Review of Raberg et al.,

General comments

The manuscript of Raberg and colleagues reports the investigation of the impact of different environmental parameters on the distribution of branched GDGTs (brGDGTs) in a globally distributed set of lake sediment samples. Following this analysis, the authors propose new calibrations for the use of brGDGTs as paleo-proxies. Branched GDGTs are lipid biomarkers that are ubiquitous in continental settings and are increasingly used in paleo-studies to reconstruct past air temperatures (and sometimes pH) from lacustrine archives. However, the organisms producing brGDGTs are still unknown so the relationships between their distribution in a sample and environmental parameters (temperature, pH …) remain empirical. In this context, Raberg and colleagues provides a very comprehensive analysis of the relationships between the lipid distribution and a wide range of environmental parameters. Furthermore, they extend the latitudinal coverage of the worldwide sample set typically used to establish brGDGT calibrations to high latitude lakes. This study is thus of great interest for the community. The manuscript is clear and well written although it would benefit from reducing some parts (see below). I thus consider the manuscript to be suited for publication in *Biogeosciences* after minor revisions. The authors will find below a list of specific scientific comments and a list of technical ones.

Specific comments

-The approach of dividing the compounds into subsets to isolate each structural variation is interesting and valuable as it enabled the authors to reveal some physiological links between the lipid structures and the environmental parameters. I would suggest the authors to more specifically explain the rationale for their groupings in the introduction (l. 111-112). It would also be interesting to further discuss the relationships revealed by their approach in light with the literature on other biomarkers, such as isoprenoid GDGTs or alkenones. Are the observed lipid structure adaptations coherent with the homeoviscous membrane adaptation theory?

-Moreover, I strongly recommend the authors to better emphasize why the calibrations they set up are better than the previously established ones, especially the one, still under review but available as preprint, proposed by Martinez-Sosa and colleagues. I am, up to now, not convinced that the authors temperature calibration would perform better than others. Eventually, the paleo-community needs to know which calibration is the best suited for their archive(s). The authors should thus clearly and specifically state in their introduction and in the discussion part, the benefits for the paleo-community of using the calibrations they defined in the present manuscript over the others previously published calibrations.

-The proposed calibration with the conductivity of the lake water column is novel and of high potential for paleo-studies. The authors note that, in their dataset, pH and conductivity covary but they never suggest an explanation for this covariation and treat them separately all along the manuscript. In fact, it is not surprising to observe links between pH and conductivity in a lake water column. This aspect should be further discussed in the manuscript.

-In line with the previous comments, parts three and four seemed often redundant and sometimes too descriptive. The manuscript will benefit from a reorganization/condensation of these two parts. This reorganization should put forward comparisons of the study results with previous literature.

-In the introduction, the authors should briefly describe the four temperature indices they used and the differences between them (l. 114).

-Also, the authors should mention in the introduction the previous studies that evidenced the multiple sources of brGDGTs in lake sediments: from the lake catchment but also from *in situ* production in the water column or in the sediment. This aspect will have implications to define the environmental parameters the producers are effectively experiencing and could warrant further discussion in their discussion part notably in l. 592 (can the depth habitat of the producer have a role?) and in l. 601-614 (could the export mechanism also play a role here?).

-In the material and methods part, I wonder if the paragraph 2.3 really belongs there. Maybe the authors could put it in the supplementary material instead. Also, some details on their statistical procedure are missing. It would be important to know if their variables were all normally distributed or if the authors transformed and centered them before defining the linear models, for example.

Technical comments

l. 54: move "indices" before "methylation"

l. 63-64: "e.g." should be added before the reference cited

l. 67: "of the dependencies of brGDGTs on…"

l. 114: no S at "temperature"

l. 377: provide reference for the existing correlation.

Figure 7: replace r^2 by R^2 in the caption. In the legend *p-value* should be italicized and with a small p.

l. 428: R^2 is relevant only to evaluate the quality of a regression model. To discuss the correlation between two variables it is more appropriate to mention the correlation coefficient (r) and its *p-value*.

l. 459: explain what DC stands for

l. 593: same remark for HP5