Cryosphere
Response to reviewers

Defining Coastal Antarctic polynyas in satellite observations and climate model output to support ecological climate change research

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Thank you to the reviewers for careful and considerate reading of our submission and thoughtful feedback that will make this a stronger paper. Thank you also for your general support of the manuscript and overall suitability for publishing, and the opportunity to revise the manuscript and resubmit.

We have expanded our analysis of polynyas identified from satellite products to include the European Organization for the Exploitation of Meteorological Satellites (EUMETSAT) Ocean and Sea Ice Satellite Application Facility (OSISAF) CDR (Lavergne et al., 2019). The OSISAF retrieval algorithms represent a "hybrid" approach that combines algorithms in the retrieval process to better account for both atmospheric conditions over open water, and detection of sea ice in non-open water conditions (e.g. Ivanova et al., 2015 and references therein). Inclusion of the OSISAF has added length as well as depth and coverage to our manuscript. Results including OSISAF also allow for a more direct comparison with previous work (e.g. Mohrman et al., 2021), a greater understanding of impacts of SIC retrieval algorithms on polynya identification in satellite products, and also frame results of polynyas in the model within a range of satellite based sea ice concentration products.

Below we highlight further revisions based on reviewer feedback showing *reviewer comments* and our response.

Reviewer 1.

I see four main results in this paper:

1. *Sensitivity analysis of the various choices involved in polynya detection using observations (thresholds, concentration vs thickness, grid type, daily vs monthly, CDR vs NASA Team);*
2. *Assessment of the representation of polynyas in CESM2-LE and the effect of internal variability;*
3. *Comparison of the representation of polynyas between the fully-coupled CESM2 and the forced JRA-CESM, i.e. detangling the contributions of ocean/ice and atmosphere;*
4. *Polynyas as a hotspot for ecosystem productivity.*

All these four are interesting and important areas of research.

Unfortunately, the first three are mixed together in a story that I struggled to follow. In particular, the representation of polynyas in the models is presented as a detection issue; the fact that "there may be model biases" is only finally acknowledged line 618. Yes, one needs to know how to detect polynyas in models before being able to evaluate them, but that's why you are doing all these tests on the CDR where you degraded the resolution and/or changed the low values to reproduce known model/observation differences: You have a reference to compare your tests to. I would recommend you set the detection method, using the one most adapted to the models' detection based on the many tests you did on the observations, and only then look at the models, using this on method only. The model comparison of the two temporal resolutions would remain useful though, since different communities use the monthly and daily output.

It is also confusing that for most of the analyses you present only the results of JRA-CESM, when 1. Figure 1 suggests that CESM-LE has a more accurate sea ice and 2. The NPP analysis at the end is done on CESM-LE. That is, the case for using JRA-CESM is not well motivated. As I said above, the comparison between JRA-CESM and CESM-LE would allow you to discuss the effect of forcing vs full-coupling, but you do not discuss this for now. Removing JRA-CESM from this paper could allow you to keep this discussion for a dedicated study. You could even consider using only the subset of ensemble members that are the most accurate for your NPP analysis; according to Figure 8b, bottom panels, some are ok-enough.

Despite how this may sound, I suspect that all the work is there already, and that it is only the text that needs re-arranging for your argument to be convincing. The rewriting would be substantial though, so I do not provide minor comments that could become irrelevant this time.

Thank you for pointing out how mixed up our messages were in the original manuscript. To increase clarity we have added more discussion in the introduction on potential model bias in simulating Antarctic Sea Ice (L51-63), a bit more on retrieval algorithms (L67-72) and clearly state our 5 primary goals up front and in the introduction (L109-119) and hope that this adds some clarity and structure to our intent and the narrative. We clearly stated in the introduction why a comparison in polynyas in observational products and the model should start with the forced ocean-sea ice (FOSI) model forced by atmospheric reanalysis (L180-183).

