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Interactive comment on “Assimilating high horizontal resolution sea ice concentration data into the US Navy’s ice forecast systems: Arctic Cap Nowcast/Forecast System (ACNFS) and the Global Ocean Forecast System (GOFS 3.1)” by P. G. Posey et al.

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Addressing the revisions recommended by the Reviewer #1 (Author’s Response follow "AR:" in text.

Anonymous Referee #1 Received and published: 26 April 2015

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GENERAL COMMENTS The authors present a method to create a blended high resolution sea ice concentration product from AMSR2 and MASIE/IMS and model results from US Navy's sea ice forecasting system (in hindcast mode) that assimilates this newly developed blending concentration data. This newly blended data includes information of human analysis, and has a very high horizontal resolution of 4km, and hence are well suitable for using in the forecasting model of the Arctic Ocean with the high horizontal resolution. Comparing with the independent NIC data, the new assimilation decreases the predicted sea ice edge error significantly. In recent years, US Navy updated its operational system from PIPS to ACFNS. This paper describes their latest advances over an earlier sea ice concentration data assimilation used in their operational system. This manuscript is well written, and the results are clearly presented. Although the paper is quite technical, I think the focus on improving high resolution sea ice edge forecast using this innovatively high resolution sea ice concentration data justifies publication in TC.

However, some points should be addressed before publication: 1) In the Introduction, the US Navy's forecasting system is described in too many details.

AR: The system information has been moved from the introduction to section 2 (renamed to "System descriptions, data and methods").

But for a scientific publication, it would be also helpful to add some overview of the advances of the sea ice data assimilation in the current scientific community.

AR: While such a discussion could be useful, we think that including this information is not pertinent and too tangential to the scope of this paper.

At present, there are already a lot of sea ice data assimilation method and related research, e.g., nudging, OI, 3D-Var and EnKF, why you still use this simple approach of weighting technique? Is this method particularly suitable for your operational use?

AR: Text was added in the manuscript (section 2.4) describing the assimilation tech-

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nique in more detail. It's a 2 step process: 1) reading in ice concentration observations (AMSR2/blended product) into NCODA (3DVAR) that produces an ice analysis, and 2) which then gets read into CICE where the concentration is blended with the model ice concentration along the ice edge. This simple methodology has been used in the past forecast systems and is continued here.

AR: We will soon be developing a more advanced technique for assimilating the IMS sea ice mask (along with new data sources, i.e. VIIRS) within NCODA. During this time, the blending that is currently implemented within CICE will be moved into NCODA. Adjustments to other ice variables will also be investigated in this work.

Further, I would suggest the authors to re-organize the structure of the paper, e.g., to move the description of the forecasting system from the “introduction” to the “data and methods”.

AR: Done.

2) In Part 2 and Figure 5, you show that the blended concentration varies from 70% to 100%, and there are no concentration values below 70%. Is this a reasonable approach? Could this method introduce additional errors to the model? E.g., it seems not realistic that the concentration data within the sea ice edge are as high as 70% in the blended data, but you fuse this information into your model.

AR: The second to the last paragraph of Section 2.4 now addresses this topic. We tested other values, and more sophisticated schemes, but settled on 70% as the overall best approach.

3) In Part 3, more details of assimilation method is strongly required in the MS. Do you update the ice thickness and water temperature during the initialization? Is this initialization introduces inconsistency to your model physics?

AR: More details on the assimilation were added in section 2.4. It is a well-known disadvantage of 3D-VAR that it always introduces an unbalanced state. However we

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have not seen any large consequences from this even though we are using direct insertion of the analyzed ice concentration. The other prognostic fields are updated based on the new ice concentration. SST is only adjusted when switching between ice free and ice covered or visa versa.

4) In Part 3, you show the substantial improvements in the sea ice edge area, but besides this, sea ice concentration and ice thickness are also very important information for the forecasting use. So how about the sea ice concentration change in the sea ice edge area? Can you also compare your results with some other sea ice concentration data set, for example, NSIDC, OSISAF, ESA CCI or ice analyze charts from Canada. It would be also interesting to investigate the effect on the sea ice thickness. Can the new assimilation further improve the ice thickness forecast over the earlier approach? I notice that you had done such comparison in Posey et al. (2010), so in this MS, I would suggest you also do such comparison with in-situ observations, especially in the sea ice edge area. If you cannot show the improvements in the sea ice concentration and thickness, I would suggest you change the title of the MS to show the limit of this data assimilation study, e.g., Improving the Arctic sea ice edge forecasts by assimilating high resolution sea ice concentration data into the US NAVY's ice forecast systems.

AR: We agree that further testing could be done to show the improvements of other forecast fields such as ice concentration and ice thickness but for this paper we focused on the improvements of the ice edge error, which is of importance to the Navy. Because of this, as suggested, we changed the title of the paper.

SPECIFIC COMMENTS:

1) Page 2349, line 4, “difference” should be “different”

AR: Done.

2) Page 2350, line 12, (29 vs. 45 km, a “)” is missed.

AR: Done.

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3) Page 2362, the color bar in Figure 5 is not clear. Please redraw this figure.

