'Pacific Decadal Oscillation modulates the Arctic sea-ice loss influence on the mid-latitude atmospheric circulation in winter'

<u>Review 1</u>

Summary

This paper used coupled and atmospheric-only model experiments to investigate the interaction of Arctic sea ice loss and the PDO. A range of sensitivity experiments were performed where sea ice was artificially reduced. It was shown that the mid-latitude and stratospheric response to sea ice loss was similar to the response to the warm phase of the PDO. A linear regression algorithm was used to determine that the effect of sea ice and PDO was not additive, and the atmospheric response to sea ice loss was dampened by the PDO.

Overall, I thought this was a well written paper with interesting results. I recommend publication with minor revisions. Mostly I have only minor comments and clarifications, however, I was unsure about the results in Figure 9 which seemed to contradict earlier key results.

We appreciate the reviewer's positive assessment of our work, and we are grateful for the time spent reviewing our manuscript and for providing constructive comments.

General comments:

Figure 9: If the combined effect of future sea ice and the PDO is not additive, and the PDO dampens the sea ice response (e.g. Figs. 3-7), why does Figure 9 not show a difference between the gradient of the lines in Fig. 9? For example, Fig. 4, bottom row, shows a difference in the combined Aleutian Low/PDO response between PD:PDO and FUT:PDO, since it's a linear regression would that also mean that the response of the PDO is dampened by reduced sea ice (apologies if I am misunderstanding this)? Which would imply that in PD should have a steeper gradient that FUT in Fig. 9.

We thank the reviewer for this comment. Indeed, Figure 9 was erroneous in the previous manuscript, as the points marked as PI, PD or FUT were in fact mixing the results of the three simulations. This was corrected in the revised manuscript. Figure 9, involving composite analysis, now clearly shows that the response of the PDO is dampened by the sea ice loss, except for the AO that the slopes are similar for both FUT and PD. We added to the figure the regression lines, to make a more straightforward comparison between the composite analysis and the regression analyses shown in previous figures.

We also reformulate and improve the description of the composite analysis. The legend of Fig. 9 was modified: "*Composites of the AO index (top-left; unitless), Aleutian low (top-right; in m) and polar cap 50 hPa geopotential height (bottom, in m), for members sorted following their PDO index in the PI (green lines), PD (blue lines) and FUT (red lines) ensembles. The dashed lines show the regression lines of the corresponding ensemble. The triangles indicate the value for each composite, constructed using* $PDO<Q_{1/3}$, $Q_{1/3}<PDO<Q_{2/3}$ *and* $PDO>Q_{2/3}$, *where the threshold are given by* $Q_{1/3} = -0.43$ *and* $Q_{2/3} = 0.43$, *the first and second tercile of a standard normal distribution. The error bar provides the 95% confidence interval.*"

We also modify the related text (Lines 496):

“*Therefore, we performed a composite analysis averaging members of the PI, PD and FUT ensemble simulating warm, neutral and cold phases of the PDO. The composites are built using members with a PDO index lower than -0.43 (cold phase), between -0.43 and 0.43 (neutral phase) and higher than 0.43 (warm phase). The values of -0.43 and 0.43 are chosen as a threshold as they correspond to the first and second tercile of the standard normal distribution. For gaussian climate indices, this leads to a composite of approximately the same size.*”

My other general comment concerns the PDO in the experimental setup, if the experiments consist of 200 x 12 month periods, how can there be decadal variability?

The PDO pattern is commonly calculated from an EOF analysis based on yearly data. The resulting principal components then usually show large decadal variability. In our experiment, we perform the EOF with yearly data, but on the members dimension instead of the temporal dimension. This is equivalent to a classical EOF. As stated in Maher et al. (2018), ‘’When using the ensemble-dimension to compute EOFs (EOF-E), all time scales of variability are captured’’. The pattern obtained by our analysis is well representative of the PDO. We also verify that a similar pattern can be obtained by a classical EOF using the preindustrial control run of the IPSL-CM6A-LR model (Fig. R1, left). Such a pattern is the PDO and is known to have an important decadal mode of variability. We verified that it is the case with our model IPSL-CM6A-LR, that found important decadal variability for the first principal component of the North Pacific SST (Fig. R1, right).

Figure R1. (Left) First EOF of the annual mean North Pacific SST (north of 20°N) with the preindustrial control simulation of the IPSL-CM6A-LR model. It explains 23.8% of the variance. (Right) Variance spectrum of the corresponding first principal component.

