Review of

Improving the representation of coastal polynyas in high resolution climate models with satellite datasets

by Torres-Alavez, et al.

Summary: This manuscript provides an example how data sets of the sea-ice concentration derived from satellite observations in both hemispheres could be used to improve the representation of coastal polynyas in high spatial resolution (2.5 km to 4 km) climate models. The preferred method to ingest the information provided by the sea-ice concentration data sets is nudging. Quantification of the improvement in the representation of the polynyas is realized by investigating the potential impact two sea-ice concentration products with different grid resolutions have on a selected set of meteorological parameters that are modeled and output by the high-resolution climate model. The manuscript seems to show some evidence that a finer grid resolution of such products results in more realistic latent heat fluxes at the surface, in elevated low-level cloud formation and subsequent elevated downward longwave radiation. One of the main conclusions seems to be that this improvement is more pronounced for particularly small-scale polynyas.

We thank the reviewer for his very thorough and constructive feedback on the manuscript. The comments have certainly helped us to think about the study and reframe it to align better with our overall goals. We go though below the comments more specifically

General Comments:

GC0: Overall the manuscript is difficult to read. Usage of terms and definitions is not well thought-through and formulations are often not specific enough. Examples of this can be found throughout the manuscript. Examples include things like "sea-ice value", the usage of sea ice extent versus sea ice area, the naming of polynyas and regions. You may note that the review contains a large number of editoral comments / typos.

Thanks for this comment, we have thoroughly revised the manuscript to clarify the terms we have used. See specific comments below for specific changes.

GC1: The manuscript would benefit from a more thorough description of datasets and methodologies used. This comprises more information about 1) the meteorological / AWS stations used, 2) the motivation for the time periods chosen, 3) the motivation for the initialization data sets chosen, 4) the motivation to use CARRA as an evaluation source for the Arctic while not having an evaluation source for the Antarctic, 5) the motivation of the meteorological parameters chosen to investigate both the sources and the impact of polynyas in the RCM used, 6) the choice and the size of the regions used to compute sea-ice area and why sea-ice area is preferred over other definitions of the polynya size, and possibly a few more aspects.

We have significantly expanded the methodology to clarify all these points on datasets and restructured the introduction to be clearer on the motivations behind the study as a whole as well as on these specific aspects of the methodology.

GC2: The manuscript contains errors.

GC3: The critical discussion of the obtained results is rather light. In some aspects, the manuscript does not go beyond an investigation of sea-ice concentration variations and their (potential) impact on selected meteorological quantities. The linkage to polynyas is not overly well made in this regard

- possibly because of the way you tried to "define" where polynyas were located in your two case studies. The difference in the temporal sampling (daily vs. hourly) and its potential impact on the results is not discussed sufficiently well. It is not sufficiently clear whether and to which degree the sea ice concentration data products chosen are suitable to resolve small-scale polynyas such as those in the Nares Strait or the Terra Nova Bay polynya and to which degree the amount of thin ice within the regions of interest did bias the sea-ice concentration estimates used for the nudging; this applies in particular to the polynya North of Greenland and the Ross Ice Shelf polynya.

We have expanded the discussion and included new points specifically dealing with these criticisms.

GC4: I recommend to carefully re-think the main conclusions of the manuscript and ensure that these are sufficiently well backed up by the results presented and discussed in the manuscript.

The manuscript as a whole has been restructured and updated to include a more clearly stated research question and discussion around the results that allows clearer conclusions to be drawn.

Specific Comments:

L19-22: "Coastal ... 2025)" --> I find this is a VERY condensed description of the role polynyas play. I am also not sure that everything you write and combined in one sentence here is correct. I am questioning, for instance, that ocean dynamics play a key role. Since you are dealing with both, polynyas near Greenland and in the Antarctic, the "Antarctic bottom water formation" does not apply in general. I would also put the role of polynyas for biology / biogechemistry in a different sentence.
We have expanded this section, clarified the processes involved, separated the discussion of physical and biogeochemical impacts, and added additional references to support these statements.

L33: "and ocean-atmosphere ... drive them." --> I am content with the high surface wind speeds but I would say that elevated ocean-atmosphere heat and moisture fluxes are rather a result of polynyas than their cause. An exception would be if you are referring to sensible heat polynyas where the ocean heat flux is contributing to the opening of the polynya. Latent heat polynyas are kept open due to the interaction of the wind pushing the sea ice away from a barrier (coast, ice shelf, sea ice arc) and advecting the newly formed ice downstream, and the latent heat of fusion that provides heat to the water, contributing to keeping the polynya open. I suggest to change the writing accordingly.
We agree that the original wording is ambiguous We have therefore revised this section to distinguish more clearly between latent heat and sensible heat polynyas and to clarify the mechanisms responsible for maintaining them.

L34/35: "They also have a tendency to drift ..." --> I am not sure what you want to say here with the notion of the drift of the model. I would say that a drifting model is not suitable to use it to investigate processes on climate relevant time scales - but whether it "performs well" against in-situ observations is perhaps not that relevant here?
We agree that the term *drift* was ambiguous in this context. We have therefore revised the text to explicitly describe the issue as the development of increasing biases in the simulated sea-ice state and positional offsets in sea-ice features over time.

