Review : Pan-Arctic Polynya Activity and Sea-Ice Production in a Coupled Ice-Ocean Model from 2000 to 2018

Summary

The manuscript characterises the activity of Arctic polynyas, with a focus on their contribution to Arctic sea-ice production. The study relies on a simulation with the coupled OPA-neXtSIM model forced by ERA5, in a previously validated pan-Arctic configuration, run here over 2000–2018.
The simulated polynyas are compared with satellite-based observations (Preußer et al., 2019). Two polynya classification methods are used: a sea-ice thickness criterion (with two thresholds, 20 and 30 cm) and a sea-ice production criterion (with a single threshold, 3 cm/day). The main objective is to estimate the sea-ice production in Arctic polynyas relative to the total Arctic winter ice production.

General comments

The manuscript is generally pleasant to read. A study of Arctic polynyas is timely, given the many recent studies addressing polynyas with both observations and models. The paper puts forward a strong result: polynyas cover about 2% of the ice-covered Arctic Ocean but account for about 11% of total Arctic winter ice production.
However, I believe the manuscript requires further work in three respects: validating the simulated polynyas against observations more thoroughly, revisiting and clarifying the use of the polynya classification methods, and better articulating the sections. In particular, all detection criteria are used from beginning to end, with their advantages and drawbacks discussed, but no recommendation is made as to which one should be used, even though the final production estimates rely on dvi003. More generally, the text does not support the conclusions clearly enough. I detail below the points I consider most important.

1. Structure and organisation of the paper

In its current form, the manuscript is difficult to follow, with the impression that different pieces of information are mixed throughout the text. The research questions need to be stated more clearly and addressed one after the other: first, a clearer validation of the simulated polynyas, which would allow a single detection criterion to be selected; then, a clearer characterization of polynya activity and ice production.
The case studies should also be better integrated. They are not introduced among the objectives of the paper, and their results are not clearly identified in the conclusion or the abstract. In particular, I did not clearly understand: which questions do they address? Why were these regions and periods chosen? What can be learned from them?

2. Polynya classification

Two detection methods are used, based on sea-ice thickness and sea-ice production respectively. Each relies on a threshold above or below which a grid cell is considered part of a polynya: two thresholds for thickness (20 and 30 cm) and a single threshold for ice production (3 cm/day).

This number of tested thresholds is very small and does not seem sufficient to determine which one best defines polynyas in the model. I think it is necessary to explore more widely the range of thresholds classically used in the literature for each criterion.
I also do not quite understand why sea-ice concentration was not used, although, as you recall in the text, it is used in several studies (see L126). One of the stated objectives of the paper is to "examine the sensitivity of simulated polynya estimates to different polynya classification methods, offering guidance for future model evaluations" (L56–57). To complete the study, I believe it is important to include this criterion.
Finally, an important point in the comparison between thickness-based and production-based detections is that the production criterion includes the marginal ice zone (MIZ), which does not correspond to polynyas when using the thickness criterion. Could this be resolved by adopting a more elaborate method, such as the flood-fill algorithm used by Wong et al. (2026) or Mohrmann et al. (2021)? Alternatively, could the thickness criterion be complemented with other variables, such as SST, sea-ice concentration or sea-ice production? With an improved detection method, the areas identified as polynyas along the ice edge (Figure 2d,e) could be excluded.

3. Validation of the simulated polynyas against observations

In a large part of the paper, polynyas detected with t02, t03 and dvi003 are used simultaneously. This makes the reading difficult, dilutes the main results, and limits the number of thresholds and detection methods that can be explored. To lighten the text and allow a wider range of methods and thresholds to be tested, I suggest defining a dedicated validation/definition section, in which the implications of the different detection methods are compared, and then using the selected definition in the rest of the paper.
For this validation, it is necessary to add maps of ice production and polynyas from the observations, as well as polynya areas and ice production values (see Specific comments). In the conclusion, the relative polynya IP values are those obtained with dvi003, but values for all three criteria are given without a clear justification of why this one was retained.

