

Citation: <https://doi.org/10.5194/egusphere-2025-6183-RC1>

RC2: 'Comment on egusphere-2025-6183', Anonymous Referee #2, 20 Apr 2026

Improving the representation of coastal polynyas in high resolution climate models with satellite datasets - Torres-Alavez et al., 2026

This study applies two different satellite based SIC boundary conditions to a high resolution regional climate model (HARMONIE-Climate) to study how the resolution (and possible other differences in these observationally based datasets) affect the simulations of Arctic and Antarctica polynyas. These results are then compared with each other, CARRA and a few AWS observations. They find that using the high resolution SIC as a boundary condition improves the representation of certain atmospheric variables related to the formation of polynyas, such as latent heat flux and low-cloud formation. The improvements depend on the size of the polynyas, with smaller polynyas being more susceptible to the resolution of the boundary conditions.

General comments:

I find the study generally interesting and the topic important, but unfortunately its impact is hampered by issues with the general quality of writing and organization of the article in certain sections. I also have one larger question about the study set up that I would like some clarity into. This is why I recommend many major changes to the paper before it could be published.

Methods:

I found the general set up of the sensitivity experiments clear and reasonable, but didn't fully understand what was the baseline to these comparisons. In the intro the HCLIM model with the high resolution SIC was said to be compared against the HCLIM model with the 'original' lower resolution SIC. And this did appear to be the case, when the extent of the polynyas were discussed. However, other atmospheric variables were then compared with CARRA and in situ data. If CARRA was used as a baseline for Greenland, it should be marked and discussed also when polynya extent and LWD radiation are discussed (Figures 2, 3 left panel, 4 right panel).

If on the other hand, the lower resolution SIC simulation was meant to be the baseline, you can't draw "absolute" conclusions from it (such as the high resolution SIC data improves polynya representation), as you can only compare the two models to each other. I assume part of the issue is that Antarctica doesn't currently have a gridded regional reanalysis, so finding a good baseline is difficult, and why the AWS comparisons are included.

My preliminary suggestion on how to handle this would be to, first compare the CARRA with the two Arctic AWS sites, both closest grid point and the maybe with an areal average over the polynya area. Then the authors could compare the CARRA polynya average with the HCLIM simulations polynya averages (as well as make spatial maps). Lastly the AWS, CARRA grid point, CARRA polynya average and HCLIM simulation polynya averages could be compared in a same plot to help understand absolute biases. This analysis could then be repeated for the Antarctica, just without CARRA data, but this way you would have an idea how the area average over the polynya differs from the grid point value. This would then help to draw conclusions for Antarctica. Of course this is just a suggestion that I came up pretty hastily, so there might be many better ways to address this.

We thank the reviewer for this helpful suggestion. We have added the CARRA time series to the corresponding figures, as suggested. However, we did not include CARRA in the station-based evaluation because the automatic weather stations used in this study are assimilated into the CARRA reanalysis. Including CARRA in this comparison would therefore be largely redundant, as the reanalysis is expected to closely match the assimilated observations and would not provide an independent evaluation of the model performance.

On a related note, how were the polynya line plot values calculated? Was it area-average over the exposed ocean? This should be written in the methods.

We have clarified this in the Methods section. The polynya values shown in the line plots represent the total sea-ice area within each predefined polynya region. They are calculated by summing the product of sea-ice concentration and grid-cell area over all grid cells contained within each polygon.

Writing:

Unfortunately the article was quite difficult to read. There were many typos and missing words that made it in cases difficult to understand what the writers were meaning. Also, some sentences were quite long and lacking punctuation. I would suggest the authors go through the text over again with the line by line comments I've provided, and especially pay attention to any repeating mistakes.

I also think some re-organization would make the paper easier to follow. The introduction and background sections included a lot of repeating information and I think these 2 sections could easily be combined and made more concise. This way there would also be more space to emphasize what is specifically the scientific question/gap that this paper is filling. This was kind of reference throughout the two sections, but was never clearly articulated.

Lastly the results section would benefit from being combined with the discussion section, and the have a separate summary section. Now it seems the reader is just reading through a list of figure descriptions before getting to what these results actually mean. The authors might also consider if all the figures need to be in the main text for both poles, as there are very many figures to go through, and including some in the appendices instead might make the flow of the paper better.

