RC1:

General comments

In this study, the authors describe a new operational sea ice forecasting system for the Southern Ocean using a regional MITgcm ocean/sea ice/ice shelf general circulation model along with an ensemble based localized error Kalman Filter data assimilation system that assimilates sea ice concentration on a daily basis. Results from forecasts ranging from 24 to 168 hours are compared against different observational products to show the model performance in terms of RMSE of sea ice concentration, integrated ice-edge error, mean absolute error of ice thickness, and mean absolute error of sea ice drift. There was also a comparison of model sea ice convergence forecasts versus changes in MODIS imagery for one particular event (a sea ice opening in November 2021 that would be relevant for navigation to a particular coastal station).

I thought the manuscript was mostly (see below) clear and easy to understand. A regional sea ice forecast system for the Southern Ocean would certainly be useful, not only for the scientific/resupply missions for different nations, but also for the many private operations (i.e. fishing and tourism) that are becoming more numerous in Antarctic waters. MITgcm is a great tool for the ocean and ice shelf modeling. While the embedded sea ice model may not be the most "up to date", I think it is fine for these purposes, especially in the Antarctic where I do not think the lack of different ice thickness categories is such an issue where there is not much multi-year ice. I am not an expert on data assimilation and cannot comment on the appropriateness of the method used here (hopefully there will be a reviewer who can). All in all, this seems like a solid numerical setup for a sea ice forecast system (although I do have some questions below).

Response:

Dear reviewer, thanks a lot for your time and valuable comments on this manuscript. Our replies to your comments and suggestions are as follows.

My two concerns for a manuscript that is describing a forecast system is that I think more needs to be added to the model description and that it is hard to tell how well this setup is performing compared to either a simple forward model with no data assimilation or other existing global sea ice forecast models.

1) I think there are important aspects of the forward model that are relevant to dynamically moving ice around that are not described. Is there any parameterization of landfast ice processes? I know one exists in MITgcm, but that is meant more for the Arctic and it is not mentioned here, or in the Zhao et al. 2023 paper describing the forward model, whether it is used (and/or modified). Are icebergs (especially grounded ones that can limit ice transport) represented? Is tidal forcing included? All these processes would be relevant to ice motion and divergence.

Response:

Itkin et al. (2014) proposed a landfast ice parameterization for low salinity shallow shelf water in the Arctic marginal seas and tested its impacts on the stability of the Arctic halocline based on the MITgcm. Taking into account the unresolved shallow water topography and landfast ice internal strength, the parameterization sets the maximum compressive strength of landfast ice to the double of the drift ice inside the prescribed maximal landfast ice edge mark. Liu et al. (2022) proposed a more complicated landfast ice parameterization that uses lateral drag as a function of sea ice thickness, drift velocity, and local coastline length. Their simulation suggested that the parameterization leads to an improved and realistic landfast ice distribution in most marginal seas in the Arctic.

Our model does not include landfast ice parameterization. Although without landfast ice parameterization, the model still has a capacity to simulate the nearly immobile sea ice zone attached to the coast of the Antarctica, which can be inferred from Figure 10 in the original manuscript. The capacity originates from the correct simulation of sea ice thickness and drift velocity in the landfast ice zone.

Iceberg parameterization and tide forcing are also not applied in our model. We admit that all these processes participate in regulating sea ice motion and divergence, and we thank you for pointing out these as our future model developing direction.

We have clarified relevant model settings. The statement of "Neither specific landfast ice parameterization, iceberg parameterization, nor tide forcing has been involved in the SOIPS." has been added into the revised manuscript.

References:

Itkin, P., M. Losch, and R. Gerdes (2015), Landfast ice affects the stability of the Arctic halocline: Evidence from a numerical model, J. Geophys. Res. Oceans., 120, 2622–2635, doi:10.1002/2014JC010353.

Liu, Y., M. Losch, N. Hutter, and L. Mu (2022), A new parameterization of coastal drag to simulate landfast ice in deep marginal seas in the Arctic, J. Geophys. Res. Oceans., 127, e2022JC018413, doi: 10.1029/2022JC018413.

2) The manuscript has several descriptions of the performance of the data assimilative forecasts, but it is difficult to tell how much the data assimilation adds to the forecast skill. Was there a control run (like in Liang et al., 2019, JGR, which has some of the same authors) without data assimilation over the same dates as the forecast runs? If so, what does that look like compared to observations? What do the ice concentration and integrated ice-edge errors look like over time with no forward modeling and just the initial analysis (i.e. persistence of the initial sea ice state throughout the forecast period)? I do not expect the authors to go through all the different existing global sea ice forecasting systems, but I think it might be helpful to a reader if some information were given on how this system compares to others. For example, the 168-hour sea ice concentration RMSE for this model (Figure 3) looks considerably better for most months than the GIOPS for the Southern hemisphere (Figure 3b in Smith et al., 2016).

Response:

Thanks a lot for this comment. We renamed the original experiment as DA_Forecast in the revised manuscript. According to your suggestions, we have conducted a control run without any data assimilation (NoDA_Forecast) and also performed persistence forecast (PE_Forecast) in the analysis. We believe that these additional analysis can substantially improve the quality of our manuscript.

Regarding to sea ice concentration forecast (Figure R1), the DA_Forecast run performed best and the NoDA_Forecast run performed worst in most time except during late March–early June. Since the PE_Forecast run uses the initial condition of the DA_Forecast run which assimilates sea ice concentration observations as forecasts of the following 168 hours, the PE_Forecast run generally performed better than the NoDA_Forecast run and worse than the DA_Forecast run. During late March–early June, the PE_Forecast run performed worse than the other two runs at lead time of 168-hour, suggesting that the sea ice changes rapidly in response to the oceanic and atmospheric forcing during this onset–to–fast freezing period.

