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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.

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Author's response to the comments posted by three anonymous reviewers for the manuscript "Bacterial GDGTs in Holocene sediments and catchment soils of a high-alpine lake: application of the MBT/CBT-paleothermometer"

We would hereby like to thank the three anonymous reviewers for their helpful comments and criticism as well as the Prof. Fuchs for his commitment as the editor for this manuscript.

The three reviewers commonly acknowledge the scientific significance- and the quality of the discussion paper but they share the opinion that temperature (T)- and pH-

reconstructions for the early to mid Holocene should be discussed in more detail. In the revised version of the manuscript, we will address this issue particularly with respect to the HTO and uncertainties of the age horizon captured by single samples in our record. We also propose to expand the discussion with respect to potential forcing mechanisms which may have led to the reconstructed T- and pH fluctuations. The reviewers conjointly expressed the need for a better documentation of the GDGT indices (MBT, CBT, BIT), which are used for the calculation of Mean Annual Air Temperature (MAAT) and for the assessment of the original lipid source (terrestrial, aquatic). Tables listing BIT, MBT and CBT indices (for sediments and soils) will be included in the revised manuscript. Finally, the reviewers argued that some of the statements are too strong. We will moderate these initial statements in the revised version of the manuscript. However, our record, to say the least, provides putative evidence (in agreement with independent proxy data) that seems to confirm the applicability of the MBT/CBT-paleothermometer, at least to Lake Cadagno sediments.

In the following we address the reviewer's concerns point-by-point. Reviewer's comments (RC) are followed by our response (author comments = AC).

Referee #1

RC

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.

AC

The good match of soil and sediment derived branched GDGT patterns indeed indicates that a substantial contribution of these compounds originating from in situ production in the lakes water column and/or sediments is unlikely. A significant effect of (early) diagenesis on the distribution of branched GDGTs in the sediment matrix appears for the same reasons unlikely. The only alternative explanation for our findings would be that in situ production in the lake (water column or sediments) produces a GDGT-signal similar to the soil-derived pattern, or that early diagenesis proceeds but deteriorates the relevant GDGTs in equal measure. Of course, we can not out-rule this scenario entirely as we did not assess in situ production (e.g. by measuring sinking OM flux in the lakes water column) or early diagenesis and we see the need to address this in future studies (our ongoing research seeks to address these aspects). However, we argue that this alternative is rather hypothetical and seems unlikely but we will mention it in the revised version of the paper, as we can not entirely out rule it. Thus we will moderate our initial statement accordingly.

RC

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)...

AC

We will add a table with MBT, CBT and BIT indices in the appendix. We will also provide information on the Corg content of the sediments. However, as our T- and pH-reconstruction show no recognizable dependency on Corg, and because the distribution of Corg is part of another publication (Wirth et al., in prep), we are hesitant to show an additional figure with Corg. Similarly, we refrain from showing GDGT data from slump deposits as these are out of the chronology of the sedimentological sequence (as already explained in detail in the manuscript). These data are hence of no value for

paleothermometry and pH reconstructions, which are the focus of this present study.

RC

...and I am left with the impression that the applicability of the new proxies is sold too uncritically.

AC

See our initial statement. Our aim was to be very transparent with our data set, comparing it with a wide range of independent continental proxy records from the Alps and Europe as well with N-hemispherical marine records. Nevertheless, because of the multitude of records, we focused on time series data, which either span the entire Holocene or the last 2 kyr. However, we acknowledge that further discussion, particularly with respect to the HTO is needed and we accordingly will revise the text (see our comment below).

RC

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.

AC

This is one of the few lakes analysed today that shows this match in distribution. In the majority of the lakes studied so far, the distribution of the branched GDGTs in lake sediments is different from that in soils from the lake watershed (eg. Sinninghe Damste et al., 2009; Tierney et al., 2010), as we have explained in the discussion paper. However, as we can not out rule in situ production completely, we will moderate our statements accordingly. Early diagenesis is not likely to change the distribution of the branched GDGTs (except that IPLs may be transformed into core GDGTs) since this will need to affect the carbon chains of the branched GDGTs, which is highly unlikely to happen under anoxic conditions on these time scales and the present pressure/temperature conditions.

RC

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 $\sim$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.

