RC1: 'Comment on egusphere-2023-1327', Stefan Kern, 09 Aug 2023

Review of “Sea ice variations and trends during the Common Era in the Atlantic sector of the Arctic Ocean” by Lindroth Dauner, A. L., et al.

Summary:

This manuscript deals with the compilation, discussion and analysis of a set of marine sea ice proxy records investigating the approximate development of the sea ice cover in the Atlantic Sector of the Arctic Ocean during the past 2000 years - aka the common era. The existing records are analysed by statistical means such as cluster, EOF, and wavelet analysis to find out dominating modes of sea ice variability. The results are compared with climate simulations by two climate models of the CMIP6 group. Analysis and model results are in good agreement. The sea ice cover information inferred from the marine proxy records tends to reflect the general trends well. However, deficiencies are discovered when looking into shorter-term and/or regional fluctuations in sea ice cover as suggested by the marine proxy records. Potential causes of these deficiencies are discussed.

General comments:

I have two general comments.

One is that I found it difficult to figure out what are the contributions and findings of the author team and what is the information that was taken from other sources / previous work. Don't get me wrong here: You cite and give credits to this other work very well but this information is so much mixed with the description of your results that I found it hard sometimes to disentangle these.

> **Reply**: Thank you for the constructive review. We tried to clarify the results and discussion section to separate better the results from the original datasets and our compilations based on clusters (lines 2446-247, 266, 274, 304, 327, 332, 340-341, 355, 357 and 366-367 in the marked up version). See more detailed responses to your comments below.

The second general comment exactly hooks up on this last impression. I was wondering whether the overall results of your manuscript could be worked out more clearly by finding a way to better separate your results and their description / interpretation by you on the one hand from the critical discussion of the results and their limitations plus hypotheses developed based on combining your results with those of previous work on the other hand.

> **Reply**: See previous reply. We think it is important to discuss the original data and methodological discrepancies (see also comments from Reviewer 2), and thus we discuss the original data when interpreting sea-ice trends in the clusters produced here. Presenting the results from the local data, without directly interpreting it regarding its local informational value, would not be very meaningful.

Specific comments:

L67/68++: I can agree that there is truly very limited information about the past sea ice cover in the Arctic; there are however some useful extensions of information about the sea ice cover from the satellite era back into the past - especially in the European Arctic - based on observations. For instance: Divine and Dick, 2006, Historical variability of sea ice edge position in the Nordic Seas, J. Geophys. Res.-Oceans, 111; England et al., 2008, A millennial-scale record of Arctic Ocean sea ice variability and the demise of the Ellesmere Island ice shelves, Geophys. Res. Lett., 35; I am sure there are more.

There are also reconstructions of, e.g. the Arctic sea ice volume, based on numerical modeling carefully tuned to present day conditions (see Schweiger et al., 2019, Arctic sea-ice volume variability over 1901-2010: A model-based reconstruction, J. Climate, 32).

Not sure whether you'd consider it worthwhile to harvest archives for more published work of others in this respect. It might be quite interesting.

> **Reply**: The lines you mention list the very few *compilations of proxy-based sea-ice reconstructions*. It is true historical observations exist and we will mention this in the Introduction. However, because the focus of this study was on sea ice reconstructions for the whole Common Era, we did not include any data that didn't cover at least 80% of it. Historical reconstructions don't fit this criterion and therefore were not included in the statistical analysis. Unfortunately, the ice-shelve reconstruction focused more on the ice-shelve establishment than on sea-ice variability, and on longer time-scale trends. The Arctic sea-ice volume reconstructions from Schweiger et al (2019), on the other hand, dealt with sea-ice changes over the last century, over an annual to decadal timescale. Therefore, the timescales were not compatible with the criteria set (see methods). However, we included the study of the historical sea-ice edge position in the Nordic Seas in the discussion (lines 517-518 in the marked up version).

L184-186: "Sea-ice extents ... gives the annual extent". In the sea ice community, sea ice extent is defined as the sum of the area of all grid cells covered by at least 15% sea ice. In contrast, sea ice area is the sum of the area of all grid cells covered by any sea ice weighed with the actual sea ice area fraction. From what you describe you seem to have computed the sea ice area and you should name it like this henceforth - aka use "sea ice area" instead of "sea ice extent". You might need to correct this in the entire manuscript.

> **Reply**: Indeed, we used the sum of the area of all grid cells covered by any sea ice weighted with the actual sea ice area fraction. Thus, the terminology was corrected to "sea ice area" throughout the manuscript. Thank you for pointing it out.

