Anonymous Referee #1
General comments

I thank the authors for all their efforts in response to my previous comments, especially the new control run with no data assimilation and the new persistence forecast. I know it was a fair bit of work, but I do think those two runs will be quite helpful in enabling the audience to judge the utility of the data assimilation in this forecast system.

I still think a little more could be done to compare this system to other ice forecasts systems. The additional comparisons to the MOI product in the Supplement are great, but there is no mention in the main text of what these show (just a statement that comparisons are made in the Supplement). I think a simple sentence or two of what Figures S4 and S5 show could be added to the main text. With respect to GIOPS, I understand that the data could not be downloaded, but I wasn't expecting the authors to download the data and make new figures. Smith et al. (2016) already include some of the same comparisons for the Southern Hemisphere as done here (sea ice concentration RMSE as a function of lead time, 168 hr lead time ice concentration RMSE as a function of time of year) that could just be mentioned in the this text.

There still are several minor awkward usages or subject/verb agreement mistakes. Again, I am not going to explicitly comment on most of them, and they still do not make the manuscript more difficult to understand, but I really think some effort should be put into cleaning them up.

I have a few new specific comments and suggestions below, but all of these are minor and should be easily dealt with by the authors. I still think some minor revision is necessary before publication, but I also think this is close to being a good guide for readers into how well SOIPS works (pretty well in my estimation) as a forecast system for sea ice in the Southern Ocean.

Response:
Dear reviewer, thanks a lot for your time and valuable comments on this manuscript. We have added several sentences into the main text to show the comparison among the SOIPS forecasts, the MOI product and the GIOPS forecasts.

"Additionally as a reference, the sea ice concentration RMSE of the GOIPS forecasts at lead time of 168-hour maintains below 0.35 in the year of 2011 with respect to the Interactive Multisensor Snow and Ice Mapping System ice extent product (Helfrich et al., 2007). With respect to the OSISAF data, the sea ice concentration RMSE of the SOIPS forecasts at lead time of 24-hour is larger than that of the MOI product. It should be mentioned that the MOI product has assimilated the OSISAF sea ice concentration data, which leads to a lower RMSE in comparison to the SOIPS forecasts (Figure S4 in the supplementary material)." **(L: 414–419)**

We have also proofread the manuscript. Our replies to your comments and suggestions are as follows.

Specific comments

Line 62: I still think the fact that this regional model has about the same resolution as some of the global models should be explicitly mentioned. Perhaps changing "coupled models with lower computational cost" to something like "coupled models at a similar resolution, but with lower computational cost".

Response: Revised. **(L: 64)**

Line 99: Suggest adding "for vertical mixing" after "K-profile parameterization".

Response: Revised. **(L: 101)**

Lines 107-110: I don't understand what the authors mean by "a boundary layer between the ice shelf and ocean is formed". Are they referring to the oceanic boundary layer underneath the ice/ocean interface where the turbulent transfer is parameterized by the three equation formulation (Losch 2008, Holland and Jenkins 1999)? Also, since this paper is focusing more on sea ice outside the ice shelf cavities, is the sentence about partial cells necessary?

Response: We revised the sentence to "An oceanic boundary layer underneath the ice-shelf/ocean interface is formed following three physical constraints: the interface must be at the freezing point and both heat and salt must be conserved at the interface (Holland and Jenkins, 1999)". We deleted the sentence about partial cells. **(L: 110–111)**

Lines 126-129: What does the simulated ice shelf basal melt look like? I did not see it mentioned in Zhao et al. (2023).

Response: Zhao et al. (2023) did not provide any information on ice shelf basal melt. Figure 1 shows the modeled ice shelf basal melting rate during 2003–2020 derived from the data used in Zhao et al. (2023). The ice shelf basal melting rate is larger in the western Antarctic than that in the eastern Antarctic.