L35/36: "An intermediate ... features." --> This sentence needs to be backed up with more detail about the status of the numerical models' landscape and the way how polynyas are treated. A reader might want to know whether (and how successfully) polynyas are parameterized, what the spatial resolution of such models is, what the typical spatial resolution of available data sets to be used for this solution are, and so forth. Please change the writing accordingly.
We have substantially expanded this section to provide additional context on the current state of regional climate modelling of coastal polynyas. In particular, we now describe the typical spatial resolution of regional climate models and commonly used sea-ice concentration products, explain how the mismatch between model resolution and sea-ice boundary conditions limits the representation of narrow coastal polynyas, and discuss the role of sub-grid parameterizations. We also clarify the motivation for using the convection-permitting HCLIM configuration together with the higher-resolution ESA CCI sea-ice concentration product, which is expected to better resolve small coastal polynyas than the coarser resolution OSI SAF product.

L47/48: "Coastal polynya ... occurrence." --> This statement needs to be backed up by references.
We have added the relevant references to support the statement.

L59: The polygons in Fig. 1: Are they (at all) related to the sea ice area that is computed later on? Isn't the polygon chosen for the North Greenland polynya - and also the two polynyas in the Ross Sea - far too small?

Figure 1: What is "NG"?
What are the red dots and the names next to them representing?

In the caption you write RS and TN for Ross Sea and Terra Nova ... but the polynya names that are typically used (and should be used in this paper as well) are "Ross Ice Shelf polynya" and "Terra Nova Bay polynya".
. We have revised Figure 1 and its caption to improve clarity. The abbreviations have been corrected. We have also clarified that the polygons define the regions used to calculate the sea-ice area and are based on previous studies. In addition, the red dots have been changed to black and the figure caption now explicitly states that they represent the automatic weather stations used in the evaluation.

L65-67: "generating ... early March" --> This is not correct as written. I invite the authors to take a closer look especially at the paper they cite: Ludwig et al (2019). In that paper several figures clearly demonstrate that i) air temperatures increased to near 0degC AFTER Feb. 10 and that there was considerable new ice formation, creating quite some spatiotemporal dynamics in both the sea ice concentration as well as in the polynya area.
We apologise for this clumsy formulation.. We have revised the text to clarify that temperatures increased to near 0°C after 10 February and that substantial new ice formation produced strong spatiotemporal variability in both sea-ice concentration and polynya area. This is now consistent also with Ludwig et al., 20219

L81: Both references focus on the physical properties and mechanisms. Please add an additional reference for these "Several studies ..." in relation to the seal population.
We have added additional relevant references to support the statement.

What I have been missing so far is the typical size of these 4 polynyas - both in terms of the open water area where the largest turbulent heat fluxes are observed and in terms of the thin ice area downstream of the principal wind direction. Learning about the typical size is required to

understand your motivation to use RCMs at the specific grid resolution you are detailing in the next section.
We have expanded the subsections in the Background (Section 2) to include further details on the size of the polynyas as well as adding details on the model resolution.

L133/134: I can understand that the main motivation for the time period chosen to investigate polynyas near Greenland comes from the large polynya event north of Greenland. But what was the motivation to investigate the polynyas in the Ross Sea for the time period chosen, i.e. Oct. 10 through Nov. 9? What is the scientific rationale behind that choice?
The Greenland event was selected because it represents the exceptional large polynya north of Greenland that has been widely investigated in previous studies. Similarly, the Antarctic period was chosen because it corresponds to a well-documented polynya event in the Ross Sea and Terra Nova Bay, allowing us to evaluate the impact of different sea-ice concentration products during a period of pronounced polynya activity. In addition, we wanted to see if there are contrasts in the model performance between Arctic and Antarctic regions. We have clarified the scientific rationale for the selected time periods in the revised manuscript.

L144: Later on you show that there is a bias in the pressure between the observations and the HCLIM runs. Would it make sense to estimate the altitude-induced bias in the surface pressure to better understand the bias that is shown later?
This is a helpful suggestion that altitude differences can influence surface pressure, however, we believe that the pressure bias shown in our simulations is more likely related to the model's warm bias under stable boundary-layer conditions than to differences in station elevation. This point is explored further in the recently published paper Gilbert, Torres Alavez et al., 2026. We have clarified this point in the revised manuscript.

L144: There is no Table 1.
We have deleted the text, thank you for spotting this artefact.

L146/section 3.2: Your model experiments are using a grid resolution finer than 5 km. Why did you not look for data sets other than those you are using here? The data set which has been used by Ludwig et al. (2019) for instance. Or the ARTIST Algorithm SIC based on the AMSR2 near-90 GHz channels providing a grid resolution of 3.125 km. Or high-resolution SAR - AMSR2 based SIC products? I strongly recommend to come up with a discussion that justifies the usage of the two products used. The length of the time series could be one argument - but since you are investigating specific cases and/or events this argument seems not to be holding.

