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The Cryosphere
Discussions

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 #2

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Summary

In this work the authors demonstrate the synthesis of hydrographic and sea ice concentration data into a 16-km horizontal resolution Arctic and North Atlantic coupled sea ice-ocean model. The reduction of an uncertainty-weighted model-data difference cost function was achieved by iteratively optimizing a set of adjustments to a set of atmospheric and initial condition control variables using gradient information provided by the adjoint of the numerical sea ice-ocean model. The final multiyear state estimate was constructed by optimizing each single year between 2000 and 2008 in succession - the final optimized state of year X is defines the initial state for year X+1.

The authors demonstrate improvements of the model's reproduction of the data. The largest reduction in terms of percentage is found with sea ice concentration and SST

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with lower relative cost reduction for other data, including T and S profiles, SSH, and mean dynamic topography. The largest sea ice concentration cost reductions in terms of RMS are found during summer months. Discrepancies between simulated and observed sea ice extent are found to increase in some months even when discrepancies in simulated and observed total sea ice area decrease. After synthesizing ocean and sea ice data, little impact is seen in ocean volume, heat, and freshwater fluxes through Fram Strait and Davis Strait.

Specific Comments

1) With respect to the title, assimilation is not "into a Coupled Ocean-Sea Ice Adjoint Model". The assimilation is "into a Coupled Ocean-Sea Ice Model using its adjoint".

2) Abstract: Better to provide the actual spatial resolution of the satellite sea ice concentration data that is assimilated rather than refer to it as 'high resolution'.

3) Page 1, Line 6: 'values of sea ice extent become underestimated' doesn't define a metric. Is the metric the sum of model minus data or weighted model minus data difference or the RMS of model minus data or something else?

4) Page 1, Line 6-7: Characterizing a state estimate of a system as complex as the Arctic Ocean requires that one analyzes a suite of metrics. The author's statement that one the sea ice extent metric is "not suitable to characterize the quality of the sea ice simulation" is odd and out of place. To whom is this statement aimed? This seems to be a straw man argument.

5) Page 1, Lines 10-11: The atmospheric control variable adjustments that one finds during any optimization are intimately related to the magnitudes of the prior uncertainties of the individual terms of the first-guess atmospheric state. The author's statement that biases in sea ice are reduced 'mainly due to corrections to the surface atmosphere temperature' is difficult to interpret because the reader does not know the magnitude of prior uncertainties used during e optimization. Are you referring to the sum of the

squared normalized adjustments? How is surface atmosphere temperature identified as the main control variable correction since atmospheric forcing units are arbitrary?

6) Page 2, Line 8. The authors may consider using the term 'state estimation' to describe the model-data synthesis methodology used in this study instead of the term 'data assimilation'. An uninformed reader may think that the work conducted here referring to sequential data assimilation, a technique that has been applied to sea ice data for decades. The adjoint method used in this work is rather special and yields quite a different product (namely a physically-consistent ocean and sea ice state). Below I post an excerpt from Wunsch and Heimbach, 2007 in which they argue for their choice of the term 'state estimation' when describing the application of the adjoint method to combine data with a model (emphasis mine):

"In physical oceanography, the problem of combining observations with numerical models differs in a number of significant ways from its practice in the atmospheric sciences. It is these differences that lead us to use the terminology "state estimation" to distinguish the oceanographers' problems and methods from those employed under the label "data assimilation" in numerical weather prediction. "Data assimilation" is an apt term, and were it not for its prior use in the meteorological forecast community, it would be the terminology of choice. But meteorologists, faced with the goal of daily weather forecasting, have developed sophisticated techniques directed at their own particular problems, along with an opaque terminology not easily penetrable by outsiders. Because much of oceanography has goals distinct from forecasting, the direct application of meteorological methods is often not appropriate."

7) Page 4 Line 10-11: List the control variables.

8) P4 Line 20: Describe why the atmospheric control variable frequency was changed to daily.

9) P4 Line 23: As atmospheric adjustments are an important control parameter in this work, the authors should (a) explicitly state how they were derived as Kohl (2015): "For

the atmospheric state, errors are calculated as before from the [standard deviation] of the NCEP fields." And (b) show maps of their magnitudes in the main text or in supplemental materials. Also, because they are so important, more discussion about your choice of standard deviation of NCEP fields is appropriate. The standard deviation of Arctic near-surface atmosphere temperatures is considerable given the large seasonal cycle. In much earlier versions of ECCO/GECCO the use of atmospheric state standard deviations could be justified because in mid-latitudes and the tropics they partially captured "random" variations due to synoptic variability. At high latitudes the standard deviation for near-surface atmosphere temperature and shortwave radiation is mostly due to the seasonal cycle.

10) Page 4, Line 24: Why are the sea ice data assigned a constant 50% error? Satellite SIC products have errors that are far smaller than that everywhere except in the MIZ and in summer when meltponds are present.

11) Page 4, Line 27: To clarify, each year after the first uses initial conditions that are identical to the final state of the previous year, correct?

12) Page 4, Lines 3-4: Some SST products have nonzero values beneath sea ice. Is that the case in the RSS dataset?

13) Table 1: I understand that the PHC climatology had large biases relative to modern Arctic T and S because it was derived with observations mainly from the 1970's and 1980's and before the recent shifts in Arctic heat and freshwater (McPhee et al, 2009). Can you comment on how the simultaneous use of the PHC climatology alongside contemporary data may have affected the T and S cost reduction?