AC

We agree that our data match other T reconstructions best for the last 2-3 kyr. However, we think that this is related to the huge research effort that has been committed for reconstructing this time period. For the early to mid Holocene, far less climate reconstructions are available, particularly from continental settings. We have provided evidence that our temperature estimates from MBT/CBT paleothermometer matches modern, instrumental data and that it matches the "consensus" paleo-T for the last 2 kyr. From these findings we conclude that the paleothermometer works for the Lake Cadagno record at least as far back as 2 kyr BP. We furthermore provide evidence that limnological conditions (meromixis and euxinia), which lead to the superior sediment, and, most likely, GDGT perseveration did not change during the Holocene. Thus, we deduce (as clearly stated in the text), that MBT/CBT indices from sediments older than 2 kyr can also be used for T-reconstructions. We could also show that at least the rough timing of climate undulations match published reconstructions. However, we agree that an expanded discussion with regards to the differences between our and other T records is needed and we will thus address this in the revised version of the manuscript. In Europe, the HTO was a time period ($\sim$8 to 5 kyr BP) of relative high summertime warmth that was most pronounced in higher latitudes and in Easter Europe, while it was weaker in South and West Europe (Davis et al., 2003; Battarbee and Binney, 2008). Since Lake Cadagno is in central Western Europe and we reconstructed mean annual T (and not summertime T), a comparably weak HTO signal is to be expected in our data set. Furthermore, we have indications for an overall warming during the time period 5-8 kyr BP, which agrees with pollen based data (Davis et al., 2003). We will clarify this aspect in the revised version of the manuscript. We agree that our initial statement with respect to a 2 kyr cyclicity needs moderation, which will be done in the revised version of the manuscript. However, we are still convinced that the general timing of climate undulations agrees with the other proxy reconstructions discussed in the initial version of the manuscript.

RC

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

AC

Slumps were recognised visually (e.g. massive texture, deformation structures, mud clasts) which is a common methods applied by experienced sedimentologists as well as by analysing elemental (C/N ratios) and stable carbon isotope compositions. We will clarify this in the revised version of the manuscript but we refrain from showing sedimentological and bulk geochemistry data as these will be provided elsewhere (Wirth et al., in prep).

RC

Did they set the sampling boundaries accordingly?

AC

No, because sedimentological examinations were carried out after sampling for lipids (as described in the manuscript). However, for T and pH reconstructions we only considered samples containing background sediments and flood material; thus we discarded the samples from slump deposits which is the reason for many of the data gaps in our record. We will clarify this in the revised version of the manuscript.

RC

Did they obtain GDGT results from the slumps as well? Wouldn't it be interesting to present and discuss those values?

AC

See our comments above.

RC

Similarly, what about the flood layers? Do these show distinctly different values? How do they compare to sediments without flood layers?

AC

Floods are a regular feature in lake Cadagno because of its mountainous nature (snow melt and hard rain can lead to sediment transport from the nearby acclivity). Hence, all of the 10 cm horizons that we analysed for GDGTs contains flood layers (however, also the background sediment is mostly composed of allochthonous material). We could not find a dependency of the overall GDGT pattern to sedimentology. However, this information is not relevant for the manuscript and we therefore refrain from adding it the text.

RC

This comparison could provide additional insights concerning in situ production.

AC

Given the above statement (similarity between flood and background sediments), we

cannot see how this would provide additional insights into the origin of the GDGTs.

RC

2. Core chronology: The authors mention that they have dated material from the slump deposits. Why don't they present and discuss those data?

AC

The data are presented in Table 2 (indicated by an asterix). We also mentioned in the text that the dating of this material revealed that slump deposits are out of chronological sequence (because the slump material is older than the underlying sediment).

RC

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?

AC

The age-depth model is indeed not linear, indicating variations in the sedimentation rates. During time periods characterised by high flood activities we determined particularly high sediment accumulation. One of these high-accumulation periods occurred at the end of the Bronze Age ($\sim$ 4000 yrs BP), which is illustrated by the steepening of the age-depth function at about 3 m sediment depth. We will clarify this in the revised version of the manuscript. We have also improved our age model by dating another terrestrial macro fossil that we found in the lower section of the composite core and by tying the sedimentology of the core used for GDGT analysis more closely to the core used for the development of the age/depth model. Also, we applied a modified spline, so that the edges depicted in Fig. 2 of the original manuscript are smoothed out now. The overall pattern of the modified age model is very similar to the original model except that the age horizons covered by the samples from the lower section of the composite core are shorter. Yet, this will not change the conclusions/findings of the paper. One site effect of slight shifts related to the improved tying of the sedimentology is that some sediment horizons in the time window 9-8 and 7-6 kyr BP are now in the region where we detected mass movements. Consequently, we will exclude the GDGT contents from these samples from our paleo-climate reconstruction. The improved version of the age/depth model will be included in the revised manuscript. We also plan to provide an overview on the recognition of different sediment types. Nevertheless, as already mentioned, the sedimentation and flood history of Lake Cadagno is subject of another manuscript (Wirth et al., in prep), thus we would like to restrict this information/discussion to a minimum. âĂÍRC

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?

