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Interactive comment on "Climate reconstructions based on GDGTs and pollen surface datasets from Mongolia and Siberia: Calibrations and applicability to extremely dry and cold environments" *by* Lucas Dugerdil et al.

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[12pt]article

We thank the reviewers for their attentive reading and their accurate comments. We certainly appreciate the feedback they provided, and have strove to improve our manuscript according to their suggestions. We also provide a point-by-point account of our rebuttal, please see below. In addition to the changes suggested by the referees, we also modified the following items:

- 1. The annexes have been reorganized and the references in text to tables and figures have been updated and sorted.
- 2. Some recent references have been added to reinforce our assumptions.
- 3. The formula of the MAAT - brGDGT calibration used in Figure 7 was added in the Table Annex A3.

Responses to the comments of Reviewer 1 (Anonymous Referee)

General comments:

I have finished a review on the manuscript " *Climate reconstructions based on GDGTs and pollen surface datasets from Mongolia and Siberia: Calibrations and applicability to extremely dry and cold environments* ", submitted to biogeosciences discussions by Lucas Dugerdil and co-authors. The paper represents an interesting dataset, and great care is taken to develop models based on two different independent paleoclimate data: pollen and brGDGT lipids.

I have few comments to make that will hopefully make the manuscript even more relevant for readers interested in using pollen and GDGTs in cold areas. Investigating Mongolian brGDGT lipid and pollen distributions illustrates local dependencies that are different from global dependencies. To frame the impact of this observation, the authors need to be more transparent whether and why current global and regional calibrations fail in Mongolia.

Response: Local and global dependencies are actually not totally disconnected from each other. The Sibero-Mongolian system is indeed driven by the global climatic sys-

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tem, elevation gradients and its ecological responses. However, because of the extreme values of all these parameters, the local dependency has to be investigated in details. Basically, if the global calibration could be used in palaeoclimatic reconstructions for the Sibero-Mongolian lakes, the accuracy and the interpretation of the results has to be questioned (cf. Figure 7).

Applied changes: To emphasize the limits of local applications of global calibrations, we have added a sentence at the end of the Global vs. Local Calibration paragraph : " *Tangibly, for the two proxies, even if the global calibrations can operate on our study area, the local calibrations reach higher accuracy* " (L. 401). Moreover the following sentence has been added in conclusion. " *Even if global and regional calibrations could be applied in such a setting, local calibrations provide enhanced accuracy and specificity.* " have been introduce within the conclusion. (L. 485)

I would suggest the authors to move the section 5.4. " *Issues in Modelling Mongolian Extreme Bioclimate* " forward, as this section highlights why dry and cold areas represent challenging conditions for current proxy calibrations. It also allows to present some of the limitations of this study (existing error on climate parameters) before diving into technical model selection steps.

Response: We acknowledge this remark but we rather prefer to discuss the details and technical limits of our methodology first and then to extend these limits latter to the global context of Mongolian climatic system and to global palaeo-proxy calibrations. We therefore choose, not to modify the structure of the discussion. About the existing error on climate parameters, we add information (following a comment in the review of D. Naafs)

Applied changes: paragraph 5.4 " *According to the authors, the interpolation model* *used in the Central Arid Area (which include our study area) gives R^2 = 0.99 and RMSE = 1.3 C for MAAT and R^2 = 0.89 and RMSE = 23 mm.yr-1 for MAP. Whenever the Siberian-Mongolian calibrations are used for palaeoclimatic reconstructions, the RMSE of the climate parameter have to be added to the model RMSE.* " (L. 416)

For brGDGTs I would recommend to include a figure that shows the variation of MBT'5ME and MAT of soils on a global scale, with the Mongolian soils plotted in this dataset. If the Mongolian soils plot within the global variation of soils, the observed environmental dependencies might be extrapolated to other cold areas.

Response and applied changes: In order to test whether our dataset was included in the world trends for MBT and MBT'5ME–MAAT relationships, we plotted these graphs in an early stage of our investigations based on the global and regional chinese databases (Naaf et al., 2017 - Dearing Crampton-Flood et al., 2019 and Yang et al., 2014, respectively). On both panels A and B, our dataset plot within the global trend, but the vertical dispersion is larger than the horizontal one. This leads us to the conclusion that MBT and MBT-derived indexes are hardly applicable for local climatic calibrations. As suggested by the reviewer 1 we added this new figure in the Annex B2, here Fig. 1 in the interactive discussion.

