and changes in insolation.

Minor comments:

I. 85: The publication by Halamka et al. (2021; doi: 10.7185/geochemlet.213) showing production of brGDGTs triggered by oxygen limitation in an Acidobacteria should be cited and discussed here.

- We are grateful for this feedback and added a sentence: “A recent study by Halamka et al., 2021 reports that Acidobacteria produce certain brGDGTs under oxygen limitation.”

I. 93: Bale Mountains are cited here for the first time, without being introduced before. The authors should introduce why the Bale Mountain area is an interesting location to study the Horn of Africa temperature variability earlier in the introduction.

- We are grateful for the feedback of referee #2 and added a sentence in the introduction (line 73): “The Bale Mountain, situated in the East of the Rift Valley, are a valuable study site with the potential to enhance the paleoclimatic knowledge in an understudied region.”

Material and methods: it’s not clear whether the authors extracted and analysed themselves the surface sediment samples or if they just used already published data.

- Thank you. We clarified this by rephrasing one sentence (line 147) to: “In this study, we used the published data of 76 surface sediment samples from eastern African lakes. The data of these lakes, located mainly in Ethiopia, Uganda and Kenya, were published by Loomis et al. (2014, 2011, 2012), Russell et al. (2018), Eggermont et al. (2011) and Baxter et al. (2019).”

Figure 2, 3, S1 and S2: the colors described for the datapoints in the captions do not seem to fit the colors in the figures.

- Thank you. Has been done.

I. 208-210: I do not understand the distinction between Kenyan, Ugandan lakes and the East African ones. Are the samples named “Kenya” and “Uganda” samples from the East Africa dataset of Russell et al. with T<10°C (higher altitudes) or are these not included in the dataset of Russell et al. and new samples analysed by the authors? This must be better explained by the authors.

- We tried to clarify this by rephrasing a sentence in line 225 to: “Compared to similar high altitude lakes (above 3500 m and MAT < 10°C) in eastern Africa (situated in Kenya and Uganda published in the East African lake dataset (Russell et al. 2018)), the percentage of IIIa and IIa is lower, and the IIIa’ and IIa’ is higher in the Bale Mountain lakes (Fig 3).”

I. 228-230: It is hard to draw such a hypothesis from the data distribution in the PCA. The Bale Mountain samples are vertically distributed which suggests that they have the same
proportion of IIIa but it is their proportion of IIIa’ (and IIb) that varies most. Moreover, it is clear in Fig. 3b and 3c that it is the East African lakes that are responsible for the good correlation between the lipid fractional abundance and the MAAT. It is difficult to see any relationship between the lipid fractional abundance and MAAT in the Bale Mountain dataset (as well as in the datasets of Kenyan and Ugandan lakes with T<10°C).

- Here we kindly disagree because without the Bale Mountain lakes the correlation of IIIa to MAT is slightly better ($r^2=0.86$) than for IIIa + IIIa’ ($r^2=0.82$). This changes once the Bale Mountain lakes are included. Therefore, supported by Fig. 3A, we still believe that at least in some Bale Mountain lakes, IIIa’ is produced instead of IIIa. We also tried to formulate the sentence very carefully using “hypothesise”.
- However, to account for the valuable feedback of Reviewer #2 we rephrased the sentence to “We hypothesise that the 6-methyl compound (IIa’ and IIIa’) might be produced instead of their 5-methyl counterparts (IIa and IIIa) or in higher abundances in some of the Bale Mountain lakes.”

I. 235-237: and so? Is this important for the rest of the manuscript?
- We believe that mentioning regional differences in the current data is important for the manuscript to point to regional differences in the lake records.

*Figure 4: captions of A and B are inverted. The color codes in the barplot should be explained.*
- Done

*I. 277: no “s” at Bale Mountain.*
- Done

*Figure 6: again the colors mentioned in the caption do not fit with the colors of the figure.*
- Done

*I. 300: to be replaced by “we will only discuss”*
- Done

*I. 320-322: this sentence is not clear, the authors should rephrase it.*
- Thank you very much. The sentence was not necessary and we deleted the sentence not to cause confusion.

*I. 347-351: are the authors suggesting that the brGDGTs are recording the lake water temperature rather than the MAT? (see my major comment above)*
- Yes, in the case of the Garba Guracha, we assume that the mainly aquatically produced brGDGTs are recording lake water temperature.

*Section 5.3: the country where each lake is located should be added to help the reader orientate him/herself.*
- We added the countries to Fig. 8.

*Fig. 8: for the stack record of Ivory and Russell, does the y-axis show absolute temperatures or delta of change?*
We thank the reviewer for pointing out this mistake. We have changed this to “Average Temperature Change (°C)”.

l. 401-406: here as well, the authors suggest physical phenomena within the water column that may influence the lake water temperature. But aren't the brGDGTs supposed to be correlated with air temperature? In this regard, physical phenomena such as water stratification or ice formation should not influence the brGDGT signal (supposed to come from soil weathering within the lake catchment).

- We have addressed and answered this comment above.