

Reviewer 1 (Darci Rush):

This paper entitled “Biomarker characterization of the North Water Polynya, Baffin Bay: Implications for local sea ice and temperature proxies” submitted by Harning et al. presents the distribution of lipid biomarkers in marine surface sediments taken in the area in and surrounding the largest arctic polynya (North Water Polynya). They assess some of the most commonly evaluated lipid classes in organic geochemistry: highly branched isoprenoids, sterols, and GDGTs. Combining their results with data generated from previous studies in the area, these authors evaluate the use of highly branched isoprenoids and sterols as biomarkers for sea ice productivity. The authors also compare the estimated temperatures generated by biomarker (GDGT) proxies with measured data from the World Ocean Atlas Data from 2007 – 2017. The authors make several astute observations about the environmental controls that may govern the distribution of GDGTs involved in the temperature proxies in the Arctic. They have a reasonable argument for the power of regional calibrations in reducing uncertainty and errors in temperature estimates, but I feel that they must also acknowledge that the sample size of these observations is small. Overall, I am satisfied with their discussion of GDGT-based temperature proxies in the Arctic and sea ice cover/productivity and recommend this manuscript for publication. I do have a few comments for minor revisions, nevertheless:

We greatly appreciate Darci Rush’s time and consideration of our manuscript and thank the reviewer for a constructive critique that will lead to a stronger paper. Below we address each comment individually.

Results

Line 257 onwards. It is not clear to me which sediment set you are discussing in terms of the GDGT and OH-GDGT data. Is it the 13 “Baffin Bay” samples collected for your study or are you including the 70 previously published samples? It would be good if you make the sample number clear in the text and in the figures. It is also confusing when the authors switch on line 283 to talking about “northern Baffin Bay”. I assume that you mean just the 8 NOW sediments, but this needs clarity. Including number of samples with the correlation coefficients would make it clearer for the reader when you are examining 13 sediments, or just the 8 in the NOW sample subset and if you are also ever including previously published data. Indeed, if all your GDGT and OH-GDGT data analysis is for the 13 samples then you aren’t saying anything with the GDGTs about the NOW (as you did do for the HBIs and sterols) but rather about the entire Baffin Bay area. This seems to deviate from the title and aim of the manuscript.

Thank you for providing the opportunity to clarify this. The only available GDGT and OH-GDGT data for Baffin Bay is from this current study (n=13). The other study we reference (Kolling et al., 2020), where we use n=70 data points, is focused on sea ice proxies and only reports HBI and some sterol concentrations. We will make it clear up front in the Materials and Methods as well as throughout the text and figure captions how many samples are being used for which biomarkers. Regarding L283, we are referring to the entire sample set, so we will remove “northern” to avoid any potential confusion. Finally, while yes, we agree that including samples outside the NOW extends the region beyond our focal point, we believe it is necessary to expand the temperature range of our calibration. In this sense, we also use HBI and sterol data from outside the NOW to provide greater context for those biomarkers. As suggested in the following comment, we will add some figures and discussion on the spatial distribution of GDGT and OH-GDGT in Baffin Bay, particularly regarding the NOW.

It is a pity that the authors have not presented more of the GDGT dataset. For example, the GDGT & OH-GDGT fractional abundance for each of the 13 stations in a table or in a supplementary figure (perhaps similar as to that done for the sterols and HBI concentrations; Fig. S1 - S3). While Figure 6 provides an overview of the entire data set (although I'm still unclear as to whether this is 13 samples or not), I think it would be useful to know whether the GDGT and OH-GDGT distribution varies spatially inside and outside the NOW. Perhaps something similar to the way Spencer-Jones et al. (2021) presented the distribution of archaeal lipid (headgroup)s in different polar water masses. I expect that spatial differences in GDGT & OH-GDGT (core) distributions would not be great in surface sediments, but as you have the data it is a pity not to present it, or at least include a line about any spatial homogeneity or heterogeneity in the results.

Apologies for the confusion, however, the complete sample datasets will be made available on the PANGAEA online repository upon acceptance of the manuscript. We will add similar spatial figures as provided for the HBI and sterol datasets in the supplemental and add some text in the discussion on the spatial distributions of GDGTs and OH-GDGTs throughout Baffin Bay.

Discussion

Overall, the discussion of GDGTs and OH-GDGTs is clear but I have a few suggestions. The sentence on lines 413 – 415 about the cren isomer reminded me of the analysis in Bale et al. (2019) of the cren' ratio in 58 thaumarchaeotal cultures. They reported that growth temperature has a larger effect than thaumarchaeotal phylogeny on the proportion of the cren isomer.

We appreciate this insight and will take this into consideration during the revision of our manuscript.

I found this BGD preprint and discussion <https://cp.copernicus.org/preprints/cp-2022-19/> useful when thinking about the relationship between hydroxy-GDGTs and shifting archaeal species composition and/or salinity. Perhaps if it is in print in time, the authors could reference it in relation to the statement on lines 378 – 380 and perhaps in relation to the statement on line 456.

We were not aware of this preprint, thank you for bringing this to our attention. While we can reference the preprint as it has a DOI, we will hold off until it is published and officially passed the peer review process. If that fall within our revision window, we will certainly reference it.

A note throughout the manuscript. The authors refer to the cren isomer as a regioisomer, whereas this has been proven not to be the case and is more likely a stereoisomer (Liu et al., 2018; Sinninghe Damsté et al., 2018).

Thank you and edited.

One final suggestion I would like to put forward is based on a number of recent articles about decolonization of geosciences, for example Liboiron (2021). Would the authors be prepared to acknowledge the indigenous people that traditionally occupy the regions they sampled? I believe that Inuit Nunangat occupy the West (Canada) and Kalaallit the East (in Greenland). More information about the people in the sample areas and how to acknowledge them can be found at <https://native-land.ca/>

We very much appreciate this suggestion as we are always striving to learn and better ourselves for the greater decolonization of geosciences. We had not fully recognized that marine locations could be considered as regions traditionally occupied by indigenous people, which exemplifies the progress that is inherently needed. Acknowledgement and appreciation of the indigenous people has now been added at the end of the paper and will remain in all future relevant publications of ours.

Minor edits

All minor edits corrected for as suggested below.

Line 124 – change to “depth”

Line 135 – change to “planktonic”

Line 189 – remove extra)

Line 260 – the second half of this sentence is confusing.

Lines 262 – 263 – This is a confusing sentence as figure 7 only presents temperature correlations and the other variables listed are shown in the supplement. Perhaps easiest here to expand the figure range (e.g., Figs 7, S4 – S6).

Line 265 – change to “their correlation”

Line 269 – is this line missing the word temperature, SST perhaps?

Line 272 – change to either “autumn subsurface temperatures between 60 and 80 m bsl also feature similarly significant correlations” or “autumn subsurface temperature between 60 and 80 m bsl also features similarly significant correlations”. I’m not sure which of these you mean but you currently have a mix of both.

Lines 283, 285 – This should be a reference to Figure 8 not 9.

Line 380 – Could you add the actual samples size (i.e. $n = x$) and temperature range here?

Line 425 – this should be Fig 8c?