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.

Major comments:

-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.

-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? For example, records in Fig. 8 could be classified from the closest to the furthest from the Bale Mountains and countries should be specified for each record to help the reader orientate him/herself. 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.

Minor comments:

l. 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.

l. 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.

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.

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.

l. 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.

l. 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).

l. 235-237: and so? Is this important for the rest of the manuscript?

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

l. 277: no "s" at Bale Mountain.

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

l. 300: to be replaced by "we will only discuss"

l. 320-322: this sentence is not clear, the authors should rephrase it.

l. 347-351: are the authors suggesting that the brGDGTs are recording the lake water temperature rather than the MAT? (see my major comment above)

Section 5.3: the country where each lake is located should be added to help the reader orientate him/herself.

Fig. 8: for the stack record of Ivory and Russell, does the y-axis show absolute temperatures or delta of change?

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).