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Comment on egusphere-2022-953

Anonymous Referee #2

Referee comment on "On the linkage between future Arctic sea ice retreat, Euro-Atlantic circulation regimes and temperature extremes over Europe" by Johannes Riebold et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-953-RC2, 2022

Review of the article entitled "On the linkage between future Arctic sea ice retreat, Euro-Atlantic circulation regimes and temperature extremes over Europe" by Riebold et al.

General comments:

This study examines how projected Arctic sea ice decline might affect the large-scale atmospheric circulation over the Euro-Atlantic region in terms of frequency of occurrence of weather regimes and temperature extremes. It is based on the analysis of sensitivity experiments conducted with the ECHAM6 atmospheric model within the framework of CMIP6 PAMIP coordinated experiments. Several sets of experiments are analyzed: present-day simulations (pdSI/pdSST) and idealized simulations in which Arctic sea ice is reduced either over the whole Pan-Arctic region (futArcSI) or only in the Barents/Kara Sea region (futBKSI). Each experiment consists of 100 members of 1 year. In order to assess the role of future Arctic sea ice reduction on large-scale atmospheric circulation, five weather regimes over the Euro-Atlantic regions are computed and their relationship with cold and warm temperature extremes is examined. The authors show that the frequency occurrence of three weather regimes, SCAN, NAO+ and ATL is affected by Arctic sea ice reduction. The change in the frequency of occurrence in the model experiments is compared to observations using ERA5. The authors compute the regime occurrence frequency in ERA5 for lower than averaged and above average Arctic sea ice conditions and compare these two situations with the present day and future simulations. Only the Scandinavian Blocking and the Atlantic Ridge regime show some significant results that are comparable to observations and in general only for one month among the 4 winter months. Hence the signal appears to be quite weak and only detectable for specific months. The comparison between futArcSI and futBKSI indicates that most of the frequency changes can be explained by the regional contribution of the Barents/Kara Sea sea ice reduction. The authors then apply a storyline approach using the conditional extreme event attribution framework described in Yiou et al. (2017) to identify the respective contribution of dynamical and non-dynamical changes in the modeled response of extremes to see ice reduction. They show that European cold extremes during winter can be mainly attributed to changes in the occurrence of the Scandinavian blocking as well as to a non-dynamical thermodynamical component. The authors also compare the sea-ice

induced atmospheric changes to global increase of SST to evaluate the importance of Arctic sea ice decline in future climate changes. This comparison suggests that sea-ice decline is of secondary importance compared to future SST change.

The topic of the paper is important because the role of Arctic sea ice loss on midlatitude climate is highly debated and deserves attention. The analysis conducted in this study are very thorough with a comprehensive description of the mechanisms that might be at play in the atmospheric response to sea ice reduction. The paper is well written, I really enjoyed reading it. Some of the figures could have been clearer in particular the statistical significance that is often difficult to see in most figures. My main concern is the fact that the paper is based only on the analysis of one model experiments. While I can understand the value of analyzing experiments from a single model when it is the first time a protocol is used, the experiments analyzed in this paper have been conducted by many climate centers as part of the coordinated multi-model PAMIP within CMIP6 and hence not taking advantage of this unique database is to my opinion a strong weakness of this study. This is even more important that several studies have shown that 100 members were not enough to show a robust response to Arctic sea ice decline (e.g. Peings et al. 2021) and that models may underestimate the atmospheric response to sea ice loss (Smith et al. 2022). The authors themselves state in their conclusion that "the sign and significance of the signals highly depend on the respective month". Having several models and more members would likely have increased the signal to noise ratio and could have allowed to see a more robust response in terms of changes in weather regimes frequency occurrences and the associated temperature extremes. Hence, I strongly recommend extending the analysis conducted in this study to more models before allowing the publication of this paper.

More detailed comments:

l.1-24: The abstract is quite long and dense. I suggest reducing it to better emphasize the novelty of the work described in the paper.

-l.6, l.43, and at many other places in the manuscript, the term Barents/Kara Sea is written Barents/Karasea. I suggest writing Kara Sea with two words.

l.40: A reference to Smith et al. (2022) should be added here as they analyze the wave activity response to Arctic sea ice reduction in about 16 models and provide an emergent constrain based on eddy feedback.

L45: I suggest adding here a reference to Blackport and Screen (2020) who also addressed extensively the lack of consensus about sea-ice induced atmospheric linkages.

-l.60 "effecting" should be replaced by "affecting"

-l.66: “effected” should be replaced by “affected”

-l.75 “ “Climate model simulations typically suffer low signal-to-noise ratio” . It would be relevant to add here two references: Smith et al. (2022) and Scaife et al. (2018).

l.75-77: This sentence would strongly support the use of more than one model to address issues like the one investigated in this paper.