Regarding to the IIEE forecast (Figure R2), the DA_Forecast run performed best and the NoDA_Forecast run performed worst over the whole study period.

Regarding to sea ice thickness forecast (Figure R3), the DA_Forecast run also performed better than the NoDA_Forecast run at lead time of 24-hour. Sea ice thickness changes relatively small in one day, and sea ice thickness forecast of the PE_Forecast run is approximate equal to that of the DA_Forecast run at lead time of 24-hour, so the PE_Forecast run is not marked on this figure.

We also compare our forecasts to the physical analysis field of the Antarctic Ocean produced by Mercator Ocean International (MOI; accessed at https://data.marine.copernicus.eu/product/GLOBAL_ANALYSISFORECAST_PHY_001_024/description). The MOI product is physical analysis data and free accessed. We can not download the GIOPS data from the internet.

With respect to the OSISAF data, the RMSE of sea ice concentration forecasts of the DA_Forecast run at lead time of 24-hour is larger than that of the MOI product (Figure R4), while the IIEE of the DA_Forecast run at lead time of 24-hour is close to that of the MOI product (Figure R5). Note that the MOI product has assimilated the OSISAF sea ice concentration data, the SOIPS assimilated the AMSR2 sea ice concentration data, thus the MOI product has a lower RMSE of sea ice concentration when uses the OSISAF data as validation reference in this study.

Detailed comparison among the different runs and validation of our system against the observations (Figure R1, R2, R3) are presented in the revised manuscript. We put Figure R4, R5 into the supplementary material.

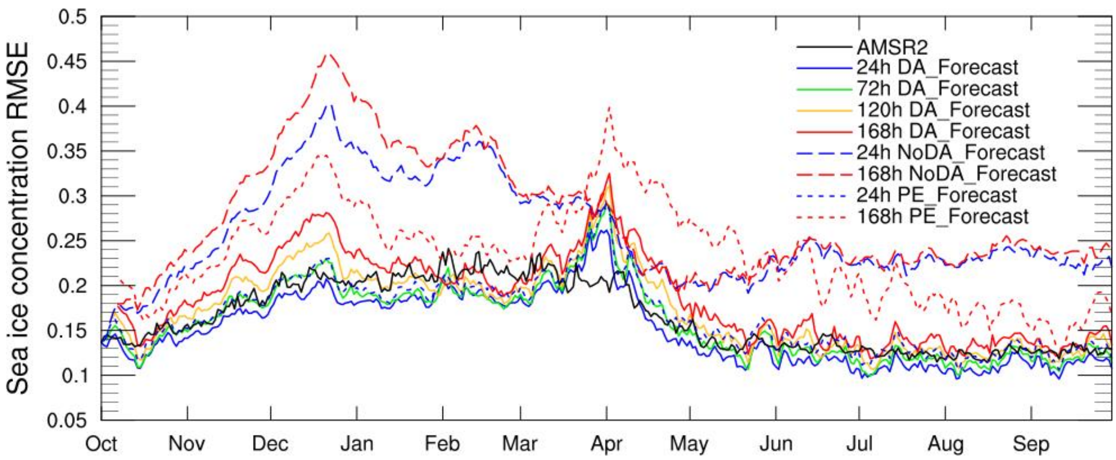

Figure R1. Time series of the RMSEs of the assimilated AMSR2 data and sea ice concentration forecasts at different lead time with respect to the OSISAF data. The blue, green, yellow, red, and black solid lines denote the sea ice concentration forecasts of the DA_Forecast run at lead time of 24-hour, 72-hour, 120-hour, 168-hour, and the AMSR2 data, respectively. The blue and red long-dashed lines denote the sea ice concentration forecasts of the NoDA_Forecast run at lead time of 24-hour and 168-hour, respectively. The blue and red short-dashed lines denote the sea ice concentration forecasts of the PE_Forecast run at lead time of 24-hour and 168-hour, respectively.

