Dear Referee, Thank you for the time that you have spent on our manuscript. We are happy with your positive response and grateful for your comments and suggestions. These certainly contributed to improving the quality of our manuscript.

Below you will find a summary of the changes that we have made throughout the manuscript to address all your suggestions. The replies to your comments are written in blue, while your comments are reproduced in black. Please, notice that line, page, and figure numbers mentioned in our rebuttal letter refer to the new version of the manuscript.

Yours sincerely and on behalf of all the co-authors,

Guillian Van Achter

## Anonymous Referee #1

## GENERAL OVERVIEW

This brief communication analyses the variability of Arctic sea ice thickness in pre-industrial, historical and future climate simulations from the CESM1(CAM5) coupled model. Both temporal analysis of the timeseries of sea ice volume, and spatial analysis of the sea ice thickness are presented, and results from the two analyses are brought together in the discussion. The main findings are that this model shows two peaks of temporal variability (8 and 16 years) in the pre-industrial simulation, which persist in the historical simulation, and until the middle of the 21 st century. The first mode of spatial variability is a dynamic mode related to the AO, and corresponds to the 8 year peak in temporal variability. Both the spatial and temporal variability change significantly from the 2050s when the summer sea ice is lost.

In terms of the originality, scientific quality, significance and presentation quality I asses this communication as good. The application of wavelet analysis to the SIV timeseries is interesting, and it is really good to see the temporal and spatial analysis brought together in the same piece of work.

Overall, I feel the paper could benefit from

- More clarity in the details of the analysis, and some of the explanations.
- Some improvements to figure 1
- A better bringing together of thoughts at the end of the conclusions.

Again, we thank the referee for her/his time and the detailed revision of our manuscript. We appreciated very much her/his comments, which were all taken into account in the revised version of the paper. Below, we answer point-by-point all specific comments.

## SPECIFIC COMMENTS

Lines 46-50: While the model has been well validated for the mean state of ice thickness and extent, and for the declining in ice extent, it is of course not possible to validate the variability in the ice thickness/volume, and so I think the statement that it can be assumed that the modelled time series is an adequate proxy is perhaps too strong - the assumption is a caveat of the work.

We agree with your comment. We reformulated this statement in the new manuscript version "While it is not possible to validate the data in terms of SIT and SIV variability due to a lack of continuous observational data, the model was well validated in terms of mean state of ice thickness and extent as well as regarding the recent trends in the latter." [pg. 2, 1, 49-52].

Lines 53-54: Was the analysis done using just one of the historical and future climate ensemble members? I assume so, but it would be good to clarify this. As an aside, it would be interesting to know how robust this analysis is if it is applied to different members of the ensemble.

Since there is only member that spans from 1850 to 2005, we had decided to use only one member of the historical and future climate periods for the analysis. Thanks for your suggestion, we have now performed the multi-ensemble analysis for the study of both the temporal (wavelet) and spatial (EOF) variabilities. Please, notice that your comment agrees with one of the comments from the 2nd Referee, which also flagged the possible lack of robustness of using one member only.

We tested the robustness of our one-member EOF and wavelet analysis compared to the 30 other ensemble members. Since only one historical member is spanning the 1850-2005 time period, the historical period is now 1920-2005. For this analysis, we removed the ensemble mean from each member to obtain detrended SIT anomalies. In order to apply the EOF to the 30 members, the members were appended together over time (this method has been suggested by reviewer #2 and has already been used in the literature (Labe *et al.*, 2018)).

Figure 1 presents the first three modes of SIT variability over the historical (1920-2005) period. The modes are similar to the one of the study for the historical period (1850-2005). Figure 2 presents the first three modes of SIT variability over the future (2005-2050) time period. The first mode is similar, the second has the same pattern with small differences and the third has a different pattern of variability.

We conclude that our results for the EOF analysis over one-member are robust with the other ensemble members. The first and second modes that were described in the previous version of the manuscript are still present in the historical analysis over 30 members and the first one is still present in the future analysis. In the new manuscript, the EOF analyses for historical and future periods have been changed from a one-member to 30-member analysis.



Figure 1: Modes of Arctic SIT spatial variability. First (a), second (b) and third (c) EOF of Arctic SIT over the historical period (1920-2005). EOFs are performed over 30 ensemble members by appending them over time before applying the EOF analysis.



