REVIEW – HEUZÉ et al. 2026

„Review article: Arctic polynyas in a changing climate"

The manuscript by Celine Heuzé and co-authors constitutes a long "overdue" new review article on the evolution of Arctic polynyas over the last two decades (and beyond), thereby connecting to previous reviews / inventories from 2007 (Barber and Massom; "*Polynyas: Windows to the World*"), 2004 (Morales Maqueda; "*Polynya dynamics: A review of observations and modeling.*") and 1990 (Smith et al.; "*Polynyas and leads: An overview of physical processes and environment*").

In this review, the authors span the arc from setting the scene on recent climatic changes in the Arctic over to their influences on the development and behaviour of Arctic polynyas and how we can observe this using earth observation data. Finally, they discuss the effect of polynyas on different compartments of the Arctic climate system and how this might change in the foreseeable future.

The text is generally well written, understandable and hence nice to read, with only a few remarks from my side noted below under "Specific comments, incl. Figures and Tables". Otherwise, please find my more general and main points below under "General comments". The manuscript is already in a good shape, so after addressing these few suggestions and minor hiccups, and maybe some more from other reviewers, it is well ready to be published.

GENERAL COMMENTS

- Although the review focuses on Arctic polynyas, I would argue to pay a bit more attention to the differences and linkages between polynyas and leads, as related physical processes often overlap. Given the northward shift in ice edges in both summer and winter, polynyas and leads in close proximity of the ice edge are likely to become more prominent in future, as you also indicate in the manuscript (one could refer to this as a "MIZ-ification" of polynyas close to the wintertime ice edge, e.g. Franz Josef Land, Storfjorden and polynyas around Bering Strait). However, there were a few occasions in the manuscript where a note on related lead dynamics is missing so far – potentially something to keep in mind in the upcoming revision.
- On the topic of trends: there is currently no notion on the potential linkage between trends in polynya sea ice production and sea ice export from the Siberian shelf regions, feeding into the Transpolar Drift. Are there any, based on published literature?
- In the current version of the manuscript, I am personally missing an update of the (spatial) polynya inventory of Barber and Massom (2007), i.e. adjusting areal extents and indicating other changes (such as new/emerging polynyas around the CAA and Greenland). So similar to Fig. 2, but with polynya names, increased spatial detail, etc.
- In terms of the overall coherence in the manuscript regarding definitions, explanations, citations etc. – please check carefully that there are no doublings, contradicting statements/quotes or explanations of processes/wordings that were already mentioned earlier in the text. I noticed a few of these instances while reading through your manuscript, some of which are mentioned below under specific comments. I know this is particularly difficult for a review article due to many quotes and cross-references, but another careful read-through by the authors could definitely help to correct most of these instances.

SPECIFIC COMMENTS, including FIGURES and TABLES

P.2, L.19 & L. 49: A short differentiation to "leads" might be useful right in the introduction, either here or a bit later. This includes a note on the difficulty separating both at some locations, for instance in terms of SIP and MIZ dynamics.

P2, L. 28/29: How about the earlier polynya and leads review by Smith et al. (1990; DOI:10.1029/jc095ic06p09461)? Would likely be a reasonable assessment to compare your updated findings/summary to, or complement some of the statements in Ch. 2.

P.3, L.53: There is (kind of) - https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/joc.5729 (Conclusion: "*In contrast to the near-surface warming in the Arctic during the last decades, the 95% percentiles of daily maximum wind speed show no significant trends for most parts*.").

P.4, L.99: One could add Spreen et al. 2011 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2011GL048970); and/or Kwok et al. 2013 (https://doi.org/10.1002/jgrc.20191) here, on the topic of accelerating TPD speeds. Also – repeated on P.6, L. 125.

P.4, L. 117/118: Is there any published literature to back that statement on increased likelihood for ice breaking (e.g. from ice mechanics)? I agree that it is rather logic and obvious, considering field observations from the last 1-2 decades, but still – I think we lack proper evidence/metrics for that (also in terms of what defines "thin" in that context). Willmes et al. (2023)

(https://doi.org/10.5194/tc-17-3291-2023) show that there are at least no significant trends in wintertime lead dynamics observable between 2002 and 2021. Maybe something to dig a bit further into in future studies.

P.5, Figure 1: I think this figure could be improved a bit, most prominently by adjusting the green shadings for different ice extents (the difference is quite hard to see).