Additionally, we restructured the results section with separate sections to add clarity and aid in cohesive narrative:

4.1 large-scale sea ice properties

4.2 polynyas identified in the satellite products

4.3 polynyas in the forced ocean-sea ice hindcast CESM2 compared with those in the satellite products

4.4 sea ice concentration definitions and degrading model output to more closely resemble satellite-based SICs

4.5 relationships between polynyas and net primary productivity in the hindcast simulation (JRA-CESM)

4.6 polynyas in the fully coupled CESM2 with comparisons during the historical period to the JRA-CESM, and polynyas and NPP in a changing climate

The figures need adjusting as well to increase readability:

- *Figure 1, black and dark blue are hard to distinguish (grey instead of black?);*
- *Figure 2, the dark blue asterisks are hard to see against the dark blue low ice concentration (have them e.g. orange, and switch red to magenta?);*
- *Figure 3 onwards, the four different shades of blue are barely distinguishable;*
- *Figures 4-6, please have bigger fonts in the legend*

Figures have been modified (and some renumbered as we have more figures):

Figure 1 changed colors

Figure 2. Changed colors for the open water polynyas to a pink for better visualization on the blue sea ice background.

Figures 3-4, 6 Colors have been changed and hopefully easier to distinguish

Figures 11-12 Colors for the SH polynya area and number timeseries have been changed for consistency with Figures 3-4, 6.

Reviewer 2.

The study under review presents an analysis of the sensitivity of polynya occurrence and area to polynya definition thresholds in satellite observations and climate model output. The authors subsequently use this analysis to construct methodological recommendations for future studies on polynyas. In my opinion, these two aspects represent the main two contributions of the study; methodological advice for polynya studies, and numerical evidence for the way in which resolution/data-type/season influences polynya statistics. This submission is original, being the first such study to quantitatively demonstrate the influence of resolution and data type (e.g. model/observational) on polynya number and area. The main significance of the study lies in the methodological recommendations and accompanying numerical justifications which can be used by future studies to consistently define polynyas without the requirement for authors to invent their own definitions (e.g. Mohrmann, 2021). In my opinion, the scientific content of the article is clearly articulated and well presented and only requires minor corrections prior to publication.

Below I have included a small number of specific issues which I would like to bring to the authors' attention.

*The authors have explained that this study is focused on coastal polynyas, (line 165), but in the study abstract and in the the conclusions (e.g. conclusions 3, 4, 6) refer to "polynyas" more generally. Given that the mechanisms which cause coastal polynyas and open-water polynyas are different, it is not necessarily possible to extrapolate conclusions from the analysis of one to the other. For example, the authors explain that SIT may be a better metric in winter due to air temperatures causing almost immediate surface freezing - I would expect this effect to disproportionately impact coastal polynya identification, because open-water polynyas feature upwelling of warm water which might less readily freeze. Related to this comment, Mohrmann et al. (2021) found that the "CMIP6 CESM2 models show coastal polynyas, but never OWPs", so extending analysis to open-water polynyas using CESM2 might not be possible. Instead, I would recommend the authors to caveat these potential differences, and clearly state in the abstract, conclusions, and main text that results are relevant for *coastal* polynyas. This may sound like a pedantic point, but since this study aims to make recommendations on polynya definition and thresholding to future studies, it is important that future studies on open-water polynyas recognise that not all of the thresholding analysis has been carried out with open-water polynyas in mind, and therefore the conclusions may not be suitable for application in their studies.*

Your point is well taken – thank you! Yes, open water and coastal polynyas typically form through different mechanisms. Our primary intent is to focus on polynyas that are considered important for marine ecosystems – i.e. coastal polynyas - and we have changed the title of the paper to reflect this. We add text in the introduction re. open water vs coastal polynyas and model biases (L52-57) and specify that we are focusing on marine productivity in coastal polynyas (L112-119). We also mention why the CESM2, with the “mushy” thermodynamic component that allows for prognostic salinity within sea ice and results in greater frazil ice production along Antarctic coasts is a good candidate for this study (L192-193)

We show both open water and coastal polynyas in our examples in Figure 2 and speak to identification of both open water and coastal polynyas in the two observational products (L339-346) to show how different retrieval algorithms and gridding can influence both open water and coastal polynya identifications and we explicitly state that the remainder of the paper will focus on coastal polynyas (L347-348).

In section 4.6, polynya number and area are analysed in the CESM2-LE, and are found to decrease towards the end of the century. I think the study would benefit from some analysis or informed speculation as to the mechanism which drives this decrease in future polynyas under emissions scenarios. The reduction could be a result of changing mean-wind fields, or more simply due to a reduction in overall Antarctic sea-ice. If the authors compare the ~2090 mean Antarctic July and November sea-ice field with the ~2020 fields, they might find that a mean field difference could explain this reduction. This might also explain why the November polynya statistics drop off more readily than the July ones.

