Southern Ocean polynyas in CMIP6 models

submitted to the Cryosphere by Martin Mohrmann et al.

This paper examines the representation of coastal and open ocean polynyas (OWPs) in the Southern Ocean in the new generation of climate models that participate to the Climate Model Intercomparison Project phase 6 (CMIP6). The authors use satellite observations to assess the main characteristics (e.g. occurrence, area, location) of polynyas in 27 CMIP6 models owing to a detection algorithm based on sea ice concentration or thickness that the authors have developed and present for the first time in the paper. Temperature and salinity profiles from an Argo float is also used to assess the representation of stratification in the Weddell Sea region under "normal" conditions versus episodes of OWP. The authors find that only half of the 27 models form OWPs and that these polynyas are underestimated compared to observations. In contrast, all models simulate coastal polynyas but the associated area is overestimated. Links between winds and polynyas, as well as between the Antarctic Circumpolar Current and polynyas are explored. Finally, the authors provide a discussion on the strategies adopted by modelling centers to reduce the occurrence of spurious polynyas in models.

The topic of this paper fits well within the stated scope of the Cryosphere and provides a valuable evaluation of Southern Ocean polynyas in the new generation of climate models. This is a subject of importance given the impact of polynyas on regional and global climate, and given the challenges the representation of these events pose to the modelling community. The paper introduces a new algorithm used for the systematic detection of polynyas in models and observations that can prove to be useful to the polar community. The paper presents a thorough evaluation of polynya characteristics, discusses a substantial amount of material, and is generally well-presented with clear figures. I have quite many major and detailed comments which should all be easy to address (see below). I feel that addressing these comments would improve the quality of the manuscript and make it suitable for publication.

Main comments

Introduction

I feel that there are some elements about the representation of OWPs in models missing in the introduction that could be added before the question or objective of the study can be stated in the last paragraph. For instance, it is well known that the modelling community struggles with the opening of spurious large OWPs as is reported in the paper through the analysis of CMIP6 models and discussed at the end. The formation of OWPs in models has been shown to be very sensitive to vertical mixing schemes (e.g. Kjellson et al., 2015; Heuzé et al., 2015), initial sea ice conditions (e.g. Kjellson et al., 2015), stratification (e.g. De Lavergne et al., 2014, Stössel et al. 2015), and model resolution (Dufour et al. 2017, Kurtakoti et al. 2018, Lockwood et al. 2021), among other things. The value of the present study is to document the representation of OWPs in the new generation of climate models and assess whether major issues still remain. I suggest the authors add a paragraph on the modelling challenges related to the representation of OWPs. This will help bring the main question addressed in the study.

Method

- How does the algorithm deal with embayments? Many OWPs become embayments as they disappear. Some models also produce large embayments with associated deep convection, very much like fully closed polynyas. Accounting or not for embayments might have a significant effect on the results (number of polynyas, areas, life time of a fully closed polynyas when it evolves into an embayment, etc). Please comment on that.
- The detection algorithm based on sea ice thickness uses 'sivol' which is the sea ice volume per grid cell area. This is not the actual thickness but rather an equivalent thickness as you explain. Have you compared the equivalent thickness (sivol) with the actual thickness (sithick) for those models which do not show the weird behaviour shown in Figure 4? It seems to me that the two variables could give significantly different results.

Results

- Modelled ocean properties are compared to that taken from one Argo float from the SOCCOM project which conveniently sampled the 2016-2017 Maud Rise polynya. The profiles taken during the Maud Rise polynya are very relevant to the present study. But I am concerned about performing the comparison based on only one float. What about also using existing climatologies that cover a larger domain to evaluate the stratification "under sea ice" (e.g. See de Lavergne et al., 2014)?
- The manuscript does not say much about the trajectory and time period covered by the float. More information is needed (e.g. coordinates of domain covered, time period, number of profiles inside and outside the polynya). Furthermore, it is unclear to me whether the comparison between the models and the float is only performed over the Maud Rise. It seems that models are averaged over the Weddell Sea but I am not sure about the float. I suggest the authors clarify that in the text so that differences between models and observations are easier to understand.
- The difference between polynya and under sea ice profiles is much smaller in observations than in models (Fig. 10). In addition, we do not see a shallower subsurface temperature maximum during polynyas in the observations as we do in the models. Please comment on that.