The calibration between currently used brGDGT based proxy values and MAT is mentioned to be plotted in Supp Fig. 3, but this Fig. was not available online.

Response and applied changes: The reference was unfortunately mislabeled. The reference has been updated (Figure B3).

I am however skeptical of using a heterogeneous brGDGT dataset (including soils, lake

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sediment data, moss) for developing a new model to predict temperature. Generally, brGDGT from lake sediments are distinct from the surrounding soils, and their different environmental dependency has resulted in the development of lake-specific and soil-specific calibrations. However, in this dataset lake and soil samples are treated as having the same environmental dependency. The MAP is expected to have the same impact on lake-derived brGDGTs as on soils, which makes little sense as there is no link between MAP and aridity in lakes. If the authors have the opinion that the brGDGTs in the lake surface samples are soil-derived and represent an average catchment signal, the authors should elaborate on that.

Response : The second reviewer raised a similar concern. Actually the calibration is mainly based upon moss and soil samples (or 98%), only 4 sediment samples were considered for pollen and 3 for br-GDGTs. Cross-values were used to check the response to all the models (the one from the literature as well as the models from this study). A sample has to be totally independent of the calibration to be unbiased, that is why MMNT5C12 was removed from the br-GDGT calibration database. The 2 others sediment samples are the lake shores, not top-cores. They are made of humid clay with desiccation cracks and embedded peat organic matter, therefore a different feature of lacustrine sediments.

Applied changes : To take this comment into account and clarify this concern, we have modified the figure 4 to highlight that the majority of the sediment samples are independent of the sample NMSDB (just displayed as a comparison). The caption of the Figure 7 has also been modified with " *cross-value on the 6 first samples of the independent core MMNT5C12, Arkhangai* " **.** We furthermore added in the 3.4 Statistical Analyses paragraph the following sentence: " *A cross-value test was performed for all the models, the previous studies as well as the models proposed in this study, using an independent set of the six first sediment core samples from* *the lake MMNT5C12, Arkhangai* " (L. 218) In the result part, we also observe no influence of the lake system in iso-GDGT relative abundances: part 4.2.1 (L. 281): " *Iso-GDGT pattern in lake sediments do not really diverge from surface samples which leads to postulate that the in-situ production of iso-GDGTs in shallow lakes like MMNT5C12 is reduced (Fig. 4.A).* " Finally the discussion part has been extended (L. 455) : " *Moreover, in the desert context of poor availability in archive sites, the edge clay samples or top-cores of shallow lakes could be a solution. The two sediment samples of NMSDB are in the soil-moss trend for all models (Fig. 5, Annex Fig. B2 and B3). Even if the br-GDGT production and concentration differ in soils and in lakes due to in-situ production (Tierney and Russell, 2009 ; Buckles et al., 2014), this effect is function of the lake depth (Colcord et al., 2015), consequently seems to be negligible for shallow lakes, and almost absent in lake edge samples (Coffinet, 2015).* "

Also, the moss samples are generally not used for brGDGT calibration. Do we expect the brGDGTs to be produced by bacteria within the moss, or to represent an average of soil-derived particles delivered by wind?

Response : Moss samples are indeed not usually used in br-GDGT calibrations, but our methodology was to develop a common protocol for pollen and br-GDGT proxies. Because in many cases the moss polsters are better for pollen surface samples than soils (less taphonomic biases for example), this type of samples is preferred. On that purpose, we wished to assess if the br-GDGT pattern and abundances would be impacted by sample type (as it is shown in Figure 4). Our conclusion is that whenever the choice is possible in the field, the best option is to take parallel samples of soils and moss pollsters for br-GDGTs and pollen, respectively. In fact, on the Annex Figure A2, we observe that the moss samples plot within the peat data set (Naafs, 2017), while the soil samples are mostly associated with Chinese soil trend confirming the reviewer comment.