4. Comparison between modeled and satellite-derived ice production

To estimate polynya ice production relative to the total Arctic production, you rightly use the production computed by the sea-ice model. However, you then compare it directly with the production estimated from satellite observations. As you note in Section 2.6 (L172–174), the satellite estimate does not account for the ocean-to-ice heat flux, which can lead to differences between modeled and estimated production.
A recent study by Noel et al. (2026, https://essopenarchive.org/doi/full/10.22541/essoar.15002648/v1, it seems this study is still under review) addresses the comparison between modeled and satellite-derived ice production in Antarctic coastal polynyas, where the impact of the oceanic heat flux is presumably smaller than in the Arctic. It shows that a meaningful model–observation comparison requires applying the same estimation method to both. To account for the methodological differences and treat the role of ocean heat fluxes consistently, I think it is necessary to apply the same ice production estimation method as Preußer et al. (2019) to the model output.

Specific comments

Abstract

- The abstract should be revised once the General comments have been addressed.
- L6: The landfast ice parameterization appears to improve the representation of polynyas, but no sensitivity experiment seems to have been performed. Mentioning this point in the abstract may therefore not be necessary.

Introduction

The introduction provides a well-written context.

- L41: Please note that the landfast ice prescription of Pirlet et al. (2025) concerns the Antarctic. There, landfast ice depends on grounded icebergs, which cannot be represented by a parameterization such as Lemieux et al. (2015).
- L49–58: This paragraph should be revised to clarify the organisation of the paper.

Data and methods

This section is well explained and clear.

- L88–93: The thin-ice concentration is described in some detail here, whereas it is little used in the rest of the paper.
- Table 1 and Figure 1: I suggest moving the abbreviations listed in Table 1 directly into the caption of Figure 1, to make the figure easier to read.
- Figure 1: To help the reader in the rest of the paper, it would also be useful to name the regions (each sub-map shown in Figure 1) and to refer to these names when discussing several polynyas together. The polynya definition areas should be more clearly labelled and delineated.
- Section 2.4: See General comment 2. In addition, the second detection method uses thin-ice growth to define ice production. Although this term is by far the dominant contribution in polynyas compared with growth under thicker ice, the observations use a single production value per grid cell identified as polynya. To follow an approach similar to Preußer et al. (2019), should the total production (i.e. open water + thin ice + thick ice) be considered instead?
- Section 2.5: Same question for the computation of IP, where it seems more appropriate to directly use the total sea-ice production within the polynya areas.

Results

This section should be clarified following the General comments.

- Section 3.1: This section is mostly qualitative and lacks more detailed comparisons with the observations. In particular, it would be valuable to add a map of the observations to Figure 3.
- Figure 4, Table 2 and Figure 7: Their organisation should be revisited. A table giving the mean polynya areas and ice production for each polynya and for the whole Arctic (possibly with trends) would provide a better overview.
- Figure 4: Why show polynya areas specifically, rather than their ice production? It would be very valuable to show both.
- Figure 7: I suggest adding the values for the whole Arctic, as well as the Preußer et al. (2019) data.
- Section 3.3: These case studies are interesting, but I found it difficult to understand their objective and what can be learned from them. This is a pity, as they add real value to the

study and deserve to be better highlighted. Both subsections also present rather qualitative validations and results.

- L293: Why the 0.3 m threshold?
- Section 3.3.2: This section presents a very interesting result but is somewhat frustrating for the reader. In particular, the statement "Overall, the model results… the cumulative winter ice production" (L328–329) deserves at least to be discussed, or ideally supported by an estimate of the fraction of polynya ice production occurring during short events.
- Figure 11: This figure could be simplified by merging panels a and b.

Discussion

- Section 4.1: As suggested above, the issue raised at L349–350 could be addressed by applying the Preußer et al. (2019) ice production estimation method to the model output.
- Section 4.2: The ice production values from previous studies could be gathered in a table, recalling the periods, resolutions, number of polynyas, etc.
- Sections 4.2 and 4.3 could be merged.
- Section 4.3: It would be very interesting to estimate from the model the ocean-to-surface sensible heat fluxes that may affect the polynyas listed at L384.

Conclusion

This section is well written but should be revised in light of the comments above. Please also make sure the conclusions are not overstated relative to what is shown in the results and discussed in the paper.

Figures

- For the figures showing curves or values for each named polynya (e.g. Figures 4, 5, 7 and 12), I suggest sorting the polynyas by region (to be defined according to the panels of Figure 1) and indicating the region names for each group of polynyas.

Technical corrections

- Figure 8: The colorbar is missing for the first row of maps (panel a).