We now add a sentence L 238: “*Performing an EOF analysis using the member dimension instead of the time dimension allows capturing all time scales (Maher et al., 2018). It is equivalent to a classical annual EOF over the time dimension. We also verified that the same pattern can be found using the control simulations of the same model, as that the associated time series have important decadal variability.*”

Minor comments:

Line 32: North Atlantic Oscillation (NAO)

Done

Line 34: "while in the *stratosphere* the polar vortex weakens"

Done

Introduction: Is there a hypothesis for the mechanism for reduced sea ice -> negative NAO, and could that be briefly stated in the introduction?

We now provide more details L 75: “*Different physical mechanisms have been proposed to explain the reduced sea-ice and negative NAO relationship. It involves a tropospheric pathway, with a reduction of the equatorward-to-pole lower tropospheric temperature gradient weakening the eddy activity, followed by feedback related to the eddy-mean flow interactions (Smith et al. 2022). A stratospheric pathway was also found, where upward propagating planetary waves into the stratosphere are intensified with sea-ice loss. Such* *waves lead to a weakening of the polar vortex propagating downward into an Arctic-Oscillation (AO) pattern. "*

Line 74: "late *autum*"

Done

Line 227: Why were concatenated outputs used for the EOF analysis? Taking the second EOF as a physical mode can be problematic (e.g. Dommenget & Latif, 2002 https://doi.org/10.1175/1520-0442(2002)015%3C0216:ACNOTI%3E2.0.CO;2) and, as stated in the text, the first EOF is due to the reduced sea ice, so would taking the EOF from each experiment first and combining them be better

We thank the reviewer for the interesting suggestion. We followed this suggestion, and performed three separate analyses using the outputs of each of the PI, PD and FUT ensembles. The first EOF obtained in each ensemble is indeed similar to the second EOF calculated using the concatenated outputs (see Fig. R2). However, there are also differences among the three EOF1 obtained. Therefore, as proposed, one has to combine the three patterns, and then calculate an index using a projection onto the combined pattern.

As the patterns obtained when taking the EOF from each experiment are similar to EOF2 of concatenated output, it is unlikely that such pattern is associated with little climate physics, as illustrated in Dommenget and Latif (2002). For the sake of simplicity, we decided to not conduct such an analysis.

We added in the manuscript L 257: ''*Although this pattern appears here as the second EOF, a very similar pattern is found as the first EOF conducting separate EOF analysis for each of the PI, PD, and FUT, or using the 2000-yr preindustrial control simulations of the same model (not shown). We also verified that the associated time series have prominent decadal variability in a preindustrial control simulation.*''

Figure R2. First EOF of the annual mean North Pacific SST (north of 20°N) using separated EOF analyses conducted on the (first line) PI, (second line) PD and (third line) FUT ensembles.

Line 228: "This EOF analysis uses the member dimension instead of the time dimension, as classically used" Since each member is one year, I assume it's equivalent to using annual means, is that right? Could that be briefly specified.

Yes, exactly. The EOF uses yearly SST on the member dimension, so it can be seen as equivalent to using annual means on the temporal dimension.

See the response to the second major comment.

Line 252: "... winter, defined as the 3-month mean in December-February-March" Is that meant to be Dec-Jan-Feb, or is there a reason for excluding January from the winter mean?

We correct it to December-January-February.

Line 269 and 273: "β_{PD} is the regression coefficient determining the effect of the sea ice in PD (FUT) when compared to PI (same for β_{FUT} with FUT);". To make it easier to read, I recommend separating the description of β_{PD} and β_{FUT} into two sentences rather than using parentheses.

Done.

Line 455: Define DCPP

Done.

Line 477: "Concerning the amplitude of the response to sea ice loss". The amplitude of what?

We removed this part of the sentence and now say (L 520): "*Observational studies estimate that winter Arctic sea-ice loss could have led to a much larger NAO-like anomaly than the one found here*"

Reviewer 2:

Summary & General comments: This is an interesting take on the multitude of studies concerning the atmospheric response to Arctic sea-ice loss where a general linear model is used to determine the response to sea-ice loss and PDO separately, and the interaction between them. The fact that PDO and sea-ice loss interact non-lineary is certainly of great interest. To me it was a novel technique that I believe should be introduced to the community, but consequently, I do think it could use a bit more clarification and fleshing out of both the methods and the discussion, and for that reason I have selected minor revisions, but also included a heading below with "major" just to separate out some of the more substantial changes I'd like to see from the smaller ones.

