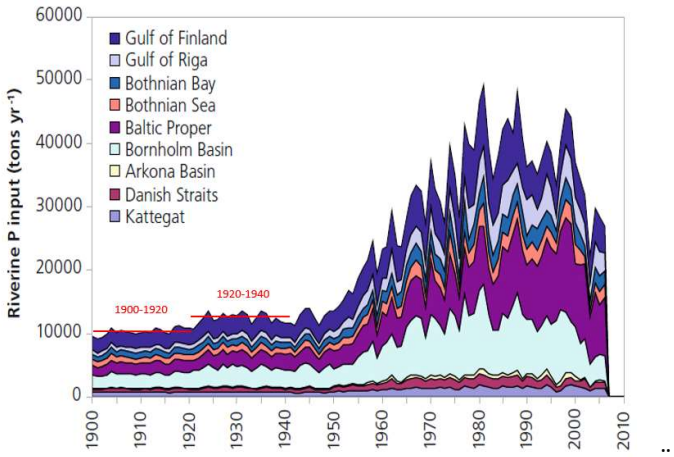
**Interactive comment by Tom Jilbert on "Reconstructing N2-fixing cyanobacterial blooms in the Baltic Sea beyond observations using 6- and 7-methylheptadecanes in sediments as specific biomarkers" by Kaiser et al.**

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 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**

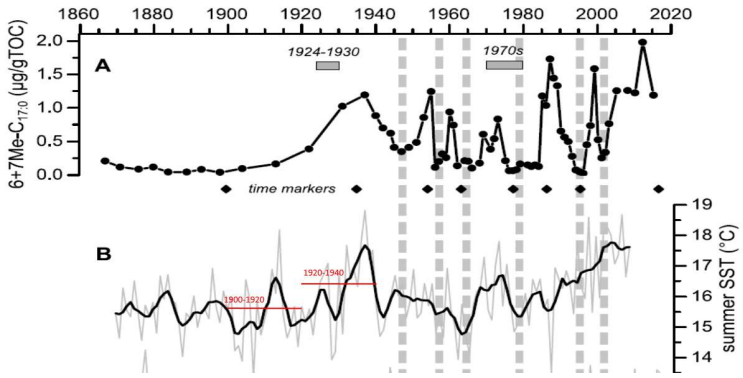
1. The conclusion stated several times in the manuscript, e.g. Line 323, "This record suggests that cyanobacterial blooms have not increased due to anthropogenic nutrient loading" is too bold considering the data presented in the study. Most researchers would agree that cyanobacterial bloom occurrence 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 20$^{th}$ 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 20$^{th}$ 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)




***HELCOM data of phosphorus loading to the sub-basins of the Baltic Sea, 1900-2010. Mean values for 1900-1920 and 1920-1940 are shown in red.***

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)




***Biomarker and SST time series presented in the study. Mean SST values for 1900-1920 and 1920-1940 periods are indicated by red bars.***

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, 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.

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.

**Minor comments**

Line 96: Replace ‘bloom’ with ‘blooms’

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

Line 106: Give more detail on the coring device

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?

Line 255: Replace ‘what’ with ‘which’