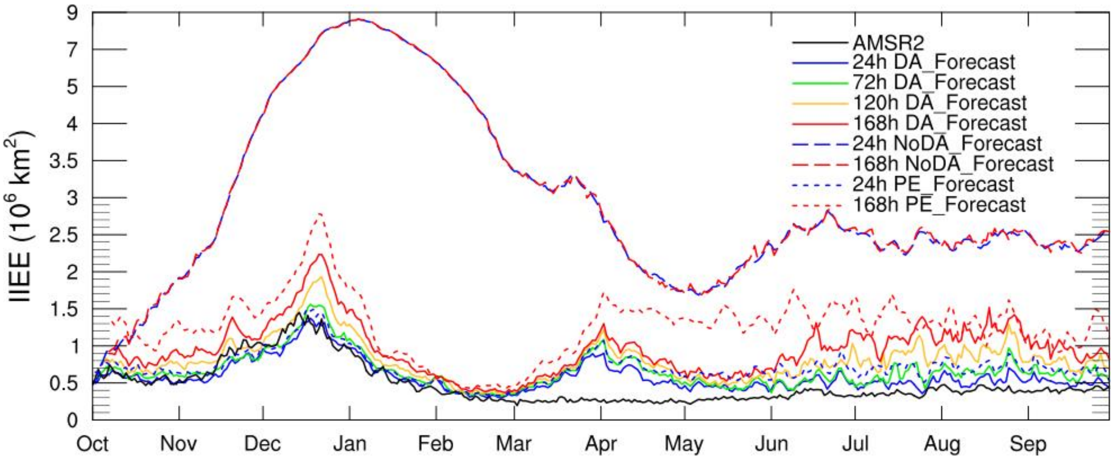

Figure R2. Time series of the IIEE of the assimilated AMSR2 data and the forecasts at different lead time with respect to the OSISAF data. The blue, green, yellow, red, and black solid lines denote the IIEE forecasts of the DA_Forecast run at lead time of 24-hour, 72-hour, 120-hour, 168-hour, and the AMSR2 data, respectively. The blue and red long-dashed lines denote the IIEE forecasts of the NoDA_Forecast run at lead time of 24-hour and 168-hour, respectively. The blue and red short-dashed lines denote the IIEE forecasts of the PE_Forecast run at lead time of 24-hour and 168-hour, respectively.

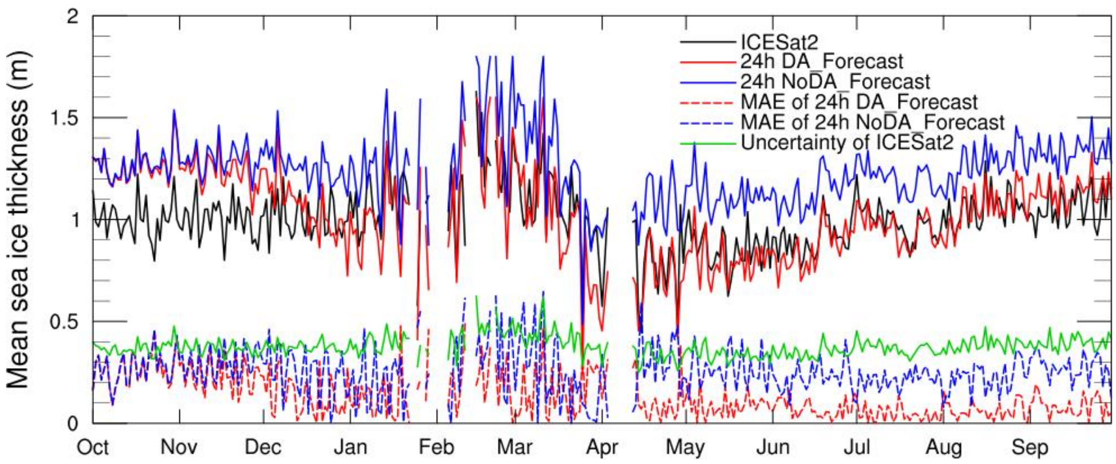

Figure R3. Time series of the mean sea ice thickness and uncertainties of the ICESat-2 observations (black and green lines), the sea ice thickness forecasts at lead time of 24-hour of the DA_Forecast and NoDA_Forecast runs (red and blue solid lines), and the mean absolute errors between the forecasts and observations (red and blue dashed lines).

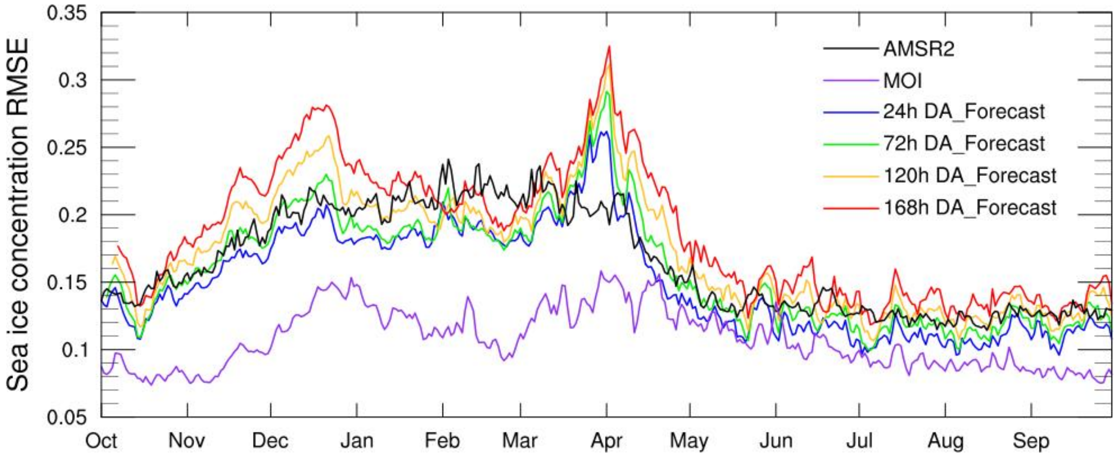

Figure R4. Time series of the RMSEs of the assimilated AMSR2 data, the MOI product, and sea ice concentration forecasts of the DA_Forecast run at different lead time with respect to the OSISAF data. The blue, green, yellow, red, black, and purple lines denote the sea ice concentration forecasts at lead time of 24-hour, 72-hour, 120-hour, 168-hour, the AMSR2 data, and the MOI product, respectively.

