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Interactive comment on "Atmospheric teleconnection processes linking winter air stagnation and haze extremes in China with regional Arctic sea ice decline" *by* Yufei Zou et al.

Anonymous Referee #1

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GENERAL COMMENTS

This paper uses climate model experiments in which regional Arctic sea-ice decline is imposed, combined with analysis of new CMIP6 data, to better understand the dynamical mechanisms by which Arctic sea-ice decline may influence winter haze pollution extremes in China. The main new result reported is that Pacific sector sea-ice loss increases the likelihood and intensity of haze pollution extremes, due to anomalous transient eddy vorticity fluxes amplifying the negative phase of the EU pattern.

Given the substantial impact of haze pollution extremes on public health, this study represents an important contribution to this research area. The variety of methods used – including targeted single model experiments, new CMIP6 multi-model data, and a variety of interesting diagnostics – is also good. The paper is generally well presented with a good quality and number of figures, and only minor alterations are required to the wording and structure.

However, while this study reports potentially very interesting and impactful results, I am concerned about the statistical robustness of some of the conclusions and, therefore, that the length of the simulations (30 years) may be too short. My specific comments below explain these concerns in detail, which I would like to see addressed.

SPECIFIC COMMENTS

Page 1, line 19: I found the use of the word 'event' a bit confusing in this study, as the extremes analysed are monthly extremes and 'event' – to me anyway – implies a shorter timescale (daily or weekly). It would be helpful to clarify somewhere what is meant by the term 'event' here, or to avoid using the term.

Introduction: This paragraph is far too long, which made the structure of the introduction – which while good – a bit hard to follow. Breaking this up into a few paragraphs would help. The same goes for similarly long paragraphs in other parts of the paper (e.g. page 4 lines 9-40; page 9).

Page 2, lines 32-35: This sentence is a bit misleading, as it implies that there is a scientific consensus that high-latitude climate change influences mid-latitude circulation and weather, when there is not (e.g. https://www.nature.com/articles/s41558-019-0662-y). There is lots of evidence suggesting that Arctic sea-ice loss can have an influence on mid-latitudes, but whether it has in the past or will in the future is more unclear (https://onlinelibrary.wiley.com/doi/full/10.1002/wcc.337). Would be good to rephrase the sentence to reflect this (e.g. 'Given the increasing evidence that climate change – especially that occurring in high-latitude regions – may have an influence on middle-latitude circulation').

Section 2.1: I found this section jumped around a bit in terms of the definitions of the EU pattern and index, the MCA_Z500 pattern, and the PPI. If possible, could this be restructured so that the definition of each is closer to where it is originally introduced?

Page 3, lines 29-30: It would be helpful to properly explain and define the WSI and ATGI.

Page 4, lines 3-5: Do you have a citation for this?

Section 2.2: Are you able to justify using simulations of only 30 years in length? To me this seems rather short, especially considering my comments regarding statistical robustness below. Indeed, Screen et al. 2014 show that the simulated circulation response to sea-ice loss is small compared to internal variability (i.e. there is a low signal-to-noise ratio), and specifically that at least 70 year-long experiments are required to simulate a robust mid-tropospheric response to sea-ice loss (https://link.springer.com/article/10.1007/s00382-013-1830-9). Similarly, simulations submitted to PAMIP (the Polar Amplification Model Intercomparison Project) are required to be at least 100 years long due to this low signal-to-noise ratio (https://www.geosci-model-dev.net/12/1139/2019/). Also, many studies using WACCM to investigate the response to sea-ice loss use longer simulations (e.g. England et al. 2019 use 151 years, https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-17-0666.1; Sun et al. 2015 use 161 years, https://journals.ametsoc.org/doi/full/10.1175/JCLI-D-15-0169.1; Zhang et al. 2018 use 60 years, https://advances.sciencemag.org/content/4/7/eaat6025).

Page 6, lines 4-6: You say that you have 90 samples when conducting this statistical test, and so I presume you are assuming 90 degrees of freedom. However, have you checked whether the MCA_Z500 and/or ECP_PPI indices are autocorrelated (e.g. between consecutive months or lag-1), and therefore whether 90 degrees of freedom is an overestimate?