Figure 1: Please note the source of the bathymetry used and also note where the arrows denoting the currents stem from. If you plotted the latter by yourself you might want to at least state which source you used as blueprint. What is the source of the sea-ice median extent?

Reply: All the sources were included in the revised figure caption.

Figure 2:

- I was wondering whether it would make sense to explain in a bit more detail what a dendrogram in general and in this case tells us.

Reply: We changed “Dendrogram” to “Dendrogram from cluster analysis” in the caption. And a more detailed explanation was added in the items “2.3. Statistical analyses” (lines 181-183 in the marked up version) and “3.1. Proxy-based sea-ice reconstructions” (lines 246-247 in the marked up version).

- I can see that panel b) shows a standardized quantity that somehow seems to be related to sea ice (area?) reconstructions. But the y-axis annotation says "Standard deviation" and the text in the caption speaks of "average composite ... solid ... mean values of all records .... dashed values represent the amplitude". These descriptions seem not to match well with each other and some explanation might be helpful.

Reply: The y-axis label was fixed to “standardized sea-ice reconstructions” and the individual reconstructions were plotted in the figure to improve the understanding. The same was done for Figure 3.

L225: "Thus, an increase ..." --> I agree that this interpretation can be made. I was wondering however, to what degree a change in the dominant ice type, i.e. from multiyear ice to seasonal ice, could also have resulted in an increase in the seasonal sea ice - being interpreted as an overall increase in sea ice - which would be contrary to what we have been observing over the past decades in the Arctic.

Reply: Most of the cores are located in regions that have not been covered, during the Common Era, by multiyear sea ice (line 228 in the marked up version). The only cores located in areas that might have been influenced by multi-year sea ice are cores D and M. And, for these two cores, the change in the dominant ice type is included in the discussion, which was especially relevant for site M (composite core 03TC-41GC-03PC) (lines 355-358 in the marked up version).

L282: "The similarity ..." I am not so sure I go with this similarity here because G1 seems to have an accelerated increase from -1 to +2 until 1700-1800 CE and a rather moderate decrease afterwards while G2 kind of ramps up from -1 to near 0 around 300 CE, followed by a rather linear and comparably weak increase to values around +1 in1500 CE followed by a quite remarkable decrease to values below 0 until present day. The time of the maximum value appears to coincide with a remarkable ramp up in G1 values. This is what I see there and I am not sure this could be explained by large-scale climate forcing that easily.

Reply: We agree that the sentence is a bit vague, and we now described the details more clearly in the revised version (lines 315-322 in the marked up version). There is a common long-term trend from around -1 to +1 until around 1500 (so ¾ of the Common Era) in both

clusters. G2 is less smooth compared to G1 including your mentioned jump at 300 CE and hence includes temporal deviations from the overall similar trend. A clear divergence dominates the last 500 years. We will edit the text accordingly. Partly the reason can be methodological, i.e., the sea-ice maximum was not captured by the IP_{25} proxy.

L289: "It was probably caused by ... of the coast." --> I take this sentence as the example to express the impression that a lot of what is written in this paragraph is less the presentation of results but rather a lot of discussion and hypothetical statements.

Reply: We intentionally chose to merge results and discussion. Representing only the results for such local variations would be very descriptive and we would need to repeat all details in the discussion again, if separated. In our view, these local variations are only interesting or relevant if they are directly explained. Hence the direct inclusion of original references and/or own explanations. We tried, however, to clarify the reasoning behind the “hypothetical statements” throughout the manuscript.

I note that section 3 is indeed entitled "Results and Discussion" but I was wondering whether a more distinct separation of what are your results (i.e. what is new) and what are the points of discussion and hypotheses where you mix in a lot of information from other authors. I find it difficult to focus and lose traction on the results of this paper.

Reply: See previous reply. Our result is the synthesis of local data to identify common vs. local changes which still requires to account for the local information and/or specifics of the sea-ice proxy from original studies. We are wording out “the original authors” when we look at their interpretations of the data; we think it is important to carefully consider the data origins beyond the cluster behaviour. But we changed the wording in some places to highlight better our results/interpretations and the interpretations by the original authors.

If you would try to separate discussion issues more clearly from the results issues it might also become more clear what the different influencing factors as well as the various limitations of the approach used actually are. Among these would be the first-year ice - multiyear ice issue repeatedly mentioned, or issues like changes in the location of the land/ice - sea ice transition zone and thereby in the strength of katabatic winds / polynya existence by changes in ice sheet extent.

Reply: See our response above.