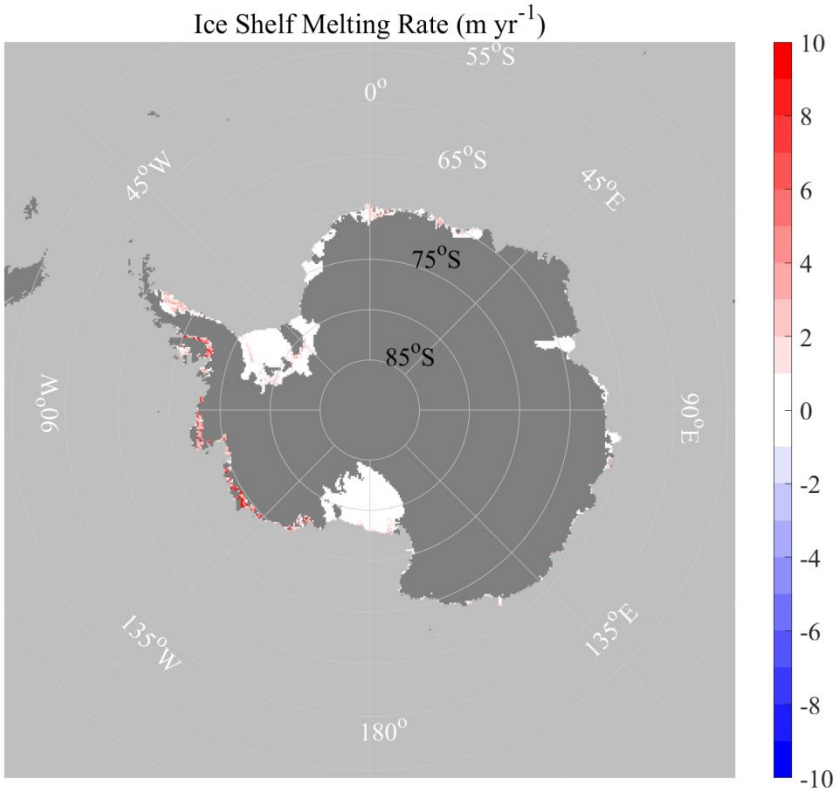

Figure 1. Ice shelf basal melting rate during 2003–2020 in the model free run.

Line 138: Do the authors have any references for studies that show LESTKF is suitable as mentioned here?

Response: We cited "(Vetra-Carvalho et al., 2018)". **(L: 141)**

Lines 162-163: I just wanted to thank the authors again for conducting the NoDA_Forecast and PE_Forecast runs.

Response: Thanks for your suggestion on these two runs. Your suggestions substantially improve the quality of this manuscript.

Line 250 and Figure 5: Why are the 24 hour and 168 hour NODA_Forecast IIEEs almost exactly the same?

Response: In the NoDA_Forecast run, the sea ice and ocean are strongly coupled, and the ice-ocean state evolution is uninterrupted, thus the difference between the 24h and 168h IIEEs is small. In the DA_Forecast run, we assimilate sea ice concentration observations to update the modeled sea ice concentration and thickness, but the top-layer ocean temperature is not updated. For example, we assume a case that the data assimilation step eliminates sea ice in a grid, but the top-layer ocean temperature of the grid is still close to freezing point, which leads to fast formation of sea ice in the following numerical integration steps. The data-assimilation-induced decoupled

sea ice and ocean states produce the obvious difference between the 24h and 168h IIEEs in the DA_Forecast run. Since we have already developed sea ice concentration-sea ice thickness-SST data assimilation scheme in the ArcIOPS system (Liang et al., 2019), the above-mentioned decoupled problem in the SOIPS will be solved in future work.

Line 295-297: Could the positive thickness bias in the western Ross Sea be a dynamic effect where the sea ice growth in that region is actually OK, but not enough ice is being advected out of that region? Figure 10 shows the magnitude bias in the Ross Sea sea ice drift is negative almost every month. Have the authors compared satellite-based estimates of the ice production and/or transport in the area (e.g. Drucker et al., 2011; Nihashi et al., 2017; Tian et al., 2020) with their estimates?