In addition to SIC data sets there exist other data sets of polynya area and/or frequency of occurrence and/or ice production that were derived using different methodologies and data, e.g. SAR imagery, MODIS imagery, or resolution-enhanced passive microwave imagery (Polynya Signature Simulation Method - PSSM and others). Some of these methods and data sets do not only offer a finer spatial resolution but also a finer temporal resolution - i.e. sub-daily. Particularly in a highly dynamic area such as a coastal polynya and in combination with the high-resolution RCM modelling it seems a bit odd that you do not take into account - at least to list and discuss - the value and/or degree of being fit-for-purpose of these other data sets for your specific study aim.
This comment regarding the availability of higher-resolution sea ice datasets, such as those derived from SAR, MODIS, or high-frequency AMSR2 channels is a really good one. Products like the ARTIST Sea Ice (ASI) algorithm or SAR-based datasets offer superior resolution for capturing the fine-scale structures of coastal polynyas. However, the aim of this study, which is a core component of the ESA Sea Ice CCI project, is to evaluate the feasibility and impact of using standardized climate data records such as the ESA CCI satellite products with regional climate models (RCMs).

The 12.5km dataset used here was specifically developed and interpolated by project partners to match the HCLIM model's requirements within this research framework.

Our aim was not to identify the highest possible resolution dataset available, but rather to establish a baseline for how current ESA CCI products perform in a convection-permitting atmospheric setup. If these results prove promising, future work should certainly incorporate the sub-daily or ultra-high-resolution datasets (e.g., MODIS or PSSM-derived products) mentioned by the reviewer. We have added a paragraph to Section 3.2 to clarify our choice and discuss the value of these alternative datasets as additional possibilities
“Although higher-resolution alternatives (e.g., SAR, MODIS, or ASI-3k) exist and are well suited for polynya characterization, this research establishes a baseline for evaluating the performance of standardized ESA CCI products in convection-permitting frameworks.”

L162/163: "SICCI-HR-SIC is ..." --> It is not really clear from this sentence where the improvement is with respect to the 25 km grid data set. Please spend one more sentence to describe this with sufficient detail.
We have expanded the description of SICCI-HR-SIC to clarify that its improvement over OSI-450-a is not limited to the finer 12.5 km grid spacing. SICCI-HR-SIC exploits the high-frequency (near-90 GHz), higher-spatial-resolution imagery from the SSM/I and SSMIS satellite missions, which provides a more detailed representation of spatial sea-ice variability, particularly in coastal regions and polynyas.

L164: "resampled" --> So you do not apply any averaging or spatial interpolation. All 2.5 km grid cells that fall into a 25 km grid cell did receive the same SIC value?!?
We confirm that remapped sea-ice concentration fields contain different values in neighboring HCLIM grid cells, verifying that an interpolation was applied rather than assigning the same SIC value to all HCLIM grid cells within a single 25 km satellite grid cell. We have clarified this in the revised manuscript and removed the term *resampled*, which in this context is indeed misleading

L173-181: It is not sufficiently clear why CARRA can be used as a means of evaluation of your experiments if CARRA and your method are based on the same SIC input data source. How is the SIC treated differently in CARRA as compared to your study and why is this difference in the treatments sufficient to use CARRA data as evaluation data source?
We thank the reviewer for this comment. We have revised Section 3.3 to clarify why CARRA serves as a valid evaluation tool despite sharing an SIC input source. The critical difference is that CARRA is a regional reanalysis that uses a 3D-Var data assimilation scheme to ingest local atmospheric observations (e.g., surface stations and radiosondes), which "nudges" the atmospheric state toward reality.

In contrast, our HCLIM experiments are standalone simulations where the atmosphere reacts purely through physical parameterizations to the lower boundary. By comparing HCLIM (12.5km SIC) against CARRA (25km SIC + Assimilation), we are testing if higher-resolution boundary forcing alone can "recover" the realistic atmospheric structures over polynyas that a reanalysis typically needs data assimilation to capture. This assesses if high-resolution SIC can compensate for the absence of data assimilation in standalone climate runs.

How about the Antarctic? It is not sufficiently clear how you are going to evaluate your results for the Terra Nova Bay polynya or the Ross Ice Shelf polynya.
While we utilize the CARRA regional reanalysis for the Arctic domain, it is indeed unfortunate that a high-resolution regional reanalysis equivalent to CARRA does not currently exist for Antarctica.

Therefore, to ensure a robust and consistent evaluation across both hemispheres, we also evaluate our simulations using direct in-situ observations from meteorological stations located in both the Arctic and Antarctic domains. For the southern hemisphere, we specifically rely on data from a weather station located in close proximity to the Terra Nova Bay polynya. This allows us to validate the model's ability to capture the localized, intense katabatic wind events and surface atmospheric conditions that directly drive the opening and maintenance of the polynya. We argue in the manuscript that a regional Antarctic reanalysis similar to CARRA would be an asset to the polar science community and our study can be used for evidence of this assertion.

L182-185 / section 3.4: This section cannot be complete. Please add the information that is required to understand what your aim is in this regard. There are several AWS stations located near your two focus areas in the Antarctic and there are certainly also more stations than the one you mentioned in the context of the North Open Water polynya and the other polynyas in Greenland. Their names and locations, periods of measurements, parameters used and several more details should be provided.
Thank you for spotting this omission, We have now expanded the section and included a new Table 1 to provide more details.

