Review of “Enhanced Predictability of Antarctic Sea Ice through Sea Ice Thickness Assimilation” by Williams et al.

This manuscript presents a novel Antarctic sea ice data assimilation experiment, which assimilates the newly-available LEGOS sea ice thickness (SIT) observational product into the NorCPM model. The authors evaluate the reanalysis and seasonal prediction skill impacts of adding these new SIT observations. They find that seasonal predictions of regional Antarctic sea ice extent (SIE) and SIT are both improved by adding SIT assimilation, relative to the control experiment.

This manuscript addresses a critical topic for the Antarctic sea ice prediction community, which is the effective use of SIT observations for seasonal sea ice predictions. I appreciate the careful methodology employed in this study, including a sufficiently long hindcast period to derive statistically robust results. However, I do have some concerns about the manuscript in present form which I think should be addressed before the manuscript is published. My recommendation is Major Revisions.

Major Comments:

1) Observational sea ice thickness uncertainty

This study is based on the assimilation of LEGOS SIT observations, and shows improvements in SIE and SIT prediction skill associated with this SIT assimilation. A key question that remains unclear is the degree of observational uncertainty in the LEGOS SIT product, and the role that this uncertainty might play in the results presented. In particular, Figure 4 shows very large RMS differences between LEGOS SIT and IceSat-2 SIT and SMOS SIT. These RMSE values are even larger than the difference between the data assimilation experiments and SMOS/IceSat-2 SIT. The differences with SMOS are somewhat unsurprising given its known biases for thick ice, however, the differences relative to IceSat-2 are quite surprising. These differences are also notably larger than the uncertainty values provided in Bocquet et al. (2024). This raises the question: to what degree can LEGOS SIT be considered an observational “truth”? Some discussion on this point should be added throughout the manuscript, especially in sections 2.3, 4.1, and 5.

To better address this point, I suggest adding LEGOS SIV and also SIV based on IceSat-2 SIT to Figures 2 and 9. This would give a better sense of the observational uncertainty, and it would be helpful to see how the assimilation runs compare to these observational values.

Even if the LEGOS SIT product has systematic errors, I believe that seasonal prediction skill evaluation can serve as a useful and quasi-independent assessment of the value of this observational dataset. In particular, LEGOS SIT improves predictions of observed SIE, which suggests that there is certainly some meaningful information on SIV interannual variability that is being assimilated. I invite the authors to make this point somewhere in the Introduction or Discussion and Conclusions section.

2) Comparison to reference forecasts

It is encouraging to see the level of seasonal prediction skill for regional SIE and SIT. It would be useful to place these skill values in context by comparing the skill to an anomaly persistence or damped anomaly persistence forecast. I suggest adding this information into Figures 11 and 12. One way to do this would be to use upward facing triangles for ACC values that are statistically significant and exceed persistence skill, and downward facing triangles for values that are significant but have lower skill than persistence.

Minor Comments:

Title: "Predictability" is an inherent property of a dynamical system, and therefore assimilating SIT does not enhance the predictability. Rather, assimilating SIE enhances the prediction skill. Therefore, I suggest changing the title to “Enhanced Prediction Skill of Antarctic Sea Ice through Sea Ice Thickness Assimilation.”

Line 31: I would argue that at least three primary mechanisms on the seasonal time scale have been demonstrated in the literature: 1) OHC persistence, advection, and reemergence, 2) SIT persistence and advection, and 3) SIE persistence. I suggest rephrasing this sentence to make it more clear that multiple mechanisms exist. Also, I suggest adding a sentence or two on previous findings related to SIT as a source of predictability.

L111-112: Is there a reason why the CMIP5 forcings are still being used? It seems like a missed opportunity to not use the additional 9 years of historical forcing data (up to 2014) available from CMIP6.

L132-137: Figure 2 shows that NorESM is biased high in terms of sea ice extent in all months of the year. Does this bias affect the anomaly assimilation methodology? In other words, the observed anomalies may occur at a different spatial location than the model’s sea ice edge. If the model is biased high, this might lead to unrealistic amounts of SIC variability within the ice pack. Is there any indication of this type of behavior occurring in these experiments? I suggest adding a few lines discussing this issue.

L196: It would be helpful to quote some typical uncertainty values here, including any spatial and seasonal variations that exist.

L210: Please add the forcing used (I believe it is CMIP5 Historical+RCP8.5).

L212: There is no atmospheric observational constraint in these experiments, correct? Suggest mentioning this explicitly here, or in the paragraph above.

Figure 1: What motivated this definition of the Weddell Sea? The region is small compared to the typical definition considered (e.g. Holland et al., 2013). In particular, the location of the Weddell polynya near Maud rise lies well outside this domain.

Line 248: I assume that this should be 95%?

Figure 2: It appears that the values for extent and volume are both off by a factor of 10 here.

Figure 4: What spatial domain are the RMSE values computed over?

Lines 286-288: I agree that EXP-OCT provides a clear improvement over EXP-OC. However, these errors are still very large. The errors are roughly 2 times the observed values from SMOS, even over the thickness ranges where SMOS is considered reliable. Some comments should be added on this, making clear that this product is not really doing a very good job at capturing the SMOS thickness values.

Figure 5: Are these RMSE values for the climatological thickness, or RMSE for the time-varying thickness (which includes interannual variability)?

Lines 326-335: It would be helpful to place these IIEE values in context by including reference values from the IIEE of the observed SIC climatology in Figure 10.

Typos:

L88: “observational datasets”

L185: Remove “and” at the start of this sentence.

Please feel free to reach out if anything in the review is unclear.

Best Regards,
Mitch Bushuk