Discussion

- Caution should be taken when inferring causes of differences between climate models (CMs) and Earth System models (ESMs). CMs and ESMs are different in many aspects (including additional model components but not only). Given that 1/ There are only a couple of CM/ESM pairs that can be compared (that belonging to the same family) across CMIP6 and 2/ Occurrence of polynyas is very sensitive to stratification (among other factors), it is hard to be make a conclusive and general statement on the representation of polynyas in CMs versus ESMs.
- Have you investigated the role of the representation of convection (convection schemes used in models) on the formation of OWPs? If relevant, information about convection schemes could be added in Table 1 for instance.

Language

- Please check the use of acronyms (e.g. OWP, SAM) as they are used inconsistently across the manuscript.

Detailed comments

Abstract

- p.1, l.7: I suggest you change "The coastal polynya area in contrast is often overestimated" to "In contrast, the coastal polynya area is overestimated in most models."
- p.1, l.10: I do not believe that introducing the ACC acronym is useful at this stage since it is not used in the abstract.
- p.1, l.13: What is meant by "vertical discretisation"? Do you mean new or more adequate vertical discretisation (for e.g. hybrid vertical coordinate system)?

1. Introduction

- p.1, l.23-34: Replace the "/" by "or".
- p.2, l.3-31: The statement about the influence of coastal polynyas on intermediate water formation only applies to the northern hemisphere I believe. Please verify this statement, and if it only applies to the northern hemisphere remove it as it is confusing.
- p.2 , l.34-35: It is unclear whether the statement applies to coastal polynyas, to open water polynyas, or to both. Please specify.
- p.2, l.35: "The Weddell Sea Polynya has been the largest OWP to date" → "The Weddell Sea Polynya has been the largest OWP **observed** to date".
- p.2, l.38: "More than forty years later, in 2016 and 2017, the Maud Rise polynya re-opened": This sounds like the Maud Rise polynya had never reopened before. I think what made the 2016-2017 exceptional is the size of the polynya as mentioned next in the sentence. Please rephrase to convey more clearly that point.
- p.2, l.40: I believe you mean "Weddell Sea and Maud Rise polynyas" by "OWPs" (I do not think the statement applies to all OWPs).
- p.2, l.40-41: How about the hypothesis made by Gordon et al. (2007) on the role of the transition of the Southern Annular Mode from a prolonged negative phase to a positive phase in explaining the Weddell Sea polynya of the 1970s? Even if you choose to keep the sentence as is I would add the reference to Gordon et al. (2007) to the other references.
- p.2, l.41-43: The correlation between OWPs and SAM/westerlies suggest a causality (winds driving the opening of OWPs). The correlation between open ocean convection and the ACC strength also suggests a causality, but the other way round: open ocean convection strengthens the meridional density gradient which in turn strengthens the ACC transport. I find the sentence about correlation between OWPs and ACC ambiguous in the sense that it does not say clearly what this correlation suggests. The reader could infer that the ACC transport might be driving the opening of the OWPs which I do not think is what Behrens et al. (2016) argued.
- p.2, l.45: "open water polynyas" should be "OWPs" for consistency.
- p.2, l.48: Are you really going to determine the causes of the OWPs for each of the 27 models? This seems to me unrealistic as causes are often multiple and complex requiring an in-depth analysis of a simulation which seems hard to carry out for all these models in one paper.
- p.2, l.58: "open water polynyas" should be "OWPs" for consistency.
- p.2, l.54-55: "We also analyse indicators of deep convection and upwelling as possible causes for the polynya formation and maintenance in CMIP6 models." I feel this is a

bit of an overstatement as Section 4.3 presents results on stratification but does not investigate causes. The diagnostics presented do not really allow that.
- p.2, l.55: Do "polynyas" refer to "OWPs" only or to both coastal and open water polynyas?