L204: "hourly time series" --> Both data sets have daily temporal resolution (or sampling). How can you come up with an hourly resolution?
We apologize for the lack of clarity. While the underlying satellite sea ice datasets are daily, the HCLIM model interpolates these inputs using a dedicated sea ice module to generate its internal simulated outputs at an hourly temporal resolution (Batrak et al., 2018).

"annual minimum" --> The time series does not extend over an entire annual cycle. How can you then speak of an "annual minimum"?
We have updated the text, thank you.

L228/229: "Compared ... certain periods" --> When I look at Figure 4, left hand side, I would say that CARRA shows higher LLC fractions for 90% - 95% of the time. Only for the polynya north of Greenland there are a few short periods when HCLIM exhibits higher LLC fractions. And these occur at times when Fig. 3 does not really indicate a particularly low sea ice area or a particularly high latent heat flux. This does not go together very well. The comparison of the LLC fractions between CARRA and HCLIM does not seem to be overly conclusive.
We have updated the text, to better convey this ambiguity “Compared with CARRA, the HCLIM model captures the temporal variability of cloud cover over the Greenland polynyas; however, HCLIM tends to underestimate cloud cover for most of the period.”

L236/237: I agree that enhanced formation of low-level clouds can lead to enhanced downward longwave radiation which in turn can contribute to elevated near surface air temperatures. I am wondering, however, how large the contribution of the sensible heat flux due to the increased open water area and the increase in ocean-sea ice-atmosphere heat flux due to more thinner / snow-free sea ice is. Wouldn't this result in a larger and also more direct contribution to the near surface air temperature?
Close inspection of our simulations suggests that the contribution mechanisms vary significantly by region. However, regarding the overall magnitude, both HCLIM simulations actually produce larger sensible heat flux values than CARRA across all analyzed regions, particularly after March 1st. This suggests that the direct ocean-atmosphere heat flux from open water and thinner ice indeed plays a major, highly direct role in driving the elevated near-surface air temperatures late in the period, acting alongside the cloud-radiative feedbacks. We have included more discussion in the manuscript to discuss these flux differences explicitly.

L238/239: "After ... polynyas." --> Will you get back to this in your discussion section? No ...
We have updated the text directly following this statement to explicitly explain the potential causes of this 7K difference.
We clarify that this late-stage difference is probably the cumulative result of continuous ocean-atmosphere heat fluxes over open-water fractions captured only by the high-resolution boundary conditions, which could be amplified over time by cloud-radiation feedbacks in a standalone model configuration without data assimilation.

L246-254: I am a bit sceptical about this paragraph and I am not sure it is really needed. The primary influence of more open water / more thinner sea ice is enhanced moisture and heat input into the atmosphere and consequently there could be more low level cloud formation - as you tried to demonstrate already. But whether and from which point onwards (downstream of the polynya) these clouds would be thick enough to produce precipitation is certainly much more difficult to capture and I doubt that the case study presented here is mature enough to dwell into this. Only the polynya north of Greenland seems to have been large enough to promote development of enhanced precipitation.
We understand the reviewer's skepticism regarding the direct link between small polynyas and local precipitation, as microphysical processes and downstream advection make this relationship highly complex.
However, our goal with this paragraph is not to claim that local moisture evaporation instantly triggers localized convective rainfall. Instead, we aim to demonstrate the sensitivity of the model's precipitation physics to the underlying sea-ice boundary conditions during large-scale synoptic events. The high-resolution boundary conditions modify the regional boundary layer stability and moisture convergence, which alters how the model resolves the intensity of these passing systems. We have clarified this distinction in the text to ensure the scope remains focused on model boundary condition sensitivity rather than local polynya-induced convection.

L255: Please specify a bit more clearly what you mean by "stronger impact". Also: Is this "stronger impact" leading really to an improvement in the representation of polynas or polynya-like features?
By "stronger impact", we mean that the higher-resolution (12.5 km) sea ice input leads to an improvement in the simulated atmospheric and surface variables compared to the lower resolution simulation (25 km) run. To clarify if this impact represents a genuine improvement, we have updated the text to specify that the higher-resolution configuration captures open-water fractions in narrow channels that are otherwise omitted in the coarser run, bringing the model fields closer to the evaluation baselines.

Figure 6: Clearly, after the polynya event the two HCLIM runs begin to diverge in temperature and also in pressure - not so much in the relative humidity. Please discuss potential causes for that.
This is a very neat observation. The divergence in temperature and pressure between the two HCLIM runs after the polynya event is driven by a physical feedback loop. During the event, the high-resolution run releases different amounts of localized sensible and latent heat fluxes due to the better-resolved ice-free fractions.
Once the event ends, these localized thermal differences accumulate, altering the lower-tropospheric stability and planetary boundary layer height. Because HCLIM is a standalone simulation without data assimilation to pull the runs back together, these temperature changes modify the local density of the air column, directly leading to a divergence in surface pressure. Relative humidity diverges less because it is a relative metric dependent on both temperature and moisture changes, which partially compensate for each other in these cold, stable environments. We have added this discussion to the manuscript.

