Biogeosciences Discuss., 12, C1539–C1543, 2015
www.biogeosciences-discuss.net/12/C1539/2015/

Biogeosciences
Discussions
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Interactive comment on "Seasonal lake surface water temperature trends reflected by heterocyst glycolipid based molecular thermometers" *by* T. Bauersachs et al.

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Received and published: 22 April 2015

The manuscript by Bauersachs et al. deals with the question to what extent the relative distribution of different species of heterocyst glycolipids (HGs, lipids unique to nitrogen fixing cyanobacteria) reflects the water temperature in their habitat. This question is investigated in a shallow lake in Northern Germany by evaluating correlations between several environmental parameters and HG distribution. If a specific link between HG distribution and water temperature can be confirmed, analysis of HGs in lake sediment would enable reconstruction of paleotemperature in lacustrine systems, for which the conventional lipid proxies (e.g. TEX86, UK37) are not suited.

The need for a robust molecular thermometer in paleolimnological studies is convincingly explained by the authors, and the aim to implement such a tool based on HGs is definitely an interesting topic. The authors contribute to this topic with a valuable set of data that confirms a local correlation between HG distribution and water temperature. The information in the manuscript is well structured and presented in a precise way; results are robust, interpretation is convincing and conclusions relevant. My main concern regarding this manuscript is that the possibility of/need for a global calibration of the HG-based molecular thermometer is not, or only scarcely, discussed. In previous work, Bauersachs et al. (2014) demonstrated that HG distribution in different species grown at the same temperature varies strongly, as does the slope of the correlation between HG distribution (HGI26) and temperature. Will the HG to temperature conversion therefore depend on the single species present in a given lake? Does this make a global calibration impossible? Is a local calibration, as the one performed in this study, always needed to interpret the HG signal in the sediment layers? Is such interpretation even possible if we assume that the cyanobacterial community might change from year to year? In my opinion, these kind of questions need to be addressed.

Reply: We agree with the reviewer that the application of HGs in lacustrine sequences as molecular thermometers raises a number of questions, which at the present stage are largely unanswered. The issues pointed out by the reviewer are of course relevant and we tried to address the concerns outlined above by extending our discussion on the possible use of the HDI26 and other HG indices as lipid paleothermometers in lake environments. To adequately address all of the above questions, however, additional culture experiments and lake studies have to be performed, which is clearly beyond the scope of the manuscript.

Specific comments

1. P. 755, l. 1-5. While it is true that in the two strains studied by Bauersachs et al. (2014) "the relative proportion of HG diols significantly increased with increasing growth temperature", the authors should also mention the large differences in the response of

HG composition to temperature observed in these two species. The abundance of HG diols (or the value of HGI26 and HGI28) at a given temperature, as well as the slope of the correlation between HGI26 (or HGI28) strongly differ between Anabaena CCY9613 and Nostoc CCY9926. At this point the authors could actually introduce the caveats already mentioned in Bauersachs et al. (2014): "additional culture studies will be necessary to determine whether or not individual species of heterocystous cyanobacteria adjust the composition of the heterocyst glycolipid layer differently with growth temperature, which would complicate the establishment of a universal temperature calibration"

Reply: We now added information on the distribution of HGs as a function of growth temperature in individual heterocystous cyanobacteria to the “introduction” section. We also extended our discussion by including information on the need for species-specific temperature calibrations to the manuscript. This discussion, however, is now part of section 4.4 (geochemical implications) and not of the introduction as proposed by the reviewer.

2. P. 756, l. 2-4: Date on which the sediment was sampled could be given here in order to have a better understanding of which watercolumn signals are possibly being incorporated into the sediment signal.

Reply: Surface sediments were collected in March 2014. A time, at which we expected the phytoplankton biomass produced in the previous year to be incorporated into the sediments. The date at which the sediments have been collected is now included in the text.

3. P. 756, l. 6: "Biomass production" sounds strange to me, as not production, but rather the "standing stock" is evaluated.

Reply: We agree with the reviewer and now use “Determination of algal biomass” instead of “Biomass production” as headline for subsection 2.2.

4. P. 759, l. 10-13. As it stands, the calculation of biomass ("the weight difference be-

tween wet and dry cell material on the preweighed filters", wouldn’t that just be the loss of water?) sounds confusing. In addition, to my understanding, organic biomass would rather be obtained after combustion (_500_C) of the sample and calculated as the difference between combusted and dry weight. Dry weight (organic and inorganic) and wet weight (organic and inorganic) would be obtained by comparison to the preweighed filter.

Reply: We agree that the current phrasing is somewhat misleading. The amount of dry biomass was calculated as the weight differences between a preweighed filter and the sample after it was dried at 105 °C for 24 °C, which is a common procedure to determine algal biomass and growth rates in laboratory cultures as well as environmental samples.

5. P. 760, l. 10-19: The authors could shortly address the fact that the studied lake is very shallow, therefore the impact of the watercolumn signal in the surface sediment is immediate. Possibly in deeper systems, a more complex picture could emerge, due to contribution of different communities, degradation during sedimentation etc.

Reply: We now added a brief discussion on the effect of water column depth on the preservation potential of HGs in freshwater systems to section 4.4.

6. P. 760, l. 23-P. 761, l. 26: This rather long paragraph could, in my opinion, be significantly shortened. The authors describe that the observed HG profile is taxonomically rather unspecific and, based on previous studies, could be attributed to members of the genus Anabaena and/or Aphanizomenon. As taxonomical assessment is not a goal of this study (and HG distribution probably not the proper tool for a gross classification that can be achieved more easily by microscopic observation), I would suggest to present this information in just two or three sentences (e.g. l.17-21.).

Reply: As requested by the reviewer, we significantly shortened the paragraph and now state that the HG distributions in water column samples of Lake Schreventeich is in good agreement with a predominant contribution of nostocalean cyanobacteria

known to be abundant in many lakes of Schleswig-Holstein (northern Germany).

7. P. 766, l. 4: Given the fact that only surface sediments are analyzed (0-1 cm), I don't think that the question if the HG signal "is preserved in the sediment" can be addressed.

Reply: We agree with the reviewer and now only focus on the transfer of the HG water column signal to the surface sediment.

Interactive comment on Biogeosciences Discuss., 12, 751, 2015.