Response: We did not validate the modeled ice volume seasonal evolution due to the lack of satellite sea ice thickness observations with high spatial-temporal coverage in the Southern Ocean. In Zhao et al. (2023), we compared the modeled ice area seasonal evolution with the AMSR family data, and the result shows that the modeled sea ice area evolution is generally in line with the observations in the Ross Sea sector (Figure 2). In the model physics, sea ice dynamic equation determines that positive ice thickness bias is always accompanied with negative ice drift speed. The biases in ice thickness and ice drift speed forecasts could be reduced by assimilating sea ice thickness observations in future work.

We revised the sentence to "Satellite observations of sea ice concentration, thickness and drift have been used to estimate sea ice production and transport in the Antarctic coastal polynyas (Drucker et al., 2011; Nihashi et al., 2017; Tian et al., 2020). However, due to the relative scarce coverage of satellite observations in the Antarctic, especially in sea ice thickness, the evaluation of the SOIPS sea ice forecasts in this work still has considerable uncertainties". **(L: 438–441)**

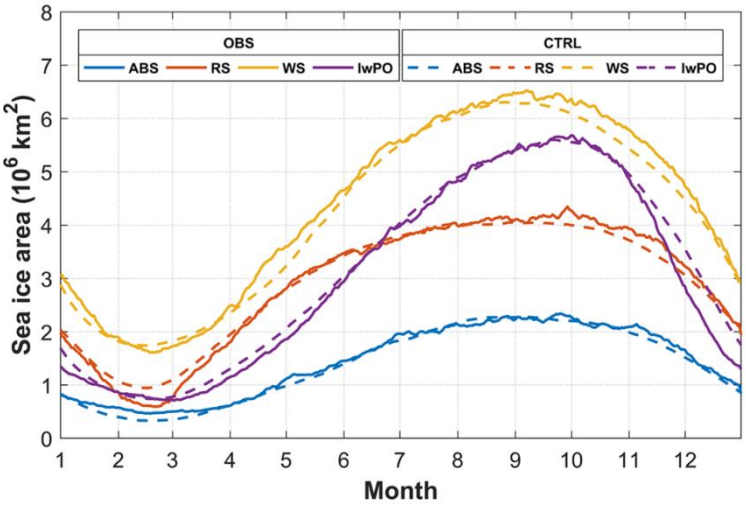

Figure 2. The 2003–2020 mean annual cycle of the regional sea ice area in 10^6 km^2. The solid and

dashed lines denote the observations and the model free run. The blue, orange, yellow, and purple lines denote the Amundsen-Bellingshausen Seas (ABS) sector, the Ross Sea (RS) sector, the Weddell Sea (WS) sector, and the Indian-Western Pacific Oceans (IwPO) sector, respectively. The observations are derived from the AMSR family sea ice concentration data between 2003 and 2020. (This figure is the same to Figure 3b in Zhao et al., 2023)

Lines 348-349: In January-March, the drift magnitude bias (Fig. 10) appears to be relatively small compared to the other months, not large.

Response: We revised the sentence to "During January–March, the Antarctic sea ice zone shrinks to its annual minima, sea ice drift magnitude bias appears to be relatively small compared to the other months". **(L: 353–354)**

Technical corrections

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

Line 17: Suggest changing "the OSISAF data" to "non-assimilated OSISAF data".

Response: Revised.

Line 32: Suggest changing "safety maritime navigation" to "safe maritime navigation".

Response: Revised.

Line 123: "have been successfully" should be "has been successfully".

Response: Revised.

Line 214: Suggest changing "forecasts mainly locate" to "forecasts are mainly located"

Response: Revised.

Line 218: "that in some nearshore areas" should be "in some nearshore areas".

Response: Revised.

