Review of: Sea ice data assimilation in ORAS6

by Phillip Browne, Eric de Boisseson, Sarah Keely, Charles Pelletier and Hao Zuo

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Manuscript Synopsis

This paper is a description of the sea ice data assimilation process in ORAS6. While being an overall description of the sea ice data assimilation, the paper's main emphasis is to document the procedure of propagating the sea ice concentration increments into sea ice thickness category increments. The authors describe their method of distributing the total sea ice concentration increment to the thickness categories via equally distibuting it across existing categories very similarly to Smith et al. [2016]. They then demonstrate the improvement over an earlier strategy [Peterson et al., 2015] to only add or subtract the sea ice increment to the lowest available thickness category. Their demonstration of the improvement using their method is compelling, and on this basis alone I would recommend publication. However, I do have a comment concerning the explanation of perturbations employed to the sea ice observations (last paragraph of Section 3, ll. 100–104) to generate the ensemble analysis. While this does not have application for the results in the manuscript, which only use the control, unperturbed analysis, it may impact how the results will influence the wider ensemble, and aid in how they are interpreted.

My recommendation is Minor Revisions

Major Comments

1. The results from the manuscript are based on the non-perturbed control member of the ensemble ORAS6 system. Therefore the short existing description describing the perturbations to the sea ice concentration observations could be removed. As it stands, I found the ensemble perturbation description just informative enough to not understand what has been instituted. However, for selfish reasons – I really wish to understand this – I would prefer they firm up the description somewhat, even if it does not have direct bearing on the manuscript. The alternative might be to refer the reader to some other reference, either past or future (tech documents being acceptable).

   The tech document [Zuo et al., 2017] gives a detailed description of the random superobbing of OSI-SAF sea ice concentration used in ORAS5. I assume this corresponds to the one sentence statement (l.100) "The data is randomly thinned to boxes of 0.25/0.25 degrees."

   I have many questions regarding the further description detailing randomly sampling both the structural error determined by differences between OSTIA and ESA SST CCI v2, and the analysis error determined by variance in ERA5.

   - Are the structural and analysis errors actually independent. Doesn't ERA5 use OSTIA as the lower boundary condition?
   - Are the differences between OSTIA and ESA SST CCI v2 a good representation of the uncertainty in observed sea ice concentration. Do they not use the same OSI-SAF sea ice product, or at least similar OSI-SAF products? I researched this, OSTIA uses the near realtime OSI-401-b and ESA SST CCI v2 presumably (no clear link to which product used) uses one of the climate reanalysis products of OSI-450-a1 or OSI-430-a. These will differ, but only by the choice of the long term anchor mechanism use to produce a self-consistent climate product, but not in the actual retrieval method.

- How do you build a database from each of these? I envision this could be as small a database as two numbers – a difference, and a variance. However, I suspect it must be a much fuller database that somehow incorporates temporal and spatial covariances into it. A more detailed explanation, or reference to a more detailed explanation of what is done here would be beneficial.

Why this might be important: There is a large uncertainty between the sea ice analysis products [e.g. Peterson et al., 2022, Niraula, 2023]. This large uncertainty, and in particular the fact that the sea ice edge from the OSI-SAF products appears to be too diffuse Renfrew et al. [2021], could have an impact on your results.

I want to be clear: I completely agree that it is best to test the methodologies for distributing the sea ice into thickness categories in the control, unperturbed, deterministic simulation, and then validating the sea ice concentration fit against the assimilated observations. (I.e. I am not suggesting any changes to your methodology.) However, I believe results such as the degraded (Atlantic) sea ice edge thickness (Figure 4) when employing the gamma thickness distribution are likely related to uncertainties in the ice concentration observations – and this should probably be acknowledged. Similarly, the need to suppress temperature increments at the expense of sea ice increments (Section 6.3) is again likely a result of uncertainties in both the sea ice observations and SST analysis close to the sea ice edge as covered in my next point.

