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Interactive comment on "Seasonal sea ice predictions for the Arctic based on assimilation of remotely sensed observations" *by* F. Kauker et al.

Anonymous Referee #2

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Review overview: The paper needs substantial editing and better communication. There may be some useful work here, but it is difficult to tell. If readers do not understand the work, it will go to waste.

This paper investigates the utility of data assimilation strategies within the NOASIM ice/ocean model via experimental procedures.

Several model parameters are calibrated using observations form the period 1990-2008. Experiments are then performed for 2012-2014 where data is assimilated during March-April and the model is evaluated in September. CryoSat-2 ice thickness, ice concentration (OSI SAF), SST's (OSI SAF) and snow depth (Uni. Bremen) are assimilated at times. Using a "straightforward" assimilation strategy in March-April still produces biased results in September.

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"Reconstruction" runs are then performed, which aim to find March-April ice thicknesses that give good results in September. The ratio of CryoSat-2 ice thicknesses and those found in the "reconstruction" runs is then used as a form of "bias correction" in a final set of assimilation experiments. The final runs give better September results than the original ("straightforward") case, however, the March ice thickness used in this case seems to be unrealistically thick, with vast areas being +3.5m thick.

The overall concept of calibration, then assimilation and analysis is quite good. Unfortunately, it is difficult to learn much from this paper. The description does not effectively communicate exactly what was done, nor is there an analysis of why the assimilation failed to make expected improvements. The primary recommendation of this review is that the authors put themselves in the position of someone trying to replicate their experiments and edit their paper accordingly.

My impression after reading the paper is as follows: since the "straightforward assimilation" of realistic data did not produce a good result and the "bias correction" assimilation uses ice that is too thick, the NAOSIM model suffers from structural, parameter or input errors. Data assimilation can be used to highlight these issues (which is perhaps the most useful contribution made by this paper), but it should not be used as a crutch for trying to correct such systematic problems - that goes against the theory and underlying assumptions of optimal data assimilation.

It seems that a final conclusion could be that either: 1) the CryoSat-2 ice thickness data is incorrect thus adds no useful information or 2) the NAOSIM model has issues that need to be rectified before it can make a reliable forecast... Case (2) seems more likely, but one cannot say based on the information in this paper and it is up to the authors to demonstrate either case.

Some more specific (though not exhaustive) comments are given below.

Section 2.1 - How does the variational code deal with potential data inconsistencies? What is the error assigned to the model in the C(Xo) matrix? Results in Figure 5 would

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suggest that the model error is set to be very high as very little of the total cost function is from this term (Figure 7).

One suggestion would be to make a table with four columns: Assimilated data, State/control variables impacted by the data, Observational error, Model error. This would give the reader a better picture of what was used. Maybe even consider schematic diagrams to explain the assimilation experiments.

The M operator needs to be described. Does M treat all control variables independently or are there other matrix elements which allow information from one variable to propagate to others? In either case, how does the M operator impact the final result? For example, when ice thickness is NOT assimilated, but concentration is assimilated, what happens to thickness when concentration is increased or decreased? If new ice is added, what is its thickness? If concentration goes down does thickness stay the same?

Section 2.3 - The sea ice portion of the model is the part receiving assimilation so it would be good to give some information about it. How many ice layers are there? Are there any thickness categories per grid box? Is snow a bulk layer? How does the M(x) function map the model states to the observed quantities?

Only parameters related to dynamics are changed in the calibration; what was the reason for excluding the thermodynamic parameters? For example, albedo? The result of the calibration is that thickness is better, but extent is not - are you not just pushing the ice around at this point rather than removing it via a thermodynamic process?

"As this strong deviation is absent when forced with NCEP reanalysis this can be attributed to deficits in the CFSR surface forcing" - this goes against the vast majority of investigations which indicate that the NCEP forcing is rather poor. As one recent example, see Lindsay et al., 2014 which show that CFSR is superior to NCEP. My guess would be that the authors interpretation is not correct. Was the model was tuned for NCEP and only limited correction could be made for the CFSR forcing? In any case,

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better justification or explanation needs to be given for such a statement.

Section 3:

"the price we have to pay for more reliable ice margin is misfit to the CryoSat-2 ice thickness" - this statement suggests to me that there are some fundamental problems with either the model, the CryoSat data or the asimilation scheme. Ideally the rest of the paper would go on to investigate where the problem lies rather than trying to find a factor to compensate for this problem. It seems that the authors have side-stepped the real problem.

"prize" should be "price"

For the "straighforward" case, how often is an assimilation step performed? You are using daily and monthly average data - how are these items assimilated in the same system? My guess at the moment is that it is once a day for March 1 - April 30, but with a special step once a month where thickness is assimilated (and somehow the monthly averaging process is accounted for)? - but I could not tell from the paper. Where does the extra weight of 180 get applied? From the text, I cannot tell how this experiment is performed. It might be a good experiment for other model groups to try, but we can't tell.

For the Reconstruction runs: If ice concentration and thickness are not assimilated in March, how/why are the concentrations in these simulations so different from the control experiment? Where did these new concentrations and thicknesses (as suggested in Figure 9) come from? Are these initial thicknesses simply guesses? A better explanation is required.

If the model was well calibrated (as described in Section 2), why does it need further bias correction? The purpose of calibration is to remove biases (low frequency error) so that data assimilation can account the more random and higher frequency error (e.g. initial conditions error). This point should be reconciled either with reference to the model or the observations. A situation requiring double bias correction often points to a case of overfitting.

As noted before, ice thickness and concentration are related variables - it seems unlikely to have a concentration of 20% and 3m thick for a grid box. Updating one of these variables without consideration of the other can lead to physical conflicts and/or unlikely situations that would not happen in an "open loop" simulation. How is this accounted for?

Figures 7 & 9: To many readers the CostFunction might be an abstract quantity, particularly when it is not scaled for ice thickness, thus the Y-axis has no real meaning. These values might be better understood as percentage changes from the control? However, what readers would be most interested in (and comprehend the easiest) is if you were to transform the cost function back to quantities such as mean increment in ice thickness and snow depth etc.

A.G.S.

Interactive comment on The Cryosphere Discuss., 9, 5521, 2015.