Line 254: Suggest changing "biases locate" to "biases are located".

Response: Revised.

Line 281: Suggest changing “changes relatively small in” to “changes a relatively small amount in”.

Response: Revised.

Line 289: Suggest changing “locating” to “located”.

Response: Revised.

Lines 334-335: Suggest changing “Along with the prolong of the forecast lead time” to “As the forecast lead time increases”

Response: Revised.

Line 344: Suggest adding “A” before “Previous study”.

Response: Revised.

Line 358: Suggest changing “locates at” to “is located at”.

Response: Revised.

Line 394: “find that the SOIPS” should be “finds that the SOIPS”.

Response: Revised.

Line 397: Suggest changing “mainly locate” to “are mainly located”.

Response: Revised.

Lines 421-423: I do not understand the point the authors are trying to make with this sentence. Is this related to the preceding sentence about needing to add landfast ice processes or the following sentence about static ice shelves?

Response: We deleted the sentence.

Line 430: I am not sure what the authors mean by “coastline sharp”.

Response: Revised to “coastline”.

Line 441: Should “dealing oceanic boundary” be “dealing with oceanic boundary”?

Response: Revised.

Anonymous Referee #2
Review comment for 'Southern Ocean Ice Prediction System version 1.0 (SOIPS v1.0): description of the system and evaluation of synoptic-scale sea ice forecasts' in Geoscientific Model Development
[Round 2]

The manuscript by Zhao et al improves significantly after some careful modification. Specifically, the authors compared the data assimilation (DA) experiment with the model free run (NoDA) to show the benefit of sea ice concentration DA in prediction. Also, comparison with the persistence forecast highlights the advantages of dynamical prediction, especially when the sea ice changes quickly. The authors also dig into the model's systematic error and use it to explain the model's deficiency in the onset-to-fast freezing period. I appreciate the author's effort in addressing my concern. I suggest the manuscript be published after some extra modifications.

Response:
Dear reviewer, thanks a lot for your time and valuable comments on this manuscript. We have revised the manuscript according to your precious comments. Our replies to your comments and suggestions are as follows.

Major comment:
1. As the revised manuscript includes some more content, e.g., a comparison with the model free run and the persistence forecast, the abstract needs to be updated accordingly.

Response: We added the statement of "The comparison among the persistence forecasts, the SOIPS forecasts with and without data assimilation further shows that both model physics and data assimilation scheme play important roles in reliable sea ice forecasts in the Southern Ocean." into the abstract. **(L: 23–25)**

2. According to Figure 9, the sea ice drift prediction improves a lot due to the assimilation of sea ice concentration. Could you explain the reason for it? Is this related to the improved sea ice thickness, the sea ice concentration, or both (Chapter 6.1.1 in Leppäranta M. The drift of sea ice[M]. Springer Science & Business Media, 2011)?

Response: We added the statement of "The improvement of sea ice drift forecasts originates in principle from the enhancement of the SOIPS forecasts on sea ice concentration and thickness induced by data assimilation of the observed sea ice concentration, since sea ice drift is impacted by both sea ice concentration and thickness (Leppäranta, 2011)." into the main text. **(L: 336–339)**

3. For Section 3.5, a specific case is shown to illustrate the skill of the system to predict the sea ice convergence and divergence. Please elaborate more on the source

of this skill. For example, does this skill appear in the NoDA experiment? If so, we can suspect that this skill originates largely from the atmosphere forcing.