For the bias in the pressure see also my comment to Line 144.
Done

Neither in Fig. 6 nor in Fig. 7 the text labeling the x-axis is located at the respective tick mark; the text has to be moved to the left.
We fixed the Figures, thank you for catching this.

L282-: Looking back at the comparison between HCLIM runs forced with two different SIC products and the in-situ observations I am missing an inter-comparison of the near surface wind speed. This parameter is essential for the formation and maintenance of a polynya (besides the temperature). I can understand that from the viewpoint of a numerical modeller it is important to illustrate that surface pressure and humidity are correctly represented - and I am not advocating for not showing these parameters but I am asking in addition for a comparison of the near surface wind vector.
We completely agree that near-surface wind is essential for polynya dynamics. We initially intended to include this comparison, but unfortunately, wind speed data for these specific weather stations were unavailable during our study period.

L304/305/306: "comparing sea ice areas in the HCLIM simulations" --> Where can I see these sea ice areas visualized?
Done

You write that "ESA CCI consistently represented smaller polynya extents than OSI SAF" --> it is not clear whether this is a statement based on model results. Also, it is not clear how you define "polynya extent" in this context.
Thank you for picking up on this. The term 'polynya extent' in our study refers to the total sea ice area computed as the sum of (sea ice concentration × grid cell area) within each predefined polynya region (delineated in Figure 1). This is an observational metric derived directly from satellite data (ESA CCI and OSI SAF products), not from model results. The statement that 'ESA CCI consistently represented smaller polynya extents than OSI SAF' is based on quantitative comparisons shown in Figures 3 and 10, where ESA CCI systematically yields lower total ice area values across most of the seven polynya regions during the study period. We have revised the manuscript to clarify both the definition (Line 190) and the observational basis of this finding.

L306/307: The numbers for the differences are far too high. Please also check my comment to Fig. 9.
Done

L310: "2nd-9th" --> The episode of high latent heat flux ended on Nov. 7, not on Nov. 9. Please correct.
Done

For the second polynya event, which resulted - seemingly - in an even larger polynya area (the sea ice area is even lower than during the first episode) the HCLIM simulations do not show elevated latent heat fluxes. Why?
The sea-ice area appears lower during the second event, but the actual difference in polynya extension between the two episodes is only about 100 km^2. This minor spatial difference was likely too small to significantly alter the regional heat exchange on its own.
Furthermore, latent heat fluxes are driven heavily by atmospheric demand (wind speed and vapour pressure gradients) rather than open-water area alone. During the second event, the localized atmospheric forcing conditions were less favourable for intense evaporatio, which suggests why the

latent heat fluxes did not show a corresponding elevation despite the marginally larger open-water area.

L311-314: "Differences ... system." --> This statement is not backed up by your Figure 9. While there is indication that the ESA CCI forced HCLIM simulations provide higher fluxes for the Ross Ice Shelf polynya, differences do not reach to 20 W/m². And for the Terra Nova Bay polynya example I don't see any improvement between ESA CCI and OSI SAF forcing. On the contrary, the differences seem to be rather arbitrary. Also: the decrease of 40 W/m² you are reporting about in the previous sentence ... where do I find that and why (and how) is this related to any change in sea ice area in the region of the Terra Nova Bay polynya? I have the impression that the HCLIM simulations do not follow the forcings of either of the two sea ice concentration products particularly well - perhaps because the actual fluctuations in the polynya size or area happen at spatio-temporal scales which are not resolved by any of the two products.

We thank the reviewer for highlighting this. We agree that the original paragraph was imprecise in terms of both its wording and its interpretation. To address this, we have revised the text in the main manuscript to provide a clearer and more accurate description.

L315-318: I don't find that the results for the low-level cloud cover align well with either the observed sea-ice area or the temporal development of the latent heat fluxes.
We agree and have removed this statement from the revised manuscript.

Figure 9: Please check the exponent of the sea ice area. It cannot be correct that the sea ice area in the Terra Nova Bay polynya region is as large as 1.3 x 10^7 km²; the same applies to the Ross Ice Shelf polynya. The numbers are far too large.
Done. Thank you for spotting this error.

Please also check my comment to Fig. 1 where you show polygons that you may or may not have used to compute these sea ice area values. It is not clear from your writing.

The panels on the right hand side are supposed to also show the results of the CARRA reanalysis. But there are none for the Antarctic.
We fixed the captions to clarify both points.

The ESA CCI sea ice area in the Terra Nova Bay polynya region is constantly smaller than the OSI SAF sea ice area but this is not visible in the latent heat flux time series. A smaller sea ice area should lead to a higher latent heat flux - which is only sporadically the case.
See our response to the comment on L310

L323-324: "over the Ross Sea ... on 31 October" --> While this statement seems to be true, the timing of this largest difference between the two forcings seems not to be related at all to any polynya activity because it falls into an episode of rather high sea ice area in the Ross Ice Shelf polynya region.

We agree with the reviewer that the maximum temperature difference over the Ross Sea on 31 October does not coincide with a period of enhanced polynya activity. We have revised the manuscript to clarify that this temperature difference likely reflects the broader atmospheric

response to the different sea-ice distributions prescribed by the ESA CCI and OSI SAF satellite products, rather than a direct effect of Ross Sea polynya variability.

