

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 #2

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The authors address the impact of assimilating merged sea ice thickness data into an operational system in order to improve challenging and important forecasts of sea ice thickness in the Arctic Ocean. They use the CS2SMOS data for this purpose and perform one year (March 2014–March 2015) Observing System Experiment (OSE) with the TOPAZ system. The assimilation of CS2SMOS data improves the modelled sea ice thickness when compared to CS2SMOS and independent data and, to a less extent, the modelled sea ice drift. Compared again to CS2SMOS merged data, the total sea ice volume also is improved and, as expected, no improvement of sea ice extent but without noticeable degradation. A quantitative impact of the observational network with the Degrees of Freedom for Signal (DFS) approach shows the dominant source of information of the sea ice thickness in the central Arctic Ocean.

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The paper is comprehensive and shows the positive impact of assimilating the merged CS2SMOS ice thickness product in reanalysis mode. I recommend publication with minor revisions considering the points below, and especially parts of the work with in situ analysis.

Even if it has been already found by Mathiot et al. (2012) with a different approach and data, the authors should highlight more the important finding of improvement of the system (SIT and SIV) outside the observed period, e.g. in the abstract for instance. The impact analysis with independent data deserves the use of all available data. In situ sea ice thickness data from the “Unified sea Ice Thickness Climate Data Record” such as Air-Em data (http://psc.apl.uw.edu/sea_ice_cdr/Sources/airborne_em.html) are available within the time period of these experiments. This wouldn't need any re-run of these experiments and may reinforce assessments. The impact onto the sea ice drift is low but need better relative quantification. And comparisons with independent IABP drifting buoys deserve a better methodology, suggested below.

I'm not a native English-speaker but I think the English should be corrected in few parts mentioned below.

1 Introduction

Page 3 line 63: replace by “. . . and the use of reanalyses. . .”

Page 3 line 83: replace by “. . . with high accuracy the sea ice freeboard . . .”

Page 3 line 85: replace by “. . . because of approximations. . .”

The context in the introduction part should include the recent work made by Mu et al. (2018) using the same data in their LSEIK filter.

Page 4 line 114–115: add recent paper of Uotila et al. (2018) in their intercomparison of reanalysis including TOPAZ in polar oceans. Uotila et al. (2018): An assessment of ten ocean reanalysis in the polar regions, *Climate Dynamics*. Doi:10.1007/s00382-018-4242-z.

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Page 4 line 120: replace by "...too thick ..."

Page 5 line 123 : " ... and for reanalysis (Chevallier et al., 2016, Uotila et al., 2018) Chevallier et al. (2016) : Intercomparison of the Arctic sea ice cover in global ocean-sea ice reanalyses from the ORA-IP project, Climate Dynamics, doi: 10.1007/s00382-016-2985-y

Page 5 line 138: "strongly" really?

2 TOPAZ system descriptions and observations

Page 6 line 159: Chassignet

Page 6-7-8: the description of the assimilation system in TOPAZ in paragraph 2.2 could may be shortened by giving relevant references to Sakov and Oke (2008), Xie et al. (2017), Evensen (1994) for the Kalman Gain, etc ...

Page 8 line 224: Details of assimilated data in Table 1 doesn't give much information for the scope of this paper, you may remove it.

Page 8 line 225: "superobed"?

Page 8 line 231-232: sentence to be rephrased

Page 8 line 241: rephrase "... is on the low side".

Page 8, end of paragraph 2 and paragraph 3: this part should be more explained, the initial observed error used in the sensitivity assimilation experiment is the one from CS2SMOS? It seems that you use Desroziers to inflate observations errors but Desroziers could deflate initial errors; rephrase the methodology please. Authors attribute to SMOS the presence of discrepancies in Desroziers diagnostics in Figure 2, but what is the source of abrupt changes in Desroziers diagnostics above 3m thickness of sea ice?

Page 9: The estimated observation error (eq. (6)) is pretty large compared to initial

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CS2SMOS error, how this minimum threshold of 50cm has ben chosen, with short sensitivities tests?

3 OSE runs and validations

Page 10 line 296: "...is set to 0.1 m, ..."

Page 10 line 307: "... is the total number of time steps ..."

Figure 1: locations of IMB buoys are hardly detectable, use another colorbar ? How coastal areas are defined?

3.2 Validation against CS2SMOS and innovation diagnostics

Page 11: line 328: "... have all been improved" but how much is the improvement? Line 335: "... but is thickened in the Test run." But once again how much? The assimilation of CS2SMOS in the Test run definitely show improvements but Figure 3 doesn't help to know how much and where are these changes compared to CS2SMOS? 2D maps differences with CS2SMOS in Figure 3 (replacing mean values) will help to better quantify these regional changes and better understand the Figure 4.

Page 11 line 349: replace by "... is not significantly impacted by..."

Figure 3: Color bar of SIT is unreadable, Put row and columns titles instead of yellows labels which are hardly readable.

RMSE or RMSD?: RMSD or RMSE are used all along the paper (text and figures), please be consistent.