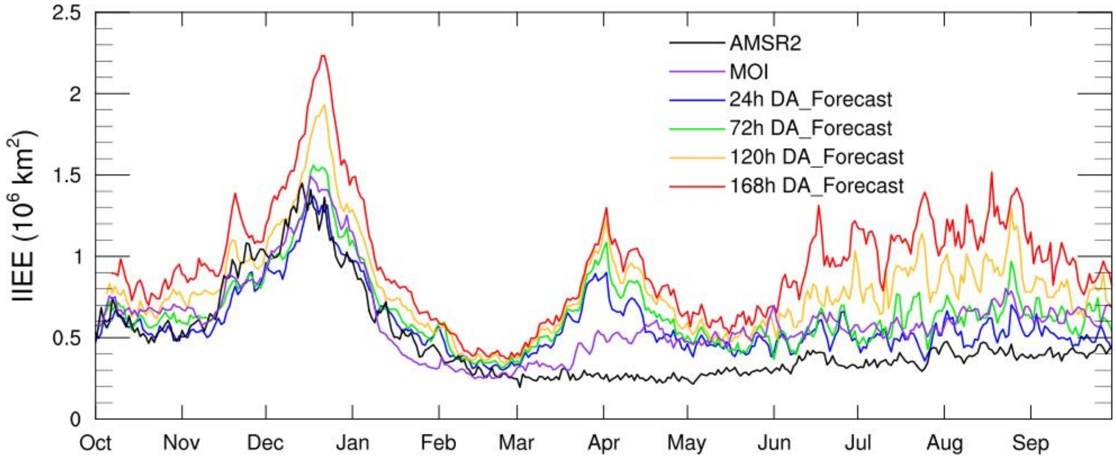

Figure R5. Time series of the IIEE of the assimilated AMSR2 data, the MOI product, and the forecasts of the DA_Forecast run at different lead time with respect to the OSISAF data. The blue, green, yellow, red, black, and purple lines denote the forecasts of the DA_Forecast run at lead time of 24-hour, 72-hour, 120-hour, 168-hour, the AMSR2 data, and the MOI product, respectively.

There are also several minor awkward usages or subject/verb agreement mistakes. I am not going to explicitly comment on most of them, and they generally do not make the manuscript more difficult to understand (I still feel the manuscript is pretty well organized and understandable), but I think they should be cleared up in the next version.

I have some other specific comments and suggestions below, but most of these are minor and should be easily dealt with by the authors. I think SOIPS may be a very good forecast system for sea ice, but this manuscript still needs some work before it can help an interested reader judge that for themselves.

Specific comments

Line 26: By "with thin first-year ice dominating the majority" do the authors mean that the majority of the ice is thin first-year ice?

Response:

Yes. We revised this sentence to “This situation is partly caused by the natural feature of Antarctic sea ice that the majority of the ice is thin first-year ice.”

Line 28: Since katabatic winds can blow sea ice away from the coast, as well as away from the front of ice shelves, suggest changing "off the ice-shelf" to "off the ice-shelf and coast".

Response:

Revised.

Lines 61-63: I agree with the authors that regional models "with higher resolution" still offer significant advantages, but isn't the resolution of this model (line 93: ~ 18 km) lower than the resolution at these latitudes of most of the global models (1/4 degree or better) listed in this paragraph?

Response:

I agree. We revised this sentence to "Although resolution of global models is constantly becoming finer, regional ice–ocean coupled models with lower computational cost still offer some advantages when appropriate initial and boundary conditions are adopted".

Line 92: I think it is also worth mentioning that the open boundaries are farther north than any likely northern extent of the sea ice.

Response:

We revised this sentence to "The ocean model uses curvilinear coordinates with the open boundaries far north away from the domain of the Antarctic Circumpolar Current (ACC) and any likely northern extent of the sea ice.".

Line 95: Large and Pond (1981) is just the bulk formula for momentum flux (I think). Is there a bulk formulation used for heat and salt/freshwater fluxes?

Response:

We revised this sentence to "The ocean model utilizing the finite-volume incompressible Navier-Stokes equations adopts the bulk formula for heat and momentum calculation at surface (Large and Pond, 1981, 1982)".

Line 99: Suggest adding the Losch 2008 reference that describes the implementation of ice shelves in MITgcm.

Response:

Added.

Line 107 (and line 376): I do not think Zwally, 1990 is the best reference for the initial ice thickness data and the URL given on line 376 is the ICESat 500m DEM, not the sea ice thickness. Is this the Kurtz and Markus (JGR, 2012) data?

Response:

Yes. We revised the reference to Kurtz and Markus (2012).

Line 128: Were any experiments done with more or less ensemble members?

Response:

NO. We have not carried out any experiment to test the impact of ensemble size on forecasting ability for the Antarctic system. The choice of 12 ensemble members is made according to the setting of our Arctic system and the limitation of computational resource of operational implementation.

Lines 129-132: I assume the ensemble is generated in the method described in the PDAF wiki (https://pdaf.awi.de/trac/wiki/EnsembleGeneration), but there is no reference, and very few details, on how it is generated.

Response:

The ensemble is generated using the method described in the PDAF wiki. We added "(Pham, 2001)" into the revised manuscript.

Lines 174-175 and Figure 3: I agree that the bias between AMSR2 and OSISAF partially explains the ice concentration forecasting errors, but the shorter term forecasts (24 and 72 hours) look to generally be better than the assimilated AMSR2 data. Do the authors have any explanation for this? What would the forecast errors be with no data assimilation?

Response:

In the DA_Forecast run, the AMSR2 data was assimilated into the ensemble of model restart fields, and an analyzed (updated) ensemble of model restart fields was generated. Initialized from the analyzed ensemble of model restart fields, each ensemble member was integrated for 168 hours driven by atmospheric forcing. So the

24-hour and 72-hour forecasts included not only the observational information, but also sea ice changes generated by model physics. The NoDA_Forecast run performed worse than the DA_Forecast run in most time (Figure R1; also see the response to your major comment 2).

