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# *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.

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We would like to thank the review for the constructive comments, and answer the points one by one as follows.

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 is RMSD compared to the CS2SMOS data. Comparison against independent NASA Operation



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Discussion paper

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 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?

-A: We have modified the sentence accordingly (P25, L810-812): "The reanalysis is currently reaching 2016 and extended by one year every year. In 2020, a new reanalysis will be provided with the upgraded version of TOPAZ5 which will include SIT assimilation from 2010 onwards."

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





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paper.

-A: We have included the new data in our validation (Section 3.3.2 in the revision). The main conclusion remains similar.

Fig. A Daily time series of SIT at three BGEP moorings (14A, 14B, and 14D, black lines) compared with the two model runs: Official (blue line) and Test (red line) and the weekly CS2SMOS satellite product (green line). The black line represents the daily average at the mooring location with the standard deviation shown as error bar. The RMSDs of the Free run, Test run and CS2SMOS (respectively) against the mooring data are indicated in each panel.

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

-A: The text was added:

P6, L168-170: “The model uses 28 hybrid layers with reference potential densities selected specifically for the North Atlantic and the Arctic regions (Sakov et al. 2012). “
P6, L175-178: “The model account for river discharge for which the seasonal climatology is estimated by feeding the run off estimates from ERA-interim (Dee et al., 2011) to the Total Runoff Integrating Pathways (TRIP, Oki and Sud, 1998) over the period 1989–2009.”

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.

-A: The relevant reference was added.

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?





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-A: The sea ice model is a subroutine of the ocean model. The coupling is done every 3 hours and exchanges ocean surface temperature, salinity, current velocity and ice-ocean stress, ice area concentration and thickness.

This has been added to P 6, L179-181: “A simple sea ice model using a one thickness category has been integrated at NERSC into HYCOM. As such, the sea ice and the ocean are coupled every 3 hours and exchange momentum, salt and heat on the ocean’s Arakawa C-grid.”

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? -A: We indeed consider precipitation to be snow, where ice is present, if air (or surface) temperature is below freezing. This has been added to the text P6 L182-183: “The sea ice thermodynamics, described in Drange and Simonsen (1996), treat the precipitations on ice as snow whenever the surface air temperature is below zero.”

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. -A: Yes, the bias is decreasing in the Official run at the end of 2014. The main reason is that the overestimation in the Beaufort Sea (see Fig. 5-7) balances the underestimation to the north of Canadian Arctic Archipelago and Greenland. We also indicate that the reduction of the bias in the TEST run is not due to the impact of assimilation as the same trend can be seen in the Official run.

To avoid confusion, the concerned statements are changed as P13, L413-418: “In Fig. 4 we can see that the pink line and the red line are evolving reasonably in phase but that the diagnosed error $\sigma$diag is much larger than the RMSD meaning that our system is overdispersive. The error budget shows that the observation error ($\sigma$o) is too large,





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suggesting that offset term in Eq. 4 is overestimated , which we do not expect as a serious problem as explained above."

Page 15 line 451: Here you use RMSE, while the rest of the paper you use RMSD. Be consistent throughout the paper. -A: RMSE has been replace by RMSD.

Pay 17 line 540: Why do you filter buoy trajectories with ice concentration > 0.9? Why such a high cutoff? -A: Yes, we agree that the cut off was large, it was originally motivated by the wish to focus on areas where the ice rheology is active. In the revision, we use 0.15 to replace 0.9 to filter these buoy trajectories for quality control, which does not affect the result.

This was changed as P19, L613-617: "Only trajectories longer than 30 days and reporting more than 5 times per day are used to estimate the daily drift speed of sea ice. To avoid buoys in open water, the observations are selected based on sea ice concentration (>0.15) and ice thickness (>5 cm) at the nearest model grid cell in both runs."

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 in significant. -A: In the whole Arctic, the monthly RMSDs for sea ice drift have a little reduction due to the assimilation of CS2SMOS. However, if we notice the fast drift area shown by the 3m isoline of the deviation in Fig.9, the overestimation patterns have been reduced and the regional RMSDs of ice drift around the North pole are reduced obviously (about 8-9%). We agree with the reviewer and we have soften this statement as P20, L626-633:

"The difference of drift distributions between the two runs is minor compared to the difference to the IABP data. Restricting the analysis to the area North of 80 degrees, the two runs show larger differences in SIT with a Test run about 30 cm thicker (Fig. 10d), the resulting difference in SID in that area is small (0.2 km d-1) and tends to





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degrade slightly the performance by slowing down the drift speed (Fig. 10c). This is somewhat contradictory to the analysis with OSI-SAF data which indicated a too fast model drift and smaller errors in the Test run"

Figure 1:IMB locations are difficult to see. Can a portion of this figure be enlarged? -A: This has been done.

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. -A: The model is initially biased high in both runs because of their common initial conditions. The differences are only taking place gradually as can be seen on Fig. 3. It should also be noted that the 2013F data is not assimilated but the CS2SMOS data may differ substantially from the buoy values.

Technical Corrections Page 3 line 73: replace "take" with "play" -A: Thanks, it is replaced.

Page 4 line 120: replace "tick" with "thick" -A: Thanks, it is corrected.

Page 8 line 224-225: reword to "Table 1 presents an overview of the assimilated observations utilized in the TOPAZ4-system. -A: We have removed the table according to the suggestion from another reviewer.

Page 8 line 228: spell out OSTIA, OSI-SAF -A: Thanks, it is replaced.

Page 8 line 249: replace "carried out" to "performed" -A: Thanks, it is corrected.

Page 9 line 266: replace "means" with "represents" -A: Thanks, it is corrected.

Page 11 line 321: spell out OSE -A: The acronym was already defined in page 5.

Page 16 line 491: replace "Hunker" with "Hunke" -A: Thank, it is corrected.

Page 17 line 526: delete "are" after improvements -A: Thanks, it is corrected.

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





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reference to both dotted and solid. -A: Thanks, we adjust this figure to easily distinguish the different lines.

Page 41 line 1154: replace “test run (blue)” with “test run (red)” -A: Thanks, it is corrected as the reference in Figure 11.

Page 42: provide dates for the 3 weekly SIT plots -A: Thanks, the first three weekly SITs are respectively stated in Figure 12.

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