Editor decision: Publish subject to minor revisions (review by editor)

by Sebastian Gerland, 10 Jan 2024

Public justification (visible to the public if the article is accepted and published):

Dear Ana Lucia Lindroth Dauner,

thank you for the revision of your manuscript "Sea ice variations and trends during the Common Era in the Atlantic sector of the Arctic Ocean". With the changes you made, in the next step only minor revisions would be necessary. Please see the new comments by the two reviewers and take them into account in your new revision.

Best regards

Sebastian Gerland

> **Reply**: We appreciate the additional comments and took them into account in this latest version. The lines refer to the marked-up version.

Report #1 - Anonymous referee #1, Submitted on 20 Dec 2023

Summary:

Dear author team,

thank you for taking into account the review comments provided. I only found a few technical / minor things that you might want to correct and/or comment on.

Specific comments:

L40 / L43 (as an example, please check the entire manuscript): I find "palaeo" and "paleo" ... what is correct?

> **Reply**: We kept "palaeo" to keep consistent with the British spelling (lines 43, 95 and 553 in the marked-up version).

L110: You explained the meaning of IP25 already in L78; perhaps it can be deleted here?

> **Reply**: Done as suggested (line 110 in the marked-up version).

L330: While you give Smith and Barber as a reference here I was wondering whether this statement really holds the way as written. The two main polynyas around Greenland at the NOW = the North Open Water polynya which forms regularly southwest of Nares Strait and the NEW = the North-East

Water polynya close to the Fram Strait where grounded icebergs in combination with perennial sea ice block the everlasting southward ice export through Fram Strait. These are just two comparably small polynya areas - in contrast to, e.g. the Eastern Antarctic where polynyas are really abundant, or the Arctic flaw lead / polynya system. Given the forcing conditions on Greenland's eastern side I suggest to stress here that the polynyas you hypothesize to have formed in the past all formed along Greenland's western side.

> **Reply**: Done as suggested (line 330 in the marked-up version).

L412-418: If you would have used sea ice area data (from satellite observations) like available from https://www.cen.uni-hamburg.de/icdc or from https://met.no then you would have been able to make a clean 1-to-1 comparisons instead of rambling about one is area but the other is extent. This is a bit sub-optimal.

> **Reply**: We have now used the sea ice area data from https://www.cen.uni-hamburg.de/icdc to compare the numerical model results to the satellite data (lines 418 – 424 in the marked-up version).

L555: Looking back at the discussions provided, I note that you did not take into account any potential changes in the snow cover on top of the sea ice. Snow also influences light availability under the sea ice quite a bit. Possibly you have a reason for this?

> **Reply**: We have added snowpack thickness into the discussion (lines 553 – 555 in the marked-up version).

Report #2 - Referee #2: Dmitry Divine (dima@npolar.no), Submitted on 10 Jan 2024

Summary:

The authors adequately addressed the comments/suggestion made to the original version of the manuscript. Please find below a few more generally minor comments I would suggest the authors to consider before the draft can be published. The line numbers refer to the new version of the document with changes highlighted.

Comments:

Line 300: “Interestingly, G1 demonstrates a higher rate of sea-ice increase from 1600 CE until the LIA, which is not apparent in the general trend (Figure 2).”

Period of 1600 until late 1800s is associated with LIA, so the statement "from 1600CE until the LIA" needs to be corrected/rephrased. Otherwise, it appears from this statement that LIA is associated with a specific time point, not a period.

> **Reply**: We rephrased the sentence as suggested (line 295 in the marked-up version).

Line 423: “The satellite data, on the other hand, is the sea-ice extent, which considers the sum of the area of all grid cells covered by at least 15% of sea ice.”

Why did the authors then used the extent, while the area is also available directly from the NSIDC SSMI data? This would make the comparison way more consistent. For winter the difference between the two metrics will not be very substantial, but in the summer this is not the case. I recommend the authors to switch to similar variables.

> **Reply**: As suggested by the other referee, we now have used the sea ice area data from https://www.cen.uni-hamburg.de/icdc to compare the numerical model results to the satellite data (lines 418 – 424 in the marked-up version).