Note: This comment applies also to the previous and the following paragraphs

Table 2: I was wondering whether you at all thought about comparing the output of these two models also against observations of the sea-ice concentration from satellites. There you could only use data from the late 1970ies onwards but it might provide you with an idea whether any of the models is potentially biased in its representation of the sea ice extent (or area?) See my previous comment about your description of how you compute sea ice extent as well.

Reply: We are not aware of any notable link between biases and differences in long-term variations and trends. However, we agree that it is a relevant point to add a verification of the simulations. We hence did such a comparison now (lines 418-424 in the marked up version) and added the comparison between the models' results and the satellite data, for the period between 1979 and 2000. We included the table as Supplementary Material.

Sea-ice area (10^6 km^2) between 1979-2000		Northern Hemisphere			Greenland		
		Minimum area	Annual average area	Maximum area	Minimum area	Annual average area	Maximum area
CESM1	Summer	4.8	6.0	6.6	0.9	1.2	1.5
	Winter	15.0	15.6	16.2	4.1	4.3	4.5
MPI-ESM	Summer	3.6	4.2	4.8	0.4	0.6	0.8
	Winter	14.1	14.6	15.1	3.5	3.8	4.0
Satellite data	Summer	6.2	7.1	8.0	1.7	2.3	2.6
	Winter	15.7	16.2	16.9	6.7	7.3	7.7

Table S2. Comparison between simulated sea-ice areas (in million km^2) between the two models (CESM1 and MPI-ESM) and sea-ice extent (in million km^2) from satellite data (obtained from the National Snow & Ice Data Center website) for the Northern Hemisphere and for around Greenland (see mask in Figure 1) for annual, summer and winter areas, over the period between 1979 – 2000 CE.

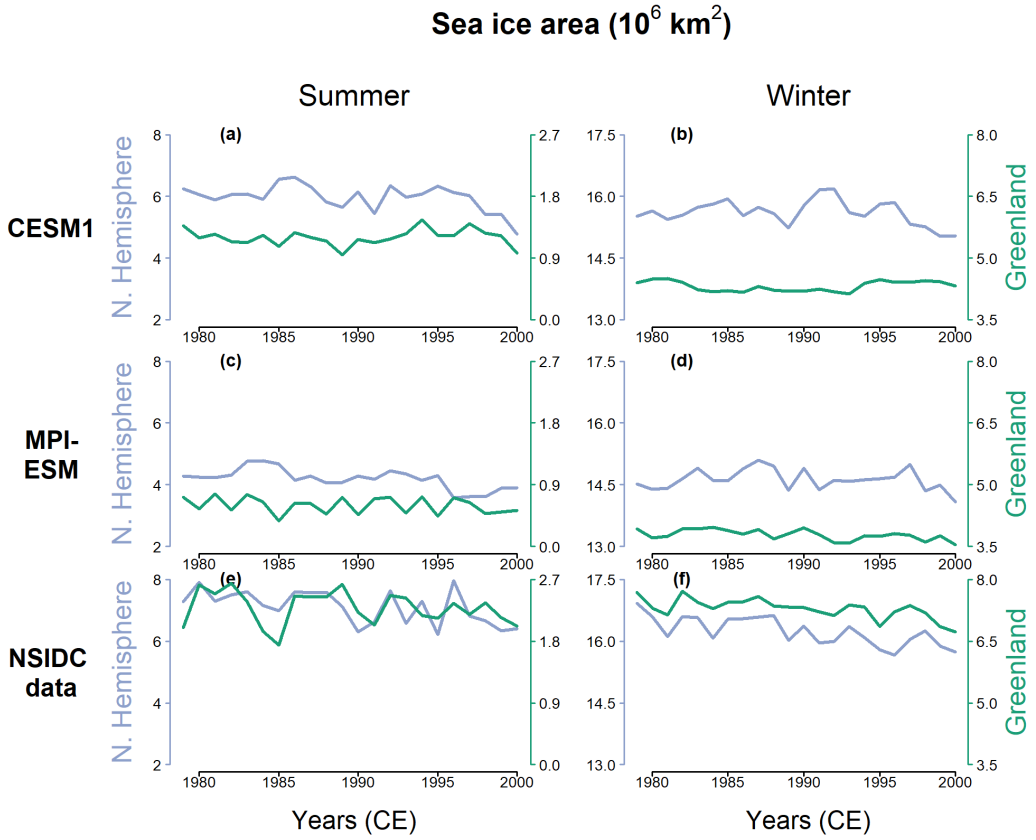


Figure (extra). Sea-ice area (in million km^2) in both models and from satellite data, around Greenland (green) and in the whole Northern Hemisphere (blue), for boreal summer and winter seasons, over the Common Era.

