Response to the specific comments and corrections of the reviewers

(Italic: comment of reviewer, bold: our reply)

Reviewer 1:

Dear Yoshi

Using the MITgcm, this study presents a regional ocean-sea ice-ice shelf configuration of the Amundsen and Bellingshausen Seas, downscaled from the ECCO-LLC270 global state estimate. The authors provide an extensive evaluation of the model's performance in simulating sea ice, ocean hydrography, and ice-shelf basal melting, and demonstrate its utility through passive tracer experiments, sensitivity simulations, and Lagrangian particle tracking. The model outputs and configuration are made publicly available, supporting reproducibility and multidisciplinary use.

I am very pleased to have the opportunity to review this study. The authors have already made an important contribution to the field. This is a well-written and timely contribution that addresses a critical need in the Antarctic modeling community: a standardized, well-evaluated, and accessible regional model configuration. The manuscript is thorough in its evaluation, transparent in its methodology, and forward-looking in its emphasis on community use. The inclusion of multiple evaluation datasets, interannual comparisons, and process-oriented applications strengthens the paper significantly. The authors also clearly outline model limitations, which is commendable.

I have also provided a few suggestions, including some general and specific comments, with the hope of improving clarity, completeness, and alignment with the standards of EGUsphere.

Overall, this study is certainly worth publishing in Geoscientific Model Development after some minor revisions.

Cheers

Chengyan Liu

Thank you very much for your encouraging comments.

General Comments:

Lines 68-72: More clarification of the surface forcing might be helpful.

I am not sure if I correctly understood the application of the surface forcing using the output of a coupled ocean-sea ice model (ECCO-LLC270). Do you mean that you use the output file EXF (e.g., EXFtaux, EXFtauy, EXFatemp, and so on) from the ECCO-LLC270 to force your model? If so, why not directly use the same atmospheric forcing as in the ECCO-LLC270 rather than the output of ECCO-LLC270? I also cannot fully understand the strategy of the surface forcing in the extension period (2018 and onward). Does it mean that the forcing is a sum of the ERA5 and the climatological seasonal variability of ECCO-LLC270? If so, the surfacing forcing seems to be artificially amplified. I may have misunderstood this part, and I would appreciate further clarification very much.*

To clarify your questions, we will include the following sentences in the revised manuscript

"Although our atmospheric forcing may appear somewhat complex, it can be regarded as being very similar to the 6-hourly ERA-Interim reanalysis and ERA5 reanalysis products, with only minor adjustments applied. These adjustments, calculated within the ECCO framework, are small (see Fig. 2 in Zhang et al., 2018) and do not substantially affect the simulated output."

Lines 73-74: It would be very nice if the open boundary conditions could be shown; e.g., you can add some figures of open boundary conditions of potential temperature, salinity, and velocity in the supplement.

We will include supplemtary figures for open boundary conditions.

Lines 200-201: Tables 2-3 compared the maximum potential temperature and salinity rather than the average over a specified domain. I think it is very important to mention this point when you first introduce the evaluations based on Tables 2-3. Yes, you mention this at lines

216-217, yet it would be nice if these descriptions could be clarified early.
We will revise this text as suggested to emphasize that we compare maximum potential temperature and salinity.

"The simulated mCDW exhibits a maximum potential temperature bias of +0.15 °C, while the salinity shows no difference greater than 0.01, as maximum potential temperature and salinity are summarized and compared in Table 2."

Minor comments:
Lines 78-79: Just want to express thanks for introducing different transfer coefficients. It is really useful!
Thank you.

Line 140: 'February: r=0.39, p<0.05;' may not be appropriately considered 'a significant correlation'. It is nice to see the quite good correlation of the sea ice between the simulation and observations, and then we can just admit that the model is still deficient in the simulated sea ice in winter. I also believe that it is really difficult.
We will revise your text following your suggestions.

Line 141: 'to reproduce the interannual variations of sea ice.' can be revised as 'to reproduce the interannual variations of sea ice in the austral winter.'
We will revise the text as suggested.

Line 162: 'but their areas are quite smaller'; should it be 'larger'? The red lines in Figs. 4e-f are larger than the blue lines.
Thanks for spotting this. We will revise the manuscript as suggested.

Line 182: The halocline also deepens westward. Do you think that you can also mention this here?
We will revise the text as suggested.

Line 260: Fig. 8 is present later than Figs. 9-10, and thereby it would be nice to rearrange the figure number.