AC

The content of figure 5 accurately shows the quality of the data. The grey line describes a 1:1 relation of instrumentally measured and GDGT inferred pH values. Clearly, the inferred values are higher than the actual values, on average by 0.85 pH units with most extremes of 1.5 pH (rather than 2 pH) units, which is just outside the calibration error of the CBT proxy (ca. 1 pH unit). Most importantly, the GDGT inferred values are offset (as stated in the text) but this offset is systematic, which, for paleo-pH reconstructions implies that trends (pH drops or increases) are probably recorded correctly. However, we will erase the adjective "slight" in order to avoid confusion. As for MAAT, we cannot really follow the reviewers arguments. We have discussed reconstructed MAAT values in relation to instrumental data and well-constrained proxy records at length (Section 4.2.1). We agree that it is necessary to reconstruct MAAT and soil pH from more than one sample because individual measurements can be relatively far off from the

average value of the soils surrounding lake Cadagno. It is also advisable to focus on trends rather then single extremes (as we have done in the manuscript). Yet, this is true for any proxy reconstruction.

RC

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.

AC

Regarding TOC-data, see our comments from above. With respect to representing 'bad' data/results, we disagree with the referee: Data that cannot be trusted should not be included in a scientific manuscript.

RC

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.

AC

The BIT data will be added to the revised manuscript. Indeed OM in sediments of a lake situated in an environment with no/young soils (as one would expect for the time 10 kyr BP) may originate to a comparably high degree from in situ production, thus leading to comparably low BIT values. We will include this argument in the revised version of the manuscript.

RC Variations later on should provide some indication about run-off. Do the BIT values change in the slumps or the flood layers?

AC

As already mentioned in the discussion paper, BIT ratios do not vary significantly and as Lake Cadagno is strongly and continuously influenced by allochthonous material (in contrast to larger lakes, where in situ production plays a more significant role, and the influx of allochthonous material occurs rather sporadically, mostly during flood events). Again, we could not identify a dependency of GDGT distribution and sedimentology.

RC

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.

AC

We will include a detailed overview of GDGT ratios in soils and sediments in the revised version of the manuscript.

RC

What about the GDGT concentrations? They should be shown and discussed as well.

AC

We do not see the added value of reporting concentrations as many factors can impact concentrations of compounds such as production, preservation and accumulation rates. Interpretation of these data would not be straightforward and unlikely to impact discussions on our paleo T and pH reconstructions. Presenting concentrations would thus exceed the scope of the manuscript.

RC

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.

AC

See our comments above; we will rephrase our statements in a more moderate fashion.

RC

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).