Line 523: “This difference is likely caused by internal climate and ecosystem dynamics where proxy-based sea-ice reconstructions were affected by complex environmental parameters, while the physical models directly responded to large-scale atmospheric forcing from variations in solar, volcanic, and orbital forcing. Furthermore, while the proxy records used here have relatively high resolution (< 100 years), the resolution is generally not enough to capture multidecadal oscillations.”

In addition and in my personal opinion, are likely most important, chronology errors of various nature (core sub-sampling itself, delta R uncertainty, depth age modelling method and the associated uncertainty etc) is also a serious obstacle when comparing various oceanic proxy series on subcentennial time scales.

> **Reply**: We have now addressed the chronology errors in our discussion (lines 525 – 528 in the marked-up version).

Line 531: “dominance” appears twice.

> **Reply**: The repeated word was removed (line 532 in the marked-up version).

Line 531: “…while periods with high resolution confirm the dominance of dominance of multidecadal variability …“

The authors can easily estimate how correct this statement is directly from wavelet analysis by summing the variance over the band of timescales corresponding to multidecadal variability. See example in Torrence and Compo, (1998). It is also useful to indicate (again) that this dominance applies to the timescales over a decade, since the analysis is applied to filtered series with sub-decadal variations removed. Otherwise, annual to intra-annual variations generally contribute most to the total variance.

> **Reply**: This sentence refers to the wavelet analysis based on the proxy records, which were not filtered to remove the sub-decadal variability. Most of the sediment core resolutions don’t allow the analysis of sub-decadal variations. But we reinforced that the model data for the wavelet analysis was filtered to remove the sub-decadal variations. And the variance over the band of timescales is represented in panels C in the Supplementary Figures S8 to S14. The

periods marked with the red dots in those panels represent the same as the periods above "the upper dashed line is the 95% confidence spectrum" in Torrence and Compo, (1998).

Line 535: "The spatial distribution of EOF2, associated with the multidecadal to centennial-scale fluctuations…"

Better, in my opinion, refer instead to a "Spatial structure of EOF2" or a "Loading pattern".

Reply: Changed as suggested (lines 475 and 537 in the marked-up version).

Line 537: "This inconsistency is probably caused by the low explained variance of PC2 (15% for CESM1 and 5% for MPI-ESM), incorporating a large degree of residual noise. Although the multidecadal and centennial variability of Arctic summer sea ice has been linked to changes in the northward Atlantic and Pacific heat transport and in the Arctic dipole pattern, there is still some significant variability between mean states of Arctic sea ice simulated by different models (Li et al., 2018)."

Another possible explanation is purely methodological, namely the orthogonality, by definition/construction, of EOFs in the basic EOF analysis. It means, in plain words, that EOF1 has a unipole spatial pattern, EOF2 - bi-pole, EOF3 4-pole etc. For the region used sea ice is mainly present on both sides of Greenland, this is where EOF2 will form its two poles with loadings of the opposite sign.

Reply: This explanation was added in our discussion (lines 539 – 541 in the marked-up version).

