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Biogeosciences Discussions

Interactive comment on "The Holocene sedimentary record of cyanobacterial glycolipids in the Baltic Sea: Evaluation of their application as tracers of past nitrogen fixation" *by* Martina Sollai et al.

Anonymous Referee #1

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This paper reports on the downcore distribution of heterocyst glycolipids (HGs) in the Baltic Sea in an attempt to evaluate the utility of HGs as tracers for past nitrogen fixation by cyanobacteria. The rationale is very well formulated and the data are unique and precious given the high-resolution sedimentary record and the limited information on these biomarkers in paleo-environment studies. I am very impressed with the depth of analytical analysis involved and the motivation of research. However, I must admit that I am not convinced that the data deliver the conclusion described in the abstract. I have two major concerns.

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Discussion paper

My first concern relates to the preservation of HGs in sediments (as is briefly discussed by the authors in the text as well). How does HG decomposition vary in freshwater versus brackish water systems? In modern freshwater and brackish water systems, does HG composition show the same pattern as observed in the sediment core? Is it possible that HGs are better preserved in brackish waters, leading to their higher abundance as well as stability compared to in freshwater systems? If so, HGs in sediments are not only related to their inputs but also to their decay. As both processes are influenced by temperature, the presence of O2 and possibly salinity, it is very difficult to conclude on “the potential of HGs as specific biomarker of heterocystous cyanobacteria in paleoenvironmental studies”. Instead, I would suggest considering whether there is a proxy or indicator that may be used to (even roughly) assess the preservation or degradation stage of HGs in sediments? In lines 27-35 (pg 9), it is mentioned that sea surface temperatures reconstructed using HGs were too high to be realistic and the causes were not clarified. To me, this seems like a hint that HG signatures in the sediments may be subject to diagenesis-related alterations and that different molecules have be influenced differentially. I think the authors need to clarify this possibility before making conclusions and in the abstract as well.

My second concern relates to the influence of multiple environmental variables on HG composition and distributions. As the authors mentioned (several times) in the text, HG variations may be related to temperature variations as well as salinity changes. I think that control experiments are needed to prove that HG shifts are related to cyanobacteria community changes only instead of being affected by physiochemical processes also.

A minor point: I am not sure if Figure 6 provides any new informationâĂŤthis is just another form of Figure 4. The authors may consider removing this figure.

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