Figure 4: I think it would be helpful to have a figure like this (monthly RMSE for ice concentration) for the AMSR2 vs. OSISAF for comparison. I can certainly understand the authors not wanting to add any figures to the primary manuscript, but perhaps in a supplementary material section?

Response:

The monthly patterns of the RMSEs of sea ice concentration between the AMSR2 and OSISAF data (Figure R6) generally resemble and set the base for those between the 24-hour forecasts and the OSISAF data. We put Figure R6 into the supplementary material.

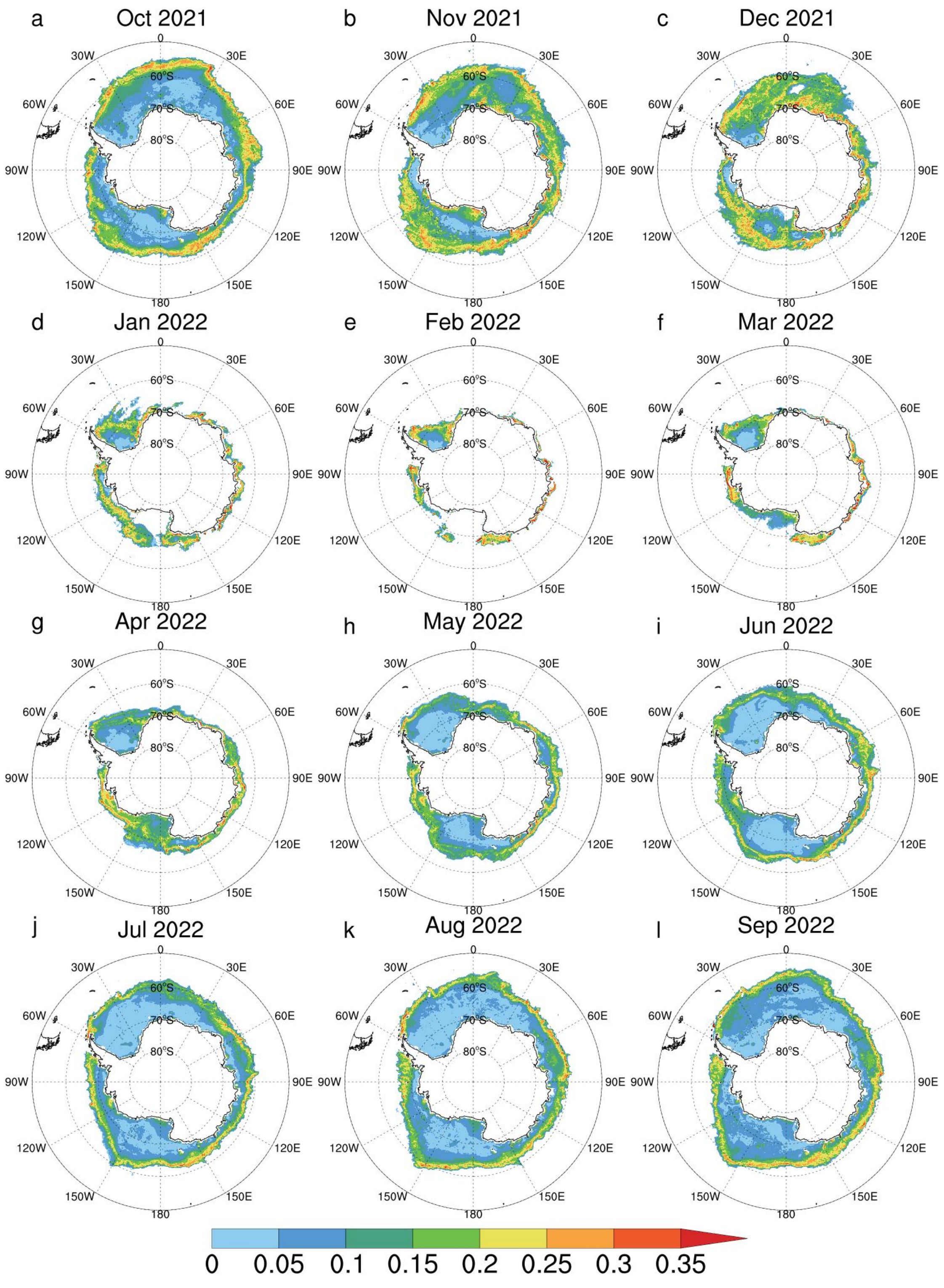

Figure R6. Monthly patterns of the RMSEs of sea ice concentration between the AMSR2 and OSISAF data. (a)–(l) denote October 2021–September 2022.

Lines 218-221: As mentioned above, what about other non-simulated (I think, it is never explicitly stated one way or another) barriers to sea ice free drift such as fast ice or grounded icebergs?

Response:

Thanks for the suggestion. The statement of "The lack of specific landfast ice parameterization may lead to unrealistic landfast ice zones around the Antarctica, which possibly also contribute to the mismatch of sea ice edges." has been added into the revised manuscript.

Figure 6: The sea ice edge forecasts look really good at 24-hours. If the authors do create a supplementary material section, I would be curious what the ice edge looks like at 168-hour lead time.

Response:

Comparing to the sea ice edge forecasts at lead time of 24-hour, the biases of sea ice edge forecasts at lead time of 168-hour with respect to the OSISAF data (Figure R7) are larger in November–December, March–April, and July–August. The areas with obvious bias amplification locate in southeastern Atlantic Ocean sector, southwestern Indian Ocean sector, and the southwestern Pacific Ocean sector. We put Figure R7 into the supplementary material.

