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Interactive comment on "A 900-year New England temperature reconstruction from *in situ* seasonally produced branched glycerol dialkyl glycerol tetraethers (brGDGTs)" *by* Daniel R. Miller et al.

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We would like to thank Referee #2 for their suggestions and detailed review of our manuscript. Referee #2 requested several revisions, primarily within the discussion section. One of the main issues that referee #2 raised was the lack of applying a current published temperature calibration to the MBT'5me record. After consideration, we have agreed to provide an additional plot of our Basin Pond MBT'5me record with two different temperature calibrations (Dang et al. 2018, and Russell et al. 2018) that were developed using analytical methods directly comparable to our own, that allow for

the separation of 5me and 6me brGDGTs during HPLC analysis. Referee #2 stated that we should develop a calibration of our brGDGT observations to local temperatures, and also noted that we do not have enough data for such a calibration to be robust. We would like to emphasize that the goal of this study was not to provide a brGDGT to temperature calibration for Basin Pond. Rather, the goal of this study was to understand the timing and depth profile of brGDGT production in Basin Pond in order to address potential seasonal bias in the 900-year climate reconstruction. We agree with referees #1 and #2 in their assertions that a focused effort to develop a local calibration would be a useful contribution to our understanding of brGDGTs. Although this is beyond the scope of the main manuscript, our data have been plotted against local temperature in the supplementary materials, as we believe future efforts to develop a local calibration may benefit from these data. The main purpose of including this in the supplement was to highlight the potential Basin Pond holds in producing a temperature calibration in the future.

All other comments have been addressed in line below.

- The interpretation of the brGDGT data along the sedimentary core is only based on the MBT5Me index. Absolute temperatures based on the calibration developed by Sun et al. (2011) or Russell et al. (2018) are comparable (difference of ca. 1 âŮęC, within the range of the uncertainty associated with brGDGT calibration) and should be provided in the main text, all the more as it does not change the interpretation of the data.

We present our data as MBT'5Me values rather than applying a temperature calibration because the two existing calibrations based on methods that separate 5me and 6me brGDGTs were developed from lakes located in very different regions (China and Africa) (Dang et al. 2018, Russell et al. 2018) than Basin Pond (NE USA). Furthermore, brGDGT distributions in Basin Pond differ significantly from those of the Chinese and African Lakes (we will add a new ternary diagram plotting the hexa-, penta-, and tetra-methylated brGDGTs to clearly illustrate this- see attached). For this reason, these calibrations should be treated with caution when applied to Basin Pond sediments to reconstruct temperature. However, based on the request of the referee, we will plot our data on these 2 different temperature calibrations (Dang et al. 2018, Russell et al. 2018) in an additional figure that will be provided in the main text of the manuscript, while also continuing to emphasize our reasons for exercising caution when using these calibrations at Basin Pond. To summarize, our data will be presented as 1) an additional figure with MBT'5me, growth temp (Dang et al. 2018), and mean annual air temp (Russell et al. 2018), 2) a ternary diagram (see attached figure) in the supplementary information showing that the Basin Pond samples and the Chinese and African Lakes samples are characterized by different brGDGT distributions,, and 3) as MBT'5me and deviation from mean, on figures 7 and 8. Other temperature calibrations mentioned by the referee (e.g., Sun et al. 2011) are based on different analytical methods that did not fully separate 6Me isomers, and are therefore not suitable calibrations to apply to our MBT'5me record.

- I would be very cautious about the preliminary calibration between MBT derived from SPM samples and temperature, as it is based on only 4 samples.

We agree - because we are also very cautious about this preliminary calibration based on very few data points, we placed it in the supplement rather than the main text. We will revise the main text to better emphasize this important point.

- A local calibration between seasonal temperature (fall temperature, when brGDGTs are preferentially produced in this New England lake) and brGDGT distribution should be developed and used for temperature reconstruction along the sedimentary core.

We agree and this study in an area of our ongoing research. However, this is beyond the scope of the presented work. Here, the main focus in on presenting our new 900-year reconstruction.

