

Interactive comment on “Benefits of assimilating thin sea-ice thickness from SMOS-Ice into the TOPAZ system” by J. Xie et al.

Anonymous Referee #1

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Review of “Benefits of Assimilating thin sea-ice thickness from SMOS-Ice into the TOPAZ System”, J. Xie, F. Counillon, L. Bertino, X. Tian-Kunze, and L. Kakeschke.

General Comments

This paper examines the assimilation of SMOS derived ice thickness into the TOPAZ modeling system. Assimilating ice thickness is a timely subject, and very much needed to help constrain model runs, especially short term forecast systems. Two modeling experiments were performed spanning Feb-Mar and Oct-Nov 2014, with and without SMOS thickness. Comparisons to daily SMOS thickness values are made. Also, a degree of freedom test is performed to investigate the impact SMOS ice thickness has on the model data.

Overall, I found this paper somewhat lacking in quality. There are several instances

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where terms are not defined and instances where the same mathematical symbol is used for different terms, making some of the equations confusing to follow. There are also many places where key details are omitted. Therefore, I recommend the paper be returned and re-submitted after major revisions.

Specific Comments

Page 3, line 22: “Measurements of thick sea ice draft. . .” should read “Measurements of thick sea ice freeboard. . .” as altimeters measure freeboard, not draft.

Page 5, line 2: the authors state the thickness of TOPAZ was validated over the period 1991-2013 using ICESat and IceBridge. ICESat was from 2003-2009, and IceBridge started after that. What data was used starting in 1991 to validate the model? They refer to an unpublished manuscript submitted by Xie (2016) several times. I find this troubling, as it is not peer-reviewed and cannot be referred to. It also does not say where it was submitted. I suggest a different reference be used throughout the manuscript.

Page 5, line 4, the authors state “While the spatial pattern and regression compare reasonably well, large biases exist” What regression and spatial pattern are they talking about? Are the biases positive or negative? After talking about TOPAZ validation, they state inaccuracy in the ice thickness is a drawback. . . More detail needs to be added to this section.

Page 5, line 31: Is the ice model a multi-category model or one layer? This is important because it will come into play when assimilating ice thickness.

Page 6, equation 3 and lines thereafter: The \hat{P} covariance is described, but not used. I’m not sure what \hat{P} is supposed to be. Also, on line 24 the authors state “the extra term is quadratic and positive.” What term is extra, the second term?

Page 7, line 8: Another reference to unpublished/unaccepted manuscript for validation. This publication needs to be accepted first, or include the details from that validation in this paper.

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Page 7, line 12: where are the SMOS-Ice products available? A reference or website should be included. Is it available in near real time for operational centers?

Page 7, line 15: What does “TOPAZ equivalent” mean? Is there some spatial averaging or processing to match the observation location for comparison?

Page 7, line 17: The term ‘RMSD’ is used, but not defined until Eq. (6) on page 9. RMSD should be defined here.

Page 8, line 16: This is the first time the term “innovations” is used. I presume this is referring to the 2nd term on the RHS of equation (1). This should be spelled out.

Page 8, equation (4): y_{smos} is not defined. I guess this is the SMOS observation thickness. Also, is sea ice volume assimilated into the sea ice model? Is there only one ice category in their model to assimilate volume? If more than one category, how do the authors decide what category to assimilate the thickness?

Page 8, line 31. The authors state they implement an upper limit on observation standard deviation of 5 meters. This seem like a large standard deviation value (12.5 times the max observation value of 0.4 m) given they are only assimilating SMOS observations up to 0.4 meters. Why was 5 meters chosen for the standard deviation limit? Are there SMOS observations with 0.5 meters with a 5 meter standard deviation? Figure 2 suggests standard deviations less than 2 for $SMOS < 0.4$ meters.

Page 8, line 25: why use the symbol TSLA? SLA is used for along-track sea level anomaly on page 4 and 6.

Page 9, line 10: Sea ice thickness SMOS-Ice is stated to be in Table 1, but it is not listed there.

Page 9, equations 5 and 6: What is ‘H’? It is used as Bilinear operator and Obs error in previous equations.

Page 9, line 20: I find it interesting that there are minimal observations in the Beaufort

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during March. Can the authors expand on why this is? Is the ice too thick for SMOS at this time? Or not there?

Page 10: line 15: The terminology “highlighted with marked lines” is confusing to me. I think it would be clearer if the authors state something like “the averaged thickness of thin sea-ice . . . are shown with marked lines in the panels of Figure 6”.

Page 11, equation (7): does ‘tr’ mean trace? This, as well as all terms, should be defined to ensure clarity.

Page 11, line 25: On this line I think the authors are referring to equation (7).

Page 11, equation (8): This looks more like a root mean square (RMS) than a mean. Is there a reason why the authors decided to use a RMS here? Also, what are the subscripts i and j ?

Page 12: equation (9), what is subscript j ? j 'th observation set?

Page 12: line 6: Is there a reference for the ice-tethered profile data? Is this ice or ocean profile data?

Page 13: line 30: I would say the blended sea ice thickness has been “tested with” the U.S. Navy Arctic Cap Nowcast/Forecast system. The term “implemented” implies the blending is currently being used operationally, which is not the case.

Figure 6: If I understand correctly, the blue line with triangles is the test run where ice thickness is assimilated once a week. There does not seem to be any evidence of the assimilation. I would expect the blue triangle line to get closer to the SMOS line at the assimilation interval. Why does this not occur?

Technical Corrections

In many places throughout the document (e.g., page 2 lines 13 and 18, page 3 line 16, page 4 line 19,) words are run together without proper spacing. I don't know if this is an artifact of the submission formatting or something else, but the word and sentence

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spacing needs to be verified.

Page 3, line 29: "Cryostat-2" should be "CryoSat2"

Page 4, line 13: Yang reference is (2015), but in bibliography it is 2014. Please check on the year of this publication.

Page 4, line 14: Define LSEIK.

Page 4, line 16: should read "This study is a follow up and assesses..."

Page 4, line 16: What is this a follow-up of? Yang (2015)?

Page 4, line 24: should read "... and does not apply post processing..."

Page 8 and after: no need to bold "official run" and "test run". I find this distracting in the rest of the document.

Page 12, line 25: need units after 0.4 (should read thinner than 0.4 m)

Page 12, line 32: Yang reference is (2015), but in bibliography it is 2014. Please check on the year of this publication.

Figure 1: the words of the regions are hard to see (especially Kara and Beaufort). I suggest putting a white background for these words.

Figure 3: Don't need the word "resp" when doing comparisons. Usually when comparisons are done in this manner you just state the contrasting item. The green (red) line represents the mean bias for March (November) of each year.

Figure 4: Bottom row. It is hard to see the orange line. I suggest choosing a separate color not in the colorbar. Same comment about "resp" as above. Say Top Row, Middle Row, Bottom Row.

Figure 5: The vertical axis changes on each plot. Please plot each item with the same vertical axis.

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Figures 6, 7, 8: same comment about "resp" and bold test run, official run.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-112, 2016.

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