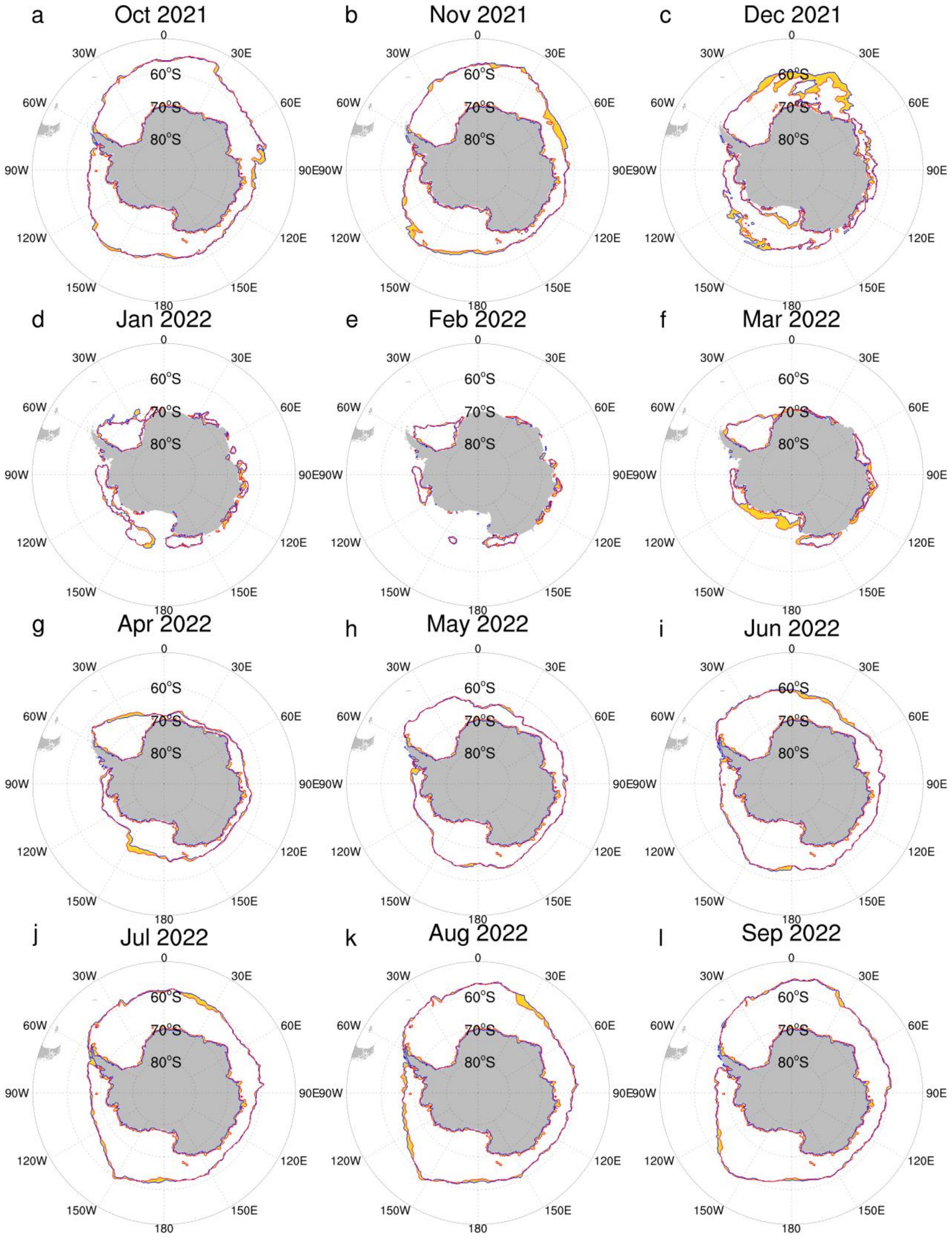

Figure R7. Monthly patterns of sea ice edge forecasts at lead time of 168-hour with respect to the OSISAF data. (a)-(l) denote October 2021–September 2022. The blue lines denote the DA_Forecast run. The red lines denote the OSISAF data. The gold contours denote the IIEE.

Figure 8: As for figure 6, what does the ice thickness look like at 168-hour lead time?

Response:

The biases of sea ice thickness forecasts at lead time of 168-hour with respect to the ICESat2 data (Figure R8) do not change obviously in comparison with those of 24-hour. We put Figure R8 into the supplementary material.

