

Interactive comment on “Impact of assimilating a merged sea ice thickness from CryoSat-2 and SMOS in the Arctic reanalysis” by Jiping Xie et al.

Anonymous Referee #1

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General Comments

In this paper, a reanalysis with TOPAZ4 (HYCOM-ice) is performed with weekly updated CS2SMOS ice thickness fields assimilated into the modeling system using a Deterministic Ensemble Kalman Filter technique. The CS2SMOS data is updated weekly in non-summer months. The results are compared against the operational control run which does not assimilate this data. Assimilation of this data shows a reduction in the sea ice thickness bias (from 16 to 5 cm) and a 28% reduction in RMSD compared to the CS2SMOS data. Comparison against independent NASA Operation IceBridge data shows a 11% decrease in the RMSD compared to the control run. A significant improvement is shown for IMB 2013F which covers the entire period, while other buoys did not show much improvement. Ice drift speeds did not show any meaningful

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improvement when compared against IABP observations. Qualitatively, some improvement was shown with comparisons against OSI-SAF ice drift, especially in December 2014 for the Central Arctic. There was a noticeable improvement in ice volume for the period of December 2014-March 2015 compared against the CS2SMOS data in the “test” case. Finally, an analysis of the DFS contributions clearly showed the impact of assimilating the CS2SMOS data in the Central Arctic versus all other observations. Overall, the impact of the ice thickness assimilation is evident in the IceBridge and IMB 2013F comparisons. The ice drift analysis is disappointing (using IABP) where no improvement is demonstrated.

This is a well-written paper which shows the utility of assimilating a blended CryoSat-2/SMOS ice thickness product. I recommend publication with minor revisions. See comments below.

Specific Comments

Provide more information on the TOPAZ4 reanalysis. From an operational perspective, how often is the reanalysis performed or updated? Based on results presented in this paper, are their plans to adopt this technique to “re-run” the reanalysis, say from 2010 onward?

The authors have examined the impact of the merged CS2SMOS data into the TOPAZ4 system by examining 4 CRREL IMB and IceBridge data for 2014 and 2015. Please add an additional analysis of the model ice thickness versus the WHOI ULS data for the same period. See <http://www.whoi.edu/page.do?pid=137076> where ULS ice draft data is available at 3 locations (“A”, “B”, and “D” moorings). No additional model simulations should be required. This would complement the existing analysis presented in the paper.

Page 6: Is river discharge include in HYCOM? Mention the number of vertical levels in HYCOM used in this study.

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Page 6 line 159: I suggest adding the following HYCOM reference “Metzger et al., 2014)”

Metzger, E.J., O.M. Smedstad, P.G. Thoppil, H.E. Hurlburt, J.A. Cummings, A.J. Wallcraft, L. Zamudio, D.S. Franklin, P.G. Posey, M.W. Phelps, P.J. Hogan, F.L. Bub, and C.J. DeHaan. 2014. US Navy operational global ocean and Arctic ice prediction systems. *Oceanography*, 27(3):32–43, <http://dx.doi.org/10.5670/oceanog.2014.66>.

Page 6 line 171: Provide more information on how the two models are coupled. Which information is exchanged between the two models. How often does the coupling occur?

Page 7: Precipitation perturbation is discussed. How is snowfall addressed in the ice model used in this study? Do you take precip and convert to snowfall rate if Tair is at or below freezing?

Page 12 line 380-381: The paper states “the bias gradually decreases after the aforementioned spike and stabilizes close to zero in the end of 2014”. It is apparent that the bias is much reduced in the “test” run beginning in late November 2014. Please comment on why the bias for the “official” run is near zero by the end of the period.

Page 15 line 451: Here you use RMSE, while the rest of the paper you use RMSD. Be consistent throughout the paper.

Page 17 line 540: Why do you filter buoy trajectories with ice concentration > 0.9? Why such a high cutoff?

Page 18 line 554: Explain where you see a “clear advantage” to the OSI-SAF product wrt ice drift? I see some improvement in the Central Arctic in Dec 2014; but for Apr '14 and Jan '15 results look very similar. Also comment on how the ERA-Interim atmospheric forcing impacts your results. A 2 km/day shortfall is significant.

Figure 1: IMB locations are difficult to see. Can a portion of this figure be enlarged? (e.g., Fig. 1b).

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Figure 5: Please comment on why the model (for 2013F) is biased high beginning in January 2015 for either test case. The assimilation does not appear to have any impact here.

Technical Corrections

Page 3 line 73: replace “take” with “play”

Page 4 line 120: replace “tick” with “thick”

Page 8 line 224-225: reword to “Table 1 presents an overview of the assimilated observations utilized in the TOPAZ4-system.

Page 8 line 228: spell out OSTIA, OSI-SAF

Page 8 line 249: replace “carried out” to “performed”

Page 9 line 266: replace “means” with “represents”

Page 11 line 321: spell out OSE

Page 16 line 491: replace “Hunker” with “Hunke”

Page 17 line 526: delete “are” after improvements

Page 35: I can not distinguish between dotted and dashed line. I suggest you remove reference to both dotted and solid.

Page 41 line 1154: replace “test run (blue)” with “test run (red)”

Page 42: provide dates for the 3 weekly SIT plots

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