Typos / editorial comments:

Line 47: "preventing heat and moisture transfers" --> perhaps better: "reducing or even almost preventing heat and moisture transfers"

> **Reply**: The text was changed as suggested (lines 47-48 in the marked up version).

Line 95: Suggest to replace "high-resolution" by the actual resolution in years.

> **Reply**: We cannot inform one specific resolution value because each of the 14 sea-ice reconstructions have their own resolution, which also varied through the records. But the average resolution was added to Figures S8 to S14 in the Supplementary Material.

L104 / L141: "original authors." --> Not clear what this means. The co-authors of this manuscript? Are the data associated with this part of the archived data you downloaded or is this additional data. If the latter I recommend that you emphasize this more.

> **Reply**: In the first case (L104), it was additional data that were not found in the searched databanks (now line 106 in the marked up version). In the second case (L141), the original age-depth models were used (now line 145 in the marked up version). In both cases, the sentences were rephrased to improve readability.

L195/196: "offset of 0.5 to avoid zeroes" --> So you are actually working with sea ice concentration data sets that have values ranging between 0.5 and 1.5, is this correct?

> **Reply**: The offset is an arbitrary number to allow for a log-transformation of fractional data for statistical analysis (lines 207-208 in the marked up version). So, the used numbers are the log-values derived from the fractional data across the range 0.5 to 1.5. It is a common procedure when using fractional data like sea ice or similar.

L251/252: "common non-linear trend" --> Two questions here: 1) why non-linear? and 2) would you mind to share the values of these trends?

> **Reply**: The “non-linear trend” is the average composite observed in Figure 2 (line 276 in the marked up version). The values of all the composites were included in the Supplementary Material of the revised manuscript.

L277/278: "since most of the results ... period" --> You could put more emphasis on this issue by providing information and/or referring to the actual time coverage.

> **Reply**: We tried to emphasize the information regarding the cores that do not cover the last two centuries of the Common Era (lines 312-313 in the marked up version).

Figure 5: I strongly recommend to have identical ranges of the extent displayed at the respective axis, i.e. N. Hemisphere Summer sea ice extent has the same axis range, N. Hemisphere, Winter sea ice extent has the same axis range.

Reply: The idea behind the variable Y-axis ranges was to emphasize the similarity between sea-ice evolution from the whole Northern Hemisphere and from the area around Greenland. However, we agree and set the same Y-axis ranges as suggested as it is still possible to observe the similarity between the two regions.

CC1: 'Comment on egusphere-2023-1327', Kirsten Fahl, 03 Sep 2023

Review of “Sea ice variations and trends during the Common Era in the Atlantic sector of the Arctic Ocean” (Lindroth Dauner, A. L., et al.)

As an organic geochemist, my comments will mainly relate to this field of the manuscript.

Summary:

The article of Lindroth Dauner et al. focused on the currently relevant issue sea-ice variation in the Northern Hemisphere during the Common Era (past 2000 yrs). The sea-ice variation studies were conducted using proxy-based sea-ice datasets (already published data) and by different statistical means. In this context, long-term trends and low-frequency variability show different results. While long-term trends of both approaches are in good agreement, the short-term variability results of both approaches are less coherent.

Comments:

- Generally, the manuscript is well written.

-The literature referenced is current.

- The database of proxy-based sea-ice reconstructions is solid and the generation of the data is consistent with established methods of organic-geochemical analysis.

> **Reply**: Thank you for the constructive review.

-The authors have used nearly all relevant proxy-based sea-ice reconstruction datasets currently available for the Common Era in this area. The only data set, I’m missing, is from Core MD99-2275 (Iceland) published by Massé in 2008 (Guillaume Massé, Steven J. Rowland, Marie-Alexandrine Sicre, Jeremy Jacob, Eystein Jansen, Simon T. Belt; Abrupt climate changes for Iceland during the last millennium: Evidence from high resolution sea ice reconstructions. Earth and Planetary Science Letters 269, 564-568).

> **Reply**: We looked at the Core MD99-2275 data. Unfortunately, it does not fit our time coverage requirements (80% of the Common Era), since the data ranges between 800 and 1950 CE. Adding shorter timeseries would deteriorate the consistency of long-term trends as these would then partly depend on data availability rather than only climate. But we now reference this publication in the discussion (lines 298-299 in the marked up version).

- Personally, I would have liked to see some additional explanations of the figures so that also non-specialists could somewhat better evaluate the inferences made based on the results. Thus, at its

present stage, it limits the readership, even though the topic is certainly of interest to a broader community due to its topicality.

Reply: See our responses to Reviewer 2. We changed axis title names and add explanations to the captions.