AR: We re-made both figures 4 and 5 to ensure both plots and legends are more readable.

Interactive comment on The Cryosphere Discuss., 9, 2339, 2015.

TCD

9, C1278–C1284, 2015

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C1282

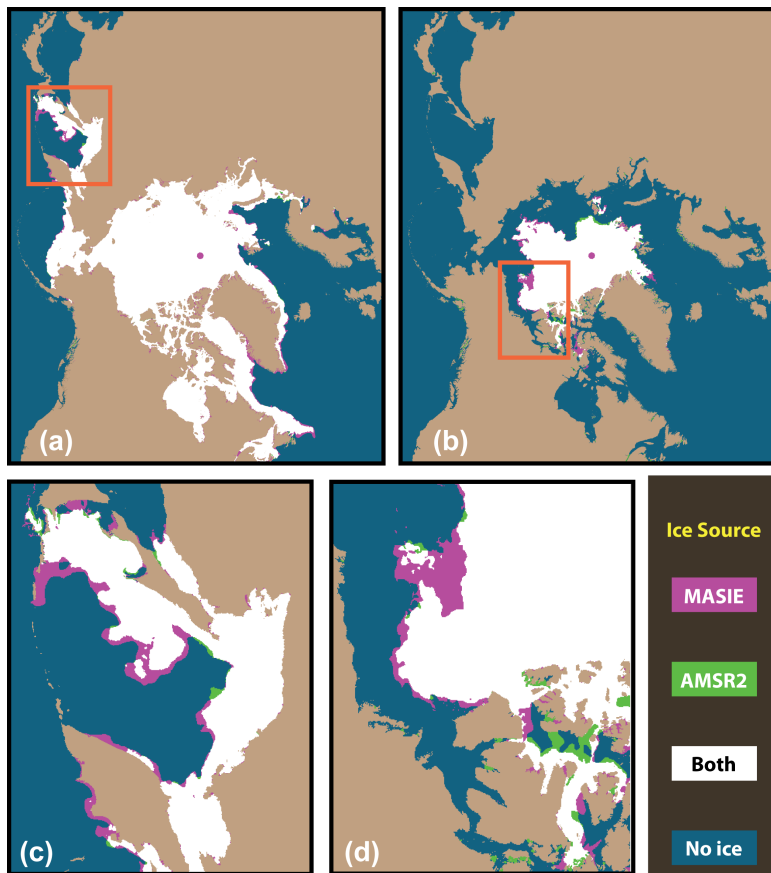


Fig. 1. re-do of figure 4

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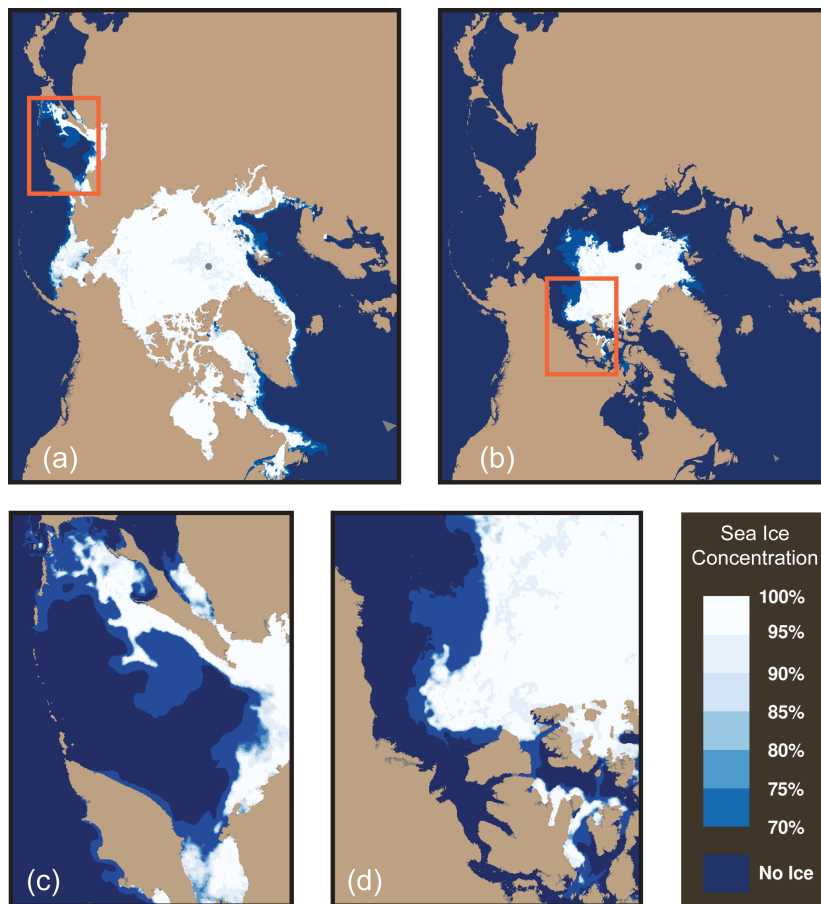


Fig. 2. re-do of figure 5

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