Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-455-AC2, 2020

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Reconstructing N_2-fixing cyanobacterial blooms in the Baltic Sea beyond observations using 6- and 7-methylheptadecanes in sediments as specific biomarkers" *by* Jérôme Kaiser et al.

Jérôme Kaiser et al.

jerome.kaiser@io-warnemuende.de

Received and published: 7 February 2020

Referee #2 Tom Jilbert This manuscript presents several interesting datasets concerning the past abundance of diazotrophic cyanobacteria in the Baltic and Bothnian Seas on various timescales. The datasets are derived both from direct observations (water column- and sediment trap- monitoring of genera abundance, as well as satellite-based observations of bloom frequency) and from a new organic proxy in sediment trap and core samples, namely the abundance of mid-chain branched alkane (6+7Me-C17:0) lipids. The main goal of the study is test the applicability of these biomarkers for the

Printer-friendly version

Discussion paper

reconstruction of past diazotrophic cyanobacterial abundance, and indeed the authors present one such long sediment core record from the Bothnian Sea. The authors also use their biomarker data, along with instrumental and proxy-derived time series of climatic parameters, to investigate the potential climatic forcing of bloom occurrence on various timescales. The intrinsic value of the proxy seems to be high, and the paper is well written. However I have concerns over the authors' conclusions about the drivers of cyanobacterial bloom occurrence on various timescales, in particular their strong favoring of temperature over nutrient dynamics. Major comments 1. The conclusion stated several times in the manuscript, e.g. Line 323, "This record suggests that cyanobacterial blooms have not increased due to anthropogenic nutrient loading" is too bold considering the data presented in the study. Most researchers would agree that cyanobacterial bloom occurrence during recent decades is influenced by both temperature and nutrient dynamics, so without a strong piece of evidence to refute one or other factor, I suggest to moderate the wording of these sections. I also suggest that the authors should add the 20th century nutrient loading time series to Figure 5, in order for the reader to see how this compares with the other data presented. Some further considerations related to this: - The principal reasoning for stating that blooms respond more to temperature than nutrient loading is the "early onset" of blooms in the 20th century as implied by the peak in 6+7Me-C17:0 lipids in the period 1920-1940 in the Gotland Basin core. However, monitoring data show that phosphorus loading during this period increased by some 20% with respect to the period 1900-1920 (see below). This increase of 20% is greater in absolute terms (and certainly less noisy) than the SST increase over the same period (approx. 1°C +/- 2°C, see below). Of course, in the case of both nutrient loading and temperature, the response of cyanobacterial blooms is expected to be non-linear, e.g. a threshold-type response, related to the fact that these organisms are competing for resources within an ecosystem, and above certain thresholds of certain environmental variables may gain significant competitive advantage over other primary producers. Hence the difficulty in making direct linear correlation analyses with time series of those environmental variables. In summary,

Printer-friendly version

Discussion paper

I would like to see a more balanced acknowledgement that both nutrient loading and temperature may have influenced bloom occurrence during this period, and that these responses are likely strongly non-linear.

Answer: In agreement with Mr. Jilbert's comment, we have now included Gustafsson et al. data of riverine phosphorus input to the Baltic Sea in Figure 5. We have added Gustafsson et al. (2012) to the reference list. The caption of Figure 5 has been modified consequently. We have also moderated our wording and suggest now that both temperature and nutrient inputs likely influenced cyanobacterial blooms. We have added a few sentences in the manuscript: - Abstract: "While the early increase in cyanobacteria may be related to a small increase in phosphorus loading, decadal to multi-decadal fluctuations are likely related to variability in the Baltic Sea surface temperature and, ultimately, to the Atlantic Multidecadal Oscillation". - Section 4.3: "However, the small increase (ca. 16 %) of riverine phosphorus input to the Baltic Sea may have partly triggered the early increase in cyanobacteria abundance in the 1920s (Fig. 5B)." - Conclusions: "This record suggests that anthropogenic nutrient loading is likely not the main trigger for cyanobacterial blooms, but may have favoured an early increase in the 1920s. Cyanobacterial biomass fluctuation seems to be at least partly related to sea surface temperature changes and the AMO climate mode at a decadal to multi-decadal timescale over the last 140 years." Concerning the possible non-linear response of cyanobacterial occurrence to environmental parameters, we have now added this sentence at the end of Section 4.3: "However, it has to be considered that the response of cyanobacterial blooms to environmental variables such as nutrient loading and temperature are likely non-linear, that may explain the relatively low correlations. Indeed, a very recent study shows that cyanobacterial blooms are highly correlated to environmental variables at a decadal timescale when considering a set of biogeochemical variables related to the amount of phosphorus and hypoxia in bottom layers, as well as surface water temperature (Kahru et al., 2020)." Note that the Kahru et al. (2020) article has been added to the reference list and is attached.

