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Interactive comment on "Sea Ice Assimilation into a Coupled Ocean-Sea Ice Adjoint Model of the Arctic Ocean" *by* Nikolay V. Koldunov et al.

Anonymous Referee #1

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Review comments on "Sea Ice Assimilation into a Coupled Ocean-Sea Ice Adjoint Model of the Arctic Ocean" by Koldunov et al.

1. Summary

The paper deals with an ocean-sea ice data assimilation experiment in the Arctic Ocean by an adjoint method. The data used for the assimilation are ocean hydrography (in-situ and remotely sensed measurements and climatology) and sea ice concentration. These data are assimilated to a regional coupled ocean-sea ice model (MITgcm + Hibler-type sea ice model) for a state estimation of 2000 - 2008 period, by adjusting the initial and boundary conditions for the model (the control vector is composed of ocean initial conditions and atmospheric boundary conditions). The authors report substantial improvement of modeled sea ice concentration and position of ice edge in summer, as

well as improvement of seasonal cycle of sea ice cover and ice thickness distribution. The authors also report that the improvements occur mainly due to corrections to the surface atmospheric temperature.

2. General comments

The adjoint method is one of the promising data assimilation methodologies for state estimations, since the method can preserve modeled physics in the estimated state. The estimated state is not only usable as a 4-dimensional interpolation of observation, but also applicable for dynamical interpretations of the system. However, setting up the adjoint model, which has the consistent physics with the corresponding forward model, sometimes requires substantial efforts, and in addition running the adjoint model with a long assimilation window needs linearization of the code. The authors seem to suffer from these technical issues, although the details were not provided in the manuscript. They run the adjoint model without some ocean modules nor sea ice dynamics. The latter issue (adjoint run without sea ice dynamics) is crucial for the current experiment design, since the study focuses on improvement of sea ice status and associated corrections to the control variables. In addition, the authors divide the 2000 - 2008 assimilation window into 1 year chunk. This division seriously deteriorates the advantage of the adjoint method, since the observed data cannot contribute to improve the model status of preceding years. This issue may affect one of the conclusions of this study that the authors don't find sizable improvements of ocean field, since the spin-up/spin-down time for ocean is much longer than that for sea ice. For these reasons, the present study did not take the advantage of the adjoint method. Since the authors did not provide the details of the experiment design (e.g., definition of the cost function, penalty term, uncertainty for each observational data), some of the results in the manuscript are difficult to interpret. As a whole, unfortunately I cannot find any new technical achievement nor sound scientific findings for Arctic ocean-sea ice system in this manuscript.

3. Major points

- Why the authors switched off the sea ice dynamics of the adjoint model? Since the main focus of this study is to examine the effect of sea ice concentration assimilation on the modeled field and correction to the control variables, the adjoint run without sea ice dynamics may seriously deteriorate the results. Particularly, I have concerns about the result that the system obtained the optimal solution of sea ice concentration by the corrections to the 2 m air temperature. Since the adjoint model doesn't take the sea ice dynamics into account, the dynamical forcing, such as wind forcing and/or ocean drag, cannot directly contribute to improve sea ice concentration by the assimilation. I would guess this is one of the reasons why the ice concentration improvement occurs mainly due to thermal forcing (2 m air temperature), and occurs not by dynamical forcing. To obtain the conclusions in this study, the sea ice dynamics is indispensable in the adjoint model.

- The division of the 2000 - 2008 assimilation window into 1 year chunks deteriorates the advantage of the adjoint method. Due to the current set-up of the system, the system cannot use the observed data to improve the model status for preceding years. I am afraid that this short assimilation window might be a cause of the small improvement of ocean status compared to sea ice in the current assimilation (ocean needs longer spin-up/spin-down time than sea ice, and 1 year window is too short even for layers shallower than Arctic halocline). I also would like to point out that even for sea ice, 1 year chunk is too short, if the authors intend to examine improvement of ice thickness. In order to extend the assimilation window, I think further linearization of the code (including consistency check with the original code) is necessary.

- The authors should describe how the cost function is defined (i.e., the objective function, the gradient of which is estimated by the adjoint), since the definition of the cost (i.e., weighting between different types of observed data, error estimates for observational data, and definition of penalty term, etc.) strongly affect the behavior of the system. Due to the lack of these information, some results shown in the manuscript are difficult to interpret.

- How do the authors control or constrain the allowable corrections to the control variables? Since the spatial pattern of the correction to the 2 m air temperature (Fig. 8) is quite similar to the bias of the free run (Fig. 3), I have a concern that the system changes the control variables without reasonable constraint. The authors cite Köhl (2015) as a reference for the atmospheric condition. In this paper, the errors of atmospheric field are prescribed by the standard deviation of the NCEP field. What does this mean? The standard deviation of the NCEP field is the ensemble spread of the NCEP climate model? If so, the standard deviation does not provide error of the reanalysis field, but provides the magnitude of the natural variability of the modeled climate system. Since the adjoint method tries to impose all the deficiencies of the model status to the control variables, the validity of the allowable corrections to the control variables should be carefully examined.