-l.80-90: I suggest adding here a reference to the work of Gervais et al. (2016) and compared the results of this study to those found in this paper.

-l.108 “analysis steps” is repeated twice.

-l.121: I don’t understand why the author refer to this model set up as high resolution as T127 corresponds to about 1º ? Please clarify or provide the resolution in km or degree.

-l.123: Shouldn’t we say “aims at” instead of “aims on” ?

-l.126-127: I suggest adding here that this is exactly what is recommended by the PAMIP protocol of Smith et al. (2019).

-l.157: Can you explain a bit more why it is chosen to merge data from the two experiments to apply the cluster analysis, instead of doing it separately for the present day and future experiments?

-l.164: “for 1000 times” should be replaced by “ 1000 times”

-l.204: Pr is not explicitly defined. Also, I find the notation here with > et < on the same line slightly confusing. It would be easier to follow if warm and cold extreme events definitions were presented separately.

-l.221: “terms” should be “term”

-Figure 1 caption: The acronyms of each circulation regime shown at the top of the panels could be explained in the caption.

-l.257: I suggest adding “Arctic” in the title before “sea ice retreat”

-l.276: It would be useful to add the spatial correlation with ERA5 as a number over each panel of Figure 1.

-Figure 2 could be improved. The labels on the y-axis are difficult to read and could be made larger and/or in bold. The triangles showing ERA5 results are difficult to see when intercepting the vertical bar (red triangle on a red bar, blue on blue). The caption is also ambiguous in some places. The word “Transparent” for the non-significant colored bar is not easy to understand. I suggest using instead “Light” bars to contrast with the “darker” bars. Further, the choice of a 50% threshold to classify ERA5 low and high sea ice conditions does not seem comparable to the present day and future experiments. The authors should justify this choice and use a threshold that is more relevant for the comparison with the model experiments. It would also be useful to add above each bar or in a separate table how many days are used for each regime and each month.

-In Figure 2, the authors choose to show the change of frequency occurrence for each month during the cold season. This results in only few situations where the fut experiments show significantly different results from the present-day experiment. The use of monthly means rather than seasonal means does not seem to be well justified. The use of seasonal means (DJFM) could have allowed to reduce the noise and potentially increase the signal to noise ratio. This is done in Figure 3 so I suggest replacing this figure with seasonal means or better justify the added value of using monthly means here. Further, as described in my general comment, I believe that at least this figure, if not all the figures shown in the paper, should be repeated using all the PAMIP model experiments that have provided daily data to check the robustness of the results and get more significant results.

l. 303-304: The authors describe here a similar feature in ERA5 and in the models for the frequency of occurrence of NAO+ pattern. It would be informative to add whether these results are sensitive to the choice of the 50% threshold in observations. In other words, if a 70-80% threshold is used to defined high sea ice conditions in ERA5, would the similarity with model simulations for NAO+ in February and ATL- in January still hold?

-Figure 3: the dots showing statistical significance are hardly visible and should be bigger or darker. Same remark for the hatching in Fig 4 and 5 and the stippling in Fig6, 7 and 8.

-l.315: the use of “observed” does not seem to be appropriate. I suggest using instead “reported”

-l.328: “descend” should be replaced by “descent”

-l.341: This sentence does not seem to be written in good English. Please check and revise it if needed.

-Figure 4 and 5 and l.355 to 360: here again as in the description of Fig 2, it would have been interesting to comment which result remains robust when repeating the analysis using seasonal means rather than separately for each month.

-Figure 6: I do not see any stippling on this figure so it is difficult to assess which region shows significant changes in cold extremes. Please make the stippling more visible if there are any on this figure.

-l.431 and Fig.9: Results are shown here for NAO- regime. The authors say that similar results are found for other regimes. It would be good to explain why SST leads to similar pictures. Is the frequency of occurrence of NAO- regime favored in FutBK ?

-Figure 9: the caption is a bit confusing as it says that blue can refer to favored occurrences of cold extremes in pdSST/pdSI but reduced occurrences in futSST. I suggest clarifying the text by referring to what is shown on the panels

-l.509. This sentence sounds a bit ackward. I guess the words “and have shown” could be replaced by “that were shown”

-l.515: I suggest replacing “used model” by “model used”

-l.526: “to unique” should be replaced by “too unique”

References:

Peings, Yannick & Labe, Zachary & Magnusdottir, Gudrun. (2021). Are 100 Ensemble

Members Enough to Capture the Remote Atmospheric Response to +2°C Arctic Sea Ice Loss?. Journal of Climate. 34. 3751–3769. 10.1175/JCLI-D-20-0613.1.

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Blackport and Screen 2020, Insignificant effect of Arctic amplification on the amplitude of midlatitude atmospheric waves. Sci. Adv., 6, eaay2880, https://doi.org/10.1126/sciadv.aay2880.

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