2. I have a similar concern with the interpretation of the long sediment core record in Figure 6, although now we are discussing natural rather than anthropogenically-impacted nutrient cycling. The authors acknowledge in the text that phosphorus regeneration played an important role in sustaining blooms during the HTM in the Bothnian Sea, as we showed in our earlier study (Jilbert et al., 2015). However I would also like to see a statement acknowledging that declining P availability was likely the main factor in the steep decline in blooms from 6500 yr. B.P., which is a dominant feature of this record (we interpreted this as due to the shoaling of the Åland Sea sills). The temperature records from the Swedish lakes presented here support the concept of warm conditions favoring blooms during the HTM, but for example, 4500 yr. B.P. shows a similar temperature to 6500 yr. B.P., yet the bloom intensity is orders of magnitude lower as shown by the log-scale of 6+7Me-C17:0. This requires another controlling factor, ie. availability of P.

Answer: In agreement with Mr. Jilbert, we have now mentioned that the shoaling of the Åland Sea sills may have declined P availability, and thus diminished the cyanobacterial blooms after ca. 6300 yrs BP. We have added this sentence in Section 4.4: "However, as suggested by Jilbert et al. (2015), the shoaling of the Åland Sea sills (Fig. 1) and the resulting decline in P availability may have been a major factor explaining the decline in cyanobacterial blooms from ca. 6300 years cal. BP." Figure 1 and its caption have been slightly modified to add the location of the Åland Sea.

Minor comments: Line 96: Replace 'bloom' with 'blooms'

Answer: Done.

Line 97: One could reasonably ask why a core from the Bothnian Sea is investigated and not from the same location as the short cores and sediment trap series Biomarker and SST time series presented in the study.

Answer: The aim here was only to illustrate the fact that the 6+7Me-C17:0 cyanobacterial biomarker is working for the complete Holocene period. We have chosen this core

from the Bothnian Sea because of its excellent age model and for sample availability. We are now working on a multi-proxy study including the 6+7Me-C17:0 cyanobacterial biomarker on this core with a higher temporal resolution.

Line 106: Give more detail on the coring device

Answer: This sentence has been added: “The short sediment core MSM51-2/20 was retrieved with a device keeping the sediment-water interface undisturbed (multi-corer). The core was sampled every 0.5 to 1 cm. The sediment samples were frozen-dried and homogenized (n = 73). The long sediment core POS435/10 retrieved with a gravity corer (Häusler et al., 2017) was sampled every 10-20 cm”. In our opinion, these coring devices are nowadays relatively standard in the field and a more detailed description of their functioning is not required here.

Line 111: Methodology for estimating TOC needs more detail. Is one of these instruments able to isolate and measure inorganic carbon from a bulk sample?

Answer: We have now given more details on the method used to estimate TOC: “Total organic carbon (TOC) was calculated by the subtraction of total inorganic carbon (TIC) from total carbon (TC) values. TC was analysed by means of an EA1110 CHN (CE-instruments). TIC was determined by means of a TIC module connected with a Multi EA 2000 CS (Analytik, Jena) elemental analyser, involving the acidic removal of carbonates from sediment samples and analysis of the CO2 released in a carrier gas stream (Leipe et al., 2011).”

Line 255: Replace ‘what’ with ‘which’

Answer: Done.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-455/bg-2019-455-AC2-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-455, 2019.

Printer-friendly version

Discussion paper

Figure 5

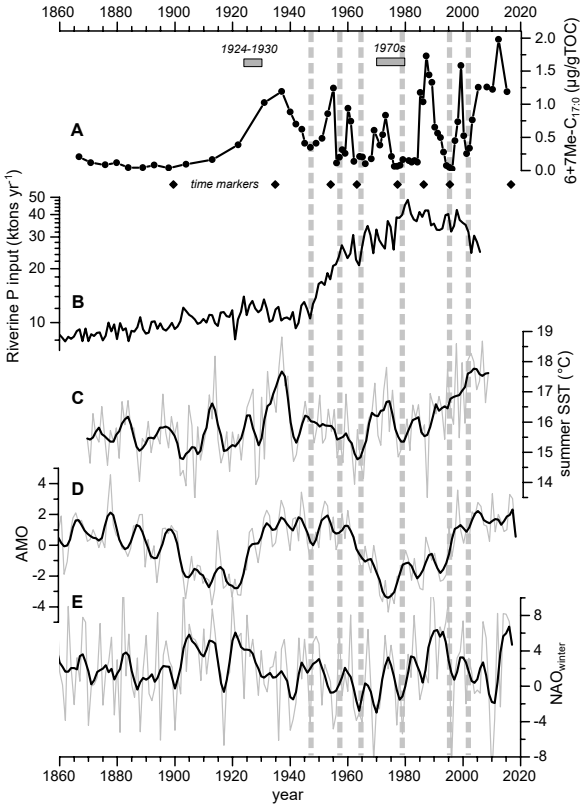

Fig. 1. Revised Figure 5

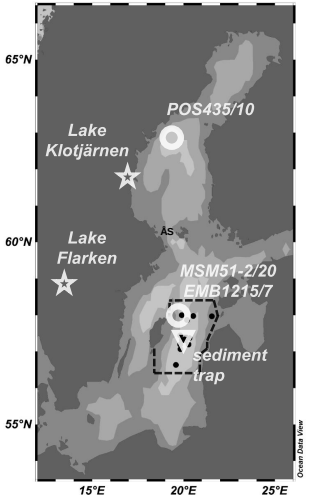

Fig. 2. Revised Figure 1