14) Page 4, Paragraph 1: Cost function reduction percentages are important but obviously they are dependent on how close to the data you were when you began your simulations. The first-guess solution of Fenty et al., (2015) could have been further from the data than your first-guess solution. While both may end up in the same state, their reduction percentage would be higher. The most important information is how

well one's final state estimate fits the data. Much less important is the magnitude of the improvement relative to one's (somewhat arbitrary) starting point.

15) Page 4, before line 31: It may be useful to mention how many iterations were conducted before the 1% threshold was achieved. In Figure 3 I see "iteration 3" as the final iteration for 2005 and 2007. That strikes me as unusual. If your cost was dominated by SIC and SST data and the adjoint method quickly reduced the misfits of those data, then I can see how you hit the 1% total cost reduction threshold quickly. However, it is possible that if those two datasets were ignored, the adjoint machinery could have continued to substantially reduce misfits in other datasets. Can you comment on that?

16) Page 5, Line 15: There may be a missing figure. I cannot match up Figure 2 to the description offered here. Fig 2 is % cost reduction in different years vs. data.

17) Page 5, Line 24: Good to additionally mention why most models overestimate sea ice in the Greenland Sea with a reference.

18) Page 5, Line 35: This is probably because in these extreme months the location of the sea ice edge is relatively stable compared to spring and fall months when the ice pack contracting and expanding.

19) Section 4: This entire discussion must be rewritten. Atmospheric control variable adjustments seem to be compared by their relative magnitudes but their relative magnitudes are not meaningful because these physical variables have different, arbitrary, units. By all means show the magnitude of the adjustments but to make a meaningful comparison one should first normalize them by their prior uncertainties. a. This includes Figure 8, which should be updated to show all control variable adjustments normalized by their uncertainties. Also include longwave radiation.

20) Page 8, Line 20-22. The "probably realistic" spatial distribution of the Kwok Arctic sea ice thickness field deserves a reference. Are the 0.7 m errors spatially correlated or uncorrelated?

21) Page 9, Line 22-24: Neither the length of the simulation nor the number of T/S profiles is a fundamental impediment to magnitude of model-data misfit reduction. An iteration 0 state with T and S close to the data as measured by the prior uncertainty could be responsible. Maybe averaged normalized costs should be added to Figure 2 for each cost category for iteration 0 and the final iteration.

22) I may be incorrect but it seems that no Arctic Ocean T and S profiles were used in this work. I do not see Arctic Ocean data in the Ingleby and Huddleston report and the NISE database doesn't show data north of the Norwegian Sea. Given that the assimilation period overlaps with the existence of ice-tethered profilers, why were ice-tethered profile data not included (http://www.whoi.edu/page.do?pid=20781)? As for the CTD data in the Arctic, both the ICES database (http://www.ices.dk/marine-data/data-portals/Pages/ocean.aspx) and the World Ocean Database v3 (https://www.nodc.noaa.gov/OC5/WOD13/) have data for the time period considered in this work. There may be perfectly fine reasons for excluding these data but the reasons should be offered.

Technical Corrections

1. Page 1, Line 5: change 'become' to 'are' as in 'values of sea ice extent are underestimated' 2. Page 1, Line 5: first comma to semicolon. Or split this long sentence into two before 'however' 3. Page 1, Line 14: strike 'to date' 4. Page 1, Line 16: reference? 5. Page 1, Line 17: strike comma before 'is therefore of utmost importance' 6. Page 1, Line 24: strike 'if not possible' 7. Page 2, line 2, strike comma before 'the community'. Strike 'heavily'. 8. Your doi for Detlef's 2016 paper is incorrect. It should be DOI: 10.1146/annurev-marine-122414-034113 9. Page 2, Line 19: strike "usually in general" 10. Page 5, Line 12: strike "are going to" 11. Page 5, Line 28: replace "very good" with "improved" 12. Page 5, Line 29: strike "thus" 13. Page 5, Line 31-32: This sentence deserves a rewrite for clarity. As mentioned above, relative percentage sea ice cost reductions are also a function of the (unknown) first guess states. 14. Add 'bears' before 'a good resemblance' 15. Page 5, Line 24: For clarity consider saying

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'since a perfect total sea ice area evolution...' and the following sentence is redundant. 16. Page 8, Line 20-22. Strike "except for the" and simply say that "Sea ice thickness are not provided by Kwok for the Barents and Kara Seas and the Canadian Archipelago because ..." with a reference. 17. Page 8, Line 26: change "variables" to "variables'" 18. Page 9, Lines 1-2: Why is it hard to provide quantitative estimates? You could plot time series of the uncertainty-weighted squared model-data misfit (normalized cost) before and after the assimilation. 19. Plotting model minus data or model minus data squared in Fig 5 might simplify comparison. 20. Section 4: Fonts on the time series of Fig 8 are also small and difficult to read. One subplot is cut off. After normalizing the summed control variable adjustments they could all be shown in the together in the same plot.

Wunsch, C., & Heimbach, P. (2007). Practical global oceanic state estimation, 230(1–2), 197-208. http://doi.org/10.1016/j.physd.2006.09.040

Fenty, I., & Heimbach, P. (2013). Coupled Sea Ice-Ocean-State Estimation in the Labrador Sea and Baffin Bay. Journal of Physical Oceanography, 43(5), 884-904. http://doi.org/10.1175/JPO-D-12-065.1

McPhee, M. G., Proshutinsky, A., Morison, J. H., Steele, M., & Alkire, M. B. (2009). Rapid change in freshwater content of the Arctic Ocean. Geophysical Research Letters, 36(10). https://doi.org/10.1029/2009GL037525

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