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Interactive comment on "Estimation of sea ice parameters from sea ice model with assimilated ice concentration and SST" *by* Siva Prasad et al.

Anonymous Referee #1

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Summary

This paper describes the use of a basic OI/nudging method to assimilate ice concentration and SST observations into an uncoupled version of the CICE model with a mixed-layer ocean parametrisation.

Unfortunately, this work is not currently up to the standard required for publication. A detailed review is given below, but the main reasons are as follows:

(1) It is unclear what is novel about this work. The conclusion states that the authors' use of a variable drag formulation is unique. However, Tsamados et al. (2014) previously incorporated a variable atmospheric and oceanic form drag into the CICE model. The paper has been cited by the authors, but they do not describe how their imple-

mentation of this method is related to any of their results. At the most basic level, the authors should show results with and without this formulation.

The statement that other centres do not provide details of parameters other than ice concentration or thickness is inaccurate, particularly as many of those centres are also using the same CICE model as the authors, with the same available parameters. The main conclusion of the results seems to be that assimilating observations brings the model output closer to reference observations, which is not a new result. Perhaps the differences in results when using thin ice of < 60 cm compared to < 30 cm might be an interesting angle, but this is not explored.

(2) The statements throughout the paper, that the model fits the validation observations well, are not backed up by the results themselves. Although the assimilation improves results, there remain clear systematic differences between the model output and the reference observations.

(3) There are many omissions in explanation of methods, several contradictions in the text, confusing wording, and the paper is missing references to current literature and relevant similar systems, e.g. TOPAZ, RIPS/RIOPS, ACNFS. Also missing is a description of how the authors' system differs and improves on these, and indeed what the purpose of the new system is. A number of the citations given are conference papers or otherwise unpublished works, which are not peer-reviewed and should not form a significant basis of citations.

General comments

The relevance of how this work fits in with the published literature needs to be discussed, along with other regional modelling systems. How do the results compare to e.g. coupled ocean-ice systems? What is the benefit of only using an SST parametrisation? Is this system to be used for operational or research purposes? A large number of the references used in this paper are unpublished or non-peer-reviewed works including conference papers. A more complete discussion of the peer-reviewed literature is necessary.

The paper needs more information on all the input and validation datasets, including descriptions and data access information. The authors also need to ensure that all the datasets used are properly cited.

Why is the assimilation system set up to weight heavily in favour of the model rather than giving equal weight to the observations?

The paper repeatedly states that AVHRR data was assimilated. Actually, it looks like the authors are assimilating the AVHRR-only OISST analysis product, which although based on observations, is an analysis product. This needs to be made clear, along with information on the temporal resolution, timeliness etc of the product. Additionally, this product uses SSM/I and SSMIS information to create proxy SST observations for assimilation at high latitudes. This means the SST observations also include input from ice concentration data. Therefore, they are not independent from the SSM/I and SSMIS data being used for validation.

More detail and justification of which thickness ranges of SMOS and CryoSat-2 observations are being used is needed.

What real benefit is the assimilation giving? Figures 5-7 show that it brings the model closer to the observations, but it still deviates and all modelled ice thickness is too high. It is not convincing to state that the M2 model has good correspondence with the observations due to being in the uncertainty range, as even the free-running model is also managing that most of the time. Assimilating SST in addition to sea ice concentration produces better results, but few if any operational centres will not already be assimilating ice concentration and not SST observations.

The authors acknowledge that the assimilation is not optimised. If changing the value of alpha or adjusting the nudging timescale is expected to improve results, why has this not been done? Similarly for the relationship between ridge and keel.

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Discussion paper

The authors need to also include RMS (or standard deviation) statistics, as well as mean difference when discussing how well models match validation observations.

For figures 5,6,7 the modelled ice is too thick for all model runs after January. Although results for the M2 model are closer to observations than the M0 or M1 models, results are still not very good, which is not mentioned in the paper. In general throughout the paper, systematic biases or errors which are large in proportion to the model variable values are not addressed, or are dismissed as being within uncertainty levels. This shows a poor understanding of the validation results.

For results where the model thickness < 30 cm (figures 9,10,11), the models seem to be underestimating ice thickness rather than overestimating. This difference to the results seen in figures 5,6,7 needs to be addressed in the paper.

For figures 9,10,11 the modelled thin ice thickness remains roughly constant from December, and also the assimilation makes little difference. Reasons for this need to be addressed in the paper.

More information on methods is needed throughout. Instances of this are given under specific comments below.

The conclusions make a number of statements which are misleading. Details are given under specific comments below.

Many of the references are missing important information.

Specific comments

Page 1

Line 6: Observations of ridge height and keel were not obtained from remote sensing data.