2. CMIP6 output fields and observational data
- p.4, l.61-76: I suggest you put that paragraph in a subsection instead of having it as a long introductory paragraph to Section 2.
- p.4, l.62-63: Please define acronyms and specify what type of data OSI-450 and SMOS are (observational/reanalysis? In situ/remote?)
- p.4, l.66: "was" → "is".
- p.4, l.70-71 & 75-76: "For the assessment of polynya activity in the Southern Ocean, we use sea ice concentration and sea ice thickness data. " This sentence appears twice.
- p.4, l.83-87: That answers my comment above about the nature of the observational products used. I still think you should briefly state what type of products OSI is in the previous section, as the reader who is unfamiliar with these products will keep wondering until reading this part of Section 2.1.
- p.5, l.96-97: It would be worth mentioning that sithick is the actual thickness.
- p.5, 100: "would be" → "is".
- p.5, l.101: "but we found it not good for our polynya detection": Could you be a bit more specific? "not good" is a bit vague. E.g. do you have false positive when using this variable?
- p.5, l.109: "that the form on the sea ice" → "that form on sea ice".
- p.5, l.112: Please remove "relatively new " as you already mention it a couple of sentences ago.
- p.5, l.116-117: This sentence belongs to the end of Section 2.1 as the advantages and disadvantages are discussed in that section and not in the present section.
- p.5, l.121: "rescaled" → " rescale to"?

3. Polynya detection
- p.6, l.141: "but not by its sea ice concentration" → "but not if using the threshold sea ice concentration criteria"?
- p.6, l. 141-142: Same comment as above. The phrasing sounds a bit off to me.
- p.6, l.150: " until the first polynya areas merge with the open ocean": Do you mean that the algorithm detects an embayment instead of a closed area of open ocean within the sea ice pack? On figure 2.b., we see that only coastal polynyas are detected (as we also see in fig 2e) for a concentration threshold of 60%. However, if the threshold further increases, then the OWP re-emerges. Could you please explain why?
- p.6, l.151-152: This seems to hold for the 60% threhsold in your example, but how about higher thresholds? I understand that the polynya would be detected but would be counted as smaller than when using a threshold smaller than 60%. Do you see similar behaviours in models (e.g. a particularly poor detection around the 60% threshold)? Please comment further on that.
- p.6, l.152: This seems to hold only until ∼85% based on Fig. 2b.
- p.6, l.153: Add "in" before "most".
- p.8, l.161-162: Please specify which "data" you are referring to. Observational data as shown in Fig. 2? Please also specify what "comparison" you are referring to (between sampling frequency)?

- p.8, l.163 (equation 1): Please describe what A and N refer to, as well as what each subscript refers to right after the equation. I am not sure to understand why you used the subscript "p" instead of "d". I thought this was referring to "daily".
- p.8, l.164: Do you mean "**coastal** polynyas grow very large in summer season"? There is little sea ice in Antarctica away from the coasts in summer to allow for a OWP to grow.
- p.9, l.172: "km^3" should be "km^2".
- p.9, l.173: Could it also be due to the fact that smaller polynyas would tend to live a shorter time than larger polynyas, hence have higher chances to be detected from daily output than from monthly output, while large polynyas persists for months, so would be equally as easy to detect using monthly or daily output?
- p.9, l.179-181: Would you have an explanation for that?
- Have you looked at the effect of sea ice model complexity (e.g. number of sea ice thicnkess categories) on the level of agreement between the sea ice concentration and sea ice thickness based algorithm?
- p.9, l.190 (equation 2): Should "A_p" be "A_m" in the second equation?
- p.10, l.204-206: Add the reference to Fig.4.
- p.10, l.205-209: What is the cause of this issue? I do not think it is expected to have $\sim$1 m of ice near the sea ice edge.
- p.10, l.226-227: I am not sure what you mean by "We have seen this". Do you refer to deep convection on the shelf? Please clarify.
- p.11, l.239-243: This paragraph needs a transition with the previous paragraph or a sentence of introduction as it is a bit disconnected from the rest.