Figs. 6-8 and Figs. 9-10 are actually introduced earlier in the text as in Line 183. "Here, we compare simulated ocean hydrography with ship-based (Figs. 6-8) and mooring (Figs. 9-10) observations across the AS and BS continental shelves (Fig. 1)."

Line 313: It may be clear to be rephrased as 'increases by 37% (respectively decreases by 43%)' to coincide with 'heat and salt transfer coefficients of Pine Island and Thwaites ice shelves by 2 and 0.5'

We will revise the text as suggested.

Line 358: Since the particles in Fig. 17 have different ages, I think that these particles are not released at the same time. So, it would be nice to mention the frequency of the deployment of these particles.

We will clarify text and figures to clarify this point.

It would be helpful to include the seafloor depth in Fig. 14.

We will revise the figure as suggested.

Reviewer 2:

General comments:

In this study, the authors present a 2-4 km horizontal resolution regional MITgcm ocean circulation model configuration, with dynamic sea ice and ice shelves, for the Amundsen and Bellingshausen Seas. The full model configuration (code and forcing files) is made available by the authors and the purpose of this paper is to show how the model could be useful to the scientific community studying this area. Results from a model run from 1992 through 2025 are presented and several sea ice (area, thickness, ice production in polynyas), hydrographic (temperature and salinity sections across the continental shelf, time series of temperature in five locations across the eastern Amundsen Sea), and ice shelf (basal melt) quantities are compared against observations to show how faithfully the model simulates the real world. Examples are given of how the model can be used to study the ocean/ice physics in this area, including: an example of a sensitivity test (model results when specific ice shelf/ocean transfer quantities are changed), examples of passive tracers (surface restoring, circumpolar deep water, ice shelf basal melt), and examples of forward and backward Lagrangian particle tracking.

I think making the model setup available for researchers, and creating this manuscript to explain how the model can be useful, is a wonderful thing and am very happy the authors decided to do this. The applications section will be very helpful to potential users (e.g. I have seen passive tracers and forward trajectories presented several times in this kind of work, but I love the addition of using backwards in time trajectories). I believe this study is worth the attention of GMD because of how useful the model setup could be for the community. The manuscript was generally clear (a few minor questions are below) and easy to understand.

Thank you very much for your encouraging comments.

I do have a few suggestions that I think will help make this study a bit more helpful as a guide for potential future users of this model.

Thank you.

The biggest mismatch between observations and results that I see is the fact that the winter water layer (at least in the Amundsen) is generally too shallow. Maybe I missed it, but do the authors have any thoughts on how this might impact other aspects of the simulation discussed here (e.g. could this be related to underestimation of summer time ice)?

In fact, this is our first attempt to conduct such a comprehensive model evaluation, and we identify a systematic bias in which the thermocline depth is generally too shallow. Although this bias is likely important, particularly in the western Amundsen Sea, where mCDW intrusions may be substantially thinner than simulated, we would like to avoid adding speculative interpretations in the manuscript. Instead, in the revised version, we will include a statement in the conclusions that highlights this issue and briefly discusses its potential implications and underlying causes, leaving a more detailed investigation to future work.

"A key discrepancy we found is that the Winter Water layer is too thin and the thermocline too shallow across much of the domain. These biases likely reflect

biased salinity stratification, particularly controlled by Ekman convergence/divergence, sea-ice formation, and surface atmospheric forcing, that have not been fully evaluated and will be a focus of future model improvements."

Also, the same lead author recently (2024) published a paper in GMD showing a similar issue with the thermocline/halocline depth for the ECCO-LLC270 state estimate over the Amundsen continental shelf. This run matches observations better over the continental shelf than the global run, but since this run is a downscaled version of the global LLC270 run (this uses the LLC270 modified atmospheric forcing and the LLC270 run as lateral boundary conditions), do the authors have any thoughts for potential users if the forcing is the issue and are the authors considering modifying the surface forcing (maybe a regional state estimate) in the future?

At present, we are unable to identify the physical mechanisms responsible for the underestimated Winter Water thickness. Addressing this issue appears to be a more critical next step than pursuing further development of techniques to adjust atmospheric forcing. We will include a sentence in the conclusions outlining directions for future improvements.

I have some other specific comments and suggestions below, but all of these are minor and should be easily dealt with by the authors. Again, I am really happy the authors are making their model available, but I do think this manuscript needs a little bit of work before it will be fully effective as a guide for possible users.

Thank you very much!!

Specific comments:
Abstract: Do the authors think it would be helpful to the reader to add the dates of the simulation shown here to the abstract?
Done.