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Applied changes : (L. 183) " *In order to compare with the methodologies developed for pollen, moss polsters, soil samples and two lake shore sediment samples have been treated for GDGT analysis.* " has been added in the GDGT methods. In Discussion 5.4 : (L. 452) " *The explanation of the signal difference between the two types of samples could also originate from the in-situ production of br-GDGTs inside the moss in front of the wind-derived particles brought to the moss net. On the annexe figure B2.A it seems that the pool of moss polster is associated with with a similar trend that the worldwide peat samples from Naafs (2017).* "

The authors mention the effect of aridity on brGDGTs (Dang et al., 2016), but don't include in the discussion whether they see the same brGDGT response in the Mongolian datasets. The authors have not attempted to make a model to predict MAT, that uses the MAP values as a confounding factor. Why not?

Response and applied changes: We agree with the referee's comment. Because the Mongolian plateau is a dry area, all the studies demonstrating that br-GDGT abundances suffer from moisture influence need to be taken into account. Based on the Dang et al., (2016) study, we justify the use br-GDGT abundances as proxy for MAP. Their results on moisture impact on brGDGT responses are discussed along two lines:

- Part 5.4 : the MAP–brGDGT correlation remains strong (Annex Table 2, Bottom Part): so mathematically we observe a positive response of brGDGTs to MAP.
- Secondly, if we cannot discuss the eco-physiological controls of MAP (or even better soil water content) similarly to what is done by Dang et al., (2016), because we lack these observations, we have proposed two models derived from brGDGTs : one for MAAT and the other one for MAP, making the hypothesis

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that the two variables are independent. It is actually not true but the specific responses of brGDGT to MAAT or MAP still remain unknown.

The authors state that MAP and MAT correlate in this dataset (this is not plotted anywhere?), but the impact on the models is not discussed. Along the same line: can a partial RDA be used to illustrate the environmental dependencies (MAT or MAP) of brGDGTs in the absence of variation in the second driving factor?

Response : MAP and MAAT are indeed not correlated in the same way in all the NMSDB samples as illustrated below (added in Annex Figure B1, on interactive comments fig. 2). In the present form of the manuscript, we discuss the difficulties in modeling the Mongolian dry plateau, but on the total range of sample sites, the R^2 is only 0.35. On the other hand, on the subset of the Mongolian sites, R^2 value reaches 0.91, which shows a strong correlation between the two variables. This tendency remains similar along the latitude gradient: decreasing precipitations is linked to rising temperatures. If the auto-correlation between MAAT and MAP represents a risk for the models reliability, because or data set employed both Siberian and Mongolian sites (R^2 = 0.35) the models stay reliable. Concerning the RDA " *br-GDGT variations linked to environmental factors* " shows a RDA1 component mainly explained by a gradient between cold-wet Siberian forests against hot-dry Gobi Desert. The second parameter (RDA2) is the altitude gradient introducing variability on the first main axis. On a biological point of view, the altitude could maybe induce a modification in bacterial communities, in their response and sensibility to climatic variations, or even a shift in the bacterial-vegetation relationship. The vegetation is indeed forced by the elevation gradient (not only because of temperature and precipitation changes but also because of the exposure, slop and soil creeping, wind intensity and 02 concentration).

Applied changes : Figure A3 has been added in Annex and the paragraph 5.4 has been modified with the addition of (L. 412): " *Issues in Modelling Mongolian Extreme Bioclimate* " and at the end of the paragraph (L. 421) " *Because the MR–GDGT models have been compiled with the group of Siberian sites which are out of the MAAT–MAP strong auto-correlation (annexe fig. B1) the reliability of the independence of the MAAT and MAP models is guaranteed.* " On the reliance of the elevation underlined by the RDA, the discussion (part 5.4) was modified (L. 424): " *Elevation as main br-GDGT drivers could also be explained by the bacterial community responses to pH, moisture and soil compound variations along the altitude gradient (Laldinthar and Dkhar, 2015; Shen et al., 2013; Wang et al., 2015) and the vegetation shifts (Lin et al., 2015; Davtian et al., 2016; Liang et al., 2019).* "

The authors present several models for brGDGT calibration. I agree that caution should be used when using multiple variables (as discussed in section 5.1 and 5.2). Would an adjusted R^2 value be useful in this case?