4. Polynya statistics in CMIP6

- p.11, l.246: "evaluate" $\rightarrow$ "compare".
- p.11, l.251-252: I do not see a coastal polynya in the Ross Sea in e.g. BCC- or CAMS models. Is it because this polynya opens later than September so is not visible in either Figures?
- p.11, l.255-257: Agreed, but note the systematic overestimation in the model. Could it be due to a lack of resolution (since 0.25° is still too coarse for the coastal region)?
- p.11, l.259: You can also refer to Fig. 3.
- p.11, l.264-265: This is an interesting point. I presume ocean properties and circulation, and atmospheric forcing are fairy similar within each model family? Have you looked?
- p.12, l.268: "by" $\rightarrow$ "with".
- p.13, l.285: " 21 concentration" $\rightarrow$ "21 when using concentration".
- p.13, l.285-286: Isn't it redundant with l.272-273?
- p.13, l.286: Please add a comma after "Surprisingly".
- p.13, l.299: "they form" $\rightarrow$ "as they form".
- p.13, l.300: "We believe that ...". This statement needs to be supported by analyses. Please refer to a figure or to analyses performed.
- p.14, l.306: Have you looked at the frequency of occurrence in these models?
- p.15, l.319: There are two "the".
- p.15, l.325: "inside an open water polynya compared to the stratification under sea ice" $\rightarrow$ "inside and outside an OWP." Some OWP might host very thin ice.
- p.15, l.326: What is the northernmost latitude of the Weddell Sea domain?
- p.16, l.330-332: Do you average the float profiles over the Maud Rise only or do you

take into account all profiles regardless of the region? In any case, please indicate the coordinates of the domain covered by the float as well as the time.
- p.16, l.334: "is resulting" → "results".
- p.16, l.339: Please add a comma after "(Fig. 10)".
- p.16, l.346-347: Please clarify what you mean in the parentheses. The available subsurface heat is mostly replenished by the CDW I believe.
- p.16, l.350: I am not sure what is referred to as "Active mixing processes". Rather "deep convection"?
- p.16, l.352-353: The float does not descend deeper than 2 km so please rephrase.
- p.16, l.353: "mixing" → "convection".
- p.16, l.357-358: Please clarify how this sentence links with the previous one.
- p.16, l.359: What does show similar mixing? The measurements from the 1970s Polynya? Grammatically, the subject of "shows" is "discussion".
- p.16, l.366: "is transporting" → "transports".

5. Discussion
- p.18, l.376: Please remove "will".
- p.18, l.376: " overestimation of OWPs in many models" seems at odds with "with the majority underestimating OWP area".
- p.18, 382-384: I am not sure I follow the argument. de Lavergne et al. (2014) also includes ESMs in the subset of models they analyze. Their conclusion about the cessation of the convection holds in general but there might be some significant differences across models, and between CMs and ESMs. Please clarify.
- p.19, l.402: 0.4 is a weak correlation. Please comment on that.
- p.19, l.309: Double comma in the parentheses.
- p.19, l.421-425: Alternatively, as most models simulate an ACC weaker than observations (presumably because of the misrepresentation of bottom water formation from coastal processes and spurious mixing leading to a too weak meridional density gradient), strong (overestimated) deep convection associated with OWPs compensate for that low bias through the formation of bottom waters that "correct" for the weak meridional density gradients. The sum of the two biases would lead to an improved representation of the strength of the ACC.
- p.19, l.414-425: In contrast to the winds, it appears that the strength of the ACC is not a cause for the occurrence of polynyas, but rather a consequence of large polynyas. I think this point should be made more clear.
- p.20, l.432: "general known" → "general knowledge"?
- p.20, l.443-444: See Lockwood et al. (2021) for a discussion on the role of coastal freshening on the occurrence of OWPs.
- p.20, l.459-460: I feel that Figure 11 could be moved to the Appendix as it is not a major figure of the paper.
- p.21, l.470-472: You might find Adcroft et al. (2019) useful for this part of the discussion (e.g. see their Section 3.8.3). The introduction of the new hybrid coordinate system in MOM6 did not solve as much as hoped.

