Reviewer 1

The authors present a comprehensive biomarker data set from three NE Greenland shelf records together with instrumental/observational data for sea ice fluctuations over the last century. A well-balanced and interesting manuscript with plenty of new datasets certainly of interest for the readership of EGUsphere. I would like to highlight a few critical points that might be addressed before the manuscript can be accepted for publication:

We are happy that the reviewer believes that this dataset is of relevance and interest to the readership of the EGUsphere.

First, the authors have access to bulk organic information including TOC, TN, and d13Corg While the bulk organics give you a comprehensive overview on the organic matter sources, the biomarkers cover only a tiny fraction of it. You could use the data better to inform the readership of dominant organic matter source in the records. You may even consider a rough semi-quantification of marine and terrestrial organic matter and use is more actively for your interpretation. The d13Corg data vary between -23 and -27 permille implying quite a bit of variation in terms of terrestrial organic matter supply to your shelf system.

We understand the idea behind this comment, however previous studies from Northeast Greenland have shown that the link between $\delta^{13}C_{org}$ from land and marine environments is complicated to decipher in this area (e.g. Andreasen et al., 2023. Boreas). We agree that further studies are needed to determine organic matter sources. However, this is not the focus of our own study, which aims to understand sea-ice conditions instead; thus, we have decided to not include this aspect in the present paper.

The authors (desperately) try to argue that the near-surficial deposits are less influenced by bio-degradation compared to the climate signal preserved within the biomarker records. Rontani et al. (2018) is often referred while only the ration of epi-brassicasterol and 24-methylenecholesterol is shown. Why don't you analyse the autoxidation products of IP25 in some of your samples? You have the co-authors to do this experiment. It would strengthen your dataset immensely and avoid mis-interpretation of your data.

Thanks for this suggestion, we agree that providing more evidence related to degradation of the biomarkers would improve our arguments relating to a climatically driven signal. As such we have now, during the review period, analysed and quantified the autoxidation products of IP_{25} (a) 2,6,10,14-tetramethyl-9-(3-methylpent-4-enyl)-pentadecan-2-ol and (b) 2,6,10,14-tetramethyl-7-(3-methylpent-4-enyl)-pentadecan-2-ol) as the reviewer has suggested. These results will be presented as a separate figure in the manuscript, plotted with the IP_{25} concentrations. As per the Bra/Me-24 data, our results from the autoxidation products suggest that degradation is not affecting the IP_{25} signal in two of the sediment cores (134R and 90R).

Clearly, from the discussion, core 109R is affected by biodegradable products. (from the Bra/24-Me) ratio. You may run some of your fractions again for potential prevalence of autoxidation products of IP25 as well.

As outlined in our previous response, we have run some of our fractions in all cores to identify the autoxidation products of IP_{25} and will present this data in the manuscript.

Also, the gradual decline in brassicasterol concentration in all records could be interpreted as a result of diagenesis.

We agree with the reviewer that this is an important point, so will add this to the discussion about diagenesis.

Perhaps the application of PIP25 is here rather speculative and taken the uncertainties of biodegradation into account, I would suggest to leave it out. You have a visually good correlation with declining sea cover from your observational data set. According to Rontani et al. (2018) this is your strongest argument against significant bio-degradational control.

We agree that the PIP_{25} index results are speculative, however we believe that they provide useful information when combined with discussion of the individual biomarker results. We believe that these values are useful for comparison amongst sites, e.g. to show lowest sea ice cover at site 134R throughout the entire study period. As such, we have used the PIP_{25} index for a small part of the discussion still, however we use the individual biomarkers for the majority of the discussion.

Minor comments

- You mention X-ray fluorescence scanning and grain size analysis in the methods, but you hardly use these data for your interpretation. Consider showing the data actively or omit. X-ray fluorescence data can also provide you with information on diagenesis (redox boundaries).

As XRF and grain size analysis was only used to correlate the gravity (DA17-NG-ST08-092G) and Rumohr core (DA17-NG-ST08-090R) we believe that it is important to include in the methods. However, we don't present this data for the other cores so have not used it to identify potential redox boundaries. We will make the purpose of this data in the methods clearer.

- You may provide more details to your bulk analysis including d13Corg measurements, uncertainties, errors, standards etc.

This will be added to the methodology accordingly (section 3.5).