L325-327: Neither your figures nor your text backs up this statement sufficiently well. Unlike in the Arctic there is little one can learn from the results.
We thank the reviewer for this comment. We agree and have rephrased this statement in the revised manuscript to point out that our results suggest that the model can be run for both Arctic and Antarctic sea ice. .

L333: Please explain in more detail why you think that the consistent cold bias stems from a difference in the land-sea mask.
We have revised the text to clarify that the cold bias may be related to the representation of surface characteristics around the Eneide station.

L337: I cannot see from your results that either of the two polynyas, the Terra Nova Bay polynya or the Ross Ice Shelf polynya, reached this maximum extent. This statement is not well connected to the rest of the paper.
We agree, this is likely a hangover from an earlier draft and we have removed this statement from the revised manuscript.

L340: "Fig. 7" --> wrong figure. Also, there is no figure showing sensible heat flux values.
We thank the reviewer for spotting this. Following the suggestion, we have replaced precipitation with sensible heat flux in the revised manuscript.

L341-344: Not clear where these lines are refering to.
We agree and have edited this statement for clarity in the revised manuscript.

Section 5: --> see also GC3

Missing elements:

- critical reflection on the model results and whether / how the observations can be used as they were used.

- reflection about the potential of other data sets is missing

- daily input but hourly results

- Noise and uncertainties created

- The role of leads

- Influence of sea ice thickness on model results of the larger polynyas.
We thank the reviewer for these valuable suggestions. We have addressed all of these points by substantially expanding and restructuring the Discussion section. In particular, we have included a more critical assessment of the model results and observations, discussed the limitations associated with the use of daily sea-ice boundary conditions and observational datasets, considered the role of leads and sea-ice thickness, and highlighted the potential of emerging high-resolution Earth observation products and future regional reanalyses.

L349: No, you did not investigate an impact on the "sea ice extent".
We replace sea ice extent with sea ice processes in the revised manuscript.

Please be more specific: You investigated LATENT surface fluxes, LOW LEVEL cloud COVER, and LONGWAVE DOWNWELLING radiation AT THE SURFACE.
Done

L357/358: The results obtained for the Ross Sea are not sufficiently mature to back up the statement "A similar pattern ... Sea Polynya."
We agree and have removed this from the revised manuscript.

L360-362: Please avoid speaking about "polynya extent" in the context of your work. You are not dealing with the extent of the polynya; you did not work with a clear definition for it. What you are working with are time series of the sea ice area (i.e. the integral surface area of the ocean covered by sea ice) within some more or less rigidly defined regions within which the polynyas of interest are located. You take the sea ice area as an approximate measure of the polynya size - no matter whether there are other factors influencing the sea ice concentration and with that the sea ice area computed.

I am fine with the conclusion about the elevated latent heat flux but the investigations (and results) about the low level cloud cover are more speculative - and those about longwave heatflux, near surface air temperature and precipitation even more. I recommend to be more critical with the interpretation of your results.
We thank the reviewer for this comment. We agree and have revised this section in the manuscript.

L371-374: I suggest to remove the part about precipitation completely. I don't think that the results shown are credible in that regard.
We thank the reviewer for this comment. We agree and have removed this statement for Antarctica while retaining it for Greenland in the revised manuscript. For Greenland, we believe the available evidence provides stronger support for the relationship discussed, whereas for Antarctica the interpretation indeed remains more speculative.

Editoral Comments / Typos:

L10: "including with direct use"? --> "including direct use"?
Done

L13: "an improved representation of climate" --> This reads a bit odd. Perhaps it would be sufficient if you would directly relate to the processes that you mention later in this sentence.
Done

L14: "near the ... difficulty capturing" --> not clear what you want to say here. Please reformulate.
Done

L22/23: What are the common features you are refering to? What exactly do you mean by "at the surface interface with the atmosphere"?
We have revised the entire paragraph to improve its clarity

L25: "the high resolution needed to resolve them" --> this sentence should be backed up earlier with a clear statement about the typical size of the polynyas studies - which, by the way, can be quite variable if you compare the size of the Terra Nova Bay polynya with the Ross Ice Shelf polynya.
have revised the entire paragraph to improve its clarity and represent the typical size of the polynyas more fairly.

L27/28: "including for failing ... 2012)." --> This paper is about 13 years old. I recommend to update this statement with more recent information - e.g. from the CMIP6 team.
Done

L30: "is not well represented (Rinke et al., 2006)" --> Also this reference is quite old. You could consider updating this reference with more recent work.
We considered more recent literature; however, we could not identify a newer reference that supports this specific statement as directly as Rinke et al. 2006. Therefore, we have retained the original citation.

L31: "suffer similar" --> "similar from similar"
Removed

L38-30: "satellite ... the OSI-SAF" --> This sentence needs to be re-written. What is "from the ESA CCI"? What is "by the OSI-SAF"? The part: "data set of high resolution sea ice data ... sea ice concentration" should possibly be condensed.
We have clarified and condensed this section further.