6. Conclusion
- p.21, l.477: "best agreement" with observations?
- p.21, l.485: Please remove the reference to the section.
- p.21, l.485: Please specify what you refer to by "upwelling" (upward mixing of wa-

ter resulting from gravitational instability or something else)? I do not recall this was specifically mentioned/discussed in the results section (the word "upwelling" does not appear there).
- p.21, l.487-488: There is only one paragraph in Section 5.3 on the comparison between the two ACCESS models, so I do not think that sentence summarizes the main point of that section.

Data availability
- Please provide the link for SOCCOM.

Figures

Figure 1
- Please specify in the caption what grey color corresponds to.

Figure 2
- I think you should add a dark blue color in your colorbar corresponding to open water.
- The colorbar is discrete but we see shades of blue around the sea ice edge or polynya edges. I assume this is an effect of the filter. Please add a note in the caption.

Figure 4
- Please explain what the two colorbars each correspond to (The caption only says "polynya areas").
- Please explain what the grey color corresponds to. Open ocean north of the sea ice edge and ice shelves both appear in grey.
- Perhaps adding a contour of the sea ice edge on panel j) would help visualize the issue described by the authors.

Figure 6
- Please add a reference to the appropriate section and equations in the third sentence of the caption.
- The fact that some models do not have daily output or equivalent thickness leads to unequal length of the x-axis across panels a) to c) which makes comparing models across the different panels difficult. To remedy that, you could keep the full list of models on the x-axis for all panels with no data for models which miss output. I believe that would make things clearer.

Figure 8
- "open water polynya" $\rightarrow$ "OWP".

Figure 9
- Which domain did you took for the Weddell Sea? Please provide the range of longitudes and latitudes.
- The labels of months on the x-axis are really packed. Maybe you could display one label every other label or rotate the labels?
- I am not convinced that the use of symbols for displaying models is better than the use of plain and dashed lines with different colors. Symbols make the shape of the plot hard to grasp.

Figure 10
- Please indicate in the caption the change of vertical scale between the first two and last two columns.
- There are some ".8" and "-0.4" here and there on the figure that should be removed I believe.
- Does the T-S diagram use in situ or potential temperature? Which flavour of the density is used? Please specify.
- Please specify the number of profiles for the float inside and outside the polynya (in the caption or in the text). It does not seem that there are many profiles inside the polynya.
- I suggest cutting the profiles at 2 km for the models to ease the comparison with the observations. The details of the Argo float profiles are hard to see and the most interesting features appear over the top 1 km for both models and observations anyway.

Table 2
- I think keeping three significant digits for all variables (especially for sa_tot that could be written in 10^6 km^2) is enough. Also, that will make the numbers easier to read and compare.

Figure A1
- The figure is of poor resolution. Could you please try to improve that?
- I suggest you color-code the numbers in the legend (e.g. red for the coastal polynyas). That will be easier to read.

Figure A2
- Same comments as in Fig. A1.

References

Adcroft, A., Anderson, W., Balaji, V., Blanton, C., Bushuk, M., Dufour, C. O., et al. (2019). The GFDL global ocean and sea ice model OM4.0: Model description and simulation features. Journal of Advances in Modeling Earth Systems, 11, 3167-3211. https://doi.org/10.1029/2019MS001726Â´.

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Kjellsson, J., and Coauthors, 2015: Model sensitivity of the Weddell and Ross Seas, Antarctica, to vertical mixing and freshwater forcing. Ocean Modell., 94, 141-â152, doi:10.1016/j.ocemod.2015.08.003.

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https://journals.ametsoc.org/view/journals/clim/31/23/jcli-d-18-0392.1.xml.

Lockwood, J. W., Dufour, C. O., Griffies, S. M., & Winton, M. (2021). On the Role of the Antarctic Slope Front on the Occurrence of the Weddell Sea Polynya under Climate Change, Journal of Climate, 34(7), 2529-2548.
https://journals.ametsoc.org/view/journals/clim/34/7/JCLI-D-20-0069.1.xml.

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