2. Section 6.3: I have a small issue with referring to the suppression of temperature increments as a balance relationship. For a balance constraint, typically one would have a balanced temperature increment dependent on the sea ice, but then an additional unbalanced term and unbalanced increments to temperature that could still improve the fit to temperature observations. But more philosophically, here you are literally placing your finger on the *balance scale* by tipping the resulting analysis fit towards the sea ice observations and away from the temperature observations. If anything, it would be more akin to introducing bogus (zero innovation) observations in the presense of sea ice. All these methods, along with still others are employed to improve retention of increments in the analysis, so I have no issue with the method, just perhaps the terminology. However, my understanding is not complete, and maybe they are all interconnected.

   When first I looked at the results it is clear that the fit to sea ice does improve with increasing alpha – but it was not clear that your choice of alpha was actually maximizing that fit (i.e. would it be a good idea to test a higher value of alpha). With better understanding, I now believe there would be no maximization (although perhaps saturation); continuing to increase alpha should continue to increase the fit to the sea ice observations, although presumably in detriment to the fit to temperature observations and presumably with lowering levels of improvement as alpha is further increased. What is shown (Tables 4 &5; Figures 6,7 &8) is only half the story as presumably this is all at the cost of sea surface and sub-surface temperature statistics (but only in the vicinity of the sea ice edge).

   This then connects with item 1: How does this connect with uncertainties in the sea ice concentration and temperature observations? Are you improving a fit to observation (in reality, that "observation" is actually an analysis) to a point the fit is well below the significant uncertainty in the observation? Figure 7, 8 & 9 of Renfrew et al. [2021] would seem to suggest this might be so. Is there a need to re-address this issue with post 2025-07-06 results and the introduction of the OSI-438 (AMSR2) product (Table 1)?

Minor Comments

1. I think it should be pointed out other methods of distributing sea ice concentration amongst the thickness categories do exist. For example, Smith et al. [2016] propose the rescaled forecast tendencies (RFT) method that is largely identical to your method. For instance their Eqn. 1 is identical to your equation 3. Please cite.

2. Joint Minimization (l. 114): It has been my understanding that joint minimimization of the univariate sea ice and multivariate ocean has always been a problem that leads to degraded fits, so I am happy that

you have managed to achieve this. However, will this not be a function of the observing system. Could improved numbers of near ice temperature observations change this dynamic? I suppose the hope is to go toward a more balanced, joint multi-variate ocean sea-ice assimilation before any substantial changes occur in the observing network.

3. Figure 4 (Section 6.1). As stated above in Major Item #1, I believe the degradation in sea ice thickness when using the gamma distribution is ultimately due to the uncertainties in the sea ice concentration and location of the ice edge. Although we are given insufficient information to confirm this, I suspect the gamma distribution has a bias towards thicker ice, at least at the ice edge (what is the hemisphere change in bias?) – which then shows up as increased RMSE there as the thickness observations would be sensing zero thickness ice. (I.e. Your decision – I believe correct decision – to not utilize the slightly better in terms of ice concentration, gamma distribution, is likely due to offsetting biases.) This might be a result that could change in the perturbed solution. It also again would likely change post 2025-07-06 with the introduction of OSI-438.

4. Open Water (Section 6.2) I could not help but think your open water fit results were one-sided. The results continue to get better as you increase the thickness ice increments in open water. While I would agree adding ice to open water into the 2nd category would seem weird – and would disconnect your data assimilation new ice thickness to your thermodynamic new ice thickness. (Actually that wording is a little ambigouous in Section 4.3.2 – the new ice thickness in the DA is or is not identical to the thermodynamic new (frazil) ice – or just the other properties?) At any rate, the same arguments that the data assimilation process is correcting for errors in dynamical movement of ice as much as errors in thermodynamics as justification for adding ice across all categories could presumably be used to justify adding higher category (relocated ice) for positive increments in open ice. Sorry: I seem to recall a statement is made along these lines, but I could not track it down in the manuscript. It might have been interesting to test adding 2nd category thickness ice to see where the fit to observations begins to decrease.

5. Table 4&5/Figure 8: If I have not missed something, α (Eqn. 5) has units of °C or K (it is a change in temperature – so your choice). The values of α in Tables 4&5 and Figure 8 – and in the text if they occur there – should be accompanied with that unit.

P.S. I will forego my anonymity as penance for my tardiness in achieving this review.

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