Considering the Figure 3 in March 2015, how do you explain in Figure 4 the bias in Official run vanishes with time? Error compensations?

Page 13 line 383: "The RMSE (RMSI?) stabilizes at a value close to 0.4m". From Figure 4, the RMSI, and total uncertainty, seem to grow with time and with the number of assimilated observations, how do you explain that?

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Page 12 lines 355-360: too long sentence, please rephrase it.

Page 12 line 364: "The innovation statistics"? Rephrase the entire sentence please.

Page 12 line 375: remove "Then"

Page 13 line 383: RMSI or RMSE?

3.3 Validation against independent SIT observations

From Figure 1 2013F and 2014B buoys seem to be located in the Canadian Basin (Beaufort Gyre), the fact that assimilating CS2SMOS improves the system in this area outside the observed period is an important finding.

Page 13 line 409: is the assimilated SIT really "pulled back" to the observations? Not clear.

Page 15 line 455: the large spread of scatterplots explains low values of R2 (give definition) in Figure 7 and then a weak significant linear regression, this should be commented.

Figure 6: the two bottom plots are nearly undistinguishable, replacing Test run plot by differences between Test and Official runs would be more helpful.

Figure 7: Lighten encapsulated text in the box and put it in the legend instead. Lines of linear regression are dotted or solid?

4.1 Impact on the sea ice drift

Addressing the impact of SIT assimilation onto the sea ice drift certainly is worthwhile and use of satellite measurements together with in situ data clearly assesses the results. But do we need such a long section by reminding classical equations such as the 2D momentum, total mass of ice and the conservative law. Please refer to adequate papers such as Hibler for example and shorten this section.

Figure 8: Idem Figure 3, color bars are hardly readable (use more ticks) and put row

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and columns titles instead of small black labels which also are hardly readable.

Figure 8: It is true that differences are pretty weak and could be found by only modifying the air-ice drag for instance. However, different ice thickness patterns could impact ice drift patterns, a plot showing Official and Test runs differences could highlight differences in large scale ice drift patterns. If patterns have no differences, just mention it.

Page 17 line 521: "... 2 days ..." what this refers to?

Page 17 line 523: insert OSI SAF data reference instead than the Table 1.

Page 15 line 527: how much this 0.2-0.3 km/day represent compared to the mean value (give a percentage for example)?

Page 18 lines 544-560: IABP essentially sample locations of important ice flows areas such as Transpolar Drift Stream and Beaufort Gyre; Sumata et al. (2014) for instance made intercomparisons with OSI SAF and IABP and found relative agreement among these products. It would have been more appropriate to collocate (in time & space) both OSI SAF and experiments into IABP space to evaluate experiments for IABP ice drift regimes. H. Sumata et al. (2014), An intercomparison of Arctic ice drift products to deduce uncertainty estimates, *J. Geoph. Res.*, 119, p. 4887-4921, doi: 10.1002/2013JC009724.

Figure 9: Meaning of "152/22329" in the top panel?

Page 18 lines 561-565: put these lines in the ice thickness validation context.

4.2 Impact on the sea ice extent and volume in the central Arctic

Page 20 lines 616-620: The spike end October-beginning November is related to SMOS measurements then? This Figure 11 should be more discussed in light of this event or removed.

Figure 10: to be corrected "... the test run (red-dotted)..."

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Figure 11: Put row and columns titles instead of small black labels which are hardly readable.

4.3 Quantitative impact for the observational network

The number of in situ data during these two months are pretty low, and more generally the years 2014 and 2015 have a pretty low number of CTD profiles compared to others years (see Behrens et al. 2018 <https://www.earth-syst-sci-data.net/10/1119/2018/essd-10-1119-2018.pdf> for instance). Given the relative importance of in situ when these data are present (Figs 12 & 13 c)), does it mean that with a more homogeneous in situ network the CS2SMOS won't be the major source of information in the central Arctic? The DFS is an indication of the impact of one assimilated observation in regards to the others. But these observations are usually complementary to each other and give different sources of information, e.g. sea ice vs water masses or surface vs vertical distribution.

Page 20 line 640: "cannot exceed"

Page 21 line 653: "...are dominant close to the sea ice edge ..."

5 Conclusions and discussions

We understand that the application of such developments is reanalysis and hindcasts experiments. According to 1) this merged data is not accessible in NRT (2) Cryosat-2 data is available in NRT (Tilling et al., 2016) (3) the recent work of Mu et al. (2018) (referenced in the paper) showing that "... the sea ice fields obtained by the joint assimilation of SMOS and CryoSat-2 data also have lower errors in thickness and concentration than those obtained from directly assimilating a statistically merged SMOS and CryoSat-2 sea ice thickness product." and 4) TOPAZ is the NRT Arctic Ocean operational system of CMEMS; it is surprising that assimilating CS-2 and SMOS together and separately is not a prospective of this work.

Page 23 line 737: "... which would reduce the IF of SIC" but also the IF of SIT because

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in situ and SIT are largely overlapped.

Page 24 line 757: "... as seen in Fig. 10".

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-101>, 2018.

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