- The discussion section, especially the comparison of the present record with other regional ones, is sometimes difficult to follow, as some explanations are missing and

all the data necessary for the understanding of the reasoning are not presented. I recommend a more careful and more detailed interpretation of the data.

We agree and thank the referee for this suggestion, and will revise the discussion text to make it easier to follow, more thorough, and more detailed. Changes to the text to address this point include: revision of Section 5.3 through 5.8.

- Several lipid biomarkers were analyzed in the lacustrine sediments and revealed some variations in the lake productivity over time, related to anthropogenic influence. Nevertheless, the authors should better discuss the potential human impact on the brGDGT signal.

We agree, and the potential human impact on the brGDGT signal will be discussed in more detail on Page 12, Lines 10–12. We have also addressed this in our response to Referee #1. However, we note that there is a general lack of knowledge on brGDGT producers, we therefore do not wish to speculate too much on this subject.

Disentangling natural and anthropogenic signals in lacustrine records is a key question which should be addressed.

We note the potential anthropogenic influence on the upper 3.5 cm of our reconstruction following the addition of Rotenone to the lake in 1955. However, the majority of our 900-year record is not subject to this kind of anthropogenic disruption. We will add text detailing anthropogenic influence in Section 2.1. We note that the precise impact of any kind of anthropogenic disturbance on brGDGT reconstructions is hindered by an incomplete understanding of the organisms responsible for brGDGT production in general. We leave this to future work in the brGDGT scientific community.

Other detailed comments are given below.

Page 2

Line 18. Late fall rather than early spring.

fixed.

Line 20. De Jonge et al., 2014 instead of 2013.

fixed.

Lines 25-30. Please also add some recent papers examining the distribution of 5- and 6-methyl brGDGTs in lakes: Russell et al., 2018 and Dang et al., 2018 both in Organic Geochemistry.

These references, which were published since the first draft of this paper was generated, will be added .

Page 3

Line 21. How long were the cores kept refrigerated? What about the evolution of organic matter (and especially brGDGTs) during storage?

The cores were collected in March 2014 (Page 3, line 16) and were stored unsplit and refrigerated (4°C, in the dark) for one month prior to subsampling (this information will be added to Page 3, lines 19-20). Although growth and alteration of the brGDGT signal during storage is a potentially important form of diagenesis for s, the sample processing in question is relatively rapid compared to some other existing studies that have utilized brDGTs (i.e. these cores were not stored for years prior to subsampling). This information will be added to the manuscript. We note that while this point should be investigated by future studies, many previous brGDGT studies have been done on old sediments and this is not believed to be a significant factor influencing the brGDT records (e.g., Weijers et al. 2007).

Line 29. All samplings were performed after a period of 28 to 40 days, except the last point. What is the reason for such a long accumulation time (264 days)?

The sediment traps, which were designed and constructed at UMass, required us to enter the water to pull up the traps and therefore were only retrieved and samples

collected while the lake was ice-free. During the winter we were not able to pull out the sediment traps because entering the water was not possible; additionally, cutting numerous large holes into the ice would have presented a safety hazard for the people utilizing Basin Pond for winter recreation. .

Page 4

Lines 5-6. These 5 dates should be specified once again in the present manuscript to make easier the reading of the manuscript.

We are unsure what the referee meant by this. Nevertheless we will make it clear when sampling took place and will repeat the sampling dates again further in the updated manuscript.

Line 9. According to Fig. 1, 20 soil samples were collected around the lake, which is not consistent with the number given in the text. This should be corrected.

Thank you- this is now correct, and the number of soil samples (10 samples) we analyzed matches the number of samples in Fig. 1.

Lines 14-17. Why were the TLE from SPM/soils on the one hand and lake sediments on the other hand separated differently?

