Weather Clim. Dynam. Discuss.,
https://doi.org/10.5194/wcd-2020-29-RC2, 2020

Interactive comment on "The role of Arctic sea ice loss in projected polar vortex changes" *by* Marlene Kretschmer et al.

Anonymous Referee #2

Received and published: 26 August 2020

This study examines the potential impact of sea ice loss on the stratospheric polar vortex (SPV) in CMIP5 model projections. The authors find that the SPV shows a nonlinear response in the ensemble mean of CMIP5 projections. The SPV initially weakens slightly before showing strengthening again, resulting in little change over the 21st century. The authors show that this nonlinearity coincides with when the Barents-Kara (BK) Sea goes ice free, and thus present the hypothesis that BK sea ice is the cause of the nonlinearity. The authors then show that there is a very weak causal response of the SPV to fall BK sea ice in historical simulations, but this weak response can explain the initial SPV weakening. This weakening is then counteracted by other effects of global warming once the BK sea ice goes ice free.

The authors' hypothesis is plausible, and the study could compliment the large body of

Printer-friendly version

Discussion paper

literature that uses targeted sea ice loss experiments in models. However, there are a lot of issues that need to be addressed before it should be published.

Major issues:

1.The authors attribute the nonlinear response of the SPV (Fig 1a) in response to global warming to the BK sea ice. While this is certainly plausible, the evidence the authors present for this is not convincing. Fig 5 seems to show that a good portion (most?) of this nonlinear response actually comes from the residual term and not BK SIC. The change in the SPV is seen at around 2060 (Fig 5a) and 2.5K (Fig 5d) and this is clearly seen in the residual term (Fig 5c,f). The nonlinear response in the BK sea ice is a lot less obvious to me (Fig 5 b,e). How can the authors reconcile this result with their conclusions? The authors might want to consider quantifying how much the of nonlinearity is attributed to BKS SIC, perhaps by simply calculating the difference in linear trends before/after a certain year/temperature.

2. The authors' estimation of the causal effect likely overestimates the strength. While I commend the authors for trying the remove the effects of Ural SLP, I do not think what they have done fully accomplishes this. It is likely the Ural SLP in November and December that would impact both the JFM SPV and the OND BK sea ice. Peings (2019) showed that November Ural Blocking impacts both November BK sea ice and December-January SPV. Thus, averaging over SON will likely underestimate the co-founding effect because it includes September Ural SLP (which likely has little impact on the JFM SPV) but does not include December Ural SLP (which likely has a larger impacts on SPV and still impacts OND sea ice). I think removing the OND Ural SLP would be better. This could remove some of the impacts of sea ice on Ural SLP, but the authors show that this is likely to be small.

3. The authors show that there is a little causal effect of sea ice on Ural SLP, but that there is an effect on the SPV. How can the authors reconcile this with the causal model in Fig 2? Shouldn't there be a stronger causal effect between sea ice and Ural SLP

than with sea ice and the SPV? This seems to suggest that the causal effect on the SPV is overestimated (see above), or there is another pathway which BK sea ice can influence the SPV that does not involve Ural SLP.

4. The scatter plots/correlations from Fig 4 and the conclusions drawn from this analysis are flawed because they do not take into the cofounding effects, which likely exist. Because the same casual effect strength is used for all models, these plots/correlations would be identical if it was done with only the BK SIC (without the causal effect strength). We would expect these same correlations to arise if a change in Ural SLP (not caused by BK sea ice) affected both BK sea ice and the SPV. We would then expect this correlation to weaken in the later period because Ural SLP will have a weaker influence on BK SIC (because there is no/little sea ice remaining in some models).

5. From Fig 1a, it looks like there are a few outliers that have strong nonlinear response. A couple of these outliers are pointed out in Fig 6c. A few outliers also appear to have a big influence on the correlations in the scatter plots in Fig 4b,c. How much do these few outliers influence the model means? How many individual models display this nonlinearity? Individual models will be heavily influenced by internal variability so the authors might want to consider looking at available large ensembles (of which there now quite a few that are publicly available, see e.g. Deser et al. 2020) to see if nonlinearity is seen in individual models after removing internal variability.