Figure 2: Modes of Arctic SIT spatial variability. First (a), second (b) and third (c) EOF of Arctic SIT over the future period (2005-2050). EOFs are performed over 30 ensemble members by appending them over time before applying the EOF analysis.

We also performed an ensemble-analysis for the temporal variability by applying the wavelet individually to each member. Afterwards, we averaged the results together (following referee #2 suggestion - Figure 3). Averaging the spectrum does not seem to be appropriate. It smoothed out any interesting information in the wavelet power spectrum (temporal-variability of

the peaks), the stationarity of the SIV is not distinguishable in that form. By examining the wavelet analysis from each of the 30 members, we noticed that, for most of them (28 out of 30), 2 peaks of variability are significant and easily recognisable, one around 5-10 yrs and another around 15-25 yrs. Figure 3 presents all peaks that are significant for all ensemble members. The number of peaks (black line) shows that, for the 30 members, most of the peaks have a period of either 5-10 or 15-25 years. Because the peaks are not centered exactly at the same periods of variability, the peaks in the averaged wavelet analysis are no longer easily distinguishable.

We conclude that most of the ensemble members have two peaks of variability within the same ranges (5 to 10 and 15 to 25 years) mentioned in the paper. Because wavelet analysis over only one member has a better representation of the non-stationarity of the SIV and because the peaks are more distinguishable, we kept the one-member analysis in the study but we added a paragraph and a subfigure about results robustness with the ensemble members [pg. 6, l. 128-139].



Figure 3: Wavelet analysis applied to the Arctic sea ice volume anomaly over the historical (1920-2005) (a) and future (2005-2100) (b) periods. Peaks of significant variability for 30 ensemble members (c) for the historical period. Wavelet analysis is applied to 30 ensemble members then the results are averaged together.

Lines 59-64: I found this paragraph a little confusing. The removal of the trend and seasonal cycle from monthly data for the SIV timeseries is clear, but I was less sure exactly what was done for the SIT fields before the EOF analysis. Later the analysis of Lindsay and Zhang is mentioned – they used annual mean data, so it would be good to clarify exactly what was done.

This was clarified in the text. "For the variability analysis, the trend and seasonal cycle are removed from the time series (pan-Arctic SIV and gridded SIT) so that we focus on the interannual variability. Since the spatial variability analysis uses 30 ensemble members, the SIT anomaly fields are computed by removing the ensemble mean to each member. When only one ensemble member is used, as for the temporal analysis, the anomaly is calculated by excluding the individual trend (provided by a second-order polynomial fit) of each month." [pg. 3, 1. 67-71].

Paragraph beginning at line 67: I found this paragraph difficult to follow on first reading. I think it would help the reader to start the second sentence in a way that makes it clearer that the discussion will initially focus on the temporal analysis (the start of the following paragraph is much clearer in this regard).

We agree that some clarification is needed here. We have restructured the paragraph to that order [pg. 3, l. 74-75].

Figure 1: I have a number of suggestions that would make this figure easier to follow:

• The discussion in the text refers to the time periods in years, whereas the scale in the figure is in months – it would be easier to follow if the scale was also in years.

The y-axis labels of Fig. 1 (bottom panels) are now in years.

• Perhaps the lines marking the areas of significant variability could be a colour not used in the scale, so that they stand out more. This is especially needed in 1c, where there is more yellow on the plot itself.

The significant levels are now highlighted in red.

• Maybe the 8 and 16 year periods could be marked by horizontal lines (on the panes representing the wavelet power spectrum).

The horizontal lines have been added to the bottom-left panel.

• I'm not sure why the Fourier spectrum is included with the time-integrated power spectrum, as I don't think it is mentioned in the text.

To avoid confusion, we have removed the Fourier spectrum and focused only on the wavelet analysis. [pg. 5, Figure 1].

In addition, the meaning of the hatched area is not explained anywhere.

Indeed, thanks for spotting that. The crosshatched areas indicate the cone of influence where edge effects become important. This is now clarified in the text and in the figure's caption. [pg. 4, l. 95].

Lines 110-111: It looks like the peaks discussed here are not significant? In the discussion of Fig 1a, the 42 year peak is not discussed because it is below the 95% red line. However, all the peaks in the time-integrated wavelet spectrum for Fig 1c are below the red line. Can this be clarified – is it that the integrated value is not significant because the peaks are only significant until 2050

Indeed, the peaks are significant only over the first 50 years, as it is shown in the wavelet spectrum by the red lines. In order to see the significance of the peaks in the time-integrated spectrum, it is required to apply the wavelet analysis over the 2005-2050 period, as shown in Figure 4 of this document. We decided to keep the analysis on the 2005-2100 period in the study since it is a demonstration of the loss of variability around the year 2050. But for clarification we modified the text [pg. 4, L 115-120].