For the purposes of this study, we are only interested in compounds that eluted in the apolar and polar fractions. Another study using the ketone fraction of the sediment core has already been published (Miller et al., 2017) and initially was designed to look for lacustrine alkenones (but they were not present). The SPM/soil samples were measured solely for this study, after we determined separating out the ketone fraction was unnecessary. However, we note that our separation procedure does not omit any compounds. For the SPM/soil samples, any compounds that would have eluted in the ketone fraction just end up in the polar fraction.

Line 19. Please specify here the different algal biomarkers which will be analyzed.

This will be added.

Lines 21-26. Please specify if the GC injections were made in split or splitless mode.

Injections were made in splitless mode. This will be added to the text.

Page 5

Lines 1-10. A short introduction of 5- and 6-methyl brGDGTs and the related indices should be added. Were some samples injected in duplicate/ triplicate? What is the analytical uncertainty on the MBT, CBT and IR indices?

We will include an introduction to 5- and 6-methyl brGDGTs on Page 2, Lines 19-24. In addition, section 4.4 "BrGDGT isomer ratios" also deals with this topic. Yes, a subset of the samples were analyzed in duplicate. Analytical uncertainty will be added in this section as well.

Lines 20-25. The relative abundances of brGDGTs in all the samples (soils, SPM, lake sediments) should be given in a supplementary table.

We plan to make these data publically available. All Basin Pond data presented here will be archived at the NOAA Paleoclimate Data Center and we plan to submit this data as soon as the manuscript is accepted for publication. If the editor wishes, we can add data tables as supplementary materials as well.

Acyclic and cyclic brGDGTs (Ia, Ib and Ic; IIa, IIb and IIc; IIIa, IIIb and IIIc) cannot be distinguished in Fig. 2. The figure should be modified to take this comment into account.

This manuscript does not explicitly discuss the differences in abundances of the a b, and c compounds (note also that relative abundances of the b and c compounds are very small; see Page 5 Lines 22–23). Therefore, we chose not to display them in Figure 2. Interested parties can access the a, b, and c data in the included data with the NOAA Paleoclimate Data Center upon publication.

The different types of green are difficult to distinguish in Fig. 2. Please choose more contrasting colours.

We will update this figure with more contrasting colors.

The brGDGT distributions are not consistent through the four collection periods: the relative abundance of GDGT I decreases from June 2014 to January 2015, in contrast with brGDGT III.

We made this observation on Page 5, lines 23-25: However, in June and July 2014, Group I brGDGTs were the most abundant, whereas in September 2014 and January 2015, reductions in Group I brGDGTs were accompanied by increases in Group III brGDGTs. We will remove the sentence about distributions being consistent through time, and add additional sentences that accurately describe the changes.

Page 6

Lines 4-5. These sentences are redundant with those in lines 1-3.

This paragraph will be rearranged for clarity and to reduce redundancy. However, we note that we separately discuss brGDGT fluxes and brGDGT distributions in this paragraph; while it may seem like there is redundancy, we find upon careful reading of these lines that all of the information presented is new and necessary.

Lines 24. These concentrations are not present either in a Fig. or a Table. Please provide the bulk data as Supplementary material.

The data will be provided upon publication with the NOAA Paleoclimate Data Center, or if the editor wishes a supplementary table.

Lines 27-29. These multidecadal events are difficult to distinguish.

We will distinguish the events by highlighting them with arrows in fig 7.

Page 7

Line 1. Please add also the recent paper by Russell et al. (2018) to the list of brGDGT lacustrine calibration.

The first sentences of the paragraph are discussing older lakes calibrations. The Russell calibration for African lakes is discussed and cited later in the paragraph after discussion of the new separation method for 5- and 6-methyl isomers.

Lines 9-13. The different arguments provided here are not convincing. The recent lacustrine calibration by Russell et al. (2018) could be applied to the NE US lakes, with all the caution needed (not the same region, difference in terms of stratification/mixing etc.). This is indeed the only calibration based on the recent analytical method proposed by Hopmans et al. (2016) allowing the separation of 5- and 6-methyl brGDGTs. Furthermore, as shown in Supp. information, the temperature variations inferred from the Russell et al. calibration are similar to those derived from the calibration by Sun et al. (2011). Therefore, the different calibrations provide different absolute temperatures (still very close, ca. 1 âŮęC difference) but similar trends.