Line 63: I know the authors are aware of this, but I think it would be helpful to potential users who do not know this area quite as well to mention that this resolution (~ 2-4 km) is *not quite mesoscale eddy resolving on the continental shelf (and provide a reference or two).*

We will include the following sentence in the revised manuscript.

"Similar to \cite{Haigh2023}, we expect that this lateral resolution may not be sufficient to resolve all mesoscale eddies on the continental shelf \cite{Stewart2015}, but we do not expect a significant impact on our simulated hydrography."

Lines 71-72: Are there any published references about the ECCO-LLC270 extension?

Unfortunately, no reference available.

Line 141 and Figures 3b-c: I agree that this shows "the reliable ability to reproduce the interannual variations of sea ice" for the changes in maximum extent, but not sure I agree about the summer minimum.

We will revise this text about summer minimum.

Section 3.1.2: Is it worthwhile to do some simple quantitative comparison (e.g. RMSE) of the sea ice thickness?

We will add some simple quantitative comparisons.

Lines 197-198 and Figures 6c-d: Is there a bias in the surface salinity between the model and observations? It is hard for me to tell from the color contours in the figure.

We will conduct additional model comparison regarding this point.

Lines 212-213 and Figure S1: I can't tell if there is a difference between the two years in the observations, but the model looks to have a stronger deep water intrusion in 2012 than 2007 when examining the difference in deep temperature near, and especially inside, the Dotson cavity.

We will revise the text as suggested.

Mooring observations section: These moorings are all from the Eastern Trough in the Amundsen. Have the authors looked at moorings anywhere else in the model domain (e.g. the mooring in the Dotson Trough from 2010-2016 analyzed in Dotto et al. 2020)?

We will include additional comparisons for moorings in Dotson Trough.

Lines 280-281: If the authors have not done so already, the quarterly ice shelf basal melt rate estimates from Paolo et al. 2024 (https://nsidc.org/data/nsidc-0792/versions/1) might be helpful for looking at basal melt variability.

We will conduct model comparison with Paolo et al., 2024.

Lines 331-334: Do the authors have any thoughts on how the fact that the model thermocline and halocline tend to be too shallow on the Amundsen shelf impacts this penetration?

We would not like to speculate too much, but we will add some text about this point.

Lines 364-365, Fig. 18, Supplementary Movies 4-6: If the particles are supposed to track ice shelf meltwater, they why aren't they released in the grid cells just underneath the ice shelf where the meltwater is fluxed into the ocean? If the reason is because Octopus does not currently deal well with advection under the ice shelf, then I think this is something important for a potential user of the model to know.

This was just an arbitrary choice, and we can release particles in the ice shelf cavities. We will clarify this in the text or redo analyses from ice shelf cavities.

Lines 367-371, Fig. 19, Supplementary Movies 7-8: These backwards trajectories are really helpful, but similar question as to above in that I wonder why the particles aren't released at depth inside the ice shelf cavity?

This was just an arbitrary choice, and we can release particles in the ice shelf cavities. We will clarify this in the text or redo analyses from ice shelf cavities.

Technical corrections:

Lines 77-79: Need to specify that the actual values given (0.25e-4 and 0.5e-4) are just the heat, and not salt, transfer coefficients.

Done.

Lines 158-161 and Table 1: Over what time periods are the observed and modeled sea ice productions computed? Is it the same period (2003-2017) as in Figure 4? Is it just winter

(JAS) of those years (like the figure legend in Figures 2m-n) or over more of the year?
We will clarify this information in the revised manuscript.

Lines 256-257: It depends on your definition of the Bellingshausen Sea, but there have been moorings near the shelf break due west of Marguerite Bay that would be inside the model domain and some would say were in the Bellingshausen. Suggest being more specific (e.g. Bellingshausen Sea west of 74W).
Done.

Line 262: Should "(Figs. 8 and S5)" be "(Figs. 8 and S4)"? If so, suggest putting "(Figs. 8 and S5)" after "Belgica and Latady troughs".
We will revise the text as suggested.

Line 322: Typo: "much stable" should be "much more stable".
Done.

Figure 1 caption: "hovmeller" should be "Hovmöller".
Done.

Figure 8: I know a reader can figure it out by looking at the longitude range on the figure x-axes, but I think it should be explicitly mentioned in the figure caption which one of these sections is closer to the shelf break and which one is closer to the coast.
We will revise the figure to make it clearer.

Figure 10 caption: "Same as Figure 11" should be "Same as Figure 9".
Done.