Review #2 of Dugerdil et al. for Biogeosciences Joseph B. Novak
Summary

I thank the authors' for their efforts to address my previous comments. After rereading the manuscript, I found a handful of issues that need to be addressed, the most critical of which is my comment regarding line 567. Otherwise, I congratulate the authors on presenting a thorough manuscript that advances our understanding of branched GDGT molecules in complex arid environments.
Warm regards,
Joseph B. Novak

Major Comments

Figure 1 and the soils dataset: some sort of justification should be given for the inclusion of the moss polsters in the soils dataset. Strictly speaking, a moss polster is not a type of soil, and I foresee this as a sample type that many who primarily work on the application of the brGDGT proxy to geologic deposits will not be familiar with. It is probably also best to mark these samples with a different marker face color so that they can be easily picked out on the map. Also, the color scheme for this figure is not colorblind accessible, particularly panel D (red symbols with green background). The simplest solution to that would be to change the marker face color of the red symbols to something else.
I agree with you that this point has to be clarified. Actually, only few samples from the NMSDB (Mongolia and Siberia) are moss polster. We detailed this point with " *The NMSDB is composed by 27 moss polsters, 15 soil samples and two lacustrine core tops. As the GDGT distribution is similar for soil samples and moss polster (Dugerdil et al., 2021a), we included the moss polsters within the soil sample type in the present study*". For colorblind accessibility, we changed the red color into clear orange. After testing this new color chat with colorblind website, it appears to be better than the previous one.

L567: It looks like a new sentence (or perhaps paragraph) was started but not finished here.
I am very sorry for this forget. It was actually a work-in-process sentence to conclude the modification of we made to answer some of your conserns from the previous review step. The idea was to clearly settle which index type (isomer or cyclisation ratios) are the most suitable to track pH change from our dataset. We removed the sentence L567 and changed it by "*From the ACADB, isomer ratios outperforms cyclisation indices to track pH variations, as isomer ratios follow unimodal relationships with pH, while cyclization indices follow dual-slope relationships.*".

Minor Comments

L14: I think "Despite this" is a bit confusing since it seems reasonable to expect a weak relationship between brGDGT methylation and environmental temperature given the significance relationship between the various aspects of the brGDGT distributions and salinity, pH, etc.
That is true, we changed it into "Thus".

L24: I think "paleotemperatures" is probably a better term here
Yes, we agree that it makes the sentence more digest.

L31–33: It might be best to specific that you are talking about the global dataset here since you highlight in the abstract that there is a weak relationship between brGDGT methylation and temperature in ACA.

True, we gave more details about the way to obtain this result by changing the sentence into: *"Analyses of worldwide calibration datasets indicate that variations of the number of methyl groups on the brGDGT aliphatic chains are primarily controlled by ambient temperature, enabling their application as proxies for past temperature (Weijers et al., 2007; De Jonge et al., 2014).".*

L43–46: "These two important indices" follows a sentence where three indices are defined. Probably best to specific which two indices you mean, or to break the previous sentence into two sentences.
Thank you for this nice reading, we change the sentence with "*MBT'_{5Me} is now widely adopted to calibrate the reconstruction of MAAT in the past by linear relationships, while CBT_{5Me} and IR are used to infer past pH variations*".

L51: "have" rather than "has"
Corrected.

L63: the first "they" in this sentence should be replaced with a noun, as it reads it is a bit confusing whether you mean the brGDGTs or the environmental variables.
Changed for "*these controlling factors*".

L76: What is meant by "specific isomer distribution?" This is a bit confusing – do you mean that the distribution of 5-methyl vs. 6-methyl vs. 7-methyl isomers is more variable?
I guess "*a particular isomer distribution"* is a better term than "*specific isomer distribution"* as we want to introduce the fact that the 6- and 7- methyl abundances over the 5-methyl's one is more important in dryland than in other context.

L82: Some additional citations are appropriate here to further support the idea that bacteria community composition complicates application of the brGDGT paleothermometer. I suggest: (Ajallooeian et al., 2025) https://doi.org/10.1029/2025JG009132 (De Jonge et al., 2019) https://doi.org/10.1016/j.orggeochem.2019.07.006
Thank you for the references, we added them into the main text.

