Ocean Science Discussions

Ocean Sci. Discuss., referee comment RC2
https://doi.org/10.5194/os-2021-65-RC2, 2021
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Comment on os-2021-65

Anonymous Referee #2

Referee comment on "Assimilation of ice compactness data in a strong coupling regime in the ocean – sea ice coupled model" by Maxim N. Kaurkin et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-65-RC2, 2021

The manuscript by Kaurkin et al. presents an application of the Ensemble Optimal Interpolation to an original Arctic Ocean-sea ice coupled model in view of operational forecasting. The methods are not new, but the implementation of the data assimilation is and the sea ice model is very advanced. The method proposed is inexpensive and therefore represents an interesting contribution to the community.
The work presented is of high technical standards and initial results do not show any sign of misbehaviour, which is encouraging for a technically demanding problem like the one addressed.
The main shortcoming of the paper is that it does not prove any added value of data assimilation. The only results presented are based on the variables assimilated and if is completely expected that these become closer to the assimilated observations than the free running model. However since the paper is extremely short, there is ample room for including additional results about non-assimilated variables that would prove the benefit of using an advanced modeling and data assimilation system.
Other works like Sakov et al. (2012, cited in the manuscript) criticised the application of an EnOI because of spurious covariances between sea ice concentrations and ocean salinity. I would therefore recommend that the authors compare the near-surface salinity output of their data assimilation system to their free run, to a reasonable climatology and other non-assimilated data to confirm that the spurious covariances are somehow absent.
Other comparisons with independent data are missing in the paper: the sea ice thickness, the ocean surface circulation. These are outputs that are of practical use and would justify the investment in a data assimilation system.

Other shortcomings are the lack of consistency in the data sources: the sea ice concentrations used for assimilation are different from the validation data. The different datasets (OSI-SAF, OSTIA, NSIDC) have large differences that may affect the results of the study. The authors should decide which of the three datasets is the more trustworthy and use only one data source consistently throughout the paper.
The choice of altimeter data is also odd: Jason-3 has a low orbit at 67 degrees North, which makes it much less relevant for the Arctic than other available altimeters: CryoSAT2, Saral/AltiKa, Sentinel-3, HY-2A all have a better coverage of the Arctic Ocean until the ice edge and are available from public databases in the period of interest. The authors should either re-run the experiment with the complete altimeter coverage or

explain why they have selected the altimeter with the least coverage of their region.
The comparison to other systems should not use the RMSE because of different definitions of the validation domain. The IIEE metrics (cited by the authors) should be used instead.

Some critical information about the method is missing and prevents potential reproduction of the results: have the authors used any localisation? Restoring to climatology? The exact definition of the correction factor for ice concentration, which seems quite important, should be expanded.

The figures are not ordered correctly and there has been mistakes in the upload of the figures on the web portal.

Detailed comments:
- Title: the authors use the word “compactness” although the ice concentration is used throughout the paper, which draws misleading attention to the complementarity of compactness and concentration. The title also forgets to mention that the model is global, and that data are assimilated in the Arctic.
- P2L26: the leap-frog scheme is a time stepping scheme. The advection of momentum needs a spatial numerical scheme, what is it?
- Figure 1 and 3 are identical. The figure comes too early because CP, A and X have not been defined.
- P3L7: What was the initial state at the start of the model spinup? A global ocean and sea ice model needs a long time to forget its initial condition so this information is still relevant after 10 years of spinup. A pinup of at least 15 years is sometimes recommended for sea ice thickness.
- P4L1: “become more gradient” -> show more gradients.
- Figure 4 has many small panels, the differences model-minus-observations would be more useful
- Figure 5 and 6: Indicate the time-averaged error between the time series.
- Section 2.3: the authors should use the notion of “multivariate” update, which is commonly used.
- Also explain in Section 2.3 if any localisation of the update has been performed and how. If no localisation was applied, an observation of the Arctic may well update the sea ice in the Antarctic, which would be unphysical.
- Table 1: The state vector has excluded important variables such as sea ice thickness, ocean current velocity, please explain upon which criterion the selection of variables was made.
- P9L16: Give an explicit expression for the correction factor: is it computed for each model grid cell and how does it handle zero values in the background?
- Section 3: the two experiments should be introduced earlier before h01 and h02 are used in the figure captions.
- P9L11: The article is so short in its present state that I recommend these results are included here.
- Table 2 is redundant and can be safely removed.
- Section 3.2, the comparison of ice concentration RMSE to other models is sensitive to the definition of the domain: the larger the area of open water is included, the smaller the RMSE. This is why the Melsom et al. 2019 (cited in the text) is recommending the use of the IIEE metrics.
- P11l6: “responseble" - > responsible.
- Data availability: the file OCN_h02_CP_2020-06-01.nc is corrupt.

- P12L15: Drvillon -> Drévillon, “Greine” -> Greiner.