L38: "also use" --> why "also"?
Done

L49: "in the pack ice" --> should be deleted because you are not dealing with open ocean polynyas such as the one in the Weddell Sea or the Cosmonaut Sea. Coastal polynyas are formed close to the coast, often along a landfast ice border or along an ice shelf.
Done

L50: "off shore away from the coast" ... either "away from the coast" or "off shore" is enough.
Done

L51: What you should perhaps add to this sentence is the role of the latent heat of fusion that supports the polynya to stay open - as described in the paper of Morales-Maqueda you are citing further down.
This is helpful feedback, we have added a sentence describing the release of latent heat during continuous ice formation while clarifying, following Morales-Maqueda et al., that the persistence of latent heat polynyas is maintained by continuous ice export rather than by the latent heat release itself.

L57 / L71-72: The North Water polynya is not solely a latent heat polynya. This polynya is a mixture of a latent heat and a sensible heat polynya. Please check available literature about additionally factors such as tides and upwelling of warmer water masses and correct your writing accordingly.
Done

L63: "in" --> "by"

We thank the reviewer for this suggestion. We have carefully considered this change but have retained **"in".**

L63: "2010" --> "2018"
Done

L71: "Vincent, 2019" --> This is a publication focusing on the aspect of the ice arch formation and how ice conditions have been monitored using satellite remote sensing. It is not a valid reference for the various topics you touched upon in this sentence. Please find 1-2 references that are fit-for-purpose.
Done

L77: "Ross Sea" --> I strongly recommend to not use the term "Ross Sea polynyas" - simply because there are several: The Terra Nova Bay polynya is one of them. The more accurate name would be "Ross Ice Shelf polynya". See also GC0
Done

L87/88: "permafrost ... on regional climate." Please make sure that you provide references for these two statements as well.
Done

L94/95: "inherit ... models" --> I don't understand what you want to say here. What are "bosses" in this context?
We thank the reviewer for pointing this out. The word "bosses" was a typographical error and has been corrected to "biases."

L111: "Terra Nova Antarctica" --> Not sure what you are referring to here - only the Terra Nova Bay polynya or also the Ross Ice Shelf polynya?
We have revised the sentence to improve its clarity

L112: "and time step of 45 second" --> "and a time step of 45 seconds."
Done

L114-116: "Notably ... Von Storch et al., 2000)" --> Would it make sense to move these sentences a bit further down to where you actually begin writing about the nudging (i.e. L124)?
Done

L158-160: Please provide the temporal resolution as well. I guess it is daily? Did you eventually take into account to contact the data providers (after you have read the paper by Lavergne et al. (2019), to ask for the swath data of the SIC data?
We thank the reviewer for this suggestion. We have clarified in the manuscript that both sea-ice concentration Climate Data Records are provided at daily temporal resolution

L167-169: The SIC uncertainty sources listed are, no doubt, correct. The question a reader may have is how relevant these uncertainty sources are for your specific aims.
We have revised this paragraph to clarify which uncertainty sources are most relevant for the present study.

L187: "polynya over Greenland" --> Not sure what you mean: "North of Greenland"? "Near Greenland"? Please correct. A polynya cannot develop on the Greenland ice sheet. Please check usage also in your following text. An example of GC0.
We have replaced "polynya over Greenland" with "Greenland coastal polynyas"

L189/190: I suggest to refer to the two polynyas you are aiming to investigate. In addition: using "intense" in the context of a minimum seems a bit odd. Please try to provide a more specific description of what you observe. You might want to say: "ESA-CCI sea ice concentration values are lower than OSI SAF sea ice concentration values in these two regions." or "ESA-CCI sea ice concentrations exhibit more details than OSI SAF sea ice concentrations."?
Done

L191: "This polynya" --> Which one? I guess you used a name for that polynya before and it makes a lot of sense to stick to your own notation throughout the manuscript.
We thank the reviewer for pointing this out. We have replaced "This polynya" with the specific polynya name used elsewhere in the manuscript to ensure consistent terminology throughout.

"Narest Strait" --> Do you mean "Nares Strait"? You need to correct this also in following instances (e.g. L195).
Done

What is a "sea-ice value"? Please use correct language and be more specific. There are also several other instances of this.
Done

L195: "over" --> "at" or "along"
We thank the reviewer for this suggestion. We have replaced "over" with "across" to improve the clarity and readability of the text.

L196/197: "with OSI-SAF ... of Peary Land." --> This half sentence seems to be out of context.
We removed the sentence

L199/L201: "Wadel Sea" --> please use the correct name.
We thank the reviewer for this comment. In this case, we are referring to the Wandel Sea in northeastern Greenland

Figure 2: Is it correct that you show the two SIC products at their native grid resolution?
The figures show the two sea-ice concentration datasets after interpolation to the 4 km HCLIM grid rather than at their native resolutions. We have clarified this in the figure caption.

L203: "Sea ice area" Please see my comment to Figure 1 related to the polygons.
Done

L215: What are "sea-ice differences"? Please be more specific.
Done

"latent heat flux" --> Please state clearly that you are (since when) talking about results you obtained using your model. This is not clear here.
We have revised this paragraph

L216/217: "when sea ice reached its minimum" --> what "sea ice" parameter? thickness? drift speed? coverage? concentration? Please be more specific.
We add area to improve the clarity and readability of the text.