We agree – and find it interesting that polynya areas show more wintertime decadal variability when using SIT as a metric than SIC. There is also tremendous interannual variability in the November (Figure 11) which is now highlighted by the light shading that shows the full range of polynya area values for all 50 members of the CESM2-LE. We did look at mean changes in sea ice fields – concentrations as well as thicknesses – at the beginning and the end of the 21st C in

the CESM2-LE and find that decreases in SH polynya areas are not easy to distentangle from the regional changes in sea ice. For example, the Indian sector shows the largest declines in both SICs and SITs, however the Weddell shows far greater changes in SIT than SIC and yet both regions have months that show significant declines in coastal polynya areas (Supplemental Figure S8-S9). We have concluded that further analysis into how and why polynyas are changing in the CESM2 is beyond the scope of this current submission (which has become very long with the addition of more satellite-based SIC analysis).

On line 312, the authors say that April to October the polynya area is higher for daily mean than monthly mean data, often by more than one standard deviation. Should this mean that the dark blue line and light red shaded areas should be separate in Figures 3a, S2 (Apr-Oct)? Perhaps the lines are too thick or the plots are too small to appreciate this, could the authors check this statement. The authors go on to conclude than monthly mean and daily mean polynya areas are comparable on a hemispheric basis, so this point does not seem critical for the main argument.

We've added an additional observational data product to the analysis and our statement in the previous version of this manuscript was inaccurate. We've rewritten this section to reflect that regridding from EASE to model grid has a discernable impact on integrated SH polynya areas identified using the NOAA product but not on polynyas in the OSISAF product (L361-372).

On line 456, the authors mention that the SIC threshold modelled polynya areas are closer to the satellite areas, this seems to be only really true for the monthly data. On this note, there seems to be quite a large difference between daily and monthly averaged JRA-CESM SIC thresholds in November. Conclusion 5) states that polynya areas are comparable between monthly/daily thresholds on a hemispheric basis, but as mentioned in Line 596, and as is visible in Figure 4c, there is a seasonality to this, and while it is true on an annual-average basis, the CDR daily to monthly cross-correlation drops to ~0.65 in December. In general, Conclusion 5) is important result, and I think the authors should consider modifying the phrasing to be '...data are comparable on an annual averaged and hemispheric basis', or similar.

Our regional results (Figures 6-8 and Table 2) and discussion (now section 4.3.2) highlight how complicated this can get. Daily SIT data in the JRA-CESM captures higher polynya areas in the Bellingshausen-Amundsen sea region than monthly SIT data, although the reverse is true in November in the Ross Sea sector. Differences in polynya areas estimated by daily vs monthly data are largest in different regions but tend to be small when integrating polynya area over the entire SH. There is not only seasonality and regionality in these comparisons, but also differences when looking at the OBS products even on an hemispheric basis. For example, total SH polynya area and variability in November in the NOAA/OSISAF products are best captures by SIC/SIT metrics in the model. So we conclude both that "polynya areas identified from monthly and daily data are comparable on annually averaged, integrated hemispheric basis" (L831-832) and that optimal metric and threshold choices will be influenced by grids, regions, and seasons of interest" (L837).

We hope that this is clearly conveyed in the greatly expanded section 4.2 (polynyas in the satellite data) and comparisons with the model (section 4.3)

Further technical corrections:

Line 162: reference should be Mohrmann et al. (2021). Please check spelling of Mohrmann elsewhere in the text.

Good catch! Thank you and we've corrected throughout the manuscript.

Line 243: could authors double-check that '≥' sign is correct. Will depend on journal precedent.
Thanks, corrected (L289)

Line 293-297: repeated point about CDR vs NASA Team polynya comparison.
Corrected.

Line 454: unmatched bracket at end of line.

Text has been substantially altered and this sentence is no longer there.

Line 547: Figure 8 caption; missing space before comma, should use 'a)' and 'b)' in caption text, formatting of following needs to be consistent "(red; 85% SIC)", "(85% SIC; black)", "(0.4m SIT, blue)".

Thanks, corrected (now Figure 11)

Line 630: long space.
Thanks, corrected (L787)

Line 715: Figure A2 caption '(REF)' left in.
Thanks, corrected

Line 735: Figure B1 caption capitilisation of 'Sea Ice Concentration' inconsistent with other figures e.g. Figure B2.
Thanks, corrected

Line 760: Figure C1 caption has no '(d)'.

