**Climate
of the Past
Discussions**

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 #1

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Niemann et al. analyzed branched GDGTs in a Holocene lake sediment core from Lake Cadagno, Switzerland, as well as in soil samples in the catchment. From the similarity of the GDGT composition, the authors infer that in situ production and early diagenesis can be ruled out. As the GDGT-temperature reconstruction over the last 2 kyr also matches other proxy-based reconstructions, the applicability of the GDGT method is considered to be confirmed, and the downcore variation during the Holocene is interpreted to document a 2 ka cyclicity of climate variability in Europe.

Overall, the manuscript is well written, structured and referenced. However, I think the manuscript could benefit from presenting and discussing more data (data table, 'bad

data', TOC, BIT, concentrations, GDGT data for flood layers), and I am left with the impression that the applicability of the new proxies is sold too uncritically. There are many remaining uncertainties and potential pitfalls with applying branched GDGTs in lake sediments, and I am not convinced that the similarity between catchment soils and surface sediments alone proofs that branched GDGTs are not produced in situ and that they are not affected by early diagenesis. I am also not convinced that the GDGT derived temperature reconstruction over the Holocene matches well with previous proxy reconstructions, and that concertedly these provide evidence for significant T-oscillations at ∼2 kyr frequency. What about the Holocene Thermal Optimum? A more critical and extensive discussion of the own record in the context of known Holocene climate variability and forcing would be nice in a contribution to Climate of the Past.

Nevertheless, after some revision of the manuscript, it can be a valuable contribution to the journal.

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The following questions/comments might help to revise the manuscript:

1. Sampling: The cores are sampled every 5 and 10 cm, but the authors don't comment on how they excluded the slumps. Did they set the sampling boundaries accordingly? Did they obtain GDGT results from the slumps as well? Wouldn't it be interesting to present and discuss those values? Similarly, what about the flood layers? Do these show distinctly different values? How do they compare to sediments without flood layers? This comparison could provide additional insights concerning in situ production.

2. Core chronology: The authors mention that they have dated material from the slump deposits. Why don't they present and discuss those data? They also describe the use of cubic splines to interpolate between 14C ages. I would therefore expect a relatively smooth age depth function for the curve 'without slumps' (fig. 2). However, there seem to be unexpected turns and steps in the age depth model. How sensitive are the results

to choosing a spline rather than linear interpolation?

3. Page 3460: The authors may want to provide some methodological uncertainties here. Given only the high analytical reproducibility, the GDGT derived MAAT and pH values do NOT seem to reasonably agree with the actual values. Particularly some of the pH values seem to be almost 2 pH units too high (fig. 5). Is that only 'slight' overestimation?

4. The authors mention that 15% of the lake sediment samples are not used for the paleoclimate reconstruction, because high organic carbon contents seem to have complicated the HPLC measurements. I think it would be important to show the TOC results as well. Maybe the authors might also want to show the 'bad' GDGT results (in grey) to give the reader an idea about the potential bias of insufficient chromatographic resolution.

5. I think it is worth showing the BIT data. In fact, lower values for the lowermost samples could be explained with young soils (little branched GDGTs) in the catchment. Variations later on should provide some indication about run-off. Do the BIT values change in the slumps or the flood layers?

6. The authors also chose not to show the soil profile data. They should consider providing at least a table with ALL data, so the interested reader can go into more detail. What about the GDGT concentrations? They should be shown and discussed as well.

7. In the discussion, the authors start with arguing that the similarity between the surface sediment sample(s??) and the soils indicate that in situ production and early diagenesis are negligible. I can follow this argument, and maybe the speculation that high sulfide concentrations make Lake Cadagno so special is indeed true, but all this is to some degree speculation and the strong wording here and there makes me feel uncomfortable (e.g. page 3461 line 16: 'rule out alternation'. Page 3463 line 21: 'evidence discussed above . . . excellent archive . . . we could show that branched GDGTs

of soil that branched GDGTs of soil origin are transferred by erosion to the sediment record where the primary GDGT signatures remain preserved, without substantial alteration by in situ GDGT production and/or early diagenesis'). It should be kept in mind that this is a novel proxy and there are still many uncertainties (e.g. no explanations for the 'irregular' soils or for the obvious pH offsets. Large 'calibration uncertainties' and unknown environmental factors controlling GDGT distributions). There is no proof for no in situ production; this can only tentatively be inferred from the similarity of the lake sediments with the 'regular' soil samples. The similarity could, however, also just be coincidence due to other (unknown) factors controlling GDGT distributions.

8. The agreement of the GDGT derived MAAT reconstruction with other proxies during the last 2 ka seems promising and provides further indication that the novel proxy can be used in lake sediments. Yet I am surprised that the authors claim to see 'good agreement' with recently published T-records for the whole Holocene. I have a hard time to see this good agreement (fig. 4), and I am left wondering how robust the conclusion is that the Holocene is characterized by ∼2 ka climate oscillations. If the authors choose to keep this conclusion, they will probably have to apply some statistics to the other records as well. I happen to know the ∼1.5 ka Bond cycles, but I am not aware of previous studies finding a 2 ka cyclicity. The authors would have to convince me with more detailed context and discussions (Wanner et al. 2011 in QSR). Maybe even more importantly: Why is the Holocene Thermal optimum not reflected in the presented records? Pollen and glacial records from the Alps and Europe seem to be consistent in that regards.

9. Paleo soil-pH: Although the authors advise caution with interpreting the reconstructed pH, I think the interpretation in terms of changing precipitation is too simplified. It should also take into account that soils developed during the Holocene and that both the soil and lake pH likely dropped in response to the production and leaching of organic acids.

10. I feel pretty uncomfortable with the strong wording in the conclusions: 'We could

show that . . . distribution of these compounds remain unaffected by early diagenesis and/or dilution by in situ produced GDGTs' and 'Lake Cadagno sediments thus provides a robust and quantitative measure . . .'. As mentioned above the similarity between sediment and soil samples could be pure coincidence. Moreover, probably one of the major potential limitations of branched GDGTs in lake sediments is mentioned only very briefly at the end: 'Soils provide time-integrated geochemical signals . . . '. I think this deserves further discussion and will also lead to a more critical attitude concerning the applicability of the method.

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Minor details:

- Page 3452 line 10: delete 'alternative'

- Line 15: delete 'reliable'. There are complications with every proxy, so the biomarkers shouldn't be sold too uncritically.

- 2.4. 'Environmental parameters' could be included in 2.1 'Site description'.

- 2.5 'independent proxies' is not really needed. This is part of the discussion anyway.

- Fig. 1 should also show the location of the research area in a larger map.

- Fig. 4f and g: typos 'Allps'

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