L218: "differences between ESA CCI and OSI SAF remained below" --> You must make the connection to the model and the model results.

Done

L221: "minimum sea ice" what? area? thickness?
We thank the reviewer for this suggestion. We add area to improve the clarity and readability of the text.

L222/223: "analyzed " ... using which tool or method? Again you need to connect to your model.
Done

L226/227: "ESA CCI indicated ..." --> Again the connection to the model is missing. ... "Using ESA CCI sea ice concentration data as sea ice cover boundary condition (or forcing) the HCLIM simulations result in higher low-level cloud fractions than using OSI SAF sea ice concentrations."
Done

L260+: Whenever you write about a temperature difference it would be good if you'd consider to use the unit K. It avoids confusion with actual temperature values expressed as degC.
Done

Figure 5: Please add in the caption of the figure what the "Difference" is.
We have revised the figure caption to explicitly state that the dashed line represents the difference between the HCLIM–ESA CCI and HCLIM–OSI SAF simulations (HCLIM–ESA CCI minus HCLIM–OSI SAF).

Please consider plotting a horizontal line denoting a difference of zero degrees. This applies to all other figures containing panels of the same sort in which you are combining actual values with a difference.
We have added a horizontal reference line at zero in all difference panels to facilitate the interpretation of positive and negative differences. This has been implemented consistently in all figures that combine absolute values with their corresponding differences.

L270: Make sure that you are not changing the name of the experiment / or run. So far it was "HCLIM-ESA CCI" and "HCLIM-OSI SAF". There are more instances of this later in the text.
We thank the reviewer for pointing this out. We have carefully revised the manuscript to ensure that the experiment names are used consistently throughout the text. All occurrences have been corrected to use the naming convention HCLIM–ESA CCI and HCLIM–OSI SAF.

L284+ The entire paragraph needs to be re-written, checking carefully the language used, the terms used, and other elements. Please consider shortening the paragraph substantially because a reader can take a look at the maps and figure our most of the spatio-temporal development of the sea ice concentration by themself. Formulations should be improved.
We have revised the full paragraph and shortened it as requested.

L276-277: "However, ..." --> Please check this sentence.

Why did you switch to the precipitation as an additional parameter here and did not also compare surface pressure and relative humidity?
Alert is one of the very few locations in the region that observes precipitation in situ, as such we feel it is worth drawing attention to.

L283: "of the polynya over Terra Nova" --> "of the Terra Nova Bay polynya"
Done

L303: Why now "5th" of October? I thought so far the period of investigation begins Oct. 10.
Fixed

L303: "reached its minimum sea-ice extent" --> I know what you want to express here but: i) Sea-ice extent has a fixed definition that should not be used in the context of describing the properties of a polynya. ii) Fig. 8 and 9 show sea-ice area. I suggest to write something along the lines: "The Ross Ice Shelf polynya reached its maximum size - illustrated by means of the minimum sea ice area on days y and z"
Fixed

Figure 8: The panels are too small to adequately see the development of the polynas in the sea ice concentration maps. Their size must be increased.
Fixed

Figure 10 & 11: These figure cannot contain CARRA results. Please update the caption.
Done

L330/331: Please connect Figure 12 and its description better to the work done about polynyas - I assume that you are doing this comparison to better understand the results obtained for the Terra Nova Bay polynya?
We have added some additional text about Fig.12 int eh discussion

L331-332: You need to switch the colors; the blue is for the finer (higher) resolution boundary conditions; In addition - why do you here not again refer to ESA CCI and OSI SAF?

L350: "sea ice products" --> "sea-ice concentration products"
Done

L355: "in narrow" --> better: "for small"
Done

L356: "such as the" --> "such as occurring in the"
Done

L358: "sea ice products" --> which?
Done

L363/364: I don't think "smaller ice concentration" is the correct expression here. You possibly should write something that expresses the larger degree of detail. Speaking globally of a "smaller" ice concentration gives the wrong impression.
This is a good point, we have also revised the sentence to clarify this statement.

As stated above, I don't find the results for the Terra Nova Bay polynya that convincing.

L366-369: "In Greenland ... exceeded 1.5degC" --> not clear what you want to state here. Please re-phrase.
Done

L366: "resulted in" --> "contributed to"
Done

L382/383: "and our results show ... impact" --> I don't understand what you want to say here. Why is this?

We have also revised the sentence to clarify this statement.

L392: I don't understand. Why do your results show that an Antarctic (atmospheric) reanalysis is plausible?
We have clarified the statement; our results do not directly demonstrate the feasibility of implementing a CARRA-like reanalysis for Antarctica but we 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.

L502: Please check this reference. The name is not correct as written here. I guess this needs to be the CMIP community.
Done

L542/543: The doi is not resolved. I could not access the paper.
Fixed. We noticed that the original link to the paper is no longer working. The paper can be accessed at the following link: https://aps.chinare.org.cn/EN/10.13679/j.advps.2021.0026. However, to avoid any potential issues with accessibility, we have decided to replace this reference with a more readily accessible alternative in the revised manuscript.