- In the text, the authors mention the Little Ice Age several times. It would certainly be helpful if such events would be highlighted in the figures (e.g., fig. 2d). As an example, see Kolling et al. (2017) figs. 2 and 6. The same applies to any recognizable warming events such as the Medieval Climate Anomaly (especially since Wang et al. 2022 is cited).

Reply: We added the indication of the Little Ice Age and the Medieval Climate Anomaly in Figures 2 and 3.

- The authors have critically discussed the differences between the matches (or non-matches) of the results of the proxy-based datasets and the modeling results of the long-term trends and short-term variability and substantiated them with recent publications.

Reply: Thank you for your kind comment. We tried our best.

-The authors have also not failed to point out the problems of interpreting different proxies, which are, for example, due to different habitats and different seasons of reproduction of the producing/synthesizing organisms and are additionally influenced by sea-ice independent parameters. In this context, I would additionally recommend Spielhagen et al. 2011 (MSM5/5-712; Enhanced Modern Heat Transfer to the Arctic by Warm Atlantic Water, 28 January 2011, vol. 331, Science) for the discussion, even though foraminifera are not used as a proxy in this manuscript.

Reply: We added the suggested reference in our discussion (lines 318-319, and 332-334 in the marked up version).

- From the proxy point of view, it would be desirable if at least one proxy record as an example of each of the three groups would be transferred from the supplement to the main body of the manuscript.

Reply: To avoid bias when choosing the only one proxy record per group, we added all the standardized proxy records in Figure 3. For the individual records with their identification, the readers are then referred to the Supplementary Material.

One last comment to Chapter 2.1. (line 131-133): ....."When available"..... In case of “not available”, does this mean, that you have used different units in some cases! This is not the usual procedure.

Reply: In those cases where the data was not normalized by TOC, we used the concentration data (µg/g dw or ng/g dw). We changed the sentence to make the information clearer (lines 135-136 in the marked up version).

RC2: 'Comment on egusphere-2023-1327', Dmitry Divine, 25 Sep 2023

Review of “Sea ice variations and trends during the Common Era in the Atlantic sector of the Arctic Ocean” (Lindroth Dauner, A. L., et al.)

Summary:

The manuscript presents a compilation of marine sea ice proxies in order to study the development of sea ice conditions in the northern NA sector over the Common era. Common variability present in the proxy series is studied using clustering. Time variability on various timescales is further analyzed using wavelets. Results inferred from the analysis of proxy data are compared against PC of transient climate and sea ice simulations from MPI-ESM and CESM1 fully coupled models. Both proxies and models share common multicentenntial to millennial scale tendencies, but diverge on shorter timescales. Causal factors for the these discrepancies are discussed.

In addition to the valuable points already highlighted by two other reviewers, I would like to suggest the authors to pay attention to a few more comments indicated below:

> **Reply**: Thank you for the constructive review and additional comments.

Comments:

Line 60: “Most human observations of sea ice are derived from satellites and date back only to the 1970s”

The authors are advised to check the older publications of Vinje et al., 2001 (J Climate , V14, p 255-), and Divine&Dick2006 (doi:10.1029/2004JC002851) where sea ice retreat after 1850 based on historical sea ice observations in the Nordic seas is discussed.

> **Reply**: We now mention historical sea ice observations and the references in the Introduction (lines 59-60 in the marked up version), and also in the Discussion (Section 3.3) (lines 517-518 in the marked up version).

Line 85: “…an integration of existing sea-ice reconstructions in a systematic way…” consider adding “for the entire northern North Atlantic”, since one can find earlier publications where regional scale compilations of sea ice proxies were made.

> **Reply**: The sentence was completed as suggested (line 86 in the marked up version).

Line 91: "… large model biases exist in Arctic regions, attributable to complex feedback framework…" consider adding "… and remaining deficiencies in the implementation of sea ice in GCMs… "

> **Reply**: The sentence was completed as suggested (lines 92-93 in the marked up version). We now also included a verification of the two used models vs. observations as suggested by the other reviewer.

Line 108: F. cylindrus was demonstrated to be rather a cold water species than the one associated with sea ice (see Oksman et al., 2019, von Quillfeldt (2004)). You mention it later in the text, anyways, so what was the point to list it here as one of the indicator species?.

> **Reply**: This is a good point, in some locations *F. cylindrus* could be a relevant sea-ice proxy and that is why we added it in the initial data search. But since we did not find these data, we edited the text as suggested (lines 111, and 124-127 in the marked up version).

Line 164: "These represent the percentage of ice cover in each grid square and both variables are on atmospheric grids"

"percentage of ice cover in each grid square" is associated with sea ice area.