We appreciate the reviewer's positive assessment of our work, and we are grateful for the time spent reviewing our manuscript and for providing constructive comments.

Minor comments:

Throughout:

Inconsistent use of sea-ice loss vs. sea ice loss: my preference is the former.

We now use "sea-ice" in the whole manuscript.

Adding 'the' in front of sea-ice loss is not usually necessary.

We removed "the" in front of sea-ice.

Abstract

Perhaps it's worth mentioning that these are PAMIP-style experiments in the abstract?

The abstract was modified L 28: "*Ensembles of simulations are performed with constrained sea-ice concentration following the Polar Amplification Model Intercomparison Project (PAMIP), and initial conditions sampling warm and cold phases of the PDO.*"

L33: I find "Weak deepening" is sort of awkward wording, perhaps something like “small increase in strength of the Aleutian Low”, or “modest deepening…”.

We now say “*modest weakening*”.

L34: In the stratospheric the polar vortex -> the stratospheric polar vortex/in the stratosphere…

We now say: “*in the stratosphere the polar vortex weakens.*”

L35: Besides: hard to know what is meant here. Is it saying that on the other hand the PDO does X or in addition, it does X?

We now use: “*On the other hand*” instead of “*Besides*”.

L38: I was confused which phase of the PDO you were referring to, upon reading the paper I understood. It could be worth mentioning that it is for both phases of the PDO here as a result of the method used.

We agree that it was a bit confusing. To make it more clear, we added two sentences in the abstract.

The text was reformulated L 37: “*The warm PDO phase therefore reduces the response to sea-ice loss, while the cold PDO phase enhances it. However, the effects of PDO and Arctic sea-ice loss are not additive, as the PDO teleconnections are damped under sea-ice loss conditions, in particular for the stratosphere.*”

Introduction

L85: There are quite a few more coupled studies you haven’t referenced here, e.g. McCusker et al 2017, Oudar et al 2017, Blackport & Kushner 2016,2017, Hay et al 2022, England et al 2020, Sun et al 2018….

We thank the reviewer for these references. We have now integrated these studies in the text, which now reads L 90: “*On the other hand, all coupled models show a negative NAO response (Deser et al., 2015; Blackport and Kushner 2016, 2017; McCusker et al. 2017; Oudar et al., 2017; Screen et al., 2018; Sun et al., 2018; England et al 2020; Simon et al., 2021; Hay et al., 2022)”*

L87: Perhaps include a reference to to Smith et al 2020 on the NAO signal to noise paradox.

The signal to noise paradox was established in the context of seasonal (Scaife et al. 2009) or more recently in decadal (Smith et al. 2020) climate forecasting. It is not clear how our results relate to these forecasts. The atmospheric response to Arctic sea-ice might contribute, but, to the best of our knowledge, we have not found references on this topic.

We added in the discussion L 525: “ *Although the effect of the surface sea ice condition is weak (Smith et al., 2022), this lack of sensitivity in models might contribute to explain the much too weak persistence of climate variability in models. This deficiency might stem from the so-called signal to noise paradox in seasonal-to-decadal climate prediction systems (Scaife et al., 2014; Scaife and Smith, 2018; Zhang and Kirtman 2019; Smith et al. 2020), which remains to be solved.*”

L92 I believe the Cvijanovic study uses a slab ocean, so it might not be the best example to include here as Deser et al 2015 showed that the nature of the slab ocean response was quite different. Hay et al 2022 also discusses the deepening Aleutian Low response and PDO-like response of SST's.

Cvijanovic et al., 2017 show the results using a fully coupled model in the supplementary file (their Figure S10 for the geopotential height). In this study, the slab and fully coupled model induces a qualitatively similar response.

In our case, we did not find a clear signal on the Arctic sea-ice response on the SST in the North Pacific, as in Hay et al., 2002 but we have a consistent response on the Aleutian Low. We have added the reference Hay et al., 2022. We now say L 98: "*Screen et al. (2018) found, in six sensitivity experiments involving different models or methodologies to melt sea-ice, a strengthening of the Aleutian low, as well as Hay et al. (2022), ..*"

L108: What is meant by December Wave 1? Wave 1 pattern in december or something else?

We meant the stationary wavenumber one in December. We reformulate the sentence to (L 115) : "*Labe et al. (2019) found that sea-ice loss reinforces the stationary wavenumber one as identified in 300 hPa geopotential height fields under the East phase of the Quasi-Biennial Oscillation (QBO) in December.*"

L119: 'oppositely' as in they will cancel each other out? Where does the cancellation occur?

