Review of “Impact of Satellite-Based Ice Surface Temperature Initialization on Arctic Winter Forecasts Using the Korean Integrated Model” by Kang et al.

This manuscript highlights the importance of improving the specification of sea ice surface temperature (IST) as a boundary condition in the polar regions for global NWP models, which have traditionally focused on performance in the tropics and mid-latitudes. To this end, the authors conducted an experiment in which the arbitrarily prescribed IST in the KIM model was replaced with a more realistic value to examine how this change affects the polar temperature bias patterns currently seen in KIM. However, the results ultimately indicate that, even with realistic IST, improvements in polar forecast skill are limited unless the model’s physical deficiencies are addressed. In this sense, the manuscript implies that better initial conditions alone are insufficient to enhance model performance in the Arctic without improvements to model physics.

Had a realistic IST been applied to an NWP model without physical deficiencies and led to improvement of model bias, it could have provided clear evidence for the importance of the initial boundary condition. However, since a model with known limitations was used and no significant improvement was observed using realistic IST, it is difficult to assess the role of the initial condition in this case. The authors conclude that improvements to the model’s physical processes are necessary in order to fully utilize realistic initial conditions. However, this claim is weakly supported by the current analysis and, as such, should be regarded as a diagnosis of the model rather than a confirmed finding. Moreover, while this may be controversial, data assimilation should be regarded as a means to improve model performance—not a justification for altering the model to accommodate more realistic observations. Doing so would, in effect, reverse the proper relationship between the model and the data assimilation process.

Therefore, I encourage the authors to consider revising the manuscript to incorporate the following critical points. As it stands, some of the conclusions are not fully supported by the analysis, and a major revision would be necessary before the manuscript can be considered for publication.

<u>General Comments:</u>

(major points)

1. The manuscript refers to “realistic IST,” but does not provide validation to demonstrate how realistic it actually is. Moreover, the algorithm used (Kang et al., 2021) was only validated using data up to 2015, which is significantly earlier than the experimental period of this study (2022). Considering the observational limitations in polar regions, some form of validation—such as comparisons with IR-based monthly means or other reference datasets—would be necessary to support the accuracy of the realistic IST used in this study.

2. Clarifying the role of microwave (MW) observations in the generation of the OSTIA dataset

would enhance the readers' understanding of the properties of the dataset and is therefore encouraged. Specifically, in the case of the "realistic IST," there appears to be a concern regarding the potential redundancy of MW data, as it is not clear whether MW observations were already assimilated into OSTIA and then additionally used in the current analysis.

3. Including a simple case study showing how the correction of a specific internal model deficiency influences the bias would provide concrete evidence to support the manuscript's claim about the need to improve the internal model processes in KIM.

(minor points)

1. Regarding lines 160–163 and Appendix A, it would be important to include further explanation for the assumed low absorption coefficient in the lower atmosphere. Furthermore, if the absorption coefficient were reversed (i.e., 0.1 in the upper atmosphere and 0.4 in the lower atmosphere), Scenario 1 would yield a Q_{net} of 5.9 W m^{-2} (HR value of 0.03), and Scenario 2 a Q_{net} of -54.46 W m^{-2} (HR value of -0.34), leading to a temperature change of approximately 0.37 K. While the values would be similar, this reversed setting appears to better explain the absolute magnitude of the shift described in Section 5.1, particularly when using absorption coefficient of 0.1 in the upper and 0.4 in the lower atmosphere.

2. To better support the discussion in lines 184–192, it would be beneficial to provide an additional figure presenting the vertical profile of the bias, which could aid in clarifying its distinct features.

3. In lines 199–206, it may be advisable to include a figure analogous to Figure 4, focused specifically on the latitudinal range of 83°N–90°N, to more clearly isolate and highlight the localized effect.

4. To enhance the interpretability of Figure 6, it is recommended to include a difference plot in Figure 3. Additionally, presenting the sea ice distribution at the corresponding time (e.g., SIC) would be helpful in explaining the occurrence of unrealistic discontinuities.

<u>Specific Comments:</u>

Line 74: "forecasting systems The remainder" → "forecasting systems. The remainder"
Line 91: "global, daily" → "global and daily"
Line 104: "background state The DA cycles" → "background state. The DA cycles"
Figure 2: This figure is not the same as Figure 1. Please rephrase this figure title.
Figure A1: This title is missing a period '.' at the end.