Why atmospheric grid (lower 2 degree resolution) is used for the analysis of sea ice data, while you state earlier that the model has a ~1° resolution in the ocean and sea?

> **Reply**: In the CESM1, the sea-ice model is coupled more "tightly" to the atmosphere and land models than to the ocean model, in order to better resolve the diurnal cycles (Craig et al., 2012). As we focus on long-term variability and trends, the spatial resolution for the analysis is not that important as detailed local variability is not the main interest here.

> Craig, A. P., Vertenstein, M., and Jacob, R.: A new flexible coupler for earth system modeling developed for CCSM4 and CESM1, Int. J. High Perform. Comput. Appl., 26, 31–42, https://doi.org/10.1177/1094342011428141, 2012.

Line 191: Some details on wavelet analysis are missing. Have you detrended the records prior to the wavelet analysis? Were the series resampled (I assume so), and to which common time increment? How the autoregression in the series was estimated prior to significance testing?

> **Reply**: During the analysis, the data was internally detrended. For that, a degree of time series smoothing of 0.75 was used over 100 simulations. For the surrogate time series, we used an autoregressive model (AR-1). The time resolution for the model data was 1 year and the time resolution for the proxy data varied among the records. We added more details about all the settings used in the wavelet analysis (lines 199-204 in the marked up version). The time resolutions for each proxy record were added to Figures S8 to S14 in the Supplementary Material.

Line 195: What was the point in log-transforming the data prior to analysis?

Reply: It is common practice to log-transform fractional data for statistical analysis to avoid the sharp cut-off at 0 and 1 that does not exist for the other variables we compare to (both proxies and simulated temperature etc.). Log-transformation helps to normalize the data distribution and reduces skewness and heteroscedasticity to make it comparable with non-fractional data (lines 208-210 in the marked up version).

Line 208: Diatoms; as an option MD99-2322 series with a relatively good resolution: Miettinen, et al., doi:10.1002/2015PA002849.

Reply: We looked at MD99-2322 data, but only the "sea ice concentration" data is available, and not the abundance of diatom indicator species. To avoid the input of more bias sources, we decided not to use quantitative estimates of reconstructed sea-ice concentrations and sea-ice duration.

Line 223: "…group refer to increases in sea-ice cover." As the authors notice later, for IP25 with its unimodal response to sea ice, the presence of perennial ice cover leads to the opposite tendency.

Reply: Indeed, that is an issue related to the interpretation of IP_{25} data. However, because most of the sediment cores were not retrieved in regions with perennial ice cover, the unimodal response of IP_{25} to sea ice was kept only in the discussion (lines 356-357 in the marked up version).

Line 284: it is not a "binary behavior", rather a manifestation of a unimodal response model quite typical for many climate proxies. See e.g. in Oksman et al., (2019) in Section 2, or elsewhere on quantitative methods of paleoreconstructions.

Reply: The term "binary behaviour" was replaced by "two end-member scenarios for absent IP_{25}", as used in Belt (2018).

While it is true that microfossil species typically have unimodal responses to environmental variables (optimum and tolerance), increases in sediment IP_{25} concentration is generally associated with seasonal sea ice but not quantitatively. Moreover, the absence of IP_{25} can indicate either absence of sea ice or perennial sea ice, described with "two end-member scenarios" or "binary" (Belt, 2018, 2019). Thus, we do not think unimodal is the correct response model here, but we changed "binary" to "two end-member scenarios for absent IP_{25}" (lines 325 and 357 in the marked up version).

Belt, S. T.: Source-specific biomarkers as proxies for Arctic and Antarctic sea ice, Org. Geochem., 125, 277–298, https://doi.org/10.1016/j.orggeochem.2018.10.002, 2018.

Belt, S. T.: What do IP25 and related biomarkers really reveal about sea ice change?, Quat. Sci. Rev., 204, 216–219, https://doi.org/10.1016/j.quascirev.2018.11.025, 2019.

Line 287: "… switch from long seasonal sea-ice cover to a multiyear sea-ice scenario" i.e. transition to a perennial sea ice cover in the area

Reply: The sentence was fixed as suggested (line 328 in the marked up version).

Line 304 "…Therefore, it does not have enough temporal coverage to register a potential IP25 decrease. "

Or this could actually be due to a more southerly location of the record. This is a very dynamic area close to the oceanic front, and spatial surface gradients are large.

Reply: Unfortunately, this line of discussion does not work because core C (MSM05/5_723-2) was retrieved from a slightly northern location than core B (MSM05/5_712-1). Figure 3 was fixed to avoid this misinterpretation.