We now say (L 126): "*Liang et al. (2021) showed that Arctic sea-ice concentration in December induces a negative NAO in late winter while the concomitant North Atlantic horseshoe SST pattern (Czaja and Frankignoul, 1999; 2002) induces an opposite NAO response.* "

L125: extension -> extent

Done.

L127: Many of the responses to sea-ice loss? There have been some efforts to make multi-model comparisons (Screen et al 2018, Hay et al 2022, though I appreciate that IPSL hasn't been used before so I understand what is meant. Though it occurs to me that since these are PAMIP experiements it would be possible to extend this analysis to other models that have done the coupled runs as well.

We agree. It would be of great interest to extend this study to other models. However, the method to constrain the sea-ice is not fixed by the PAMIP protocol and is different in each model, which might affect the results and complicate their interpretation. For all these reasons, we have not included results from other models in the manuscript.

We now say in the discussion L 534: "*The analysis presented in this paper could be repeated in a multi-model framework to investigate the robustness of these conclusions, such as through PAMIP simulations. For this, it is important to keep in mind that depending on the protocol used to constrain sea-ice in coupled model sensitivity experiments, the amplitude of the atmospheric response to sea-ice loss can vary by a factor of two (Simon et al, 2021). Moreover, sea-ice thickness was not constrained in the sensitivity experiments but might play an important role in the atmospheric circulation response (Lang et al., 2017). Great caution is therefore required when interpreting the results of different models using different ice-constraining methods.*"

Methodology

L145: CM6 -> CM6A

Done.

L154: resolution **increases**

Done.

L175: which procedures are meant here?

We meant the procedure to adjust the SST to the imposed sea-ice conditions.

We reformulated to (L 179): "*These experiments are atmosphere-only simulations, using the same SIC as the one used as target in the coupled simulations. The simulations use a repeated climatological SST calculated from HadISST and the 1979-2008 period, with a local adjustment of SST to the prescribed sea-ice (Smith et al., 2019).*"

L200: This last sentence probably belongs in the previous paragraph

Done.

L231 & elsewhere: FU->FUT

Done.

L241-: I'm a bit unclear on this due to the wording here, (esp. together with the equatorial SST anomaly), does this just mean that this is a known bias of climate models that they extend too far westward?

Yes, exactly. To make it more clear we have reformulated this sentence.

"*This pattern is similar to the observed Pacific Decadal Oscillation in the warm phase but with the midlatitude horseshoe and the equatorial SST extending too much toward the western Pacific, as found in many other climate models (Sheffield et al., 2013; Coburn and Pryor, 2021).* "

L261-262: This might be easier to read if written as two equations (assuming I'm understanding it correctly, the dummy variables effectively makes this two separate equations?)

We expand the text and add a new equation L 292: "*When using outputs from the present day experiment, equation (2) becomes:*

$$Y(n) = \beta_0 + \beta_{PD} + \beta_{PDO}PDO(n) + \beta_{PD:PDO}PDO(n) + \varepsilon \qquad (3)$$

The coefficients and are the intercept and slope of the regression lines for the PI simulations. and then quantifies the change in the intercept and slope in PD compared to PI."

L275: residue -> residual

Done.

L281: FDR is becoming more common within climate science but I think this bit will be a bit opaque to most readers and needs clarifying

We now add the sentence L 300: "*The interpretation of statistical tests at multiple grid points is often difficult. For instance, when choosing a α% level of statistical significance, if the null hypothesis is verified, it will be on average falsely rejected over α% of the grid points, but global significance requires a larger rate of rejection (Von Storch and Zwiers, 2002). The false discovery rate procedure (FDR; Wilks et al., 2016) avoids such overestimation, known as false positives, and estimates field significance over a given domain, enabling a more accurate interpretation.*"

Results

L310: Since I took issue with using 'weak deepening' in the abstract, here I want to note that you don't use it in the text, so perhaps it's not even necessary to state in that way in the abstract.

Apart from the abstract, we also talk about the deepening of the Aleutian Low in the discussion. We changed "small" to "modest" to be consistent with the abstract. We say L 460: "*The response to Arctic sea-ice loss also includes a modest deepening of the Aleutian low, as in Blackport and Screen (2019).*"

L406: What do you mean by somehow? Somewhat?

We have removed the word "somehow".