Page 6, lines 36-39: Relating to the above comment, did you account for autocorrelation when conducting this bootstrapping method (e.g. as done in your previous paper using the moving blocks method https://advances.sciencemag.org/content/3/3/e1602751)? If there is autocorrelation it may be that the uncertainties given by the bootstrap method (Tables S3 and S4) may be underestimated, and therefore the statistical robustness of the differences between the perturbation and CTRL experiments overestimated.

Page 8, lines 21-23: It should be noted that these correlations are not statistically significant at most gridpoints (but perhaps the correlation would be significant if you used an area average?).

Page 9, lines 20-23; Tables S3 and S4: Can you justify why you use the standard deviation here? The numbers in these tables for the SENSr2 experiment contain one of key results of this paper, suggesting that there is an increase in the likelihood and intensity of MCA_Z500 and ECP_PPI positive extremes in response to sea-ice loss in the R2 region. However, by using just the standard deviation it maybe cannot be said that the extremes in SENSr2 are significantly different statistically from those in CTRL. I may be wrong, but a 95% confidence interval seems more appropriate to test whether the difference is statistically robust? Since a 95% confidence interval will be larger, the 9% $\pm$ 3% figure in Table S3 for SENSr2 MCA_Z500 may not actually be significantly different from CTRL (5% $\pm$ 0%).

Page 9, line 23 to page 10, line 28: Results relating to changes in the ensemble mean of the MCA_Z500 and ECP_PPI indices are presented and discussed as if they are statistically robust (e.g. 'The differences in the MCA_Z500 and ECP_PPI responses among the four sensitivity experiments in extreme members and ensemble means also suggest complex relationships between Arctic sea ice loss and mid-latitude weather changes'). However, they are only statistically significant for SENSr1 ECP_PPI (p=0.04) – see Table S2. These paragraphs should be edited so that is clear whether the results being presented and discussed are robust or not.

Section 3.4: Why has only the ECP_PPI index been calculated for the CMIP6 results, and not the MCA_Z500 index, when both were for the WACCM results? This seems quite key, since it is MCA_Z500 that demonstrates a dynamical (and therefore more causal) connection between sea-ice loss and ECP_PPI.

Figure 1, Figure S1, Figure 5 (a) and (c): It would be useful to indicate in the captions that these plots are for observational/reanalysis data, rather than for the sensitivity experiments conducted. For Figure 5 (a) and (c) specifically this is mentioned initially, but it would be clearer to say this in the caption after (a) and (c) as well.

Figure 3: In the caption it says 'Atmospheric circulation and regional air stagnation responses to the Arctic sea ice forcing in the WACCM experiments'. However, what is in the figure is the absolute CDFs for the CTRL and SENS experiments, rather than differences between the SENS experiments and CTRL (what is normally defined as the 'response'). The use of 'response' in the caption is therefore confusing and should be changed.

Figure 4: Since these plots show the difference between the SENSr2 extreme members and the CTRL ensemble mean, rather than the CTRL extreme members, these plots do not just show the effect of the sea-ice forcing imposed, but the combined effect of sea-ice loss and internal variability (which causes extreme events without the need for sea-ice loss). The start of the caption ('Winter atmospheric response to the autumn and early winter sea ice change . . .') should therefore be re-phrased. Also - presumably 'winter' means the 'winter mean' here?

Figure 5: Why is there stippling to show statistical significance in all figures except this one?

Figure S4: 'Relative changes' to what?

TECHNICAL CORRECTIONS

Page 3, line 23: Perhaps refer to 'Fig 1 (c) and (d)' instead of just 'Fig 1', since not

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referring to whole figure. If there are similar instances in other parts of the paper, could you perhaps change these too for clarity (e.g. page 4, line 1: ‘Fig S1 (b)’ rather than just ‘Fig S1’).

Page 3, line 33: I’m not sure the definition of PM_10 would be immediately obvious to all readers, although I could be wrong. Perhaps consider including a very brief definition?

Page 6, line 11: ‘these’ should be ‘those’

Page 9, line 24: ‘of two indices’ should be ‘of the two indices’

Figures 3 and 7: ‘inlet’ should be ‘inset’

Figure 6 (a) and (b): This rainbow colour scale is not colour-blind friendly, so would be hard to interpret for some people. Perhaps use a white to blue scale, with blue indicating stronger winds?

Tables S3 and S4: ‘MAC_Z500’ in tables should be ‘MCA_Z500’

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