Minor comments:

Title: I think it would be better to use ‘Barents-Kara’ instead of ‘Arctic’ because the study is almost entirely about the Barents-Kara sea ice.

L46: The weakening of the SPV in response to sea ice loss in modelling is not nearly as robust as portrayed here. Many studies find little impact on the SPV, but these tend to not highlight this result (negative results are not that exciting!). Some examples are: Semmler et al. (2020), Blackport and Screen (2019), Sun et al.(2018). A weaker SPV in response to only BK sea ice loss might be more robust, but there are fewer studies that have looked at this, and these are less relevant on climate change timescales (sea ice loss does not only occur in the BK Sea).

L48: "possible implications of future sea ice loss have so far been only rarely studied. . ." This is not true. Many of the studies referenced in the previous sentence (and many others) have used models to study the implications of future sea ice loss.

L51: None of these three studies looked at the SPV. Seviour (2017) and Garfinkel et al. (2017) might be better for this specific point.

L67: How sensitive are the results to the exact region used to define the BK Sea, especially for the timing of when BK goes ice free? A smaller region might go ice-free sooner and larger region will go ice free later. In terms of the potential forcing I do not think there anything special about the boundaries used here.

L153: Kug et al. 2015 did not show this. Many modelling studies find reduced SLP in the North Pacific in response to sea ice loss (e.g. Screen et al. 2018), so these could be better to cite here.

L170: How important is removing the effect of SON Ural SLP here (i.e. what is the value of b)?

L194: Although not the SPV specifically, this is supported by Kolstad and Screen (2019) who found the link between fall BK sea ice and winter NAO has been nonstationary over the last century and the recent period has been especially high.

L221: But this 'problem' exists with or without a role for sea ice (see major comment 1).

L271-275: I think it important to explicitly mention here (and possibly elsewhere in the paper), that the effects of global mean warming without BK sea ice loss included sea ice loss outside of the BK Sea. A number of studies have found that sea ice loss in the Pacific side of the Arctic causes a strengthening of the SPV (Sun et al. 2015; McKenna et al. 2017), so the effects of Arctic sea ice loss are likely even smaller than the effects of BK sea ice loss.

L278: This should say BK SIC, not Arctic sea ice.

L326: These cited studies were not about the stratosphere, so are not that relevant here. Also, these studies were largely about refuting previous claims that there is evidence, and they are not claiming that the lack of evidence is evidence of no effect.

L327-328: Again, this not a very convincing argument, because BK SIC does not appear to explain a good portion of the nonlinearity. It is not completely unreasonable to think there could be non-linearity in the response of any of the many of the processes that drive the SPV, none of which were explored in this study.

L345: De and Wu, (2019) is not a good reference to make this point because it only correlations in preindustrial control simulations, not the response to sea ice loss. Also, this should be Blackport and Kushner 2017, although Screen et al. 2018 is a better reference for this point point because it used more CMIP5 models (including those used in Blackport and Kushner 2017).

Additional References:

Deser, C., and Coauthors, 2020: Insights from Earth system model initial-condition large ensembles and future prospects. Nature Climate Change, 10,277-286, https://doi.org/10.1038/s41558-020-0731-2.

Garfinkel, C. I., S.-W. Son, K. Song, V. Aquila, and L. D. Oman, 2017: Stratospheric variability contributed to and sustained the recent hiatus in Eurasian winter warming. Geophysical Research Letters, 44, 374–382, https://doi.org/10.1002/2016GL072035.

Semmler, T., F. Pithan, and T. Jung, 2020: Quantifying two-way influences between the Arctic and mid-latitudes through regionally increased CO2 concentrations in coupled climate simulations. Clim Dyn, 54, 3307–3321, https://doi.org/10.1007/s00382-020-05171-z.

Sun, L., M. Alexander, and C. Deser, 2018: Evolution of the Global Coupled Climate Response to Arctic Sea Ice Loss during 1990–2090 and Its Contribution to Climate Change. J. Climate, 31, 7823–7843, https://doi.org/10.1175/JCLI-D-18-0134.1.

Interactive comment on Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2020-29, 2020.

Printer-friendly version

Discussion paper