



EGUsphere, community comment CC1
<https://doi.org/10.5194/egusphere-2022-660-CC1>, 2022
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Reply on RC1

Jiping Xie

Community comment on "Assimilation of sea surface salinities from SMOS in an Arctic coupled ocean and sea ice reanalysis" by Jiping Xie et al., EGU sphere,
<https://doi.org/10.5194/egusphere-2022-660-CC1>, 2022

The paper "Assimilation of sea surface salinities from SMOS in an Arctic

coupled ocean and sea ice reanalysis" looks at the effect of assimilating the latest version (V3.1) of SMOS surface salinity data into the Arctic region. It does this by comparing the results to model runs which either did not assimilate SMOS data, or used an earlier version (V2.0) of the data. Validation was done against a variety of in-situ sources. The broad conclusion is that the V3.1 data does bring some benefits.

My comments, both minor and major, on the manuscript can be found in the accompanying PDF

The results in the manuscript will clearly be of interest to readers of EGU sphere. I also cannot see any major errors with the approach taken and how the results were obtained. That being said, and to be blunt, the paper is currently in a very poor state and needs to be considerably improved before publication.

Some, but not all, of my major issues are:

- The English is very poor, and nearly indecipherable in places. Most of my 230+ comments relate to the English. I appreciate that the authors are not native English speakers and that writing in English may be difficult. However, I recommend getting a native English speaker to proofread any future version before resubmitting.

Thank the reference for this comment which will be helpful to further improve our text. I together all the co-authors will improve the language more fluent and ask the native speaker to proofread the revision.

- There is a lack of care with the mathematics; three of the six equations in the paper look to be wrong.

Sorry for some technique errors in the equations. I will correct them and double check it before submitting the revision. For Eq.1 and Eq. 2 have no fault but can be explained more explicit. Eq. 3 and Eq. 6 can be adjusted more common. Eq. 4 and Eq. 5 will be corrected as the suggestion.

- The authors claim to use the DEnKF assimilation system, but their description, and

mathematics, more closely relate to the EnKF – which is not the same.

Thank for your reminding to keep the good consistence for the approach introduce. Right, DEnKF and EnKF are not equally same, but they are quite similar. The DEnKF was derived from the latter to keep the efficient analysis and more tolerant for operational forecast running. In this study, we would not like to present the method details because all the assimilation runs use the same method, and so the method illustration only in one paragraph in Section 2.1. In fact, the concerned mathematic equations of Eq. 1 and Eq. 2 are generally concept equations used by data assimilation community. At the end of Section 2.1 (line 137-138), there are mentioned “The **K** matrix (Kalman gain), is calculated as in Sakov et al. (2012) and updated in Xie et al. (2017).”

- The authors do much of their analysis on absolute fields, which all look very similar to each other. This makes it hard to believe their conclusions. It would be much more informative to look at the difference fields.

Thank this comment, but I feel a bit of disagreement for that. To evaluate the data benefits from SSS is complicated because the real observations are sparse in Arctic and contain many uncertainties due to the represented errors both on space and time. In this study, from different views, we try to include all available observations and to evaluate the model means and the deviations. Yes, most of the findings are focused on the salinity and its related fields.

- The authors need to give correlation coefficients between the model results and the in-situ observations. Regardless of the data being assimilated, some of the plots in figures 4, 6 and 7 make it look like the model is doing very poorly at representing salinity changes. It would be useful to see this quantified.

In the figure 4 and 6, the linear relationships are poor in ExpV3 if comparing the correlation coefficients. It could be related with the large spread. More discussions would be added in the revision.

Given these points, and my comments in the attached PDF, I am recommending that the paper is accepted, bit only after major, and extensive, revision.

We will work for these comments and give the official reply in PDF with one by one response.