Interactive comment on "Latitudinal distribution of biomarkers across the western Arctic Ocean and the Bering Sea: an approach to assess sympagic and pelagic algal production" by Youcheng Bai et al.

Reply to review #2 Author's replies are in Blue.

Bai et al. present a record of 52 surface sediment samples and 13 suspended particulate matter samples from the upper 5m of the water column from the Arctic Ocean, Chukchi Sea and Bering Strait/Sea. In all samples they have analyzed biomarkers for sea ice as well as phytoplankton productivity and terrigenous input, in order to investigate the spatial distribution of these compounds in surface sediments and waters.

The presented record confirms the distribution of the analyzed compounds as already published findings from this area and working group. Bai et al. add the H-Index as a relatively new method to calculate pelagic to sympagic productivity, which nicely underlines the distribution patterns and the applicability of this index.

I apologize, in case I have misinterpreted anything.

We appreciate the referee review. Please refer to the one to-one response below in blue for specific modifications and clarifications.

As mentioned before, this manuscript presents a valuable dataset, however so far, the discussion focusses mainly on the comparison with Bai et al. 2019, which raises the question of the scientific contribution of this manuscript. The presented data set however is valuable and I am sure that the scientific contribution could be higher when the discussion and data comparison is extended.

I do not see a reason why the Bai et al (2019) data is plotted for comparison but the Koch et al. (2020), Xiao et al. (2015) and Méheust et al. (2015) is not plotted if applicable. This would allow for a much better discussion of the distribution in comparison to sea ice extend and productivity. In this regard, the discussion could benefit from a discussion regarding the different seasonality's of algae bloom/biomarker production, as the study region reached from high arctic to the middle latitudes, where light availability and seasonality differ greatly.

See answer to REV#1 above. We have added more than 50 new data that essentially provide a better representation of the low end-member of HBIs (ice free). The range of IP_{25} concentrations in our surface sediments indicate comparable concentrations as in previous surface studies in the pan-Arctic (Méheust et al., 2015; Xiao et al., 2015; Koch et al., 2020).

Rather than seasonality *stricto sensus*, it is the sea ice cover that drives the relative abundances of sympagic/pelagic productions across the range of latitudes in our data set, which is the focus of this study in the context of global warming (as reflected by the title).

In this regard, it may be useful to also calculate the PIP25 indices for sea ice reconstructions. I feel the study region is not yet studies in detail since the implementation of HBI III as phytoplankton marker in the PIP25 calculation.
PIP_{25} assessment was the focus of Bai et al., (2019). Here, the purpose is to assess sympagic and pelagic algal production and the potential of H-Print and sterols to do so.

I feel that terrigenous sterol distribution is not given enough attention. Especially in regard to the influence of terrigenous input from sea ice and the influence on phytoplankton productivity.
We added a discussion on this issue in the revised manuscript (See section 5.2 Lines 511-513). Changes in sea ice conditions influence terrestrial inputs to the sediments through transport pathways such as sea ice drifting (Eicken et al., 2005; Darby et al., 2009). Regarding the role of terrestrial inputs on primary production, this is still debated including in region like the Mediterranean Sea surrounded by land masses. Nutrient availability originating from aerosols on primary production and notably the concept of bioavailability has been a research topic for many decades. This is different from coastal waters where riverine inputs of nutrients, essentially in inorganic forms, are known for having a direct impact on primary production.

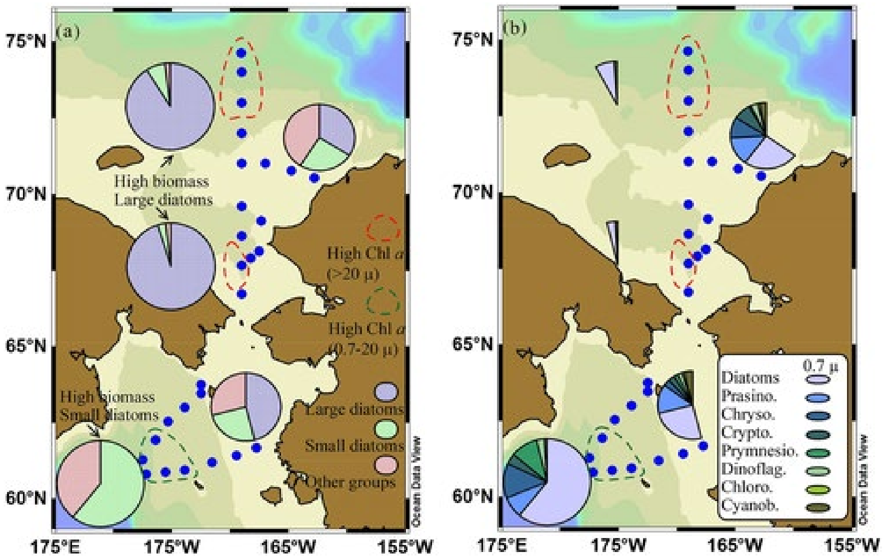
Specific Comments

Abstract

The Abstract includes a lot of detail, describing the distribution of biomarkers. I feel this could be shortened and less descriptive.
After a careful reading of the abstract we found that all sentences are needed to provide an understandable description of this work.

L35 I apologize, if this comment is based on my lack in biological knowledge: What is the connection to pelagic sterols and Chl a with diatoms. Aren't sterols and Chl a produced by a wide variety of phytoplankton species?