As mentioned previously, we will present our data in a new figure in the manuscript with the Russell et al. 2018 African Lakes calibration, the Dang et al. 2018 Chinese Lakes calibration, as these are the only calibrations based on the recent analytical method, by Hopmans et al. 2016, that we used in this study. However, as mentioned previously, we also will add a ternary diagram to the supplementary information, which shows that brGDGT distributions of the samples in this study fall outside those of the African and Chinese Lake samples. This means the brGDGT producers in Basin Pond, and their sensitivity to temperature, are potentially different from those in the lakes studied by Russel et al. 2018 or Dang et al. 2018. We therefore wish to emphasize that presenting the data in this way requires caution, as it invites a suite of misinterpretations for readers who are not familiar with the proxy. Because of this, we will continue to present our data as MBT'5Me values in Figures 7 and 8, and indicate with words and colors in these figures that higher MBT'5Me values indicate warmer temperatures, and vice versa.

Line 18. Only group I and III brGDGTs can be distinguished, not the individual compounds.

We will correct the text to reflect this. We note that the b and c compounds make up <2% of the relative abundance in our samples. CBT'5Me and MBT'5Me were inverted in Fig. 6. This will be fixed.

Line 22. Lower instead of higher.

This will be fixed.

Page 8

Lines 1-6. This is redundant with the sentences above.

We will edit this paragraph to eliminate redundancy.

Lines 6-10. In addition to the brGDGT distribution, the brGDGT concentrations in soils, lake and SPM should also be compared.

brGDGT fluxes are explicitly discussed with reference to the SPM samples (e.g. Page 5 Lines 25-27 , Figure 3). brGDGT concentrations in soils (Page 5 Lines 20) and sediments (Page 6 Lines 24) are also noted. These quantities do not have the same units and are thus not directly comparable; furthermore, brGDGT concentrations have a wide range of values in the literature, making it difficult to interpret how the soil, sediment, and SPM concentrations compare and how they represent relevant environmental parameters.

Lines 11-12. I would be very cautious about the MBT'5Me/temperature local calibration, as it is based on only 4 points.

This reasoning is why we put it in the supplement, and noted that future work should address this promising but presently thin calibration.

Lines 13-14. Where are the water temperature data?

Water temperature data are presented in Figure 4(a), and we will add a citation (Frost, 2005) which details the yearly cycle of water temperatures as a function of depth in Basin Pond.

Lines 25-26. Such a calibration should have been developed in the present study and then applied to the downcore brGDGT reconstruction.

We will remove this sentence. Because of the limited number of SPM samples we were able to collect for this study, we recognize that it is not a calibration experiment, and should not be considered as such. Instead it is a detailed assessment of seasonal production biases coupled with the downcore application of a powerful emerging paleotemperature proxy, and the main focus of this paper is on the downcore record.

Line 31. These concentrations should be provided as Supp. Material.

These concentrations will be provided in the dataset with the NOAA Paleoclimate Data Center upon acceptance for publication.

Line 33. Please remove the last sentence which is not useful.

This sentence will be removed.

Page 9

Line 9. Please be more explicit about similarities and differences between pollen and MBT'5Me reconstructions.

This will be addressed in the text in the updated manuscript (see response to referee #1 comments).

Line 16. Please provide some references here.

We will add a reference to the review by Sachse et al., 2012. Are hydrogen isotopes from leaf waxes not mainly used are hydrological proxies? Hydrogen isotopes of leaf waxes can reflectchanges in temperature (e.g. high-latitude sites) or changes in hydro-

logical processes (e.g. tropical sites), or changes in the dominant moisture source, and interpretation is dependent on site-specific parameters. This is addressed in the text but the details of these processes are irrelevant to this study - we provide the dD record from Gao et al. 2017, which the authors interpret as a temperature record, because it is one of the few available records from New England.