AC

We agree that the match between our and the other records, for the early to mid Holocene, is not as good as it is for the late Holocene. We will thus rephrase our initial statement, so that it is more moderate now. However, we still think that a similarity with respect to the rough timing of maxima shown in our and the other records is obvious: Qualitatively, our paleo T-record shows comparably high temperatures at about 1, 3, 5 and 11 kyr BP. We also see a T-high at about 7 kyr BP, yet this feature is less well pronounced, which may also be a result of the incomplete data coverage for this time period. Warm phases occurring roughly at 1, 3, 5, 7, 9 and 11 kyr BP are also evident in the other T-records. Furthermore, the temporal fit to maxima in the NAO, as recorded in SW Norwegian winter precipitation (see Fig. 4d), is striking as has also been pointed out by reviewer #2. Because a persistently positive NAO-index has previously been identified as a main driver for the MWP (Trouet et al., 2009), we think that it is reasonable to assume that earlier maxima in the NAO can have led to similar effects, an assumption that is supported by the temporal match of paleo-T and NAO maxima. It has been argued (Trouet et al., 2009) that the wide geographical extension of NAO-influence on atmospheric T, at least during the MWP, was caused by a strengthening of the NAO through tropospheric dynamics during a La Niña-like climate state in the Pacific and Indian Ocean, which may be linked to solar activity. We could find a moderate temporal match between our record and solar activity (the $\sim$ 1.5 - 2 kyr. Bond Cycle; Bond et al., 2001). However, a solar influence seems to be more pronounced in winter-time T as indicated by an δ18O-stalagmite record from the Alps (Mangini et al., 2007). We will include these aspects in the discussion of the revised text. Some of the temporal differences (i.e. timing of maxima/minima) could be related to dating issues. While we are certain that the determined ages of the collected macro fossils and wood material are correct and that the age model is reasonably well constrained (within the error range indicated in Table 2), the temporal resolution of our record is relatively limited. Firstly, we analysed GDGT distributions from relatively broad sediment horizons (5 - 10 cm), which, according to the age model, translated to a temporal resolution of decades to several centuries. Secondly, bioturbation (e.g. by rodents such as marmots, which are common in the Cadagno region) as well as erosion/re-deposition (e.g. by avalanches) probably leads to a mixing of material from different soil (and thus age) horizons. As a consequence, the organic matter content (including the relevant GDGTs) of top soils, which is transported into the lake and deposited together with macro fossils on the lake floor, comprises a relatively broad age spectrum that "blurs" the lower and upper age limit of a given sediment sample. We can only speculate on how broad the age spectrum of the soil material is, which is transported during one erosion event into the lake. Obviously, it is a function of soil accumulation- and (bio)turbation rate as well as the erosion history, all of which were not assessed for the Lake Cadagno Region. Nevertheless, as S-Alpine soil accumulation rates may be in the range of 1000 yrs m-1 (Hajdas et al., 2007), we assume that admixture of GDGTs from deeper horizons with top soil (recently produced) material could have a significant effect. We agree that this was not really clear in the initial manuscript; hence, we will describe the dating issues and "temporal blurring" in more detail in the revised version of the manuscript. Because of these temporal uncertainties, and the relatively broad sampling resolution, it is not really meaningful to attempt spectral analysis of our data set, which, as correctly pointed out by reviewer 1, would be necessary to determine a temporal cyclicity. Consequently, we will step back from our initial statement of a 2 kyr cyclicity in the revised version of the manuscript.

RC

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.

AC

The HTO seems to be related to high summertime T during the earlier Holocene (ca. 8- 5 kyr BP) and was most pronounced in NE Europe. It appeared to be much weaker in S and W Europe (see our comments from above). As our data represent a record of MAAT (including winter-time T) it is not surprising that we don't see a clear HTO signal. Furthermore, our data coverage for this time period is patchy. Still, our reconstruction indicate a general warming trend from about 8 - 5 kyr BP, in agreement with other records. Ssee also our comments from above.

RC

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.

AC

We have clearly pointed out that the soil pH is most likely not only a function of precipitation. The buffering capacities of the soil have have had a strongly moderating effect. We will mention in the revised text that the accumulation of organic acids, as is expected during the buildup of soil organic matter, will probably have added to soil acidification. However, we disagree on that this likely has changed the lake pH. Lake Cadagno is and was strongly carbonate buffered, thus a low water column pH is extremely unlikely.

RC

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.

AC

We will moderate the statements, see comments above

RC

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.

AC

See our comments from above, we will discuss this in more detail in the revised version of the manuscript.

RC

Minor details: Page 3452 line 10: delete 'alternative'

AC

will be done

RC

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

AC

will be done

RC

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

AC We prefer to leave the sectioning as it is because environmental parameters includes method descriptions for determining the reference MAAT and soil pH

RC

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

AC

True, a multitude of climate reconstructions exist but only some of them are comparably to our data set. Thus we think it is appropriate to mention what our raw data sources was. Thus, we would prefer to leave the section as it is.

RC

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

AC

will be done

RC

Fig. 4f and g: typos 'Allps' will be done

Referee #2 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 manuscript is very well-written and well structured. Detailed comments follow.

AC

We will expand the critical discussion on paleoclimatic aspects, see our comments above.

RC

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.