Thanks, corrected

Many thanks to the authors for their submission, which I feel contains a valuable contribution to the study of Antarctic sea ice, and I hope that the comments above prove useful in finalising this work.

Thank you for your kind words that are very much appreciated particularly during this time of tremendous uncertainty and changes in the research environment in the US.

Reviewer 3

The MS is comparing the polynya size and location from satellite SMMR, SSM/I – SSMIS microwave radiometer sea ice concentrations (SIC) and climate model simulations and then use the model simulations to assess the polynya ecosystems and long-term trends over the period where satellite data are available (since 1979). The topic is very welcome and the MS is well written and structured. My primary concern is the relatively poor match between polynyas detected from the satellite observations and the model simulations. This does not give high confidence in the results. Part of that mismatch is due to real differences between observed and model simulated polynyas and part of that is due to the definition of polynyas in the two datatypes, spatial resolution etc. as discussed in the MS. The satellite microwave radiometer SIC data are relatively consistent over the study period (because the SIC algorithm is using the 19 and 37 GHz channels) and it covers from 1979 until today. However, as also mentioned in some of the references (Markus and Burns), this channel combination is not ideal for mapping polynyas. For example, due to coarse resolution and sensitivity to new and mature ice types and also the way the data are processed excludes low SIC from being detected (weather filters). The NASA Team SIC is included for comparison, but it is using the same 19 and 37 GHz channels as the "CDR" and it has approximately the same spatial resolution and new and mature ice sensitivity. It also uses weather filters which excludes the detection of low SIC. The microwave radiometer swath data are resampled to daily 25 km x 25 km grids while the spatial resolution of the SIC is approximately 45 km. Other satellite radiometer datasets for detection of polynyas mentioned in the MS (Arrigo & van Dijken using the Markus & Burns algorithm and detections based on OSISAF SIC in Mohrman et al.) are not used for comparison which contributes to limited confidence in the detected location and size of the polynyas.

I would suggest to compare the detection of polynyas in the CDR dataset with other (already published) algorithms for polynya detection. Hopefully this will lead to a better match with the model simulations. I think that this is needed to give confidence in the model simulations.

This is an excellent recommendation, and we appreciate the thoughtfulness and insight of this recommendation from a clear satellite data expert.

We have expanded our analysis of polynyas identified from satellite products to include the European Organisation for the Exploitation of Meteorological Satellites (EUMETSAT) Ocean and Sea Ice Satellite Application Facility (OSISAF) CDR (Lavergne et al., 2019).

Specific comments

L51 and 53: I don't think that "Passive microwave images" is a descriptive name for the CDR sea ice concentrations. Please find a better term for it and change it throughout the MS.

We have also replaced "passive microwave data" with words such as "satellite radiometer data", "satellite-based SICs", "passive microwave algorithms", "passive microwave SIC product" throughout the manuscript.

L53: "Passive microwave images tend to underestimate SIC's… " This is not accurate as a general statement about "passive microwave" data. The sensitivity of the microwave radiometer data to sea ice concentration and the inherent noise depends on the selection of channels and the algorithm used for processing the SIC as described in Ivanova et al. I would suggest to rewrite this entire paragraph L47-L87 with reference to the specific data which is used in this study (CDR). The level of detail about the CDR data is not adequate.

We have changed our description of the satellite data for accuracy and clarity (L67-71). We have also replaced "passive microwave data" with words such as "satellite radiometer data", "satellite-based SICs", "passive microwave algorithms", "passive microwave SIC product" throughout the manuscript.

L200: Polynyas are formed in a dynamic environment and it is unlikely that nilas grows to a thickness of 5 cm on scales of the satellite radiometer footprint or resolution of the model. The type of new ice formed in a dynamic environment is pancake ice and pancake ice does not reach 100% SIC before consolidation is reached at a thickness higher than 5 cm. In addition, different SIC algorithms have different sensitivity to new and mature ice. Please specify which algorithm that you are referring to.

L246-247: Thank you for pointing out the confusion/inaccuracy. We changed the wording here to be more clear.

L258: 10-14% is that approximately 2 million km2? Please provide the difference in percent and in square kilometers.