Line 309 see my earlier comment, this is not a "binary behaviour"

Reply: As mentioned earlier, the term "binary behaviour" was replaced by "two end-member scenarios for absent IP_{25}" (lines 325 and 357 in the marked up version).

Line 324: "One possible explanation for this difference is the impact of warm waters on the melting rate of the drift ice". What is actually implied here? **Stronger oceanic fronts** in the area? Higher sea ice export to the area for this particular time period?

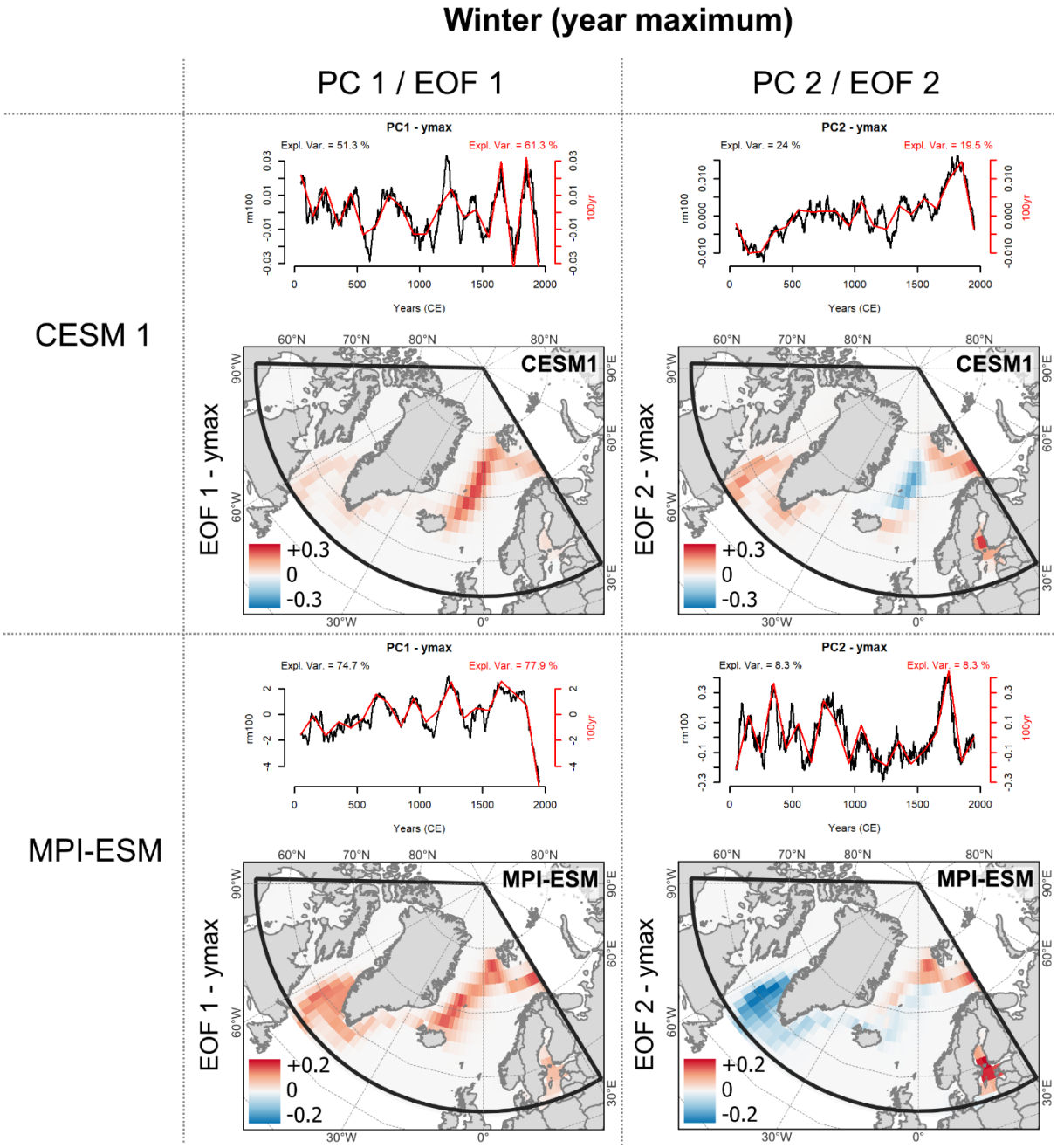
Reply: We are referring to the presence of stronger oceanic fronts. More specifically, the presence of stronger currents carrying warm waters in the Denmark Strait between 1450 and 1650 CE. Because sediment cores JR51-GC35 (record F) and MD99-2269 (record G) were collected in regions relatively more protected than sediment core MD99-2263 (record H), they were not as influenced by this intrusion of warm waters as the area of the sediment core MD99-2263 (record H). This sentence was rephrased to improve clarity (lines 372-374 in the marked up version).