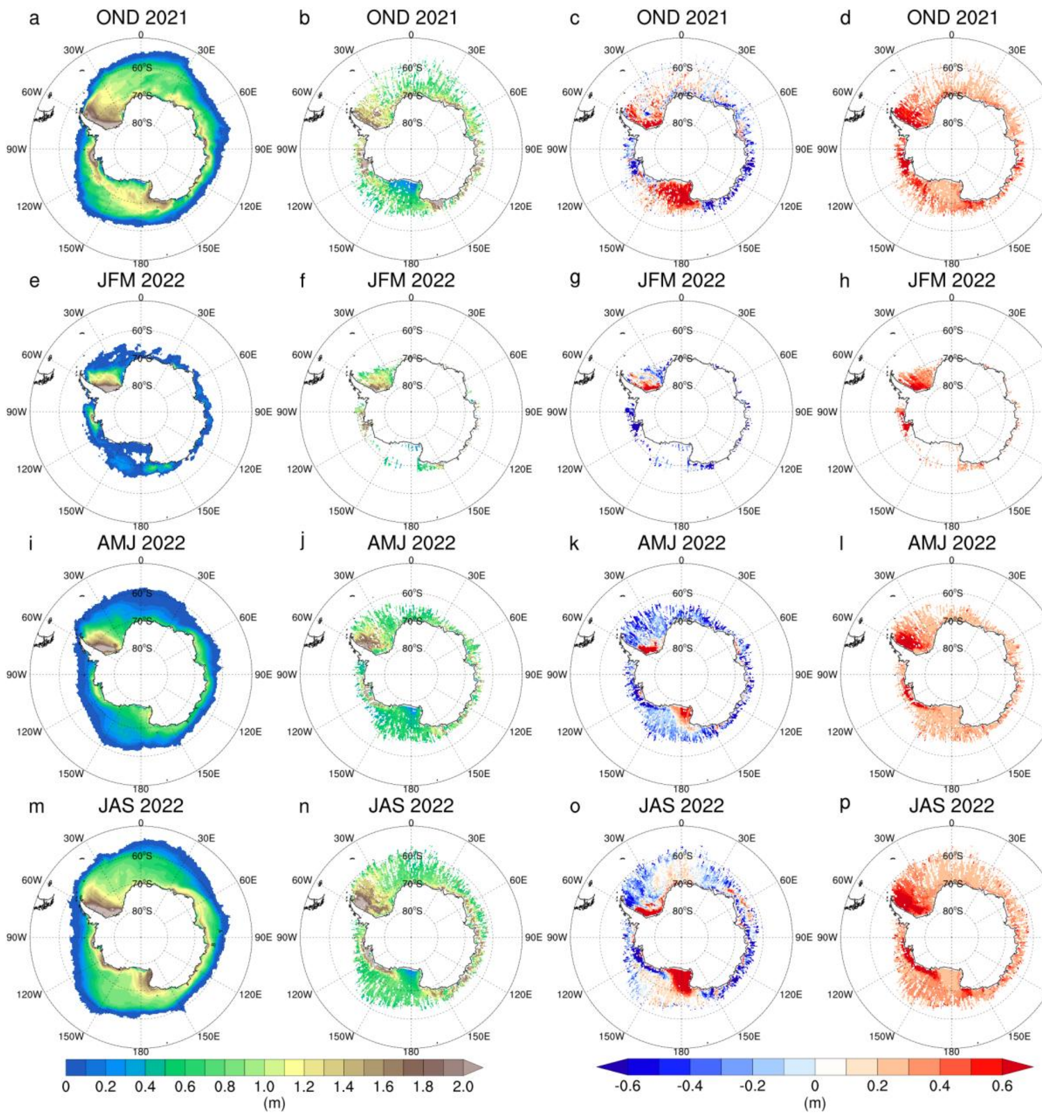

Figure R8. Seasonal patterns of the Antarctic sea ice thickness. The columns from left to right denote the DA_Forecast run at lead time of 168-hour, the ICESat2 observations, their deviations, and the uncertainties of the ICESat2 observations, respectively. The panels from top to bottom denote October–December, January–March, April–June, and July–September, respectively.

Lines 279-280 and Figure 9b: I agree that it is mostly true that "the MAEs of both magnitude and direction of the sea ice drift forecasts do not exhibit significant amplification", but the MAE of the drift angle does increase significantly at 168 hours (compared to shorter lead times) in Oct-Nov and Jun-Sep.

Response:

We revised the sentence to “Along with the prolong of the forecast lead time, the MAEs of the sea ice drift magnitude forecasts do not exhibit significant amplification, and those of direction forecasts grow significantly at lead time of 168-hour in October–November and June–September.”.

Lines 288-289: The mean absolute errors and mean magnitude of the NSIDC drift velocities are given earlier, but I did not see anything to indicate whether the mean model bias with respect to NSIDC was positive or negative until here. Apologies if I missed it, but is the mean difference (not mean absolute error) or mean drift velocity from the model given anywhere?

Response:

Thanks for the comment. We do not mention whether the mean model bias with respect to NSIDC is positive or negative in the original manuscript. The biases of sea ice drift magnitude between the DA_Forecast run at lead time of 24-hour and the NSIDC data are not uniform (Figure R9). In general, the DA_Forecast run produces larger magnitude of sea ice drift in the northern marginal sea ice zone and the coastal areas, while in between the DA_Forecast run produces smaller magnitude of sea ice drift.

The statement of “In general, the DA_Forecast run produces larger magnitude of sea ice drift in the northern marginal sea ice zone and the coastal areas, while in between the DA_Forecast run produces smaller magnitude of sea ice drift.” has been added into the revised manuscript. We also put Figure R9 into the revised manuscript.