Line 8: What is your maximum SMOS thickness data? Only thin ice thickness available from SMOS.

Line 9: CryoSat-2 freeboard observations should be mentioned.

Line 15: Citations needed.

Line 19: A 1992 reference seems a strange choice given the recent advances in ice thickness remote sensing.

Line 20: Need to specifically relate this to sea ice forecasting.

Page 2

Line 5-10: The relevance of this and how your work fits into this needs to be discussed, along with other regional modelling systems.

Line 11: Do you mean assessment rather than analysis here? Can only produce an analysis of ice concentration and thickness by assimilating ice concentration and thickness, not by modelling thermodynamics and (assimilating?) ice motion.

Line 12: which satellite?

Line 17: Before or after assimilation?

Line 19: Reference Hunke et al. (2015) the first time CICE is mentioned.

Line 22: Confusing, as you have mentioned prescribing ocean conditions but then mention you will be assimilating SST, before you mention the ocean mixed layer parametrisation below.

Line 31: "regional scale" - need to have a figure showing the domain.

Line 31: "about 10 km" - Need to mention what grid you are using.

Line 32: Should say "Density-based criteria were used >following< Prasad et al. (2015)..." and elsewhere, where the method has already been published.

Line 33: "analysis" should be "assessment" as the word analysis has a very specific meaning in the context of data assimilation.

Page 3

Lines 3-6: Citations for the sources of all the forcing data are required.

Line 5: Why use SST climatology data rather than the daily analysis fields? What sort of climatology? Daily? Monthly?

Line 7: If assuming no ice at the start of the runs, important to state the spin-up time of the model (which should be mentioned anyway).

Line 10: Assimilating AMSR2/AMSR-E data, not using for validation.

Line 16: Mentioned that AMSR-E shows best results above 65% concentration, but are validating against SMOS observations of thin ice, as found in the MIZ where concentrations are much lower than 65%. Need to discuss limitations of the AMSR-E/AMSR2 data for the ranges relevant to the paper.

Line 18: AMSR-E data is interpolated to the model grid before assimilating (what about AMSR2?). The usual method would be to interpolate the model to the observation location. What is the benefit in interpolating the observations to the model grid?

Line 22: How consistent are observations derived from the different AMSR-E and AMSR2 instruments? Information needs to be added to the paper.

Table 1: Text says AMSR-E resolution is 6x4 km, but table says 5.4 km. Inconsistent. AMSR2 resolution is 5x3 km, so needs its own entry in the table. Additionally, not only SSMIS instruments in this time period - some were SSM/I (dates will depend on which OSI SAF product was used) so specifications for this instrument need to be included in the table as well.

Line 23: Which OSI SAF product number and version?

Line 24: Also uses SSM/I sensor.

Page 4

Line 2: How were erroneous data removed? Methods needed.

Lines 2-4: Make it clear using AVHRR analysis, not SST measurements directly.

Line 5: CryoSat-2 altimeter is called SIRAL.

Line 12: Clarify what you are using the CryoSat-2 data for: validation. Why is the focus mainly on the SMOS data, and why are the CryoSat-2 SIRAL specifications not included in Table 1?

Line 14: Confusing that the SMOS thickness data resolution has a range. What is the resolution of the actual product used here? Also this is different to the range given in table 3.

Line 15-16, 19-20: Remove sentence "The ice thickness uncertainties are lower for thin ice and uncertainty increases as the ice thickness increases." as this is repeated below. Similarly for "Moreover, large errors occur during the melting period."

Line 16: Needs a line or two explaining how the SMOS sensor obtains measurements of ice thickness.

Line 20: What is the magnitude of the snow depth uncertainty?

Line 24: "...for our region of interest" - remove this, as not available in summertime for any Arctic region (and I don't think Antarctic SMOS ice thickness observations are available yet).

Line 24: Unclear what the Kerr and Barre citations are related to. Reword this.

Table 2: Caption is same as for table 3, update this.

Line 25: Show location of Makkovik Bank on map.

Page 5

Table 3: Could this information be included in table 1? Are all these specifications directly relevant?

Line 2: Confusing - state which distribution is shown in the figure, and what causes the variation in distributions.

Line 3: Why only include data for a single day?

Line 4: What are these assumptions based on? Needs more explanation.

Line 6: What sort of quality control was undertaken for this data? Needs more explanation.

Figure 1: Needs units on x-axis, and date of observations in figure caption.

Page 6

Line 7: For SST this is the AVHRR-only OISST analysis

Line 7: SST is not from model, it's a parametrisation

Line 11: "model estimate" should be "background model estimate" (as it's the background error in data assimilation terms)

Line 13: As sigma_o is different for sea ice concentration and SST and described below, remove from this line. Also "parameter may vary spatially" is confusing without additional explanation.