AC

We understand that further information on the catchment is needed and, hence, we will include this into the revised version of the manuscript. Soil in Lake Cadagno's catchment is mostly restricted to an elevation slightly above lake level, i.e. ∼1900 - 2100 m asl. The highest peak in the catchment is Pizza Taneda with 2667 m asl. as can be seen in Fig. 1. We assume that most GDGTs are washed into the lake by physical erosion, which is probably dominated by snow avalanches and flood events. Transport by the two creeks that flow into the lake (there are no rivers in the catchment) will also add to the sedimentary input. We will clarify this in the revised version of the manuscript.

RC

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).

AC

We will add MBT, CBT and BIT ratios of sediments and soils in the appendix of the revised manuscript, see our comments from above. The BIT ratios are slightly lower in the lake's sediment (>0.95 when not considering the 3 lower most sediment samples) than in the catchment soils (>0.96). We will include this indication for a dominant allochthonous source of the relevant GDGTs into the revised text.

RC

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.

AC

As correctly pointed out by Reviewer 2, lake surface T in a temperate to arctic climate will be higher than MAAT because of the insulating ice cover during winter time. Unfortunately, Lake Cadagno's annual mean (surface) temperature has, to our knowledge, not been determined, thus we are not able to address this issue.

RC

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 (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.

AC

We have clearly stated that our approach/instrumentation led to an analytical error of 0.1°C (not an overall accuracy). The accuracy of the global soil calibration is ±5°C over the full T-calibration range of almost 30°C (Weijers et al., 2007). However, this is a systematic error, which does not equal precision of the calibration. Apparently, the precision is much higher, which, for our data set is <1°C (as deduced from the comparison of our reconstructed MAAT with instrumental and independent proxy data). However, it is correct that a calibration for low temperatures, preferably from local samples would be more ideal for paleo-T reconstructions. We will clarify this in the revised version of the manuscript.

RC

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.

AC

We have discussed in detail as to why our data set displays a hiatus between 6 - 5 kyr BP. Indeed the reconstructed temperature for this time period remains speculative. Therefore, we have added a question mark in figure 4 to indicate this uncertainty. Because of this, and the fact that the data, which cover the second half of the time period indicated by the yellow bar, show decreasing temperatures from 5 to 4 kyr BP, we find it reasonable to assume a warm period at about 6 kyr BP. Thus we would prefer to keep the bar in Fig. 4.

RC

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-inferreed 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.

AC

We agree with reviewer 2 and will address the apparent differences in the long term T-trends in more detail in the revised version of the manuscript (see also our comments from above). We will also discuss the limits of our data set with respect to temporal resolution and temporal accuracy (see also our comments from above). Nevertheless, despite the dating issues and "blurred" temporal resolution of the proxy, we still think that our and the other proxy data show, in general, maxima at about 1, 3, 5, 7, 9

and 11 kyr BP. However, because of the temporal uncertainties related to our data set, we will step back from our initial assumption of a 2 kyr T-cylicity; see also our comments above. The 2σ range of our age model (as we have shown in table 2) is on average $\pm$130 yrs. The 95% confidence interval for the Lake Egelsee record was slightly higher with almost $\pm$200 yrs (Larocque-Tobler et al., 2010), while the 2σ range for the N-Atlantic record was ca. $\pm$50 yrs (Thornalley et al., 2009) and thus apparently better constrained than our record. The combined error ranges and the uncertainties of the age interval integrated in a single sediment sample only allows to compare low-frequency features of more than a few hundred years of temporal duration. We will clarify this in the revised version of the manuscript.

RC

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 can be seen?

AC

We only compared low frequency features (i.e., low-pass filtered). We will clarify this in the revised version of the manuscript.

RC

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.

AC

Apparently, it also results in warmer mean annual T (Trouet et al., 2009).

RC

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?

AC

We agree that it is currently unclear if the branched GDGTs are produced over the entire anual cycle or only during comparably warm time periods. Typically, it is assumed that microbes are dormant during winter month (i.e., if the T is below freezing point). However, this may not be true. Members of the phylum Acidobacteria, which are probably the GDGT producing source organisms in soils (Sinninghe Damsté et al., 2011) comprises strains that thrive in very cold environments, for instance the Antarctic Dry Valleys where T is typically below freezing point (Cary et al., 2010). Furthermore, recent studies in other cold environments could show microbial activity far below freezing point (Steven et al., 2008; Niederberger et al., 2010). Hence, it may well be possible that the relevant GDGTs in the Lake Cadagno region are produced over the entire annual cycle. Yet this remains speculative. Howeverr, if sub-zero T was not captured by the paleothermometer, the regression of the global calibration would be effected strongly at the lower MAAT range. As this is not the case, i.e., MBT/CBT signatures of soil samples from environments exposed to sub-zero T accurately predict MAAT, we conclude that our data set provides a record of mean annual T (which is confirmed by the good match to instrumental and other proxy data and as already discussed at length in the manuscript). The recording of MAAT (and not summertime T) may also explain, why the HTO, a summertime feature, is not well expressed in our sample (see

our comments from above). Nevertheless, as we can only speculate about the exact identity of the GDGT producing organisms and their metabolic activity, we do not plan to extend the manuscript with respect to these aspects.