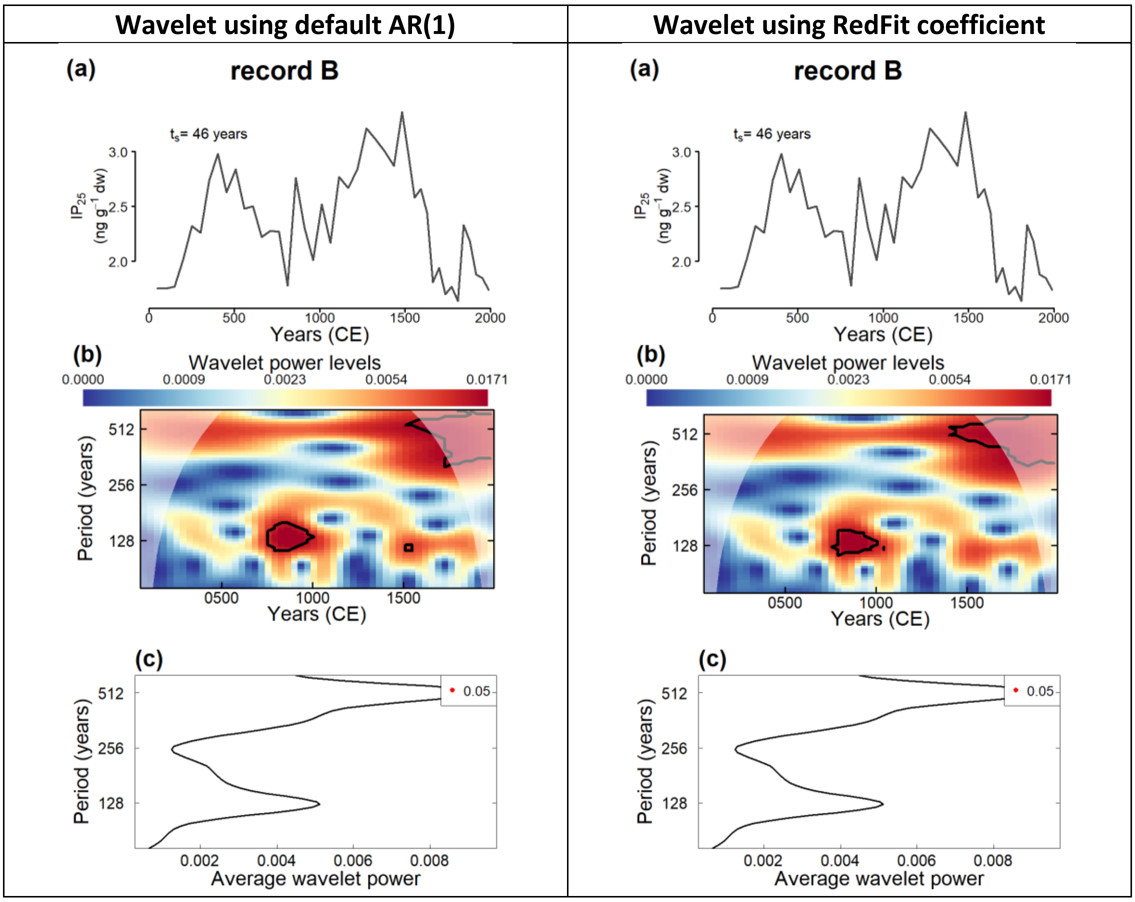
Line 577: "The recent sea-ice retreat, on the other hand, was likely initiated by a recovery from the volcanic dust emissions…"

