Interactive comment on “Impact of assimilating sea ice concentration, sea ice thickness and snow depth in a coupled ocean-sea ice modeling system” by Sindre Fritzner et al.

Anonymous Referee #1

Received and published: 16 October 2018

Review on “Impact of assimilating sea ice concentration, sea ice thickness and snow depth in a coupled ocean-sea ice modeling system”, by Fritzner et al., submitted for publication in The Cryosphere Discussion.

General comments :

The paper shows the results of sea ice data assimilation experiments into a coupled ocean and sea ice model using an ensemble Kalman filter (EnKF). Sea ice concentration, sea ice thickness and snow depth are assimilated in different combinations and verifications are performed using assimilated and independent observations. The impact of assimilation is measured on the analysis, 7-day forecasts and 5-month seasonal forecasts. The paper is generally well written. The assimilation experiments and verifications are well designed. The assimilation of snow depth is particularly original as it has not been done in other studies, as far as I know. However, some aspects could be clarified.
We thank the reviewer for the kind words and the careful and constructive feedback.

For examples, the observation-error used for the assimilation could have been explicitly specified.
Information regarding the observation uncertainty for AMSR sea-ice concentration, SMOS sea-ice thickness and the snow depth product is now provided in the observations section.

In some cases it is not clear whether the verification has been done on the ensemble mean or on individual ensemble members (and then calculating and average).
Verification is always done on the ensemble mean. This has been clarified by writing ensemble mean instead of ensemble average when applicable.

Also the error of the ensemble mean and ensemble spread relationship could have been shown, as this is usually considered a requirement for an EnKF.
We agree that this would be an interesting result to show, but we think that for sea-ice this is less interesting than it likely is for other applications. We think it would be difficult to present in a useful manner in our case. In a normal EnKF system one would expect the ensemble spread to be of the same order as the ensemble mean error, but this is not the case here. To generate the ensemble spread we use perturbation amplitudes we find to be physically reasonable, we set this independent of model error. This creates an ensemble spread significantly lower than the model error. In addition, too large ensemble spread with only 20 ensemble members would give an assimilation result skewed towards the observations.

Specific comments :

1. A couple of sentences at the end of the abstract around line 15 are confusing to me. It seems that the conclusions about the assimilation of snow depth are contradictory: “. . . while the snow

observations have a positive effect on snow thickness and ice concentration. In our study, the seasonal forecast showed that assimilating snow depth lead to a worse estimation of sea-ice extent compared to the other assimilation systems, the other three gave similar results." How come the assimilation of snow depth have a positive effect on ice concentration but lead to a worse estimation of sea-ice extent ?

We agree that this is not well formulated. The point we make here is that there is a difference between long- and short-term effect of the assimilation. Where a positive effect is seen at the shorter timescales, immediately after assimilation and for the one-week forecasts. While for the seasonal forecast over several months there is a negative effect of assimilating the snow depth observations. The text has been updated for clarification: «It is found that the assimilation of ice thickness strongly improves ice concentration, ice thickness and snow depth, while the snow observations have a smaller but still positive short-term effect on snow thickness and ice concentration. In our study, the seasonal forecast showed that assimilating snow depth lead to a less accurate long-term estimation of sea-ice extent compared to the other assimilation systems, the other three gave similar results.»

2. In section 4.1, page 7: First Pb is defined as the background-error covariance matrix. A couple of lines later, it is referred as the model-error covariance matrix. I think you should stick to background-error covariance matrix because model-error covariance matrix is usually reserved for the errors accumulated during model integration.

This has been changed.

3. In section 4.3, it is mentioned that there is 5 thickness categories; I assume they are the partial concentrations for each thickness categories and that the total ice concentration can be calculated from them. Later it is mentioned that the assimilation can result into a positive SIC but no volume. Does that mean that the 5 thickness categories and the SIC are all independent analysis variables ?

The 5 partial SICs and the total SIC is 6 different parameters in the analysis, but they are not independent, the total SIC is only a sum of the 5 partial SICs. The model uses the 5 partial SICs, while the total SIC is a dummy parameter used for assimilation. When assimilating, the total SIC is the parameter corresponding to the observations, while the partial SICs are updated based on correlation and these are the ones used in the model afterwards.

If that is the case, wouldn't it be better to only have the 5 partial concentrations as analysis variable and calculate SIC ? It seems that it would avoid the problem of having positive SIC but no volume.

If we understand you correctly this is what is already done. What we mean is that we can have a partial SIC larger than one, but the corresponding partial SIT zero or less than zero. New text: where some areas have a positive partial SIC but the corresponding partial SIT is zero or less than zero.

4. In Figure2, what are the observation uncertainty of AMSR-E/2 ice concentration used in the calculation of the RMSE ? Are they included in the product and are they constant values or are they specified for each points ?