RC

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?

AC

The glacier in the Cadagno catchment declined completely at the end of the last ice age (see our comments on the catchment area), thus there was no enhanced supply of glacial mineral material during time periods (e.g. LIA) when (higher altitude) glaciers elsewhere in the Alps advanced (e.g. the Great Aletsch Glacier).

RC

Or the development of vegetation and a soil organic carbon (and organic acid) pool through the Holocene? âĂÍAC

We will add the build up of organic acids as an soil-acidification mechanism in the revised version of the manuscript (see our comments from above)

RC

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.

AC

Our record indicates that the pH rose during the MWP and only decreased slightly during the LIA. Nevertheless, these pH changes are very subtle and most likely insignificant. In any case, this cannot be related to glacier extensions elsewhere (See our comment from above).

RC

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.

AC

Different pH buffer systems will also have an effect on soil pH in the tropics. Nevertheless we agree with reviewer 2 and will clarify our statement with respect to the alpine implications of the statement.

RC

Page 3452 line 10. New paragraph starting "A promising..."

AC

will be done

RC

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

AC

Will be done.

RC

Page 3458, lines 20âĂŤ. 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.

AC

See our comments above.

RC

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.

AC

Soils from site 4 and 8 were irregular and the environment of these sites where meadows just as site 2 and 3 where meadows. We will clarify this in the revised version of the manuscript.

RC

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

AC

Will be done.

RC

Figure 5 caption. Change 'overrated' to 'overestimated.'

AC

Will be done.

Referee #3 Review of "Bacterial GDGTs in Holocene sediments and catchment soils of a highalpine 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.

AC

See comments from above. We will moderate our statements regarding the presumably allochthonous source of the GDGTs and the probably small effect of early diagenesis on the distribution of the target compounds (for the particular case of Lake Cadagno), see our comments from above.

RC

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.

AC

We will clarify this in the revised version of the manuscript, see our comments from above

RC

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.

AC

We will step back from our initial statement of a clear T-cyclicity and we will also clarify the uncertainties related to the age horizon covered by individual samples (error range of the age model and “blurring” due to mixing of soils with different ages, see our comments from above)

RC

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.

AC

Will be done

RC

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.

AC

We will clarify in the revised version of the manuscript that in situ production has already been identified as a very likely mechanism leading to alterations of soil-derived GDGT signals.

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

AC

GDGT ratios of all samples will be presented in the appendix; see our comments from above

RC

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.

AC

We will clarify this, see our comments above.

RC

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.

AC

We will clarify that the pH estimates are overestimated, see our comments from above

RC

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?

AC

The organic carbon content of sediments was typically >5%. Nevertheless, after reviewing our data set we could not identify a clear correlation between TOC content and high background during HPLC. Since we can only speculate as to why our analytical approach was not successful for these particular samples, we will correct our initial statement.

RC

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?

AC

See our comments above with respect to GDGT concentrations. We agree that TOC-normalised GDGT concentration in soils should be of similar magnitude than those in lake surface sediments. However, we have not measured TOC in soils, and as the sediment bulk geochemistry is part of another publication (Wirth et al., in press) we do not plan to add these data to the revised version of the manuscript.

RC

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.

AC

We will moderate our statement, see our comments from above

RC

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.

AC

We will moderate our statement with respect to in situ production and clarify why effect due to early diagenesis are rather unlikely, see our comments from above.

RC

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.

AC

We will clarify that high BIT values could also result if there is a high in situ production of branched GDGTs in the lake, which would mask the effect of crenarchaeol in the BIT-ratio.

RC

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”.

AC

Will be done.

RC

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.

AC

Will be done.

RC

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.

AC

We will moderate our statement.

RC

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 CBTreconstructed pH is 0.8 pH units and discuss the uncertainty in reconstructed pH in section 4.3.