Line 393: "mean chunks" consider changing to "data segments"..

Reply: The term "data segments" would be quite unspecific. "Mean chunks" are meant to highlight that non-overlapping mean segments are used mimicking the samples of proxy records. We defined the expression in more detail when first using it (lines 193-194 in the marked up version).

Line 394: Refs to figures in Supplementary. Remarkable that CESM1 EOF1 shows very little loading south of Greenland and in the Labrador sea compared to MPI. What could be the reason for this, lack of (sea ice) related variability in the region, or it just went into another, presumably second EOF?

Reply: Since it's about the area south of Greenland, we assume this question is about the winter season (year maximum area). We checked, and the sea-ice variability was not "transferred", or better captured by the second EOF (see figure attached – "EOF1_2_CESM_MPI_winter.pdf"). The apparent lack of sea-ice variability on southern Labrador Sea in CESM1 might have been caused by two reasons. One is an artifact of the plots' scales: EOF1 ranges from -0.3 to +0.3 in CESM1, but from -0.2 to +0.2 in MPI. Another possible explanation is that the sea-ice variability in the Greenland Sea is simply stronger than in the southern Labrador Sea in the CESM1 than in MPI model. As we focus on the large-scale variability patterns, inter-model differences in local variability will have little impact on long-term variations.



Line 411: “close to Greenland east coastline” better change to the "Greenland sea", as "close to east coast" can be interpreted ambiguously

Reply: It was corrected as suggested (line 472 in the marked up version).

Line 416: "...which affects...that reaches Greenland sea. " Not clear what the authors meant here. I suggest a full stop after the "the Fram Strait" an leave out the rest of the sentence.

Reply: It was corrected as suggested (lines 477-478 in the marked up version).

Line 419: “..PC1 in both models contains some cyclicity…”. Better use the term “quasi periodic variability” for this particular case, not "cyclicity". Same in line 421

Reply: It was corrected as suggested (lines 481-483 in the marked up version).

Line 455 “…the variability was mostly concentrated in periods around 30 and 50 years.”

Since some information on wavelet analysis is missing, my thoughts below can be a bit speculative, but good to check it anyways. This "statistically significant" variability emerges only for some shorter periods when the data sampling is high enough to resolve any changes at these time scales. For significance testing, most likely an AR1 model was used based on autocorrelation coefficient estimated from the data directly. Due to data resampling/interpolation the autocorrelation will be overestimated, hence leading to lower CI 95% ranges at shorter timescales. With such background model for the analyzed time series, sporadically emerging variability at shorter time scales will be very much likely identified as significant anyways, yet being an artifact of testing procedure.

I therefore would suggest the authors to consider how meaningful is to make any numerical comparisons for the sub-centennial timescales of variability between the proxies and models since a temporal resolution of the proxy based reconstructions for most of the records is fairly low.

Reply: We agree that sub-centennial variability may not be very reliable in proxy data which we mention two sentences later and again in line 508 below (now lines 522-526, and 580-583 in the marked up version). The question to which extent it is meaningful is part of why we included it in comparison to climate models, albeit only as a side topic with supplementary figures.

As part of explaining the details of the wavelet analysis in more detail in the revised version (lines 199-204 in the marked up version), we added an additional sentence on the issue. We interpolated all proxy records onto a regular timescale using a linear interpolation before performing the wavelet analysis. So, the wavelet does not directly “know” about data sampling issues but indirectly suffers from the overestimated persistence from the interpolation. Obviously, as you correctly assume, the AR-1 test will also suffer from an overestimated autocorrelation due to the interpolation procedure. This is a common problem with all kinds of proxy data. The deteriorated signal-to-noise-ratio in combination with overestimated persistence would, however, rather underestimate significance and can be assumed to be on the conservative side. As we do not focus on sub-centennial variability in proxies in the main text, we think it is acceptable to keep the significance test as is in the

supplementary figures. But we expanded the explanation of the issue in the main text (lines 526-532 in the marked up version).

Line 508. "…variability and noise in the proxy data have a marked impact on short-them variability" Definitely in this case the lack of temporal resolution in the proxy series as well as dating uncertainties should be mentioned.

Reply: Indeed. Thus, we expanded the explanation as written above (lines 526-532 in the marked up version). We also mentioned in the conclusion (lines 580-582 in the marked up version) the importance of poor temporal resolution and dating uncertainties on the interpretation of short-them variability which is why we focus mainly on low-frequent variations in this study.