L300: Added "(~1.4-2.6 x 10^6 km^2)"

L425: Are the thin ice formation processes in the model realistic considering the dynamic environment where polynyas appear? Please, include a discussion of ice formation processes in polynyas.

We've added more description and reference for details of the thermodynamic and dynamic sea ice model (L185). The version we use includes a "mushy" layer physics scheme that allows for prognostic salinity within the sea ice and results in more frazil ice formation along the Antarctic coast in polynya-like features (Bailey et al. 2020, Singh et al., DuVivier et al, 2021) and is particularly important process in Antarctic coastal polynyas (e.g. Nakata et al., 2021). We have added more description of the sea ice model to the manuscript (L185-193).

L691: Please specify the source and date of the "satellite image" in figure AA2 (MODIS?). L705 (fig. AA2) the title is truncated and there is a reference missing in the fig-text.

Thank you for pointing out the lack of source data which has now been added (L875-878).

L735: in the figure title, I think that it should be "25 x 25 km2"

Yes, should read 25 km X 25 km and has been corrected (L900). Thank you.

L740: If polynya detection depends on grid resolution a lat-lon grid is not optimal. It never is in polar regions.

Agreed. However most earth system model (ESM) output is on lat-lon grid and thus we feel it important to include this example and give focus to this in the paper.

References

Bailey, D. A., Holland, M. M., DuVivier, A. K., Hunke, E. C., & Turner, A. K. (2020). Impact of sea ice thermodynamics in the CESM2 sea ice component. *Journal of Advances in Modeling Earth Systems*, **12**, e2020MS002154. **https://doi.org/10.1029/2020MS002154**

DuVivier, A.K., Molina, M.J., Deppenmeier, AL. *et al.* Projections of winter polynyas and their biophysical impacts in the Ross Sea Antarctica. *Clim Dyn* **62**, 989–1012 (2024). https://doi.org/10.1007/s00382-023-06951-z

DuVivier, A. K., Holland, M. M., Landrum, L., Singh, H. A., Bailey, D. A., and Maroon, E. A.: Impacts of sea ice mushy thermodynamics in the Antarctic on the coupled Earth system, Geophys. Res. Lett., 48, e2021GL094287, https://doi.org/10.1029/2021GL094287, 2021.

Hunke, E. C., Lipscomb, W. H., Turner, A. K., Jeffery, N., & Elliott, S. (2015). CICE: The Los Alamos Sea Ice Model. Documentation and software user's manual. Version 5.1. T-3 Fluid Dynamics Group, Los Alamos National Laboratory, Tech. Rep. LA-CC-06-012.

Ivanova, N., Pedersen, L. T., Tonboe, R. T., Kern, S., Heygster, G., Lavergne, T., Sorensen, A., Saldo, R., Dybjkaer, G., Brucker, L. and Shokr, M.: Inter-comparison and evaluation of sea ice algorithms: towards further identification of challenges and optimal approach using passive microwave observations, *The Cryo.*, **9**, 1797-1817, https://doi.org/10.5194/tc-9-1797-2015, 2015.

Lavergne, T., Sørensen, A. M., Kern, S., Tonboe, R., Notz, D., Aaboe, S., Bell, L., Dybkjær, G., Eastwood, S., Gabarro, C., Heygster, G., Killie, M. A., Brandt Kreiner, M., Lavelle, J., Saldo, R., Sandven, S., and Pedersen, L. T.: Version 2 of the EUMETSAT OSI SAF and ESA CCI sea-ice concentration climate data records, *The Cryo*., **13**, 49–78, https://doi.org/10.5194/tc-13-49-2019, 2019.

Mohrmann, M., Heuzé, C., and Swart, S.: Southern Ocean polynyas in CMIP6 models, The Cryo., 15, 4281–4313, https://doi.org/10.5194/tc-15-4281-2021, 2021.

Nakata, K.; Ohshima, K.I.; Nihashi, S. Mapping of Active Frazil for Antarctic Coastal Polynyas, With an Estimation of Sea-Ice Production. *Geophys. Res. Lett.*, **48,** e2020GL091353, 2021.

Singh, H. K. A., Landrum, L., Holland, M. M., Bailey, D. A. and A. K. DuVivier, A. K.: An overview of Antarctic sea ice in the CESM2: analysis of the seasonal cycle, predictability, and atmosphere-ocean-ice interactions. J Adv Model Earth Syst., 13, https://doi.org/10.1029/2020MS002143, 2020.