- What is the new technical achievement or new scientific finding(s) of this study, particularly, in comparison with the number of former sea ice (and partly ocean) data assimilation studies for Arctic region? Since the sea ice dynamics of the adjoint model is not consistent with the forward model and the assimilation window is only 1 year, I hardly find any advantages on this experiment compared to the former studies using another assimilation method (e.g., optimal interpolation, 3D-Var, Green function method and EnKF).

4. Minor points

- Page 4, line 16: Which module is excluded from the adjoint code? What effect do the authors expect by this exclusion, particularly, in relation to the conclusion of this study?

- Page 4, line 16-17: Why the sea ice dynamics are switched off? Due to the absence of sea ice dynamics, the adjoint model cannot provide the optimal gradient, and therefore the correction to the control variable may not reflect (modeled) reality.

- Page 4, line 20-23: Are the control variables used to define penalty term in the cost (object) function? Please provide the exact definition of the penalty term for reproducibility of the study.

- Page 4: Please describe exact form of the cost function (including penalty terms) used in this study.

- Page 4, line 23: How did the authors define the uncertainty for hydrographic (e.g., EN3, NISE, etc.) and satellite data? Although the author cited Köhl (2015) as a reference, the representation error of in-situ measurements should be different since the resolution of the model differs.

- Page 4, line 24: Why did the authors apply constant error of 50% for sea ice concentration? OSI-SAF provides uncertainty estimates of ice concentration at every grid points. The constant error does not take into account the large uncertainties over the marginal ice zone, while it underestimates weight of reliable data over the central arctic.

- Page 4, line 24: Why the data assimilation is performed in one year chunk? Due to this experiment design, the authors cannot take the advantage of adjoint method. Particularly, the authors cannot examine the effect of data assimilation to the ocean variables, since 1 year chunk is too short even for layers above Arctic halocline.

- Page 5, line 8-10: How did the authors define the relative weight between different types of observation? Satellite measurements generally cover the large area with constant time interval, whereas the errors of the data are not independent but covariant. On the other hand, in-situ ocean measurements are very sparse, while the measurement errors are almost negligible (and therefore assumed to be independent each other) compared to the representation error. In addition, the magnitude of the representation error depends on the size of model's grid cell. How did the authors handle these issues? Description is needed.

- Page 6, line 23-25: I don't understand the meaning of this sentence. More explanations are needed.

- Page 6, line 28-30: Please explain relation between the Hausdorff distance (Dukohvskoy et al. 2015) and the metrics used in this study (sum of the RMS errors). I do not understand why the authors introduced Dukohvskoy et al., (2015) here.

- Page 7, line 18-24: It is hard to believe that the spatial pattern of the bias of 2 m air temperature of NCEP reanalysis coincide with that of modeled ice concentration bias. If I understand correctly, the uncertainties of the NCEP reanalysis data used in this study are given by the standard deviation of ensemble runs of NCEP climate model. Does the difference between the ensembles has such a sharp gradient like the correction to the 2 m air temperature (Fig. 8 the first row, left)?

- Page 7-8: The authors described that the correction to the 2 m air temperature is the main driver to improve the sea ice concentration in the assimilated field, and the contributions from other control variables, such as wind forcing, are very small. I am afraid this may be an artifact due to the lack of sea ice dynamics in the adjoint model, as described in major point.

- Page 7, line 31-33: The wind can play a role not only in local redistribution of the sea ice along the shore and ice edge, but also in large-scale sea ice distributions, although such effect is not seeable in the present experiment design.

- Page 8, line 8: ".., both making atmospheric forcing actually worse". I do not understand the meaning of this sentence.

- Page 9, line 24-25: This is interesting. Could the authors provide specifications of the mechanism?

- Page 9, line 33 - Page 10, line 1: How much is the ratio of relative contributions to the cost function between ocean variables and sea ice variable? If the contribution from sea ice variables dominates, the system tries to change the control variables which have large impact on sea ice, and then it is natural that the changes of the ocean variables are small.

- Page 10, line 1-3: The fluxes shown in Fig. 11 are improved by the assimilation?

As far as I know there are some flux estimates through Arctic gateways based on observations (e.g., Tsubouchi et al., 2014, JGR).

- Page 10, line 25-28: see the comments above.

- Page 11, line 3-4: This result is interesting, but I am still afraid that this improvement might be achieved by wrong reason, due to the absence of sea ice dynamics in the adjoint, since the ice dynamics is important for the redistribution and accumulation of sea ice.

- Page 11, line 11-13: I agree that the estimated state of sea ice is consistent with the modeled physics, whereas due to the lack of ice dynamics in the adjoint, we cannot make sure the correction to the control variables are realistic. In other words, we cannot exclude a possibility that the estimated state is achieved by artificial forcing different from reality, and therefore by thermodynamic and dynamical balance different from reality.

- Figure 3 caption, line 3: “third” should be “forth”.

- Figure 3, 4, 8 and 10: It would be helpful for comparison of the spatial patterns, if the longitude and latitude lines (as in Fig. 9) are embedded in these figures.

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