Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-324-AC3, 2017

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.

Martina Sollai et al.

jaap.damste@nioz.nl

Received and published: 28 September 2017

We thank Dan Conley for his comments and provide answers to his two points as indicated below.

1) It appears that the 15N samples were acidified before measurement, which has been shown in the literature to result in anomalous values. If you compare their 15N data with other data from the Baltic Sea from a variety of groups their data show very little variation through time especially during hypoxic periods.

Please see our reply to a comment of referee #2; we did perform the del15N on untreated samples. The description of the experimental methods was inaccurate and will be adjusted. We apologize for this inconvenience. In contrast to what is mentioned by Conley, the range in del15N values we have measured in our core is slightly larger (1.2-5.2 permil) than that (2.0-4.5 permil) observed in the Funkey et al. (2014) study. Conley is also not right that the variation in the sediment in the main hypoxic phase during the Littorina transgression is less; it is comparable in magnitude (variations over slightly more than one permil).

2) The HG data also show a different picture than what has been observed with pigment biomarkers for cyanobacteria in the Baltic Sea. Funkey et al. (2014) – is referenced, but not discussed - showed increased cyanobacteria abundance during period of hypoxia likely due to changes in the biogeochemistry of P during low oxygen periods. I think more needs to be done to assure the validity of the 15N measurements and other proxies should be measured and compared to validate the HG data.

Conley touches here on a sensitive problem. Yes, we did reference his and his co-workers paper but we did not discuss it extensively for two major reasons:

i) The carotenoids used in their paper, zeaxanthin and echinenone, are not entirely specific for cyanobacteria. Zeaxanthin commonly occurs in various classes of algae and higher plants; echinenone has a more limited occurrence but has been reported in bacteria and marine animals. These carotenoids are certainly not limited to nitrogen-fixing cyanobacteria, as opposed to the highly specific HGs that we use.

ii) As pointed out by referee #1, diagenesis (especially post-depositional oxidation) in this environment of highly variable sediment redox conditions should be considered when the sedimentary biomarker record is interpreted. Carotenoids are amongst the most unstable organic biomarkers because of their very labile conjugated system of double bonds. Changes in redox conditions will thus have a major effect on the concentration of carotenoids and hence interpretation of their concentration profile as a

direct indication of the abundance of cyanobacterial nitrogen fixation is, at least in our opinion, somewhat simplistic.

Therefore, we don't really see any reason why the HGs should reveal a similar distribution to the much less specific and diagenetically more sensitive carotenoids. In view of this, we don't think we should measure these carotenoids as additional proxies. Our manuscript deals with the assessment of HGs as potential proxies for past cyanobacterial nitrogen fixation and, as indicated by both referee #1 and 2, provides an extensive study that does not require additional data.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-324, 2017.