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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 #2

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This paper presents one of the first applications of the MBT-CBT paleothermometer to lake sediments. In contrast with previous application studies (Fawcett et al.), the authors present a fairly detailed evaluation of the sources of GDGTs to Cadagno and assess the accuracy of the modern-day temperature and pH relative to their reconstructions. Given the novelty of the technique and the relatively careful analysis of GDGT source, I think this is a valuable manuscript that should be published in CP.

I did find, however, several shortcomings. The authors could better describe site and potential transport pathways for GDGTs to the lake. I found the paleoclimate discussion very weak- this could be strengthened substantially to the paper’s benefit. Overall the

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manuscript is very well-written and well structured. Detailed comments follow.

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Site Description. A bit more information about the catchment would be valuable here, particularly information that relates to the production and transport of GDGTs. The author argue that the GDGTs in their core are derived almost entirely from soil runoff (unlike most lakes surveyed to date). What is the mean elevation of the catchment (rather than the lake) and the temperature there? Where do the authors suppose the majority of the GDGTs come from? As GDGTs are thought to arrive bound to particles, groundwater transport is unlikely, the source is presumably the rivers and streams.

Soil vs. lake contributions. The authors present fairly convincing evidence that the branched GDGTs in Lake Cadagno are largely, if not entirely soil-derived. The authors discuss MBT, CBT, and BIT at length in their results but present only figure 3, which is somewhat difficult to read for low-abundance ions. I would suggest figure 3 be plotted on a log scale. I would also demand one additional plot- in addition to figure 3, the authors should show the MBT, CBT, and BIT values of lake sediments, regular soils, and irregular soils. The authors do not describe BIT of the soils at all, but these must be higher than the lake samples in order for the branched gdgts in their lake to be soil-derived (unlike, for instance, Lake Challa where soil BIT exceeds lake BIT).

The authors discuss the reconstructed temperatures from the Zink et al. and Tierney et al. calibrations as “unreasonably high” on the top of page 3462. What is the average lake temperature of Cadagno? The authors have quoted air temperatures only. The Tierney et al. calibration assumes that lake and air temperature are equal, which is true in the tropics due to no temperature seasonality. However, mean annual lake temperature will be much higher than mean annual air temperature at these elevations in the Alps as the lake cannot drop below freezing in winter. Lake Cadagno has been heavily instrumented, so the data should exist to address this.

Reconstructed temperatures. The authors quote temperature errors of 0.1 C for their lake. However, this is only the measurement error. Given the low errors they quote

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(0.1 C), the modern temperatures and pH do not fit with the observed values. The offset is presumably due to calibration error. My impression is that the MBT/CBT paleothermometer has a relatively large calibration error that the authors do not address in this paper at all. The authors should discuss the calibration error in this paper, as the calibration error may limit the extent to which the authors can claim to detect past temperature changes.

Paleoclimate analysis. Figure 4. The authors highlight warm intervals using a yellow bar running through their data. One of these, ca. 5000 yr BP, runs through an interval in which the authors have no data. This bar should be deleted. In general, I found figure 4 to have too many records plotted against each other. The authors have a very strong case for coherent temperature variability during the last 2 kyr (right panels on figure 4). But the mean temperature trends depicted in Figure 4A-D for the Holocene do not suggest any clear relationship between the Cadagno temperature, chironomid inferred temps elsewhere in the alps (figure 4B), and the various north atlantic records (figure 4c and 4d). I see no reason to trust the chironomid temperatures 'more' than the MBT-CBT, but the long-term mean trends of these records are clearly different, an issue the authors gloss over. Moreover, while the authors argue that the warm intervals (i.e. the yellow bars) show coherent patterns and a 2-kyr beat, even the coherency at a 2-kyr period is not clear- cooling at Cardagno at 4 kyr BP, for instance, correlates with a peak in NE Atlantic foram-inferred SSTs (figure 4C). The authors argue that this is "phase shifting" related to age model uncertainty, but they don't present any further discussion to justify this claim. They in fact present very little information on their age model, let alone the age models of the other records. How much uncertainty is there in these models (model error), and does that error allow for the amplitude of 'phase shifting' required to bring these records into agreement.

There are always elements of this in wiggle-matching in all paleoclimate papers, but in this case I think the authors are glossing over too many discrepancies. One way to look at this- plot all of the running averages on top of each other in a single figure. What

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can be seen?

Finally, and related to this, the NAO argument to be a very interesting one. The NAO is primarily a winter-time atmospheric phenomenon that, in its positive mode, warms the winter temperatures of continental Europe. It is interesting that the branched GDGTs would respond to this. I would assume that the surface soil microbes that produce these compounds (surface soils being those that would be mainly eroded and transported) would be dormant in winter, as the surface soils are frozen. While I am somewhat doubtful of the wintertime signal in this sites, the reconstructed temperature do not show features commonly associated with summertime temperatures in Europe (e.g. the early to mid-Holocene thermal optimum). It would be valuable to hear the authors' explanations regarding the seasonality of this signal- how is the signal produced during the winter when soils are frozen? Are the signals mean annual temperatures?

Soil pH. The authors argue that pH shifts in the soils are tied primarily to changes in precipitation and soil leaching. Can the authors rule out the possibility that the changes in pH are related to changes in glacial extent and the availability of fresh mineral material? Or the development of vegetation and a soil organic carbon (and organic acid) pool through the Holocene? The authors mention the glaciers for only the younger dryas, but it appears the pH also rises when glaciers expanded during the LIA. I saw no clear, critical evaluation of the importance of one process relative to another. The authors conclude this section with the statement that soil pH may have a variable response to paleoprecipitation and must thus be used with caution. I doubt they would say this for the Congo Basin record of Weijers et al- it seems more likely to be complicated in small alpine catchments where precipitation may not strongly influence soil leaching as much as temperature and glaciology.

Page 3452 line 10. New paragraph starting “A promising...”

Page 3453, line 13. Delete “until now.” After this paper is published, there are still only a few attempts to apply these methods. Also, give citations of previous pubs- Fawcett

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et al., Zink et al., Tyler et al., etc.

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Page 3458, lines 20—. How were slump deposits recognized? Simply the presence of massive vs. laminated sediment? How were the slumps excluded from the sampling strategy? It would be very interesting to see MBT/CBT temperatures in the flood deposits, as these are presumably a purely terrestrial end-member.

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Page 3459/3460. Language is confusing- rephrase. Are the samples from sites 4 and 8 or 2 and 3 irregular? Suggest deleting language about 2 and 3 in this sentence.

Page 3460 line 19. Rephrase “high background levels during HPLC”. Probably add “analysis” after HPLC.

Figure 5 caption. Change ‘overrated’ to ‘overestimated.’

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