Review of *“A comparison of the atmospheric response to the Weddell Sea Polynya in AGCMs of varying resolutions.”*

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The authors have conducted a study which identifies the direct response of the atmosphere to the 1974 Weddell Sea Polynya (WSP) in Atmospheric General Circulation Models (AGCMs), and compares this to an analysis of the response in the ECMWF Reanalysis version 5 (ERA5). By prescribing ERA5 sea surface temperature (SST) and sea ice concentration (SIC) as surface boundary conditions for the AGCMs, the authors explore the range of direct responses across three different dynamical models, and the response across two resolutions for each model.

Previous coupled simulations have shown an importance of model resolution in their representation of Southern Ocean polynyas (e.g. Lockwood et al. 2021), however there is limited research isolating the model response (as opposed to formation combined with response), and understanding how this varies with model resolution. Therefore understanding the effect of model resolution on the atmospheric response to polynyas is a valuable field of study. Comparing the direct atmospheric response to a reanalysis response can also provide some insight on the pathways for a climate response to the WSP. Finally, the study of the reanalysis response itself, with the state-of-the-art reanalysis model in ERA5 is also valuable in understanding the real-world response to WSPs.

Summarising the above paragraph, in my opinion the three main valuable contributions of this research paper are as follows:

- Exploring how model resolution impacts the atmospheric response to the WSP.
- Understanding the atmospheric-only response, and how this compares to the reanalysis response to provide hints on the relative importance of the direct atmospheric response with respect to responses which include coupled feedbacks.
- Analysing the ERA5 reanalysis response to the WSP.

I also would like to recognise that although the results of this study are primarily not unexpected or conceptually new, I still consider it a valuable contribution to repeat methodologies with state-of-the art models. Additionally, this research extends previous studies with a multi-model, multi-resolution framework to provide a comprehensive study of the direct atmospheric response to the WSP.

Suggested revisions:

In my opinion, the authors need to take care that the wording of conclusory remarks remains within the scope of this direct atmosphere-only response study.

For example, the authors' statement *"...our results suggest that a 1974-size polynya (the largest observed) cannot generate a response that would project on the scale of the Weddell gyre."* is too much of an extrapolation, the study is isolated to atmosphere-only dynamics, and so can not alone rule out wider responses which could arise from coupled feedbacks.

Similarly, I think the following statement should be reworded, its wording implies that the results of this study prohibit a large-scale interaction of the WSP with the climate, whereas in reality, the results only suggest that because the reanalysis results and atmosphere-only results are similar, that the influence of the coupled feedbacks on the atmosphere are small. The WSP may influence wider ocean circulation, which could in turn have climate impacts elsewhere, and this possibility cannot be ruled out by this study alone: *"...the WSP may not interact with the climate on a large scale and may be too far south and within sea ice edge for coupled feedbacks to have a substantial impact on results".*

Again in the statement *"In addition, our results show that the response has little memory (i.e., local in time too) and vanishes rapidly with the polynya"*, the authors need to clarify that this means that there is little memory in the atmospheric-only system. Memory in a climate system is often established via ocean-atmosphere-sea ice feedbacks, which by design are not present in this study.

The statement on line 94, *"The interpretation of coupled models is made difficult by the potential impact of ocean-atmosphere-sea ice feedbacks"*, needs to be justified. A reader might argue that a coupled model is easier to interpret, because it includes feedbacks which an atmosphere-only model does not, and the processes which drive these feedbacks exist in the real-world. Comments such as this, and as those quoted above, present the idea that these direct atmosphere-only simulations are a better alternative to coupled simulations, when in fact these different methodologies answer different scientific questions.

The authors have discussed the fact that they are using ERA5 before the assimilation of satellite data, and commented on potential data quality issues. They have also cautioned the reader that the ERA5 data is the result of one realisation. I would therefore strongly recommend that the authors utilise all ten members of the ERA5 reanalysis to improve their analysis of the response to the polynya in ERA5, and to provide uncertainty estimates on this response. ERA5 is likely weakly observationally constrained in the Southern Ocean, particularly in the pre-satellite era, which means that the underlying model may play a significant role in determining the reanalysis conditions in the area of interest.

With regards to the statistical methodology, I would suggest that the authors consider employing multiple-hypothesis testing when applying the students t-test to potentially spatially correlated data (e.g. surface temperature and precipitation response). As described by Wilks (2006), this is a mistake that is made by standard in the field. In my opinion, at least acknowledging this potential statistical caveat is an important step in improving the statistical robustness in our community.

The paper is generally well written and presented, but could also benefit from a proofread. I have included below a list of some of the minor errors which I noticed, but I did not have the time to go through the entire article from a spelling and grammar point of view:

Spelling mistake in line 16 "isolate" should be "isolates".
Spelling mistake in line 68 "impact" should be "impacts".
Spelling mistake in line 112 "your" should be "our".
Line 121 "...thus, has limitations" the reason for limitations needs to be clarified a little.
Line 180 "limiting".
Authors should decide to use "sea ice" or "sea-ice" but not both.

Finally, I would like to thank the authors for their contribution, which is a very interesting research topic.

Kind regards,
Tarkan A Bilge

Lockwood, J.W., Dufour, C.O., Griffies, S.M., and Winton, M.: On the role of the Antarctic Slope Front on the occurrence of the Weddell Sea polynya under climate change, J. Climate, 34, 1–56, 2021.

Wilks, D. S., 2006: On “Field Significance” and the False Discovery Rate. _J. Appl. Meteor. Climatol._, 45, 1181–1189, https://doi.org/10.1175/JAM2404.1.