L84: Probably best to add "in some environmental contexts" since substantial variations in IR6Me have been seen in freshwater environments also (e.g., Novak et al., 2025).
This is also an important point to highlight that the relationships between salinity and the brGDGT distribution, particularly in terms of isomer ratio are not yet clear. We changed this paragraph with "*Salinity is thought to influence the relative number of 5-, 6- and 7-methyl isomers in some environmental contexts (Wang et al., 2021). Besides, substantial variation of the 5- over 6-methyl ratio (IR_{6Me}) are also observable in freshwater environments (e.g., Nowak et al., 2025). These variations of isomer abundances impact the MBT'_{5Me}- and MBT'_{6Me}-based temperature reconstructions (Kou et al., 2022; So et al., 2023).".*

Section 2.3: Would it be appropriate to add a citation to the seminal paper by Hopmans et al. (2016) since the method used here separates the 5-methyl and 6-methyl brGDGT isomers?
Yes that is important too. We added " *This method allows to distinguish 5-methyl and 6-methyl isomers for each compound (Hopmans et al., 2016).*" in the method section after the presentation of the HPLC.

L218: I think you mean Figure 1D
Yes ! Thank you.

L283: solonchak should be defined since this is an English language journal. It can be as simple as "solonchak (salt marsh) samples."

Done.

L593–594: By “actual observations,” do you mean measurements of lake surface salinity? Also, it would be best to specific that So et al. study referenced here is not from the ACA region (which is fine, the data are obviously relevant, but the distinction should be made).
The data are water column salinity measurements. We precised this information within the citation. For the citation of So et al. (2023), we changed it to “*(e.g., in the Great Salt Lake located in North American drylands, So et al., 2023).*”.

Figure 10B: by “over cold” and “over warm” do you mean cold and warm biased?
4.4 section title and subsection titles: I suggest changing “in the past” to “in the geologic record” since I think this better describes what you are discussing here
To make this point clearer, we add two information in the caption. (1) “ *The temperature biases are expressed with DeltaMAAT = $MAAT_{observed}$ - $MAAT_{predicted}$.*” and (2) “*Annotations Over cold and Over warm illustrate cold and warm biases, respectively.*”.

L737: check subscript here
Nice catch !

Editor comments

Public justification (visible to the public if the article is accepted and published):
Dear authors,

Thank you for the first round of revision. The reviewer is satisfied with the edits made, but have few more comments/edits requests, the most critical comment pertains to L 567 (see their comments attached).

While I sent this revision to reviewers for their evaluation, I also performed some review on the manuscript itself, and have the following comments/suggestions:
(note: the line numbers I mention below refers to you track changed document)

L 4: this study investigates
Done.

Somewhere around this line, it would be helpful if the authors elaborate on the rationale and research questions that need to be addressed. Highlighting what is really the difference and novelty that this study brings relative to the previously published paper.

L 18-19: I would revise it as "indirectly influence by multiple factors"
Done.

L 35: Similar relationship (instead of "same relationship")
Done.

L69: the interaction across potential contributing factors
Done.

Data availability: I appreciate the intent for open access, and it would be helpful to have the link available in the next revision as well. Also, to consider some of the comments about the type of samples from the reviewers, I encourage the authors to clearly indicate in their compiled dataset the type of samples (for their own and the other published).

Done. We add in the *Data availability* section the PANGAEA reference and link to access the data.

Table 1: Rephrase the second sentence as Samples with an asterisks (*) represent published data from Dugerdil et al. (2025)
The sentence has been rephrased as follow : "*Data from Dugerdil et al. (2025c) are highlighted by a * for a total sum of 162 surface samples*".

Please make sure that the supplementary files are revised accordingly.
The caption of the supplementary figure S1 has been changed in "*Geographical and biological presentation of the surface sites of the ACADB analysed in this study and previously published in Dugerdil et al. (2025c)*".

Figure S1: Are these climographs based on a single year or an average of a timespan? Please clarify (also if the data are instrumental datasets or reanalysis).
The data are from the worldclim2.1 database, then there are average values over about 30 years. We added in the figure caption "Climate data represent averages over the period ca. 1970-2000 (Fick et Hijmans, 2017).". We also added precision about the worldclim2.1 database with: *"The climate parameters were extracted from interpolated climate data from worldclim2.1 (Fick et Hijmans, 2017) at the sample location of the ACADB sites.".*

Table S1: please revise the caption to accurately reflect the status. These are not new samples, but already published, right?
Yes that is true. We modified the caption (see comment above).

For Table S2: please indicate the sample type as you did for Table S1.
Yes we add a column of the number of soil and lacustrine samples for each reference.

Notification to the authors:
Your "Short summary" text in the database (MS records) contains the abbreviation GDGT. Please provide at least one written-out version to make it better understandable for non-experts. Please remember that there is a character-limitation for the short summary text of max. 500 characters (including spaces).