Line 15: If above 65% is 10%, this should be > 0.10 based on your stats given on page 3, line 16.

Line 16: I think this is intending to say something like "ensure that the assimilation is heavily weighted to the model background when there is large variation between the model and the observation." Needs rewording as it's unclear. However, method will weight towards model background even if the observation error is similar to the background. Why?

Line 22-25: Needs rewording as it's unclear what this means, and how this mechanism might directly affect the results.

Page 7

Lines 1-2: Reword this as implies model assimilates SST instead of ice in data gaps. Also gap between AMSR-E and AMSR2 should be mentioned here. Need to state that the model is free-running during periods where no data is available for assimilation.

Line 4: "error" should be "mean difference", as the dataset being used as a reference is not necessarily "truth". This needs to be changed throughout the paper. Here, this should also say "absolute mean difference of ice concentration" for clarification. "OSI SAF" should be "OSI SAF dataset" (or similar wording).

Line 7: "the results do not improve much" Is this compared to Model M1? But in some locations the difference has reduced by about 20%, which is a good improvement. However, as you are assimilating the AVHRR-only OISST analysis, it is important to note that the product makes use of SSM/I and SSMIS ice concentration data to determine SST at high latitudes (though probably a different algorithm to the OSI SAF product). This means the SST observations you are assimilating are not truly independent from the SSM/I and SSMIS data you are using for validation. However, the AMSR-E/AMSR2 data is independent from the SSM/I and SSMIS data, and this should be stated.

Page 8

Figure 3: Need to state that this is ice concentration and which product the models are being compared to in the figure caption. It also needs to be stated in the text somewhere what the spin-up period of the model is.

Lines 2-3: Need to state which instruments the assimilated ice concentration and OSI SAF data use.

Line 4: Why only giving the 2010 results? Also broken down into seasonal results would give a better picture.

Line 8: This last sentence does not relate to anything shown in figure 4, remove this or improve discussion.

Page 9

Line 2: Which model thickness category are you using for the comparison?

Line 2: observations from which instrument?

Lines 2-3: Unacceptable uncertainties in all observations? Confusingly worded.

Line 4: An uncertainty of 100 cm seems a lot for thin ice. What maximum ice thickness from SMOS are you using? From figures 5,6,7 it looks like 60 cm but this needs to be stated and explained. E.g. Xie et al. (2016; The Cryosphere, 10, 2745-2761) only use SMOS observations of < 40 cm, but others use up to 1 m thickness.

Line 5: How is model uncertainty determined?

Line 9: Add "As ice thickness increases through the season, so do the uncertainty limits."

Line 9: MO and M1 are too, except February 2013. What real benefit is the assimilation giving? Bringing closer to observations, but still deviates and are all still too high. Not convincing that it is only in the uncertainty range as even the free-running model is also managing that most of the time.

Line 10: Add "from October" before "until the end of February".

Lines 10-11: Move discussion of uncertainties to previous paragraph.

Line 12: Remove sentence beginning "Compared with the uncertainty values..." as this repeats information already stated.

Lines 16-19: This is because the assimilation is strongly weighted to model background. Demonstrates this is not the optimum set-up. If changing the value of alpha is expected to improve matters, why has this not been done?

Page 10

Figures 5,6,7: Combine these into one figure. The correspondence with the observations is poor after about January. All modelled ice is too thick, and although results for the M2 model are closer to observations than the M0 or M1 models, results are still not very good. However, for results where the model thickness < 30 cm (figures 9,10,11) the models seem to be underestimating ice thickness rather than overestimating. This difference needs to be addressed in the paper.

Figure 7 caption: make clear that M1 is not assimilating ice concentration because there is no AMSR data available.

Page 11

Lines 2-3: Why does figure 8 include regions where observed uncertainties are larger than 1 m, when on page 9 you have stated that this data has been rejected? This makes the figure very difficult to interpret, as it implies the model is underestimating ice thickness, but the comparisons in figures 5,6,7 indicate it is actually overestimating ice thickness for thin ice where the SMOS observations are more reliable - or underestimating for figure 9,10,11. Need to redo figure 8 showing only the relevant data, and also include panels with M2 differences to SMOS.

Page 12

Figure 8 caption: only showing for 3 individual dates, not 2010-2011 - update caption to reflect this.

Page 13

Figures 9,10,11: Maximum model thickness looks like 20 cm rather than 30 cm. Model underestimates thickness from December as thickness remains roughly constant throughout the year after this date. Also the assimilation makes little difference. Reasons for these results need to be addressed in the paper. Also caption states only model M2 is shown, but all models are shown on plot, update caption.