P.6, L. 150: Comparable statistics in terms of what? Frequencies of occurrence (i.e., hot spot locations) or Width distributions?

P.11, Figure 3: For Sentinel-1 – why not Sigma 0 instead of greyscale? The latter would be more reasonable for the VIS/NIR image from VIIRS.

P.12, Table 1:

a) Under "Thermal IR" → you likely meant Sentinel-3 (SLSTR), and not Sentinel-2 when referring to spatial resolutions of ~ 1km, correct?
b) You include CIMR and CRISTAL as upcoming relevant missions in the table. Fair enough and well worth mentioning, but then you should also address those somewhere in the text to explain and highlight their capabilities in terms of polynya monitoring.

P.14, L.304: Potentially add geographic reference to the Lincoln and Wandel Seas here.

P.14, L.308: Abbreviation "MOSAiC" not explained here (only later in Ch. 5.1)

P.15, L.349: It is clear that biological topics are beyond the scope of this review, but this study on polynyas and diel vertical migration (DVM) might be interesting for further reading - https://doi.org/10.5194/os-20-1677-2024.

P.15, L.351 (and generally Ch. 5): I am missing a clear description on the effect of thin ice on the ocean-atmosphere heat exchange. It is reduced when sea ice is present (low thermal conductivity), thereby acting as an insulator. This is already the case for very thin ice, as even an ice layer of only around 10 cm thickness reduces the heat exchange by more than 50% (compare Maykut 1978 and 1982; https://doi.org/10.1029/JC083iC07p03646 and https://doi.org/10.1029/JC087iC10p07971), and hence generally affects moisture-, momentum- and gas-exchange processes.

P.16, L.366: on ABL influence – the term "thermal internal boundary layer" (TIBL) is rather common in that instance (https://glossary.ametsoc.org/wiki/thermal-internal-boundary-layer/).

P.17, L.418: "…adjacent to the coast" → "and/or landfast ice" missing here (as mentioned in other parts of the manuscript)

P. 20, Figure 4 (and related text):

a) The map could be enhanced by a clearer separation between land and ocean areas.
b) "Total winter sea ice production in meter" → an accumulated number in meter is (to me) not very intuitive. Why do you avoid using km^3, as in earlier publications on sea ice production (incl. those of the authors)?

P.21, L.528: "…, but do not contribute significantly…" → contribute to what exactly (I assume the halocline)?

P.23, L.564: "reports" → report; plus: "…a plume at the bottom of Saint Anna Trough…" → can you indicate the origin of this plume? Is it linked to polynya dynamics around Franz-Josef-Land?

P.23, L.572: "…has been observed to lead to warm pulses at the bottom of the Laptev shelf…" - could you indicate which season this is referring to, in order to set the word "warm" into context?

P.24, L.621: "Both ice arches/bridges and landfast ice are disappearing in the Arctic" → at least in case of landfast ice - maybe not disappearing, but "noticeably decreasing in spatial extent and durability".

P.24, L.623/624: regarding the NOW polynya opening, gap-flow mechanics at eastern exit of Smith Sound play a or the major role here (compare Samelson et al. (2008) - https://doi.org/10.1175/2007MWR2326.1).

P.25, L.628/629: I do not fully agree with this assumption, at least not for polynyas adjacent to fast ice/the coast. The ice has also been rather thin there in the past, so most ridge height changes are more likely linked to divergent or convergent ice motions. The trend towards a general decrease in ridge-heights in the Arctic is likely to have a greater impact on the ice drift speeds (such as the TPD) and the subsequent changes in the likelihood of lead openings. The authors themselves also phrase this more cautiously in Chapter 1: " The net effect on polynyas, which primarily open in now-seasonal sea ice areas, has yet to be established."

P.26., L.670: Maybe add a note on potential future improvements through CMIP7 and/or the potential of new km-scale global climate models (ICON, IFS-FESOM, etc.) for polynya detection, despite ongoing discussions revolving around computational costs for physics-based models. In addition, a note on regional climate models (Arctic CORDEX) could also be a topic worth mentioning/discussing.

P.27, Figure 5: I am missing the polar mixed layer in this schematic – is that on purpose? Further, the development of the ABL (i.e., TIBL) could be added to this figure, highlighting the effect on the atmosphere a bit more clearly.