Lines 16-21. This paragraph should be more developed. How are temperatures reconstructed from delta D of leaf waxes?

This is beyond the scope of this study. We provide these data simply for comparison to our own record. For more information, see the study of Gao et al., 2017. However, we will make it clear in the manuscript text that Gao et al. (2017) interpret their dD record as a temperature proxy.

Line 28. Where are this information derived from? Fig. 7d?

This information is derived from comparison between Figure 7g and Figure 7d–f. This will be updated in the text.

Lines 29-32. The conclusion about the predominant human impact on fires remains speculative and is difficult to apprehend.

We will remove the speculative sentence about the causes of fire changing through time. Our reasoning was based on the fact that the regional records indicate the climate has generally been getting cooler (Figure 7a–c) and wetter (Figure 7d–f) over the last 900 years, whereas the fire record indicates higher charcoal counts in the last 200 years compared to the previous 700. We note this is beyond the scope of the present study and as the referee noted highly speculative so we will remove it from the text.

Page 10

Lines 13-15. The similarities between Basin Pond reconstruction and other northern hemisphere reconstructions are difficult to visualize.

We will make the text more explicit about the similarities and differences between the Basin Pond reconstruction and northern hemisphere reconstructions. Additionally we will revise Figure 8 to better facilitate these comparisons.

Page 11

Line 2. What do the authors mean by "not a strong cross correlation? Please provide r and p values.

Fixed, r and p values will be added to text.

Lines 13-14. This trend is difficult to visualize.

We are unsure exactly which trend the referee is talking about, but all the trends discussed are shown in Figure 9. This paragraph will be edited to focus on the trends that we are showing, and to remove superfluous information about other trends that are not relevant for the present discussion.

Lines 17-18. What is the interest of presenting local data since the authors consider them as inaccurate? I would only present statewide trends.

We agree. We will adjust the text and Figure 9 accordingly.

Lines 24-27. This is a little too short. What are exactly the mechanisms which could explain the lower MBT'5Me values in surficial sediments?

We will add a sentence which lists possible mechanisms to explain this trend without speculating too much about our own data. Furthermore, we will cite other studies showing a similar trend in the uppermost sediments, and that the mechanism causing this is not currently well understood.

Some of these "mechanisms" may be lake-/region-dependent.

We agree.

Page 12

Lines 4-16 / Fig. 9a. The different algal biomarkers are difficult to distinguish in Fig. 9a. Please use contrasting colours for each biomarker.

In order to present data in a color-blind friendly manner, we decided to stick with shades of gray. However, we agree with the referee that the way it was arranged made it difficult to distinguish the colors and link them to the respective biomarkers. We will change this figure to make it more reader-friendly in response to the referee's request.

Supplementary information

Page 1

Line 17. Where is Table S1?

Table S1 is the first table in the supplement, but was mislabeled as S2. This will be fixed.

Line 27. Where are the fall measurements?

The average measured temperatures for each of the sediment trap collection periods are listed in Table S2.

Page 2

A MBT'5Me-temperature calibration should have been developed in the present study.

While this is a fantastic goal, we found that a correlation based on only four scattered points would be spurious (see following comment). However, we note that the main goal of this study was to reconstruct temperature trends over the past 900 years.

The correlation presented in Fig. S1, based on only 4 scattered points, is not reliable.

This was our reasoning as to why we chose to include it in the supplement and not actually apply it to the data in the main text.

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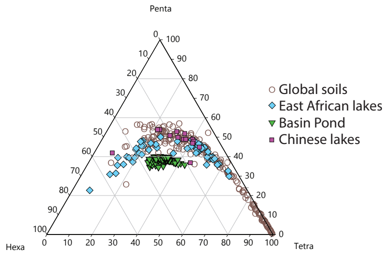
Penta
Hexa
Tetra
Global soils
East African lakes
Basin Pond
Chinese lakes

Fig. 1.