Figure 11: What is the cause of the discontinuity in SMOS ice thickness and uncertainty

between December and January? This needs to be addressed in the paper.

Page 14

Figure 12: Needs to be larger, as it is difficult to see the shaded regions.

Line 1: How is the "observation uncertainty" generated? Is this actually the AVHRR-only OISST analysis uncertainty? Add this to text. This is not independent data as it's being assimilated for model M2. Could choose a different dataset for validation.

Line 2: Sentence beginning "The SST assimilation..." does not refer to figure 12. It is confusing to have this sentence here with no context.

Line 3: The model doesn't have "outliers", results show it has systematic biases in both summer and winter.

Lines 3-11: These lines give speculation on how the results could be improved, but this work needs to be done.

Page 15

Line 2: Need to describe the method here, as Prasad et al. (2016) is a non-peer reviewed conference paper.

Line 6: Add that rho_w is the density of water.

Line 7: "about 10 cm" - give specific value (variation with season? Different years?). Need to add RMS or standard deviation.

Line 8: An error of 10 cm on a draft of 20 cm is proportionally very large, so can't be described as good correspondence.

Line 8: Reiterate here for benefit of reader that only done analysis for 2005, 2007, 2009 as this was when data was available.

Line 10: "close to the location of the ULS" - are you interpolating the model result to the observation location? If not what method is being used for the matchups and why?

Figure 13: State on figure caption where these measurements are located.

Page 16

Lines 1-2: "single melt pond" - even in winter? This method needs more description.

Line 5: H_k is not used in equation (4), remove (given below for equation (5)).

Line 6: m_r and m_k need more explanation - slopes given in degrees but what are 0.4 and 0.5?

Lines 5-8: Where are these values obtained from? Not all the variables have been given values either.

Line 12: Citation required for this statement.

Line 13: Model and observation of keel depth or ridge height? Confusing.

Line 16: Figure 14 only shows modelled and observed keel depth, not ridge height so can't see this relationship. Also need to give statistics for difference between modelled and observed keel depth.

Lines 17-19: If further work may result in a different conclusion, you need to do this further work to be able to draw any conclusions.

Line 25: How did you calculate lead fraction? Or cite existing product if that is what you used.

Line 26: Need to clarify in the text that the uncertainty given is for CS2 freeboard measurements. Need more information on the CS2 (CryoSat-2) product, e.g. how often available, where data was accessed etc.

Equation (6): I can't find this equation in Tsamados et al. (2014) but it looks like it's missing some brackets.

Page 17

Line 1: "absolute difference" should be "absolute mean difference".

Line 2: "M2 freeboard measurements are close to the observed freeboard". This isn't true - figure 15 shows that the differences between the model runs and the observations are a large percentage of the actual values. There is also variation between the different months shown. It would be better to show differences rather than absolute differences on the spatial plot to be able to see where the biases are and in which direction. Other statistics such as RMS or standard deviation also need to be given.

Page 19

Figure 16: Caption should specify Jan, Feb, March 2011 and not just 2011. Also need to show the difference plot and give other statistics e.g. RMS.

Line 4 (and line 6): The model values look systematically different to the observations in Figure 16. Figure 17 shows that the model is unable to replicate the seasonal changes in the freeboard observations, and just increases throughout the year.

Line 5: The data presumably still undergoes averaging if the points are observed multiple times within that month. Much more information on the dataset is needed.

Page 20

Lines 2-3: Needs references to back this up.

Line 6: Misleading, as have not validated the assimilation method itself, only assimilated different combinations of observations.

Lines 6-7: This sentence implies the model is assimilating all these variables, which is incorrect. Reword.

Line 8: Disagree that it is a good correspondence.

Line 10: The RMSE should be mentioned previously with the rest of the results.

Line 11: Where have you split results into below 40 cm? Seems to be 60 cm for observations or 30 cm for model category (which looks more like 20 cm).

Lines 14-15: No remote-sensing ice thickness data is available in the summertime due to the presence of melt ponds. As summertime data has been excluded in this study the error contribution from melt ponds is likely to be small.

Line 18: Agreement is not always close. This statement needs to be revised.

Lines 20-21: This needs to be done, rather than stating it as future work.

References:

Many of the references are missing important information, for example access URLs for technical reports, and format type e.g. book, report, dataset etc.

Technical corrections

There are a number of instances of citations not being in brackets when they should be, and vice versa. The authors need to go through the manuscript carefully to correct these.

The authors need to ensure that the paper is read by a native English speaker as there are a large number of minor but important grammatical errors. There are too many to list here.

There are also several areas where information is repeated within the same paragraph. Some are listed above. Correcting these instances would improve the readability of the paper.

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