Figure 4: Wavelet analysis applied to the Arctic sea ice volume anomaly over future (2005-2050) period.

Lines 116-7: I am not sure what this last sentence means – could it be clarified please. To make our point clearer in the text, we reformulated this sentence [pg. 5, l. 125-126].

Line 128: Could the sentence starting 'The disparities...' be tightened up a bit -I see what it means, but it sounds rather vague as written.

To make our point clearer in the text, we reformulated this sentence [pg. 6, l. 151-152].

Section 3.2: Mention here that the future period is analysed to 2050. It is now mentioned in the text [pg. 6, l. 142] and in the caption of figure 2.

Section 3.3: I think this analysis is just done for the pre-industrial period? It would be good to make this clear – maybe even in the section title. The analysis in this section is good, but I found the text confusing in places. Were both the thermodynamic and dynamic aspects investigated for each of the first and second modes for example?

Indeed the analysis was only made over the pre-industrial period. We have decided to keep the same section's title, but we made this point clear in the first sentence of the section ([pg. 8, l. 161]). The dynamic and thermodynamic aspects were investigated for both modes (dynamic aspect in Figure 5 in this document) and this point has been clarified in the text [pg. 8, l. 170-174].



Figure 5: Mean sea ice velocity for both high and low indexes of the first and second modes of SIT spatial variability. Analysis is performed over one ensemble members.

Lines 185-186: The analysis of spatial variability presented in sect 3.2 only covers the period to 2050 - perhaps a statement can be added there about the behaviour past-2050. We added a paragraph in the section 3.2 to address that [pg. 6, l. 159-162].

Conclusions: the first paragraphs provide a good summary of the work, but the last couple of paragraphs could be stronger. In the final paragraph do you mean the location of these devices? It would be good if this could be more explicit. We changed the sentence to make it more explicit [pg. 11, l. 220-225]. Also, the conclusion has been changed. New information about the robustness of the result with other ensemble members and the behaviour of the sea ice variability after 2050 have been added to the text.

## TECHNICAL CORRECTIONS

Line 26-27 "They enlightened..." I did not understand the first part of this sentence (relating to the historical and pre-industrial climates) – is the point that the variability for these periods is the same? It would be good if this could be re-worded to be clearer.

Yes, in Olonscheck and Notz (2017), they found the same variability for both periods. By stable, they mean that there is a remarkable similarity between the pre-industrial and the historical internal variability of Arctic sea ice volume. This point was clarified in the text [pg. 2, l. 28-30].

Line 41: It would be good to mention somewhere that this is a CMIP5 model. This information has been added to the text [pg. 2, l. 46].

Line 70: I would suggest mentioning here (with the Torrence and Compo ref) that the Morland wavelet is used, rather than in the caption for Fig 1. The medification has been done in the text and in the forme's caption [ng. 2, 1, 76].

The modification has been done in the text and in the figure's caption [pg. 3, l. 76].

Line 73: It not mentioned here which 200-year period of the 1700-year control simulation is used, although it is clear from Fig 1, maybe mention that it is the 200 years preceding the historical integration. Indeed this was not specified. We added this information in the caption of Figures 1 and 2.

Line 81: I don't think the reason for analysing the shorter period is explicitly mentioned in sect 3.1. Perhaps it can be mentioned in Section 3.2.

We thank the reviewer for spotting that gap, it is now mentioned in Sect 3.2 [pg. 6, l. 141-142].

Line 106: Sentence starting 'Those peaks and bands...' not needed - Fig 1b could instead be referenced in the first sentence of the paragraph. It has been changed [pg. 4, l. 117].

Line 125: Ref Fig2g in this sentence. It has been changed [pg. 6, l. 156].

Line 138: Sentence starting 'As the first mode....', rephrase to emphasise where there is and is not agreement in the behaviour of the first and second modes.

We agree with your comment and have improved this sentence in the text [pg. 6, l. 151-157].

Line 140: I don't think this sentence is needed (We looked at...) Agreed, this sentence has been removed from the text.