Response : We fully agree with the reviewer's comment, the adjusted R^2 is indeed particularly adapted for multiple linear regression models because it allows to check the level of accuracy of the models in one hand and to reduce the impact of the over-paramerization on the other hand. For this study, we however choose to use a combination of R^2 + AIC because (1) it leads to the same statistical control (accuracy + limitation of over-parametrization) and (2) it allows to simultaneously evaluate the effect of the two phenomena for each mode. Moreover, because the vast majority of the paleoclimate proxy calibration studies used the R^2, it appear more convenient to use the same statistical parameters for immediate comparison. When specifically tested, the use of the adjusted-R^2 provided similar results than R^2 + AIC. For instance, on a set of models, the best adjusted- R^2 model is also the model with the smallest AIC.

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I would consider moving the models that are not selected to the supplementary material, so it is clear which model has the best predictive value according to the requirements that the authors have set.

Response: We understand the reviewer's concern to improve the readiness of the manuscript. However, One of our main issues is to help model-makers to suggest a methodological path for best model selections. We thing it might be meaningful to display some of the imperfect models to discuss the way of assessing their usefulness through their formulas and statistic values. Moreover, we wish to emphasize that the model selection is context-dependent.

Applied changes : To simplify the take-home message, the best model for Mongolia (in blue in Table 2) and the best model for the " *cold and dry similar environment* " (in red in Table 2) will be spotlighted by colors in Table 2. We reformulated Paragraph 5.1 (L.345) " *To sum-up, the most universal models are MAAT mr5 and MAP mr7 (Table 2, red coloured models) but the closed models are also valuable in some local contexts, and especially in similar dry-cold regions .* "

Please also see my comment below on selecting a model that makes " biological sense ". Comments on content: The introduction introduces several concepts and biomarker lipid groups that are not included in the discussion. Please remove this information (e.g. the impact of human populations on pollen-based climate reconstructions, or the existence of H-shaped or OH-GDGTs). Removing this will open up space to explain those concepts that are used better, and make the relevant part of the introduction more specific.

Response and applied changes: We modified accordingly the text:

- even if the H-GDGT and the OH-GDGT were analyzed in the NMSDB, references to these compounds were removed from the introduction.
- similarly, BIT and IIIa/IIa indexes were removed as well as referring references.

Specific comments:

I also have several minor and " editorial " comments to make. I indicated 'vague' phrasing where it was difficult for the reader to follow the interpretation: L 13. Rephrase " *derived to the low range of climate parameters encompassed in the study area* ".

Response and applied changes: We have modified point (3) of the introduction by " *Even if local calibrations suffer from reduced climatic parameter amplitudes due to local homogeneity, they better reconstruct climatic parameters than the global ones by reducing the limits for saturation impact* " (L.11)

L 19. Rephrase " *input proxies* " and be more specific. Use fi estimates of climate parameters values. Why does the calibration of the climate proxies matter for the understanding of the interaction between climate model outputs and input? It's a bit arm-wavy, be specific.

Response and applied changes: The precision " *such as pollen or biomarker abundances* " to explain the " *input proxies* " was added (L.18). The more accurate and specific the calibration, the more we can understand on climate reconstructions by models.

L 20. Current climate changes.

Response and applied changes: Modified accordingly.

L 22. What is meant with " *degradation* " ?

Response and applied changes: According to Tian et al., (2014) this means the " *diversity and biomass production* " degradation. The sentence was added to the manuscript.

L25. It is not clear why Mongolia is a hinge area. A hinge area would be influenced by two different climatic drivers, but here the authors state only which climatic drivers do not influence Mongolia. It contradicts L 23-24?

Response and applied changes: In an attempt to better describe the specificity of the climate controls on Mongolia, we have modified the sentence by removing " *partially block the Westerlies...* " and concluded by adding " *The Mongolian system is thus driven by a mix of the distant drags of these two climatic cells.*"

L 29. Perhaps use " *environmental systems* " rather then " *climate systems* " ?

Response and applied changes: Modified accordingly: we have changed the sentence by " *climate and environmental systems* "

L 31. Please add references here that are an example of the use of pollen and geochemical proxies in lake sediments, or let the sentence reflect the references better.

Response and applied changes: To highlight the combined applications of these proxies we have modified the sentence by adding " *and more in a comparative attempt on lake sediment environmental interpretations* " and add the references to Atahan et al., 2015; Watson et al., 2018 and Martin et al., 2019.

L 34. Add " *in the absence of human influences*".

Response and applied changes: Modified accordingly.

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