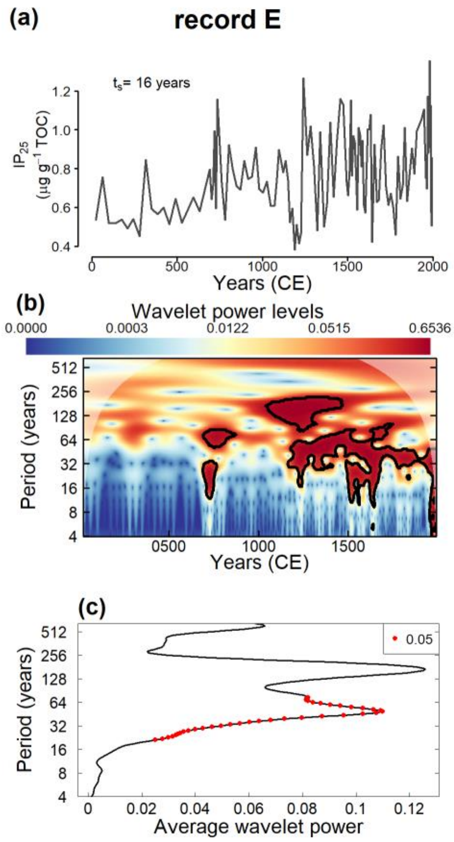
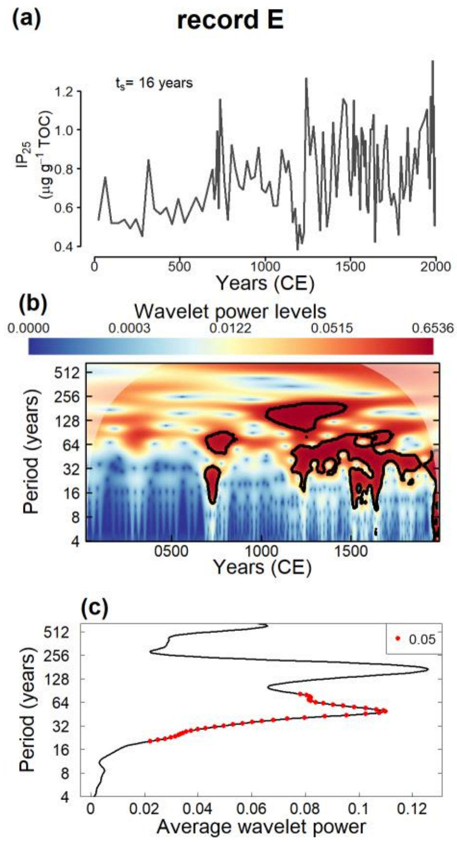
Better rephrase to "recovery after the LIA", since volcanism might have triggered a number of feedbacks that caused lasting negative SAT and positive sea ice anomalies that are now associated with the LIA manifestation. From the way it is written now, an impression of a direct liner response to continuous volcanic dust emission emerges.

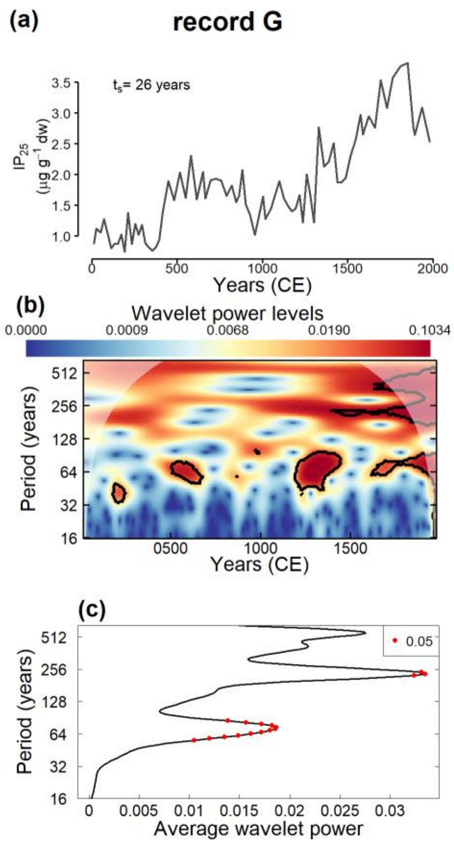
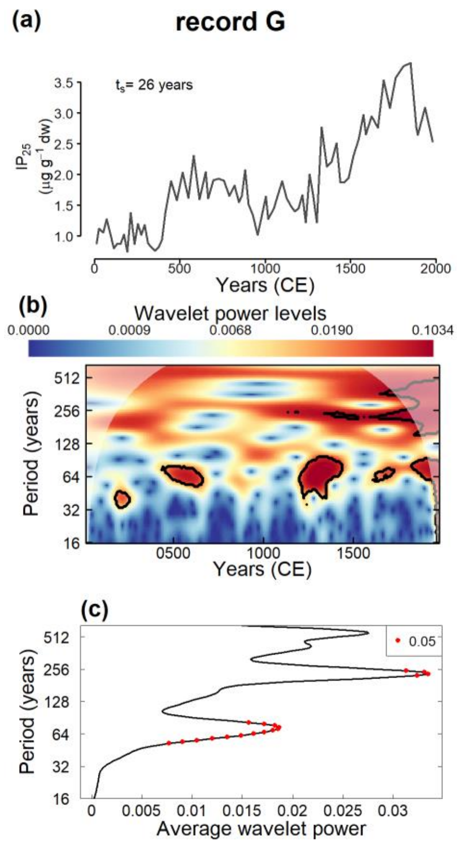
Reply: This sentence was changed as suggested (line 582 in the marked-up version).