AC

The accuracy of the global soil calibration is $\pm$ 5°C, but this is a systematical error which does not apply to within record variations, see our comments from above. Our data series shows clear trends, which apparently exceed data noise. Because the

instrumental and other proxies yield similar variations and absolute values for T, we concluded that not only the precision but also the accuracy of our record is <1°C. We will clarify this in the revised version of the manuscript, see our comments from above.

RC

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.

AC

See our comment from above.

RC

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).

AC

We agree that, for the time prior 2 kyr BP, the agreement of our MBT/CBT record with the other T-reconstructions is not perfect (in contrast to the late Holocene, where the match is very good) . However, above and in the original manuscript, we have already identified the differences and also offered a possible explanation for this phenomenon. We will extend our discussion with respect to age-related aspects of our samples.

RC

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.

AC

We have stated that, for the Holocene before 2 kyr BP, our and the other records agree with respect to the rough timing of climate undulations (with the major exception of a T-high at about 9 kyr BP which is not present in our record. We cannot explain this, as we have clearly pointed out in the text. Nevertheless, prior to 9 kyr BB, our record indicates a T-high at 11 kyr BP, which is also present in the N-Atlantic record and which also appears to be present in the chironomid record. We will clarify this in the revised version of the manuscript.

RC

Another question concerns the Holocene thermal optimum, which is not reflected in the different temperature records. This is quite surprising and should be explained.

AC

See our comments from above.

RC

Last, I think there are too many plots in Fig. 4b. The plot displaying the average temperature values might be sufficient.

AC

We disagree with this. Our aim was to present our data set (including sampling intervals and filter applications) as well as those data sets to which we compared our data as complete as possible because (unintended) artifacts may occur when applying filters. For instance excluding high frequency data by using a higher bisquare filter would have ultimately resulted in a lower signal magnitude of climate undulations. Furthermore, the different proxy records were sampled at strongly different sampling frequencies, thus

defining an “average” is tricky. We therefore prefer to keep all plots in the manuscript.

RC

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.

AC

We will rephrase the sentence. Nevertheless, within the limits of the methods addressed in the manuscript, we are confident to state that to large parts there is good (qualitative) agreement between the different paleo-records. See comments from above.

RC

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.

AC

The T-increase shown in the other records is subtle but present. With respect to the general T-increase from 8 to 5 kyr (also shown in other T-reconstructions, see e.g. pollen record by Davis et al., 2003; see our comments with respect to the HTO) and the fact that the T-estimate for 6 kyr BP is ca. 0.75 °C lower than for 5 kyr BP, we think that our speculation of a T-increase is valid. Furthermore, we clearly pointed out that our data show a hiatus in this time window.

RC

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.

AC

We have provided soil pH measurements, but we did not measure sediment pH.

RC

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.

AC

As mentioned before, we will change the text accordingly.

RC

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.

AC

We principally agree with reviewer 3 but most of the raised issues remain speculative

and thus are beyond the scope of the present manuscript. We have pointed out a possible factor that may prevent in situ production and/or early diagenesis (i.e. euxinia) and we will mention the (unlikely) possibility of GDGT in situ production leading to distribution patterns as found in the soil.

RC

2)the uncertainty in the time-scale captured by the GDGTproxies. This might complicate the reconstruction of past temperatures and pH and might notably explain the discrepancies between the different temperature records.

AC

This is a good suggestion. We have addressed this issue above and will make it more clear in a revised version of the manuscript.

RC

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

AC

ok

RC

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.

AC

As the paper primarily deals with the MBT/CBT paleothermometer and since we only used the BIT ratio as an additional indication for the presumed soil-origin of the GDGTs, we decided not to include any BIT formula in the original text. However, from the reviewers comment we see that an inclusion of the formula would help readability and we will accordingly add it into the revised text.

RC

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

AC

Our aim is to point out our major finding at this point at the beginning of the manuscript, so that the reader can critically check this in light of the upcoming method, result and discussion section. This approach is ‘a matter of taste’ but definitely not uncommon and we would thus prefer to keep this sentence.

RC

Page 3456, line 14. “0.45μm” instead of “0.45μg”.

AC

ok

RC

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

AC

ok

RC

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.

AC

In our view, this is already indicated in the sentence page 3465, line 15 of the discussion paper.

RC

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.

AC

ok

RC

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

AC

will be changed

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