We sincerely thank the reviewer for the careful and constructive review of our manuscript. We have revised the manuscript extensively in response to your comments, as well as those of the other reviewer. While we ultimately decided to retain the overall structure of the Discussion section, we have substantially expanded and strengthened it based on your suggestions. Your comments helped us improve the clarity of the manuscript, provide additional context and references, better discuss the limitations of our approach, and emphasize the broader implications of our results. We believe that these revisions have significantly improved the quality of the manuscript.

Line by line comments:

lines 22-23: this should be expanded, what are the commonalities

We have corrected this in the revised manuscript with additional text explaining commonalities

lines 23-26: sentence is too long and a bit unclear, I suggest splitting it after '... cryosphere' with a period and then rephrasing the rest of the sentence for clarity.

We have corrected this in the revised manuscript.

Line 26: add references

Done

Lines 27-30: This sentence is again very long, but also needs to be made more clear how the poor representation of sea ice results in these other issues mentioned. With references, or are these all from this single paper?

We have added this information to the revised manuscript.

Line 31: change 'driving' to 'constrained'

We have retained the term "constrained" because it more accurately describes the relationship between regional climate models and their driving GCMs through the lateral boundary conditions.

Line 31: it would also be good above or here to expand what the 'similar problems in characterizing sea ice cover' means

We have added this information to the revised manuscript.

Line 33: were you earlier talking about coupled models also?

We have also revised the sentence to clarify this statement.

Line 34: explain what is model drift

We have also revised the sentence to clarify this statement.

Line 36: add 'with the polynyas' after '...atmospheric feedbacks'

Done

line 47: needs examples of issues with sea ice modeling

We have added a larger overview of current state of the art to the manuscript

lines 44-47: repetition of introduction

Thank you for spotting this, we have removed this text from the revised manuscript.

line 47 start a new paragraph

Done

line 49: Is there a specific reason open water polynyas are mentioned? It seems a bit unrelated to the rest of the discussion here

We have refocused the manuscript more closely onto coastal polynyas

line 50: add 'area exposed to the cold air, ' after '...open water'

Done

lines 52-54: check punctuation on this sentence

Done

line 62: correct to '10th of February', '12th of March'

Done

lines 62-63: I would switch the order of these sentences: 'This notable event occurred, when this region is typically covered in thick multi-year ice, peaking between 10th of February and 12th of March.'

Done

Line 70: add 'in the area' after 'hotspot'

Done

Line 72 What is meant by 'sea ice arch', please expand

Done

line 74: Define 'DMI', remove 'and'

Done

line 77: remove 's' from 'forms'

Done

line 77: change 'It is' to 'they are'

Done

line 79: change 'it is driven' to 'they are driven'

Done

line 80: change the beginning to: 'importance of the opening of these polynyas...'

Done

line 82: spell out km

Done

line 83: add comma after 'services'

Done

line 85: add comma after 'change'

Done

line 85 change 'there' to 'their'

Done

Line 86: add comma

Done

line 87: add comma

Done

line 88: add comma

Done

line 94: what is meant with 'bosses'?

We thank the reviewer for catching this. This was a typographical error; we meant biases. The manuscript has been corrected accordingly.

Line 95: are they really only in 'development' or are some already in use?

We thank the reviewer for this comment. We have added this information to the revised manuscript.

Lines 95-98: This sentence is too long and hard to follow, please rephrase and check for any missing commas.

Done

Line 98 remove 's' from 'regional climate models'

Done

Lines 100 – 103: I don't understand this sentence. If the authors mean that global climate models do use assimilation as part of their systems I would rephrase this as: 'Global models and particularly those used for seasonal and decadal predictions do include data-assimilation systems. These models are used to study drivers of sea ice variability regionally and globally, among many other topics.'

The manuscript has been revised to clarify this point.

Line 103: start a new paragraph

Done

line 103: add comma

Done

line 110: missing closing)

Done

line 110: reference for AROME model missing

We thank the reviewer for pointing this out, it is now in the revised manuscript

line 122 – 123: SICE should be explained in more detail, as the focus of the paper is on polynyas
We thank the reviewer for this suggestion. We have expanded the description of the SICE module by briefly explaining its thermodynamic formulation, its coupling with the snow scheme, and its relevance for representing the surface heat exchanges associated with coastal polynyas.

line 125 – 126: how were these e-folding times determined?

Now added in to the manuscript

Lines 131 – 133: This description is unclear. I would change it to something like 'We conducted a series of sensitivity experiments to assess what impact the newly developed observational based SIC data (CCI) has on the model representation of coastal polynyas.' Or something alike.