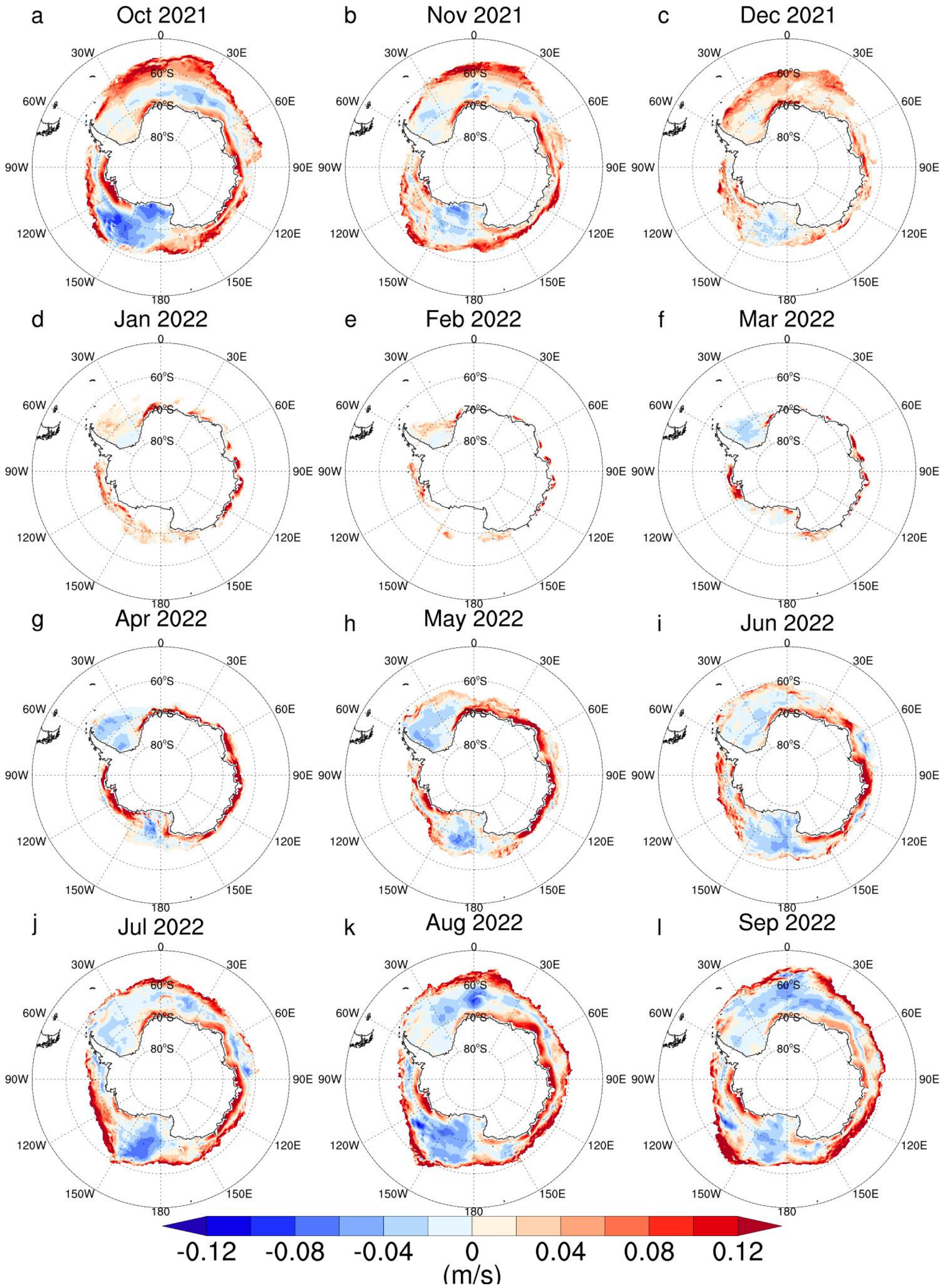

Figure R9. Monthly patterns of the sea ice drift magnitude biases between the DA_Forecast run at lead time of 24-hour and the NSIDC sea ice drift data. (a)-(l) denote October 2021–September 2022.

Line 298: Since landfast ice also floats, I suggest changing "Floating sea ice" to "Drifting sea ice" or "Moving sea ice".

Response:

We revised "Floating sea ice" to "Drifting sea ice".

Line 299: Same comment as above about "floating sea ice zone".

Response:

Revised.

Figure 10: What is the lead time for the forecasts on the left? Also, Figures c and d are really impressive!

Response:

Thanks for this comment. This typical case was derived from the operational implementation of the SOIPS forecasts. The forecast was initialized on November 18, 2021, and Figure 10a, 10c, 10e denote the 24-hour, 48-hour, 72-hour forecasts.

We added the statement of "The forecast was initialized on 2021 November 18." into the figure caption.

Lines 337-338: Same point as for Lines 218-221 above.

Response:

We revised the sentence to "It should be mentioned that mismatch of sea ice edges in some nearshore areas originates from the divergence of coastlines, ice-shelf fronts or unrealistic landfast ice zones in the model domain and the OSISAF data.".

Lines 342-343: Do the authors have any thoughts on if the forecasted convergence rates near the coast would be improved if the model included fastice processes?

Response:

Thanks for your suggestion. The statement of “Meanwhile, we realize that improvements on sea ice convergence rate forecasts may be achieved if we introduce a landfast ice parameterization into the SOIPS, which has been considered as one of future directions of model development.” has been added into the revised manuscript.

Lines 358-363: I still think more needs to be done to show if this model can do a better (or at least similar) job compared to those other forecast systems.

Response:

We briefly compare our forecasts to the MOI product, and this part is presented in the supplementary material.

Lines 380-381: The zenodo link to SOIPS (https://doi.org/10.5281/zenodo.10457661) did not work.

Response:

The link has been updated to https://zenodo.org/records/11381604.

Technical corrections

Again, this is not a complete list and there are many minor grammatical errors that should be cleaned up in the next version.

Abstract lines 18, 19, and 20 and many other places: Suggest changing "leading time" to "lead time".

Response:

All revised.

Line 36: "Grahams Land" should be "Graham Land".

Response:

Revised.

Line 83: Suggest changing "promise capacity" to "promise" or "capacity".

Response:

Revised to “capacity”.

Line 124: Should "5-order" be "5th-order"?

Response:

Revised to “5th-order”.

Line 195: Suggest changing "just a number of sea ice extent" to "just a sea ice extent number".

Response:

Revised.

Line 277: Suggest changing "In contrary" to "In contrast".

Response:

Revised.