And the final comment concerns the hypothesis testing in wavelet power spectra (Starting from line 527). In their response letter the authors agree that resampling to the annual scale causes unrealistically high values of the AR-1 coefficient used in hypothesis testing. The authors, however, are not correct stating in the response that "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." Due to a redistribution of variance towards lower frequencies in the AR1 series with a very high autocorrelation coefficient (very close to 1), the significance threshold for hypothesis testing will be underestimated in the higher frequency range, and overestimated in the lower frequency range. The actual configuration will depend on the AR(1) value of the background process that could generate the observed series. I therefore consider that the use of thresholds for significance testing calculated from the resampled data is not correct. Instead, I recommend the authors (if they would like to retain the discussion that involves significance testing from wavelet spectra of the proxy series) to use the AR(1) coefficient estimated from the original unevenly sampled data. Since the authors used the R environment in their calculations, it should not be a problem to use RedFit method (Schulz and Mudelsee, 2002) for this purpose. The implementation of RedFit can be found in dplR package (https://cran.rproject.org/web/packages/dplR/dplR.pdf). Once the AR(1) coefficients estimates are obtained, you can disable the automatic calculation of AR1 in wavelet power spectra computation/analysis procedure and type in the RedFit estimated coefficients instead. This will provide you a way more fair view on the wavelet spectra of proxy series.

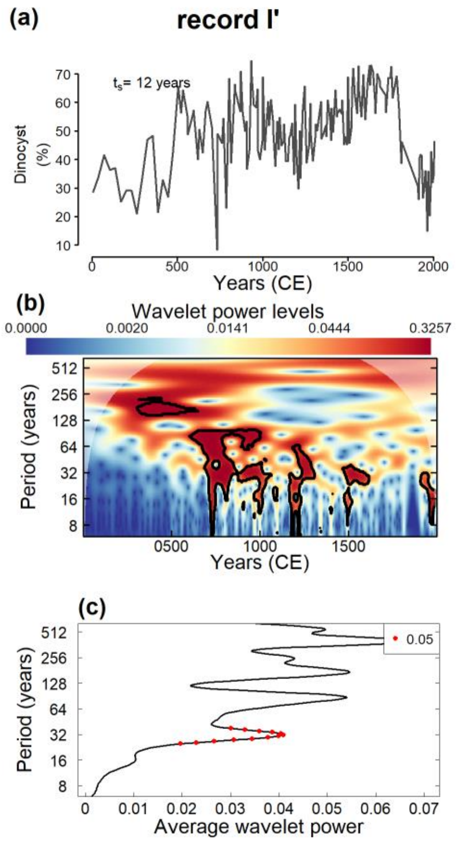
> **Reply**: We tested the RedFit method using the dplR package for a few records, and re-run the wavelet analysis using the coefficient estimated by the Redfit function, to compare with the default AR(1). As you can see below, there is not much difference between using the default AR(1) coefficient and the coefficient estimated for the original unevenly sampled data (RedFit function).
>
> Because there are no significant differences and because we do not discuss in detail the significant periods observed in each proxy record, we consider that using the automatic calculation of AR(1) coefficient is enough for our analysis.

