Reply to Jan 9 report from Maria Fernanda Sanchez Goñi

In the revised manuscript the authors have responded well to my comments and it is ready for publication after correcting the following minor problems:

a) In Figure 10, the pollen-based MTCO reconstructions from core MD95-2042 should be plotted against the updated age model that is in line with the chronology of Heinrich stadials published in Sanchez Goñi and Harrison (2010, QSR).

The figure has been updated with the latest chronology. I thank Dr. Sanchez Goñi for sending the updated data file.

b) In line 506, the DJF cooling is stronger, between 1.5°C and 4°C, compared to the interval given by the authors, 1.5-2.5°C.

This has been corrected in the text.

c) In the legend of Figure 6, the pink bands indicate the Heinrich stadials (HS) and not Heinrich events (HE). The Heinrich stadial is the climate interval associated with the Heinrich events (massive iceberg discharges from the North American ice-sheets during the last glacial period). The authors should modify the legend and the figure accordingly. The same applies for the text, for example lines 429, 506, 524, 528 and 541, where the authors should replace Heinrich event with Heinrich stadial.

We have replaced Heinrich event with Heinrich stadial throughout, unless referring specifically to iceberg discharge events.

d) In the figures, the width of the bands indicating the time intervals of the Heinrich stadials should take into account the chronology of these intervals, broadly accepted, given by Sanchez Goñi and Harrison (2010, QSR)

In general, we agree; however, the age of H3 in Sanchez Goñi and Harrison (2010) seems to be not widely accepted, as it does not fit with Iberian margin SSTs or Greenland ice core records of temperature and dust. Therefore, we use the age 30.6–28.9 b2k, which corresponds to Greenland stadial 5 (Rasmussen et al., 2014). We have updated the Heinrich bands (except H3) to match the ages of Sanchez Goñi and Harrison (2010) in all figures.

Reply to 02 Feb report by Anonymous referee #3

I am providing a review of the author replies to previous reviewers' comments and the adjustments made in the manuscript. I think that the authors have done a good job responding to the main concerns and my additional points are only minor. Notably, temperature reconstruction based on brGDGTs in lacustrine sediments is notoriously hard, so I appreciate the efforts of the authors here. Although they have done an above average job in assessing the brGDGT signals in their record, I feel like there could have been even more. Regardless, given the overall positive feedback in the first round of comments, I'll leave it to future studies with further insights into lacustrine brGDGT dynamics to possibly extract this information from the data on Pangaea.

L18: We find a bias… -> specify: bias in what?

Text modified to read “We find a negative bias in brGDGT-based temperature estimates”

L42&section 3.4: Millennial scale climate events are also detected in Lake Van (there are multiple papers from this lake, but Stockhecke et al. (2021, https://doi.org/10.1016/j.palaeo.2021.110535) specifically is on biomarkers and D-O events, but just predates the global lake calibration – still, D-O variation is presumably >>1C!)

Added citation in the Introduction and section 3.4

L142: were one (as is mentioned in the manuscript) or two (as is proposed by Hopmans et al., 2016) silica columns used for GDGT separation by the LC?

Two columns, thank you for pointing out this error, which has been fixed.

Calibration selection: I never really understand the urge to assess all possible calibrations and then continue with the one that looks most like the expected (or desired?) record. How does matching expectations validate the reliability of the record? Especially when most of the tested calibrations come down to (some form of) the MBT…

There is always information in downcore changes in brGDGT (or any biomarker) distributions. So, if one calibration does “not work” it is always worth to investigate which compound is driving this and then to assess why this compound could be behaving the way it is. Notably, the PCAs in Fig. 3 suggest that temperature is likely not recorded well by the MBT’5me, as the compounds that are in the numerator (tetramethylated 5-me brGDGTs) and the denominator (penta- and hexamethylated 5-me brGDGTs) of this ratio do not clearly plot in opposite parts of any of the PCs (specifically, IIa plots with all tetramethylated brGDGTs, which would make the MBT’5me mostly dependent on IIIa, IIIb, and IIIc).

Anyhow, I guess that this manuscript has passed the stage of this kind of comments, so keep things as is. Also, during the review process of this manuscript two new lacustrine brGDGT studies have appeared: one specifically for European lakes (Bauersachs et al., 2024, https://doi.org/10.1016/j.scitotenv.2023.167724) and a global one (O’Beirne et al., 2023, https://doi.org/10.1016/j.gca.2023.08.019) that both address some of the sensitivity issues that are also discussed in the current manuscript. In particular the European calibration study addresses the potential role and presence of IIIa’’ in lake sediments.

I note that while many calibrations were tested on modern samples, only two published calibrations were tested on the downcore sequence (Martínez-Sosa et al., 2021; Raberg et al., 2021), and these two have quite different formulas, and therefore temperature reconstructions can differ substantially.

Its unclear to me if the reviewer would like us to test the more recently published calibrations. This would represent a major revision at this late stage in the review process, and given that new calibrations are published frequently, I think it is reasonable to only consider those that were published before the paper was submitted. As a note, the new European calibration is based on the Ia brGDGT and will likely yield results very similar to the temperatures we use from the corrected Raberg21 model based on the strong correlation between Ia and our reconstructed TMAF, as shown in the PCA (Fig 3).

L370: Add reference after the statement that BIT is more likely controlled by the niche for crenarchaeol producing Thaumarchaeota (which are currently named Nitrososphaerota btw)

Reference and additional explanation added. And Thaumarchaeota has been updated to Nitrososphaerota.

L378: I agree with the interpretation that IIIa/IIa is more likely to reflect contributions from different niches within the water column than soil vs aquatic producers. Please also adjust this in the panels and captions of the figures (for those people that 'read' a paper by only looking at the graphs). Also, later in the discussion (e.g., section 3.3) the interpretation of IIIa/IIa as a soil indicator is still maintained. So, which interpretation is considered valid in the end?

Difficult to say which is valid in the end! Maybe a bit of both. Our data clearly show soils do have lower IIIa/IIa. The increase in the ratio with depth in our lake sediment transect could be driven by decreasing soil input with greater distance from shore, or by changes in aquatic communities with depth (likely both?). Nonetheless, the data from global compilations of lacustrine brGDGTs show that the thresholds from the marine environment from (Xiao et al., 2016) might not be applicable to lakes. Therefore, we have de-emphasized the interpretation of IIIa/IIa as a soil indicator by removing the thresholds in Fig. 4, and changing the labeling of the index in Fig. 7.

General comments:

Replace all apostrophes with the prime symbol throughout the manuscript.

Fixed

Fig. 4: connect datapoints with straight lines as to not infer trends.

Due to the fact that there are multiple data points at the same depth, connecting points with straight lines requires some arbitrary choices of data point ordering and can lead to false impressions of trends. We have modified the plot by using generalized additive models as a smoothing algorithm, replacing the previously used LOESS method. The GAM algorithim is less flexible and thus reduces the curviness of the lines. We have also reduced the line thickness to de-emphasize the lines relative to the data points.

Check the order of the figures to match with their occurrence in the text. For sure Fig. 3 is out of order.

Figure 3 is first mentioned in the first sentence of the results (line 215), thus it is in the correct order.