Response: Figure 1 shows that this skill originates largely from the atmosphere forcing. We added the statement of "Further analysis shows that the forecasting skill of sea ice convergence originates largely from the precise atmosphere forcing rather than the effects of sea ice concentration data assimilation (not shown)" into the main text. **(L: 388–390)**

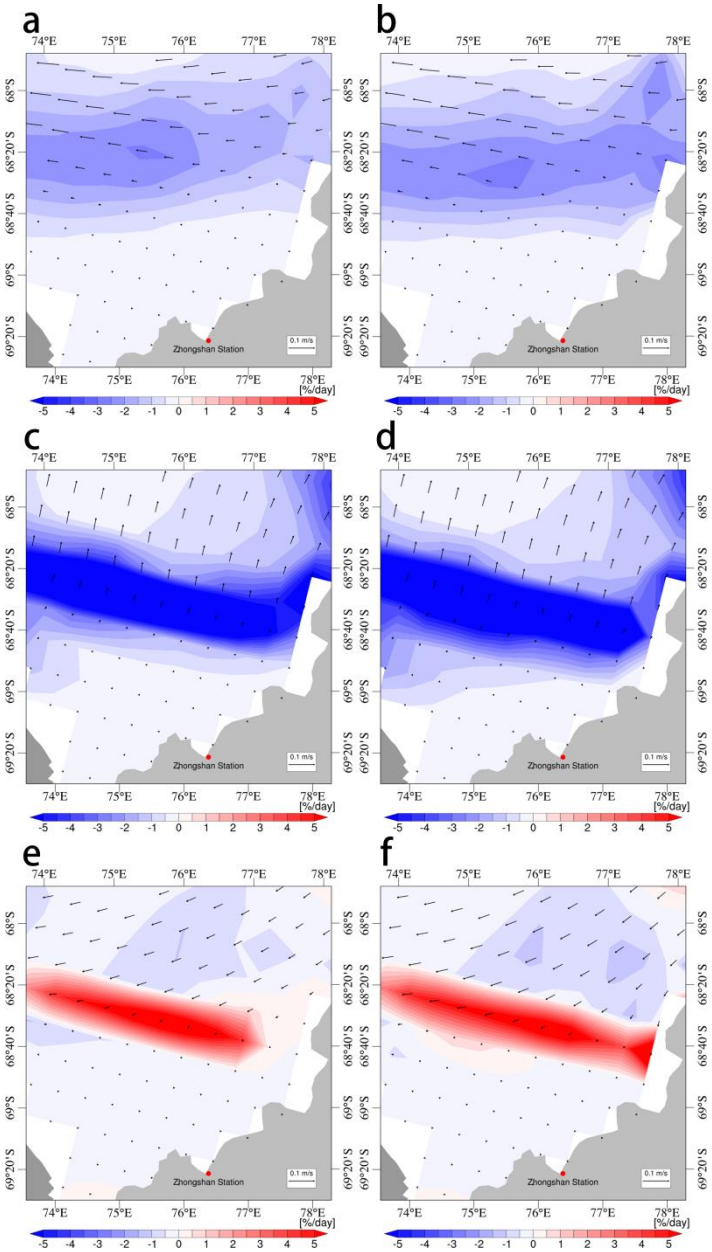

Figure 1. Sea ice convergence rate in the NoDA_Forecast (left column) and DA_Forecast (right column) runs. The top, middle, and bottom panels denote forecasts on November 19, 20, and 21, 2021, respectively. The forecasts were initialized on November 18, 2021. Black arrows denote sea ice drift vectors, while red and blue contours indicate that sea ice drift in the corresponding area tends to convergent and divergent, respectively. The red dot in each figure marks the Antarctic

China Zhongshan Station.

Minor comment:
Line 102, sever as.

Response: Revised.

Line 175, please elaborate on the purpose of comparing the SOIPS forecasts to the physical analysis field of the Antarctic Ocean produced by MOI. I didn't get the idea of this comparison.

Response: Another reviewer suggested that some intercomparisons between the SOIPS system and other ice forecasts systems are appreciated in the revised manuscript. We can not download operational sea ice forecasting products from the MOI and GIOPS. The MOI physical analysis field of the Antarctic Ocean can be accessed freely on the CMEMS website.

Line 265, by definition, the IIEE is the value of the error. The contour in Figure 6 should be the mismatch between these two data.

Response: Revised.