Summary & Discussion

See comment above in Intro section about other studies I think should be referenced here, and my thought son some of the discrepancies.

We have included more references (L 455): "*The simulations show a robust negative NAO-like pattern in response to sea-ice melting, in line with most studies (Deser et al., 2015; Blackport and Kushner 2016, 2017; McCusker et al. 2017; Oudar et al., 2017; Screen et al., 2018; Sun et al., 2018; England et al 2020; Simon et al., 2021; Hay et al., 2022).*"

Some thoughts on the discrepancies can be found here. We added the reference Hay et al., 2022 (L 460): "*The response to Arctic sea-ice loss also includes a modest deepening of the Aleutian low, as in Blackport and Screen (2019). The discrepancy with other studies in sign (Cjivanovic et al., 2017; Simon et al., 2021) or in amplitude (Screen et al., 2018, Hay et al., 2022) can be explained by the timescale investigated.*"

L446: How so?

We have changed the sentence to clarify:

We now say L 472: "*Conversely, we show that a warm PDO phase mainly intensifies the Aleutian low and the transient eddy heat flux at 30°N-40°N into the stratosphere. The wintertime tropospheric stationary wave deepens during strong Aleutian Low, which is known to lead to a weakening of the polar vortex (Nakamura and Honda, 2002; Garfinkel et al., 2010; Smith et al., 2010).*"

L478: Increase in what?

We now say L 520: "*Observational studies estimate that winter Arctic sea-ice loss could have led to a much larger NAO-like anomaly than the one found in this study*"

L503: missing -LR

Done.

L506: Besides again, not clear what it meant.

We now say "Moreover".

Last paragraph: some ocean analysis is also done in Hay et al 2022.

We have added a key result of Hay et al., 2022 in the discussion:

We now say L 592: "*In particular, the changes in the Beaufort Gyre (Lique et al., 2018), North Atlantic inflow (Simon et al., 2021), subpolar North Atlantic Ocean (Hay et al., 2022) and Atlantic Meridional Oceanic circulation (Sévellec et al. 2017) would play an important role.*"

Figures:

Fig 1: Fut-PI, and PD-PI surely? As there's a [-] in SIC? Or does that mean unitless?

We meant unitless. We have modified the figure and removed '' [-] ''.

Fig 8. Perhaps changing the vertical extent/scaling in the bottom panels would be helpful for making this easier to read

We modified the aspect ratio of the bottom panels of Fig. 8 in the revised manuscript, which enlarges the bottom panels of the figure and makes it easier to read the changes in the troposphere. We hope it will address this comment.

Fig 9. I'm a bit confused by what is meant by the triangles being associated with terciles, it looks as though they're just located at -1, 0, 1?

We thank the reviewer for this comment, as this analysis was not well presented in the previous manuscript. The triangles show the value for composites built using members with annual mean PDO index larger or smaller than a fixed value. We calculated three composites, using members with a PDO index smaller than 0.43, between -0.43 and 0.43 and larger than 0.43. The fixed values -0.43 and 0.43 were chosen as they represent the first and second terciles of a standard normal distribution, to form composites of roughly similar size.

The values of -1, 0 and 1 correspond well to the fixed values -0.43, 0 and 0.43. If the climate indices used have a standard normal distribution, the composites will have a truncated normal distribution, which will result in -1.09, 0 and 1.09 for the mean of each composites corresponding to the fixed values -0.43 and 0.43.

We modified the caption on FIg. 9:

"*Composites of the AO index (top-left; unitless), Aleutian low (top-right; in m) and polar cap 50 hPa geopotential height (bottom, in m), for members sorted following their PDO index in the PI (green lines), PD (blue lines) and FUT (red lines) ensembles. The dashed lines show the regression lines of the corresponding ensemble. The triangles indicate the value for each composite, constructed using* $PDO<Q_{1/3}$, $Q_{1/3}<PDO<Q_{2/3}$ *and* $PDO>Q_{2/3}$, *where the threshold are given by* $Q_{1/3} = -0.43$ *and* $Q_{2/3} = 0.43$, *the first and second tercile of a standard normal distribution. The error bar provides the 95% confidence interval.*"

We also modified the related text L 496: "*Therefore, we performed a composite analysis by averaging members of the PI, PD and FUT ensemble for warm, neutral and cold phases of the PDO. The composites are built using members with a PDO index lower than -0.43 (cold phase), between -0.43 and 0.43 (neutral phase) and higher than 0.43 (warm phase). The thresholds of -0.43 and 0.43 correspond to the first and second tercile of the standard normal distribution. For gaussian climate indices, this leads to a composite of approximately the same size.*"

Major

L71: "is likely" might be a bit of an overstatement/oversimplification of the large body of research debating the topic and considering how small of an effect is found (e.g. Smith et al 2022), as well this just being one driver of mid-latitude climate change, where change driven by lower latitudes may be more important and induce changes of the opposite sign (e.g. tug of war between high/low latitudes)

You are right. We now say: "might" instead of "is likely to" and add the reference Hay et al., 2022.

