Review of “Representation of polynyas in the Ross Sea coupled atmosphere-sea ice-ocean model P-SKRIPSv2” by Gossart et al.

This review is co-signed by François Massonnet and Noé Pirlet (UCLouvain)

Summary

In this study, Gossart et al. present P-SKRIPSv2, a regional atmosphere-ocean-sea ice model based on the WRF atmospheric model, the MITgcm ocean model and the Semtner 0-layer sea ice model with viscous plastic rheology, to understand the importance of coastal polynya formation processes in the Ross Sea and the sensitivity of the model results to the choice of the air-sea ice drag coefficient.

General comments

- The paper does a good job at comparing model output with a wealth of observational data (from satellite, reanalyses, cruises, hydrographic moorings, …) but is at times lengthy and quite descriptive.
- We have the impression that also the scientific question of the paper is not entirely clear to us. Part of the paper is used to evaluate the model while part of the paper is about sensitivity tests on the drag coefficient. Since the science question is not clear, the choice of the domain is not entirely clear either.
- The paper would be more suited to GMD in the present form, since it is essentially a model evaluation study and not a study about a physical process
- The structure of the manuscript should be consistent across the title, abstract, results, discussion, and conclusion. We recommend clearly defining the central research question and reorganizing the manuscript around it.
- In evaluating the realism of the simulated polynyas, the authors do not compare their extent, shapes, or the associated sea-ice production with existing observational estimates (e.g., Nihashi and Ohshima, 2015; Nakata et al., 2021) or model estimations (e.g. Pirlet et al., 2025). If the goal of the paper (and the central scientific question) is about polynya formation, then adequate diagnostics should be used.

Positioning with respect to the state-of-the-art

- The very recently published paper of (Pirlet et al., 2025) is a key one to cite. We understand that the authors may have not seen it when it was published, but it would be good to position the current paper with respect to this paper (about modeling of Antarctic coastal polynyas). We also encourage the authors to read the (less recent) papers by (Huot et al., 2021) and (Van Achter et al., 2022) where similar questions are treated.
- The paper of Pelletier et al., (2022) might also be worth having a look at since it covers similar aspects (fully coupled model) to what is encountered with P-SKRIPSv2.
- The paper Noel et al., (2025) about coastal polynya-atmosphere feedback is also missing in the introduction.

Methodological questions

- Line 70: does P-SKRIPSv2 account for snow-ice formation, which can be quite an important process in the Southern Ocean?
- Even though the study is largely based on the previously published paper of Malyarenko et al. 2023, it would be useful to repeat (1) what the boundary conditions (ocean and atmosphere) are or the model setup (especially since these boundary conditions are said to be a cause of model error, see lines 145,280,316), (2) what was the tuning procedure for the model, (3) whether the model was spun-up or not.
- The methods are a bit shallow regarding the period used. From the figures we deduce that 2017 was chosen, but then several questions come up: why choosing only one year to perform a model evaluation; and why choosing that year in particular?
- In Fig. 2, the “polynya” regime is defined as when SIC is equal to 0.6-0.8. Where does that number come from? SIC can take, on average over the domain, the same value for many possible configurations, including ones that do not have polynyas. For model data, SIT is (additionally) employed to detect coastal polynyas, as it helps mitigate the model’s tendency to overproduce sea ice and thereby prevents polynyas exhibiting near-100% concentrations for unrealistic reasons from being missed. Could another threshold or variable change your results? Can you motivate your choice ?
- How about tides, waves, ice shelves? Are these processes relevant for polynya formation, and if yes, are they captured / accounted for by the model? If not, what are the implications on the results, on the realism of the simulation?
- The area studied in this paper is infested with icebergs, which have huge impact on the landfast ice and then polynyas. Icebergs can also modify water masses when they release freshwater. Does that affect the model results?
- The Willmott index used in Table 2 is a rather unusual one, consider detailing its meaning in the section on methods. Also, this acronym is introduced before being cited.
- Subsection 2.3 is quite dense as a single paragraph; we recommend splitting it into several shorter paragraphs, for instance one for each type of dataset.

Other remarks

- The abstract is very short and does not render the breadth of the findings of this study. The authors should consider including more context, more results, and more perspectives.

Writing

The writing is in general impeccable here is the few typos we found

- L44:)) ->)
- L103:)) ->) to times
- L104:)) ->)
- L103: start -> stars (yellow) and stars -> star (purple)
- L106:)) ->)
- L113:)) ->)
- L154: “larger” than what ?

- L189: man -> many?
- L191: need a space between “Figure 5” and “as” otherwise it’s confusing
- L200: W.m^-2 is missing, also a “;” is missing after “heat”; on that line, use the proper symbol for +/-
- L202: remove “and” after “wind”
- L222:)) ->)
- L339: displays -> display
- L359:)) ->)

Suggestions:

- L6: We propose “coastal polynyas activities” instead of “polynya activity”.
- L49: You could directly mention here which are the coastal polynyas instead of later as in L52.
- L158: We do not understand the sentence “Satellite products also show insufficient variation over the year.”.
- L166: “AR” is never defined.
- L217: the acronym “WI” is used but only introduced in a table later; consider expanding it here too.
- L411: Maybe use “realistic coastal polynyas activities” with “realistic polynyas” to be coherent with the rest of the paper.

Figures

- Figure 1:
    - Change “simulated domain” to “domain of simulation” since a domain is not simulated.
    - In panel c, consider adding the text of the different items (red box, red star, green start, etc.) directly in the figures near the symbols, this would be much more visual.
- Figure 2:
    - We would propose to change the color of the lines to make the distinction between satellite, reanalysis, and model output. Cryosat, AMSR, NOAA, SSMI and Bootstrap would go in shades of blue; GLORYS and ORA would go in shades of red; and the three sims would go in shades of green (for example). The use of dash vs solid lines is a bit confusing.
    - Alternatively, consider using a shading to display the range of observations.
- Figure 5: There is a “a)” label, but no figure panel corresponds to it.
- Figure 6:
    - We suggest merging it with Figure 5, as both figures share the same data.
    - If we get it right, in the last sentence: the AMPS dataset should be indicated in blue (not pink), and conversely the AWS data should be shown in pink (not blue).
- Figure 7-8: Axis labels are not always necessary on every panel; we suggest keeping them only on the outer edges of the figures. This recommendation could also apply to other figures.
- Figure 9-10-11: We suggest merging these figures, as they provide similar information, and moving some of the panels to the Supplementary Material.

- Figure 12-13:
    - We are not clear why the bathymetry of the model and the references are so different. Can the authors clarify this?
    - The legend is not sufficiently detailed or explicit.

References

Huot, P.-V., Fichefet, T., Jourdain, N. C., Mathiot, P., Rousset, C., Kittel, C., and Fettweis, X.: Influence of ocean tides and ice shelves on ocean–ice interactions and dense shelf water formation in the D'Urville Sea, Antarctica, Ocean Model., 162, 101794, https://doi.org/10.1016/j.ocemod.2021.101794, 2021.

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