The AMSR observation uncertainty is included in the product and specified in each grid point. This is now specified in section 3: Observations. New text: «The AMSR-E/2 SIC observation product includes individual uncertainty estimates for all grid points. This uncertainty is based on the sum of algorithm uncertainty and smearing uncertainty. Where smearing uncertainty is related to the location of the observation compared to the grid.»

5. Figure 5: Over which year(s) ? Is it against IceBridge observations ? Also in the text under section 5.4, please specify what is "observed satellite snow depth", is this IceBridge ?

No, this is against the observed snow depth product used for assimilation. New caption: «RMSE of monthly averaged model SIT and snow depth averaged over all ensemble members for the years 2011-2013 calculated against the a) combined SMOS-Cryosat SIT product and b) observed snow-depth product. These are observations also used for assimilation.»

New text in section 5.4: «In Fig. \ref{fig:Snow_Thickness}b the RMSE of monthly averaged modelled snow depth over all ensembles validated against the observed satellite snow depth \citep{Rostosky_2018} used for assimilation is plotted.»

6. Page 16, line 5: “This lack of improvement can be an indication of a too simple snow component in our coupled system, only one snow layer is used.” I think that is pure speculation, unless the authors can show evidence to convince the readers. Could the reason be simply that there are large discrepancies between IceBridge and the assimilated snow depth products ? Same comment on page 21, line 25 and on page 22, line 30.
We agree that this is pure speculation and is mentioned as a suggestion to what might cause the lack of consistency between model and observations. As you mention there are large errors in the snow depth observation product too as compared to icebridge, and we agree this is a more likely reason for the large errors. We have changed the text to highlight this: «It is seen that within one grid cell, there are huge variations in the IceBridge snow observations. Such variations cannot be provided with a coarse resolution model. Hence large errors are found for the RMSE against IceBridge observations for experiments where the snow observation are assimilated, even though IceBridge is used to «tune» the assimilated product \citep{Rostosky_2018}. In addition, the snow component used in our coupled system is likely too simple, having only one snow layer, which may effect the snow cover accuracy.»

7. Table 3: It is hard to understand from the caption what are the numbers in the table. Is the averaged snow depth over a grid cell compared to the model ensemble mean ?
There was an error here, the ensemble mean is validated by observations averaged over all grid cells.

It would help rephrase the caption, maybe removing one of the 3 averages and using “ensemble mean”, if that is appropriate.
We agree, new caption: «The annual-mean RMSE of the ensemble-mean snow depth averaged over all grid cells. The five experiments and the snow-depth satellite observations are compared to the IceBridge airborne snow-depth observations.»

8. Figure 8: Are these monthly averages over the 3 years ? The caption is hard to read because the “seven day” and “forecast” are too far apart.
Yes, they are, new caption: «RMSE of monthly averaged (over three years) ensemble mean of seven-day forecast SIC validated against a) AMSR-E/2 SIC observations and b) OSISAF SIC observations.»

9. Figure 9: Please specify in the caption that (b) is the monthly averaged RMSE over the three years.
This is now corrected, new text: «The figures show RMSE of the ensemble mean SIC averaged over 3 years and verified against the assimilated OSISAF SIC.»

10. Figure 10: I think it is unrealistic to use re-analysed forcing for the seasonal forecasts, as the re-analysed forcing would not be available in an operational real time context.
Agreed, this is of course not very realistic, but more a simplification since the focus of this study is on the assimilation. Since we compare our assimilation results with a control run using the same reanalysed forcing we think that the comparison is fair.

Technical corrections :

Page 5, line 8: Change “. . .observations where given...” to “. . .observations were given...”
This is corrected

Caption of Figure 3 : Change “low concentration ice (> 50 %)” to “low concentration ice (< 50 %)”
This is corrected

Caption of Figure 4 : Change “the blue stars the OSISAF” to “the red stars the OSISAF”.
This is corrected

Caption of Figure 4 : For ice volume the units are km. Is it the volume per unit area ?
No, this is an error. The figures has been updated.

Caption of Figure 4 : It would be easier for the readers to mention that the x-labels are month-year.
Text added: The xlabel is given as month-year.

Page 14, line : Change “sea-ice extent being too large and the ice is too thick” to “sea-ice extent being too large and the ice being too thick”.
This is corrected

Page 18, line 18: “The figures show that that . . .”
This is corrected

Figure 10: Change “The blue line represents a forecast using a climatological forcing made from an over of atmospheric data for 2000-2014 with assimilation” to “The blue line represents a forecast using a climatological forcing made from atmospheric data over 2000-2014 with assimilation”
This is corrected

Page 21, line 31: Change “The main parameters analysed in this study snow depth, SIT and SIC all vary on longer time scales than one week for the spatial resolution in our model” to “The main parameters analysed in this study, snow depth, SIT and SIC, all vary on longer time scales than one week for the spatial resolution in our model”
This is corrected