Diatoms are often associated with brassicasterol (24-methylcholesta-5,22E-dien-3β-ol). Based on the similarity of this sterol and Chl *a,* we suggest that primary production is dominated by diatoms. This is further supported by studies such as Zhuang et al. (2020) who pointed out that spatial variability of Chl *a* point to a high phytoplankton biomass in the south Bering shelf (SBS), south Chukchi shelf (SCS), and North Chukchi shelf (NCS). They also demonstrated that small diatoms dominate the phytoplankton community in the SBS (> 60%), whereas large diatoms prevail in the SCS and NCS (> 90%). Koch et al. (2020) also reported that pelagic diatoms were largely responsible for the Chl *a* distribution in the northeast Chukchi Sea region from August to October 2015, based on taxonomic determination.



The above figure from Zhuang et al. (2020) shows (a) the relative contribution of different algal classes (large diatoms, small diatoms, and other groups) and (b) group composition of phytoplankton 0.7–20 µm in size to total chlorophyll a (Chl *a*) concentration calculated by CHEMTAX analyses in subsurface chlorophyll maxima layers in the Bering–Chukchi shelf.

Zhuang, Y., Jin, H., Chen, J., Ren, J., Zhang, Y., Lan, M., Zhang, T., He, J., and Tian, J.: Phytoplankton community structure at subsurface chlorophyll maxima on the western Arctic shelf: patterns, causes, and ecological importance, Journal of Geophysical Research: Biogeosciences, 125, e2019JG005570, 2020.

Introduction

L49 "annual bloom period" – To my understanding, there are several blooms (spring, summer, autumn), and this is, also in the Arctic Ocean, until September (sea ice minimum)

As the reviewer 1# suggestion, we change “annual” to “at the onset of the spring bloom”.

L51 increases **in** later summer/early autumn

Done.

L77 exchange diagnostic with geochemical

Done.

L88 This is the only time you use a hyphen for sea ice. Make sure you stay consistent.

We are very grateful to Reviewer #2 for a thorough reading of the manuscript. We have carefully examined the manuscript to correct and harmonize.

Material and methods

L179 prior **to** extraction

Done.

L179 delete “with organic solvents”

Done.

L207 heating instead of hearing

Done.

L240 I strongly disagree with the arguments given by the authors using the sea ice satellite record from 1988 – 2007. Yes, Navarro-Rodriguez et al., (2014) could show that surface records are not explicitly representing a specific time frame. But I would strongly advice against using the old satellite dataset, especially as this study often discussed the most recent changes in the region and the resulting changes in sea ice, meltwater, river run off etc., and the most dramatic sea ice changes occurred after 2007. Why not choose a 20-year time frame going back from the year of sampling (1994-2014)?

In the revised version, we have used satellite sea ice concentration data from 1994-2014 to generate average sea-ice distributions for spring (April, May and June) and summer (July, August and September). Note that it does not change much the picture and does not have implication on the discussion.

L247 A appreciate that the age uncertainty of surface sediments is mentioned here. I see that it is often not applicable to confirm the age of surface sediments - However, this study would greatly benefit from it as it will strongly differ within the presented record, which makes me doubt the comparability of the individual stations.

This has been the approach taken by paleoceanography (not even using box-corers). Surface sediment samples integrate the upper few centimeters of sedimentation, and thus represent from several years to century. This is not specific to this study.

We are fully aware of the limitation of this approach and added a sentence in the revised manuscript (more details see section 3.8, Lines 250-251). Nevertheless, distinct latitudinal variations and trends in sea ice biomarkers as well as phytoplankton productivity are consistent with similar studies in this particular region (Koch et al., 2020a, b).

Results

L307 common sterol **found** in diatoms

Done.

L307 A discussion on the uncertainties of the production of phytoplankton sterols is missing. The producers of brassicasterol and dinosterol are not as straight forwards as presented here and there are more recent publications on this than Volkman (1986), and Volkman (2003).

We agree and re-emphasized the uncertainties of the production of phytoplankton sterols and updated the more recent references (Line 307-313).

Volkman, J.K.: Sterols in Microalgae. In: Borowitzka, M.A., Beardall, J., Raven, J.A. (Eds.), The Physiology of Microalgae. Springer International Publishing, Cham,pp. 485–505, 2016.

L310 there is a problem with special characters, I only see a square throughout the manuscript

Corrected. The symbol should be displayed as α isomer.

Discussion

L400 This combination of these datasets in a figure would allow a much deeper discussion of the region.

As explained in general comments, there are methodological differences because of RF and the intent is to discuss the trends and patterns within a coherent database to assess sympagic *vs* pelagic productions.

L414ff Here, for example, recent changes in the study area are discussed in detail, this is why I see the need to include a more modern sea ice satellite dataset – and also an age control...

Limitation linked to the age of surface sediments is indeed an issue, we have updated a more modern sea ice satellite dataset (1994-2014). Here, we added a comparison with the result from the surface sediments in Koch et al. (2020b) in the revised manuscript (see section 5.1 Lines 413-416).

Koch, C. W., Cooper, L. W., Grebmeier, J. M., Frey, K., and Brown, T. A.: Ice algae resource utilization by benthic macro-and megafaunal communities on the Pacific Arctic shelf determined through lipid biomarker analysis, Marine Ecology Progress Series, 651, 23-43, 2020b.

L459 The producers of HBI III are not completely confirmed. Please be more cautious here.

Thank you for noting. The sentence has been rephrased (more details see section 5.2 Lines 462-466).

L469 “recent decades” – do you have proven that your sediments are representing these? Why are you excluding the recent decades

This has been changed by “last decades” in the revised manuscripts (Lines 474). However, we are referring here to the paper of Renaut et al. (2018).