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Interactive comment on "Bacterial GDGTs in Holocene sediments and catchment soils of a high-alpine lake: application of the MBT/CBT-paleothermometer" *by* H. Niemann et al.

Anonymous Referee #3

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Review of "Bacterial GDGTs in Holocene sediments and catchment soils of a high-alpine lake: application of the MBT/CBT-paleothermometer" by H. Niemann et al. Climate of the Past (2011).

The authors of this paper analyzed branched GDGTs in Holocene sediments from Lake Cadagno (southern Switzerland) as well as in catchment soils. The distribution of branched GDGTs was similar in surface sediments and most soils. According to the authors, this result suggests that the distribution of branched GDGTs in the lake was not significantly affected by in situ production and early diagenesis, thus allowing a successful application of the MBT/CBT proxies to the reconstruction of past temperatures

and pH. The MBT/CBT-based temperature record seems to indicate a 2 ka cyclicity of the climate in Europe, consistent with previous proxy reconstructions.

Branched GDGTs are increasingly used as proxies for paleoclimate reconstruction. However, to date, only a few studies were interested in the application of the MBT/CBT indices as proxies in lacustrine sediments, which may be complicated by the in situ production of branched GDGTs. This paper clearly deals with a subject of topical interest and shows a successful application of the MBT/CBT proxies in lakes. The manuscript is well-written and well-organized and is therefore, in my opinion, a valuable contribution to the literature. Nevertheless, I have several comments and suggestions which should be addressed before publication. First, I think that the authors are not critical enough about their data. They should be more moderate when discussing the fact that branched GDGTs are not produced in situ and are not significantly affected by early diagenesis. This is indeed suggested by the similar distributions in catchment soils and surficial sediments, but further evidence is needed to confirm this hypothesis. Second, the calibration error of the MBT/CBT proxies is quite large (5 °C), but the uncertainty in temperature and pH reconstruction is not discussed at all in the manuscript. This should be corrected in the revised manuscript. Last, I am not totally convinced by the paleoclimate discussion in the present version of the paper. The authors argue that there are coherent temperature oscillations with an apparent cyclicty of 2 ka, but I think the data presented in the paper do not fully support this conclusion.

More detailed comments are given below.

Introduction

Page 3452, lines 24-26. The authors should specify that the BIT can be biased by the in situ production of branched GDGTs. Indeed, when the BIT was defined, it was assumed that branched GDGTs were of soil origin only, but it now appears that that is not necessarily the case, especially in lakes.

Page 3453, line 21. I would insist on the fact that in situ production can strongly bias

the distribution of branched GDGTs in lakes (e.g. Tierney et al., 2010) and that further studies are needed to understand the mechanisms controlling the branched GDGT distribution in these environments.

Section 3.2. GDGT distribution in sediments and soils.

1) The authors only present the average values of the MBT and CBT in the soils and lake sediments. Nevertheless, I think that the MBT and CBT values of all soil and sediment samples should be provided in a table. The BIT values of all soil and sediment samples should also be included in this table. In the manuscript, the authors should then compare: - the MBT, CBT and BIT values of the regular/irregular soils and surficial sediments - the MBT, CBT and BIT values along the sediment core

2) In my opinion, the fact that "the composition of GDGTs in most soil samples was almost identical to lake surface sediments" does not necessarily imply "a common origin of GDGTs". It cannot be excluded that branched GDGTs coincidentally have the same distribution in lake and soils.

3) I disagree that "CBT-based pH estimates from soils are slightly overestimated", since CBT-derived pH are in average 0.85 units higher than measured pH values. I would rather say that the pH estimates are overestimated. The authors should specify if the pH estimates presented in Fig. 5 are only the values for regular soils or also include the values for irregular soils.

4) The authors state that "the matrix of the lake sediment sample was very complex, probably as result of the high content of organic carbon". What do they mean by "high content of organic carbon"? The organic carbon content of all soil and sediment samples should be provided in the revised manuscript. In addition, the authors specify that the chromatographic resolution was insufficient for 15% of the sediment samples. Is the TOC content of these samples much higher than the remaining ones (the other 85%)? Is there a real difference in chromatographic resolution between the different samples?

5) The authors only discuss the distribution of the branched GDGTs, but I think they should also show and discuss the GDGT concentrations in soils and lake sediments. The concentrations should be normalized to the TOC content of the samples. How do the branched GDGT concentrations in lake sediments evolve with depth?

Section 4.1. Origin of GDGTs in lake Cadagno sediments.

Page 3461, lines 14-17. Once again, I disagree with the authors that "the identical GDGT patterns observed in soils and surface sediments rules out alteration of the primary GDGT- signal by in situ production". According me, this conclusion is too fast and is a simplified view. The observation of identical GDGT patterns in soils and sediments is not sufficient to totally exclude in situ production. This should be acknowledged in the revised manuscript.

Page 3462, lines 17-19. The authors should be more moderate. I would rather say that the specific environmental conditions of lake Cadagno might limit the diagenetic alteration of the in situ production of branched GDGTs.

Page 3463, lines 3-5. High BIT values do not necessarily indicate a dominant soil-origin of sedimentary organic matter. Indeed, it just shows that the abundance of branched GDGTs is much higher than the abundance of crenarchaeol. A part of branched GDGTs might originate from soils, but another part might be produced in situ in sediments. This should be specified in the revised paper.

