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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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In this manuscript Dugerdil and co-authors determine pollen and GDGT distributions in a set of surface samples (mineral soil, moss, and lake sediment) from Mongolia and Siberia to establish novel (local) climatic calibrations (precipitation and temperature).

I am not an expert in pollen and therefore focus my review on the GDGT-part of the manuscript. I hope other reviewers can comment on the pollen methods and results. I congratulate the authors with writing a manuscript that reads well and with references

that are up to date. The figures are also well made. I especially like figure 7 that manages to clearly display a lot of information (but check if it is suitable for colour-blind people?). I think this is an elegant dataset from a poorly studied region that is valuable to the paleoclimate, pollen, and GDGT community. However, I have a number of comments that need to be addressed before publication.

- Firstly, the foundation for the calibrations: the instrumental data. Given the few weather stations in the region (line 169), what is the error on the instrumental data (temperature and precipitation) for the calibrations? For example, for temperature is it 1 or 5 oC or more? How confident are we that we know the temperature that the samples experienced? This error should be taken into account into the calibrations and discussed properly. Also, the discussion focusses on mean annual temperature, but did you explore warm season temperatures? Especially for the cold regions this might improve the calibration. If not, this is also interesting and should be discussed.

-I find it odd that lake sediment samples are combined with mineral soil and moss samples. Does that mean the new calibrations can be applied to both archives? We know that the brGDGT distribution in lakes differs with that in soils (compare mineral soil brGDGT data versus temperature with that of lakes). I suggest the authors split the lake and soil data into different calibrations. Does that improve the correlation for for example MBT5me'? Also compare the lake samples with the recent lake calibrations (Russell et al., 2018). In addition, for lake sediments I expect no correlation between brGDGT distribution and local precipitation (as they are mainly formed in the water column), was this taken into account to obtain the MAP calibrations?

-But then there is the more fundamental problem: Using R2 values and other statistical methods to select the best calibration. I appreciate that the authors take over-parametrization into account (section 5.1) but I think a major problem with their approach is that we end up with complex calibrations that include compounds that are not very abundant (e.g. brGDGT-IIIa' and -IIIc). Minor changes in the abundance of these minor compounds (or even slightly different ways of integrating the minor peaks) can have a major impact on the resulting temperature. I think it would be valuable to only consider compounds that have a certain relative abundance, like > 5-10 %. How would this impact the selected calibrations? In addition, this way we lose any physical basis for the proxy. The original MBT proxy reflects a decrease in degree of methylation with increasing temperature and this has a physical meaning for membrane properties and hence provides confidence that this is a true signal and not a random empirical observation. But what is the impact on a bacterial membrane if it is has a few percent more of brGDGT-IIIb'? What is the physical basis of these new calibrations and hence are we confident that these are real temperature relationships? This aspect needs a thorough consideration and discussion.

-Lastly, what is missing from this manuscript is an application of these novel calibrations. Do they provide sensible climatic signals when applied to a downcore record? If the authors do not have a downcore record, is there a published record that this calibration can be applied to?

David Naafs

Minor comments:

Line 6: delete "extremely cold dry"

Line 7: is the livestock grazing statement relevant? It does not appear to come back in the discussion, delete?

Line 16: add "environmental variations in Mongolia and Siberia"

Line 52 and 55: brGDGTs

Line 55: also cite other recent brGDGT calibrations papers, for example (Wang et al., 2019)

Line 62-63: I don't think we show this in our 2018 paper. Delete sentence or change reference

Introduction needs some rewriting to create a more natural flow between the different paragraphs. At the moment the introduction jumps from pollen to brGDGTs and back without a clear connection between the different paragraphs. A good example is the sentence in Line 71 that sort of floats by itself with no connection to the previous sentences or the next paragraph. The end of the introduction with a clear outline of the approach is good. Also, I think somewhere in the introduction the authors need to introduce the 5,6,7 methyl brGDGTs and what they mean (For example citing De Jonge et al., 2013; Ding et al., 2016). Because at the moment their use (e.g. line 276) comes a bit out of nowhere (need also somewhere in the methods explain the use of ' and '' for 6, and 7-methyl brGDGTs).

Line 265-270: this section needs a bit of re-writing (and maybe thinking). We know isoGDGTs are also produced in soils and peats, not just in lakes and not only in the water column. So that is not the reason that you see no clear correlation with climate parameters. See the discussion in (the supplementary information of) (Naafs et al., 2018) on the distribution of isoGDGTs in peat and the lack of correlation with temperature/pH. It is interesting that in these samples from dry environments crenarchaeol is abundant because the abundance of crenarchaeol in soils/peat has been inferred to indicate dry conditions (see for example Zheng et al., 2015), I suggest the authors expand on this.

Line 274: I don't understand this sentence

Figure 4: change legend to state "lake sediments, n=65"

Line 279: what kind of surface samples? Soil surface?

Line 297-298: On what basis were these 9 selected? Expand

Section 5.3: I think the assumption for this section is flawed. Of course, a local calibration based on a certain set of surface samples will have a better correlation with the MAAT then a global calibration applied to these surface samples. You chose your local calibration using these same samples.

Line 379: also see discussion in (De Jonge et al., 2014; Naafs et al., 2017) on brGDGTs in dry soils

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