We now say (L68): "*Many studies have shown that the Arctic sea-ice loss could change the mid-latitude climate, but its extent is still a matter of debate (Cohen et al., 2014; Blackport and Screen, 2020; Hay et al., 2022).*"

L257: Can this be shown? For readers not familiar with the method this might be helpful. This is an example of where I think readers would appreciate a bit more of an explanation of the method,, though I understand there's a limited amount of space. I think it's a very interesting and, to me, novel way to analyse these types of experiments. I'm not sure what sort of assumptions go in to this, for example.

We followed this suggestion and added a new figure in the appendix. Figure A1 illustrates the variance of the 500-hPa geopotential height explained using different regressors. The difference when using the interaction term was also tested with a *F*-test. The residue is significantly smaller when taking into account the interaction factors.

We added L 271 ''*We also consider the interactions between sea-ice and the PDO, as we find that it significantly improves the explained variance of the general linear model in many locations (see Fig. A1)*'' and add a figure in the appendix.

L297: So this suggests agrees with the results of Screen & Francis 2016, but shouldn't that effect have been quite hard to detect with present day sea-ice loss? I think discussing your results here in the context of theirs might be useful.

By comparing the sea-ice forcing (their figure 2 and our figure 1 for FUT- PI) and the PDO forcing (their figure 2 and our figure 2), it appears that our forcings (both sea-ice for FUT -PI and PDO) are similar but with smaller amplitude than theirs. We obtain slightly less amplitude in the response, which is therefore consistent.

We now further compare our result with Screen and Francis 2016 in the discussion part. We hope it will address this comment. We now say (L 480): *“This is consistent with the study of Screen and Francis (2016) that used sea-ice and PDO forcings larger than the ones investigated here. They consistently found slightly larger responses than ours in the near-surface temperature or zonal winds. We also found a broader near-surface temperature increase over the Arctic due to sea-ice loss and a broader PDO response in the North Pacific. The overall results agree with Screen and Francis, (2016), with an amplified Arctic Warming in response to the shift from positive to negative PDO in the recent decades. The framework proposed here also assesses the non-additivity of the responses of the Arctic sea-ice loss and the PDO and that the atmospheric response was linked to the stratospheric polar vortex changes.”*

I like that there is a discussion of the short length of the experiments, as this is an atypical way of running coupled experiments and is sure to reduce oceanic effects, particularly slow time-scale ones. Perhaps it’s not surprising that the results are not too different between ATM and CPL, but it feels like something is missing between what is stated in the abstract about comparing ATM and CPL experiments as this is just a single paragraph and doesn’t really delve in to the effects of the stratospheric weakening.

We agree with the reviewer. The comparison between ATM and CPL experiments is not fully shown in the manuscript. To support the mechanism proposed in the discussion, we added in appendix the Fig. A2 with the transformed eulerian mean diagnostics applied to the ATM experiments.

A short text was also added at L 560 to explain the different polar vortex weakening in ATM and CPL : “*The eddy heat flux reduction also extends more toward the tropics in the coupled runs compared to atmosphere only simulations (Fig. A2, bottom left). Both changes intensify the subtropical jet at 30°N and are associated with intensified upward propagation of planetary waves into the stratosphere (compare Fig. 8 bottom-left to Fig. A2 bottom-right), which might explain the reduction of the stratospheric polar vortex in the CPL experiments. “*

References:

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England, M., L. Polvani, L. Sun and C. Deser (2020), Tropical climate responses to projected Arctic and Antarctic sea-ice loss, Nature Geoscience, 13, 275-281, doi: 10.1038/s41561-020-0546-9

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Sun, L., Alexander, M., & Deser, C. (2018). Evolution of the Global Coupled Climate Response to Arctic Sea Ice Loss during 1990–2090 and Its Contribution to Climate Change, Journal of Climate, 31(19), 7823-7843