

Representation of polynyas in the Ross Sea coupled atmosphere-sea ice-ocean model P-SKRIPSv2

General Comments

This study uses the coupled P-SKRIPSv2 model in the Ross Sea region and evaluates the model's ability to simulate polynya activity (the Terra Nova Bay Polynya and the Ross Sea Polynya). A sensitivity test on the air-sea ice drag coefficient is also conducted. The results and the model are interesting. However, a major effort to improve readability is needed, including restructuring the manuscript and clarifying the use of datasets and figures. Moreover, I believe that a more thorough analysis of the impact of changes in the drag coefficient on sea ice is needed before the paper is suitable for publication. I also have other scientific questions or comments (on the impact of air-sea drag coefficient on sea ice, the characterization of the polynya activity or the characteristics of the water masses in the region). I have indicated my major comments below.

Major Comments

1. The presentation of the results is not sufficiently clear, making this section difficult to follow. I recommend splitting it into two distinct parts: one focusing on the representation of polynyas and the various comparisons with observations in each environment, and another dedicated to the sensitivity test results on the air-sea ice drag coefficient, comparing only the outcomes of the sim00x simulations.
2. Overall, across both the main article and the supplementary material, it appears that too many datasets are used (including six for sea ice and seven for the ocean—reanalyses and measurements included). Several of these datasets do not seem essential to the study's objectives, as the induced dataset-to-dataset comparisons (outside the scope of the article). Selecting a smaller subset of datasets, or presenting them in a more streamlined manner—for instance by averaging or only showing the minimum or maximum values—would improve readability and help shorten the supplementary material.
3. Additionally, this manuscript includes a very large number of figures, some of which could be removed or moved/exchanged to the supplementary material. To give a representative example, the same figure appears twice (Fig. 8 and Fig. S7). Panel numbers should be added to the figures and referred to in the text. The colorbars are not always very legible, and the plot colors are often difficult to distinguish. In some cases, labels or colorbars are missing.
4. It is not clear from this analysis how, and to what extent, changes in the air-ice drag coefficient affect the sea ice field (e.g. with maps):
 - a. How are ice thickness and concentration affected in the region?
 - b. What are the effects on ice drift and ice production within the polynyas?
5. This paper focuses on the representation of polynyas, but no quantities related to polynya activity are presented. For example:
 - a. What are the areas, shapes, and sea-ice production of the polynyas in the region compared to observations (e.g., Nakata et al., 2021: <https://doi.org/10.1029/2020GL091353>)?
 - b. How do these quantities change in response to variations in the air-ice drag coefficient?
6. Looking at the mooring measurements (Figs. 9, 10, 11), the simulated salinities appear to differ significantly from the observations. Could you present the characteristics of the water masses simulated by the model compared to observations (e.g., using T-S diagrams)?

Specific Comments

Please add numbers or letters to each subfigure (as in Fig.1)

Abstract

- You could be a bit more explicit about your results (e.g., regarding the model's performance or the impact of the air–sea ice drag coefficient).

Introduction

- L21: You may also consider adding the following references: Golledge et al. (2025) (<https://doi.org/10.1038/s43017-024-00634-x>) and Noel et al. (2025) (<https://doi.org/10.1029/2025JD043319>), which respectively review and address this question in the context of Antarctic coastal polynyas.
- L30-39: This paragraph is confusing for the rest of the study. The ocean–ice parameter seems to be introduced and mixed with the air-ice drag parameter together, even though the sensitivity test focuses solely on the air–ice drag coefficient.
- L44: Replace (with (
- L45: You may also consider mentioning the difficulty that CMIP6 GCMs have in forming dense waters such as AABW (Heuzé, 2021 — <https://doi.org/10.5194/os-17-59-2021>).
- L55: The term “response to the polynya extent” is somewhat misleading, as the polynyas’ area (or their evolution) is not provided elsewhere in the study.

Methods

- L66-67: I did not understand what you meant.
- In subsection 2.1, I would have appreciated if you specified which forcings were used, how the model was initialized, and whether a spin-up was applied for the ocean—and if so, for how long.
- L83: It is stated that the default simulation is sim001, but in several figures, sim002 is used as the reference (e.g., Fig. 5, Fig. 6, Fig. 8). Why is this?
- L84-86: The formulation is unclear.
- Subsection 2.3 should be revised or better introduced (see Major Comments above).
- Note that, to improve readability throughout the manuscript, you could consider referring to each of the moorings not by their full names, but simply as “moorings” + number/letter or by using an acronym or a short descriptive label and which could be added to the legend of Figure 1c.
- L103: Change “start” to “stars” and indicate in Figure 1c which ones are DITN and DITD.

Results

I will remain brief on this part, which should be restructured and partially rewritten by the authors (see Major Comments above).

- In subsection 3.1 on Sea Ice, too many products are discussed, which dilutes the focus on the simulation itself. In addition, the time series alone are insufficient; mean maps of ice thickness or concentration are missing, which are needed to properly discuss the model's representation of sea ice and would allow the characterization of polynyas in the simulations and observations. Including these would strengthen the results (L142–146), especially regarding the differences caused by the boundary conditions (L145). The subsection on sea-ice thickness should be shortened. The colors of the plots in Figures 2 and 3 should be changed, as the curves are hardly distinguishable.
- Subsection 3.2 is particularly interesting. In Figure 4, the x-axis should be changed to show the month names, as in the other figures (e.g., Fig. 2 and 3), and the pink color is difficult to read.
- L213-214: The terms “fairly close” and “quasi-overlap” should be qualified when comparing the distributions of relative humidity, wind speed, and pressure (Fig. 6 and Fig. S5).
- L216: For “largest RMSE,” please provide the actual values.
- In subsection 3.4, the colorbar used in Figure 7 is not clear. Similarly, Figure 8 should be revised: the maximum depth needs to be changed, there is no legend for the top panels, the bottom panels are hard to read, and the colors are illegible.
- Sections 3.5 and 3.6 are difficult to follow, and the figures are hard to read (Figs. 9, 10, 11, 12, 13), with poorly legible colorbars and colors. Additionally, in section 3.5, the errors in salinity of the simulations compared to observations (Figs. 9, 10, 11) are barely discussed. Perhaps,

before discussing salinity/temperature variability, the water masses in the region could be characterized (e.g., TS diagrams) in the simulations compared to one of the ocean products/reanalyses.

Discussion

The discussion is interesting, but it seems to me that subsection 4.2, dealing with the uncertainties of the datasets, falls outside the scope of the article.

- L339-340: Provide values for the simulations.
- L344: Values ?
- L347: Values ?

Conclusions

Very good conclusion