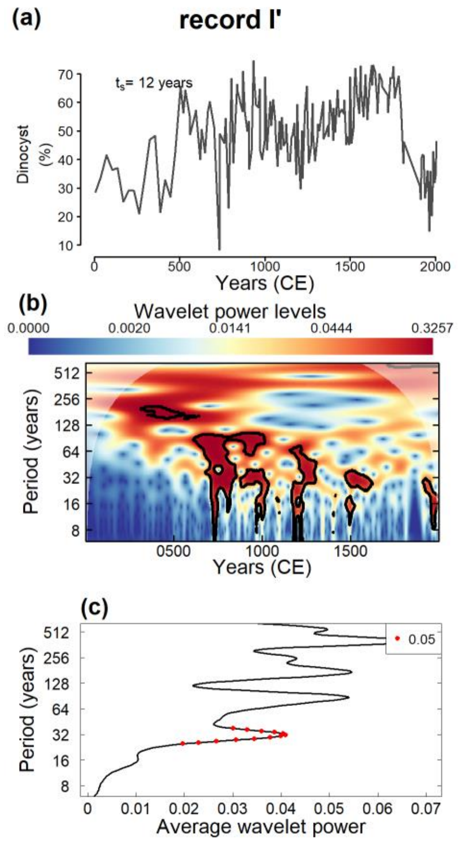


Wavelet using default AR(1)	Wavelet using RedFit coefficient
	

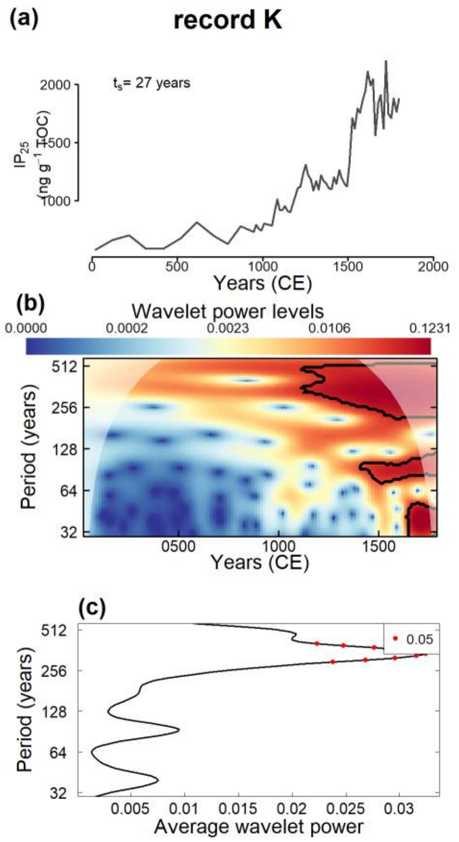
Wavelet using default AR(1)	Wavelet using RedFit coefficient
	

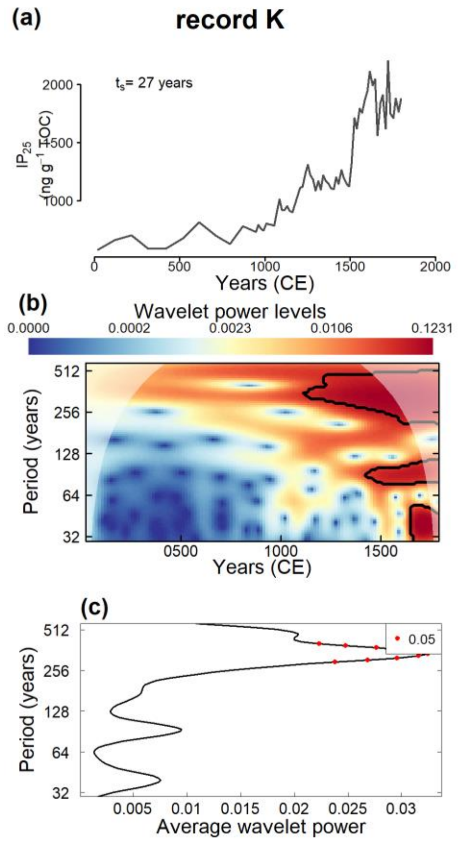
Wavelet using default AR(1) | **Wavelet using RedFit coefficient**





Wavelet using default AR(1) | **Wavelet using RedFit coefficient**





References:

Schulz, M. and Mudelsee, M. (2002) REDFIT: estimating red-noise spectra directly from unevenly spaced paleoclimatic time series. Computers & Geosciences, 28(3), 421–426.

Torrence, C., and G. P. Compo, 1998: A Practical Guide to Wavelet Analysis. Bull. Amer. Meteor. Soc., 79, 61–78, https://doi.org/10.1175