Page 3463, lines 13-15. It is unclear why the distribution of branched GDGTs differs between irregular and regular soils. It is also unclear if these irregular soils are abundant or not. Therefore, this is speculation to state that "the irregular soils are not common in the Lake Cadagno catchment". Once again, I would be more moderate and would say something like "the irregular soils may not be common in the Lake Cadagno catchment".

Page 3463, lines 19-22. There is not enough evidence to conclude that "Lake Cadagno

sediments represent an excellent archive" for the reconstruction of past temperature and pH based on branched GDGTs. This remains speculation at the moment. The authors should use less strong wording.

Section 4.2. MBT/CBT-based MAAT estimates and comparison to instrumental data and independent proxy records

Page 3463, lines 26-29. This sentence should be modified since, once again, there is no clear evidence that GDGT signature in lacustrine sediments is not altered by in situ GDGT production or early diagenesis.

Page 3464, lines 4-7. I agree that the MAAT record derived from the MBT/CBT show subtle variations. Nevertheless, the authors should discuss that fact that the MBT/CBT are relatively new proxies (with only some applications in lakes) and that the standard deviation on MBT/CBT-reconstructed temperature is quite large (+/- 5°C). The latter point is not discussed at all in the present version of the manuscript, but it should be addressed in the revised paper, due to the small temperature variations recorded by the branched GDGTs in Lake Cadagno. The authors should be more critical of their data. Similarly, the authors should clearly specify that the standard deviation on CBT-reconstructed pH is 0.8 pH units and discuss the uncertainty in reconstructed pH in section 4.3.

Page 3465, lines 4-5. Even though the MBT/CBT-derived MAAT estimate from the youngest sediment seems to reflect the temperature increase during the last century, the authors should discuss the fact that the calibration error on MBT/CBT-reconstructed temperature is much larger that this small increase (0.5°C). This is important for the reader.

Page 3466, lines 16-20. I am not convinced that "the MBT/CBT-record shows a good agreement with recently published T-records". There are several discrepancies between the MBT/CBT-record and the other temperature records. For example, the MBT/CBT-record indicates a relatively cold period at ca. 4 kyr and 10 kyr BP, whereas

the foraminifera-based record shows the opposite (Fig. 4c). In addition, there is no agreement between the different temperature records for the early Holocene period (before 8 kyr BP). Why? This should be discussed in much more detail in the revised manuscript. Another question concerns the Holocene thermal optimum, which is not reflected in the different temperature records. This is quite surprising and should be explained. Last, I think there are too many plots in Fig. 4b. The plot displaying the average temperature values might be sufficient.

Page 3466, lines 24-26. Based on the comments above, I disagree with the statement that "the GDGT-based paleo record seems to be consistent with other independent proxy records". In any case, the different paleo records are consistent between each other or are not consistent.

Page 3467, lines 4-6. The MBT/CBT-derived temperatures are not available for the time period between 5020 and 5813 yr BP, but the authors speculate that the temperature increased during this period. Nevertheless, this increase is difficult to see in the chironomid and foraminifera records and is, in any case, very limited. Therefore, I am not sure that the authors' speculation is correct.

Section 4.3. Paleo soil-pH

Page 3468, lines 6-7. As said above, the CBT-based pH-estimates are clearly higher than measured pH (0.85 pH unit). The authors give the pH estimates from surface sediments, but have they measured the pH of these samples? How do the pH estimates compare with measured values? The pH estimates and measured pH for all soil and sediment samples should be provided in a table.

Conclusions

Page 3470, lines 4-7. As previously discussed, the authors have not directly shown that "the primary distribution of soil-derived branched GDGTs remain unaffected by early diagenesis and/or dilution by in situ production". They could rather say that the

present results suggest that branched GDGTs in lake sediments are mainly derived from surrounding soils.

Pages 3470-3471. Several points addressed in the conclusion section should be discussed in much more detail in the text. 1)the environmental factors controlling the production/degradation of branched GDGTs in lacustrine sediments.This is an important question. Lakes are specific environments, and different bacterial communities might produce branched GDGTs in soil and lakes. Moreover, the environmental parameters controlling the lipid distribution of branched GDGT-producing bacteria might differ in soils and lakes. 2)the uncertainty in the time-scale captured by the GDGT-proxies. This might complicate the reconstruction of past temperatures and pH and might notably explain the discrepancies between the different temperature records.

Minor comments

Page 3452, line 6. "does not only reflect" instead of "does not only reflects".

Page 3453, line 8. The authors give the formula of the MBT and CBT indices, but not the one of the BIT. The BIT should be clearly defined in the revised manuscript.

Page 3454, lines 9-12. I would remove the last sentence of the introduction, which already gives the main conclusion of the paper.

Page 3456, line 14. "0.45 μm" instead of "0.45 μg".

Page 3463, line 13. We were not able "to" find. . .

Page 3465, line 15. The authors should specify at the end of this sentence that the temperature records for the Spannagel Cave and Lake Silvaplana are shown in Figs. 4f and 4g.

Fig. 3. In the revised version of the manuscript, a colour version of Fig. 3 should be provided. This would allow an easier comparison of the relative abundance of the different branched GDGTs in lake sediments, irregular and regular soils.

References. The title of the journal is written two times in the paper by Pearson et al. (2011).

Interactive comment on Clim. Past Discuss., 7, 3449, 2011.