We have rephrased this in the new manuscript.

Line 141: missing reference

We thank the reviewer for this comment. The manuscript has been revised accordingly.

Line 144: what corrections are the authors referencing to?

We have clarified that the correction refers to the instrument height above the snow surface when comparing the near-surface atmospheric variables with the station observations. As the expected impact is negligible, no correction was applied.

Line 177: reference needed to this claim, for example has there been studies comparing CARRA with CFSR, which is a coupled reanalysis and therefore does very well on many surface variables?

We have included this information in the revised manuscript.

3.4. Evaluation with AWS: if the weather stations are far, does this bring any additional information to the study?

Although the AWS stations are not located within the polynyas themselves, they are situated in coastal regions directly influenced by the adjacent polynyas (Cape Morris Jesup, Alert, and Eneide). They therefore provide independent observations of the near-surface atmospheric conditions affected by changes in sea-ice concentration, allowing us to evaluate whether the improved representation of the polynyas translates into improved simulations of the local atmosphere.

192- 193: difference compared to what?

We have revised the sentence to clarify that the difference refers to the location of the marginal ice zone between the two satellite products.

208 – 209: how do the authors determine the results were solely due to resolution differences?

We agree that the differences cannot be attributed solely to the spatial resolution. We have therefore revised the sentence to state that the observed differences are consistent with the combined effects of the different spatial resolution and retrieval methodology of the two satellite products.

218-219: why is this? Expand on this please.

We thank the reviewer for this comment. We have expanded the discussion to note that the largest latent heat flux differences coincided with the largest differences in sea-ice area between the simulations (approximately 600 km²). We assume that favorable atmospheric conditions during this period may have enhanced the response, although additional analysis would be required to quantify the relative contribution of these processes.

235: There is some sun during this time already, though I think

Yes, sunlight returns during the latter part of the analysis period and we have revised the text to state that shortwave radiation remains limited during the transition from polar night to early spring, rather than describing the region as completely sunless.

lines 236 -244: Needs more specific values, especially, when comparing with CARRA instead of relatively vague phrases, such as ‘smaller differences’.

We have included more details in the revised manuscript.

Line 251: overestimated by how much?

We have included this information in the revised manuscript.

Lines 251 – 254: How much closer the ESA CCI SIC was to CARRA compared to OSI SAF?

Yes, we have included this information in the revised manuscript.

Lines 255 – 258: This is good discussion, but I think it could’ve been included already earlier within the results chapter.

This is a nice idea but we have chosen to retain this discussion at its current location because it synthesizes the preceding results. Presenting it after first describing the improvements in sea-ice concentration and the corresponding atmospheric response provides a clearer interpretation of the complete sequence of processes and improves the overall flow of the Results section.

Line 260: should be ‘...temperature of more than...’

We thank the reviewer for this comment. The manuscript has been revised accordingly.

line 267: what is the basis for this hypothesis, please expand

We have expanded the discussion to clarify the physical basis for this hypothesis. Specifically, we note that numerical models often struggle to represent very stable boundary layers, leading to excessive turbulent mixing and a warm bias in near-surface temperatures under strongly stable conditions. We also emphasize that this remains a plausible explanation and that additional analysis would be required to confirm it for the present simulations.

286 – 287: I don't understand these sentences. Maybe missing words?

We have rewritten this sentence to be clearer

Line 288: What does 'five days for the whole domain' mean?

This is a stray piece of text from an earlier draft, we deleted this sentence

Line 290: missing 'concentration'

Done

Line 330 – 332: Switching to calling the simulations high resolution and low resolution this late in the paper is a bit abrupt. I would suggest changing to this naming convention from the beginning and clearly explaining it at the beginning, when the authors present the datasets.

We have consistently relabeled simulations all the way through the manuscript to be clearer.

Line 346: correct 'examine' to 'examined'

Done

Line 354: remove 'that result'

Done

Lines 392-394: How is plausibility of using similar set up to CARRA on Antarctica result of these results?

We have clarified in the revised manuscript that our results do not directly demonstrate the feasibility of implementing a CARRA-like reanalysis for Antarctica. but emphasize the potential benefits of assimilating high-resolution Earth observation datasets in a future Antarctic regional reanalysis. We also note that such a reanalysis would provide a valuable tool for investigating Antarctic weather events and physical processes in a region where in situ observations remain sparse.

Citation: <https://doi.org/10.5194/egusphere-2025-6183-RC2>