

Interactive comment on “TOPAZ4: an ocean-sea ice data assimilation system for the North Atlantic and Arctic” by P. Sakov et al.

Anonymous Referee #2

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General Comments

This study aims to present a description and evaluation of the TOPAZ4 operational sea ice-ocean assimilation system for the Arctic and North Atlantic. However no details of how the system is run operationally are given with no demonstration of the skill of the operational system. Rather, the authors use a multi-year reanalysis to evaluate the quality of the analysis system. However, they undermine the usefulness of the reanalysis by making numerous changes to the data assimilation methodology, in addition to the observations assimilated, throughout the reanalysis. Thus, making it neither straightforward to judge the quality of the analysis system nor the reanalysis itself. The authors claim that this is the only “operational large-scale ocean data assimilation system that uses the ensemble Kalman filter”. Does this not then merit a clear

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evaluation of the quality of the operational analyses? While I agree that reanalysis is a useful technique to evaluate an analysis system, a reanalysis should maintain the same analysis system throughout. If the aim is to evaluate the impact of modifications to the analysis system this should be done in parallel experiments to show clearly the impact. Doing so progressively throughout the reanalysis makes it difficult to separate the impacts of the modifications from interannual variability, temporal inhomogeneity of observations, etc. While I am sensitive to the high numerical cost of running these systems this should not be used as an excuse for a lack of rigor.

In addition to my concerns regarding the evaluation methodology noted above, I have a number of specific issues regarding the metrics provided. In particular, a number of basic evaluations are missing (see below), including spatial errors for the sea ice cover, which is presumably one of the main outputs of this system. Also, the system description is only given with respect to the TOPAZ3 system presented in Bertino and Lisaeter (2008). However, Bertino and Lisaeter (2008) do not provide a detailed description of the system, nor do they present an evaluation of the system performance. Thus, in so far as I could discern, no clear evaluation of the TOPAZ analysis system has yet to be published in peer-reviewed literature.

As such, I recommend that the authors retract the paper and consider rewriting it using a new set of experiments to clearly show the system performance and improvements in TOPAZ4. Specific experiments to show the benefits of the ensemble covariances and bias correction scheme would be of particular value.

Specific Comments

1. Pg 1520, line7: No demonstration of the spatial features of ocean circulation and sea ice cover are provided. Thus this claim is not supported.
2. Pg 1521, line 17: If this is the Arctic MFC shouldn't scales appropriate for the Arctic be mentioned?

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3. Pg 1522, line 4: While I agree that time dependent state error covariances may help to better capture features along the ice edge, they are far from “essential”. If this is an aim of the paper there should be a demonstration of the systematic impact of these covariances near the ice edge. Indeed, only a single example is given in the paper.
4. Pg 1522, lines 6-20: This description is not clear. Figure 2 needs to be complemented by maps of the background state to show how the system is affected.
5. Pg 1524, line 28: The reference should be that of Dee et al. QJRMS, 2011. The ERA-Interim fields are provided on a 0.25° grid, however, the model used is on a T255 grid, giving roughly 79km resolution (see Dee et al, 2011).
6. Pg 1525, line 3-4: Provide details of how incoming radiation is calculated.
7. Pg 1526, section 3.1: This section is difficult to read as it only provides details concerning differences with respect to TOPAZ3 without any basic description of the system itself. To the best of my knowledge, no detailed description of the TOPAZ analysis system exists in the peer-reviewed literature. As such one should be provided here. This is especially important considering that TOPAZ is the only operational ice-ocean EnKF.
8. Pg 1527, line24: Why was a factor of 2 chosen? Would 1.5 suffice? Again, given that this is the only operation ice-ocean EnKF it is important to justify clearly and demonstrate the impact of these choices.
9. Pg 1528, line 25: How were these values chosen? How sensitive is the system to their values? How does this impact on the ice-ocean covariances? Some demonstration of the sensitivity to these choices should be given.
10. Pg 1530, line 17: How are DFS and SRF useful? Fields are presented in Fig. 3 with no discussion of the values or how this is useful to demonstrate the quality of the system. Moreover, if they are so useful why simply show an example rather than a timeseries or an average map, etc.

11. Pg 1532, line 18: Why 0.7? How do the resulting representivity error values compare to the observed variances?
12. Pg 1533, line 1: What is the value of the observation error?
13. Pg 1534, line 23: Ice drift has been assimilation by Stark et al. JGR, 2008
14. Pg 1535, line 25: If the bias estimation system doesn't correct biases then won't this impact on the quality of the analysis at the ice edge where strong non-linearities around the background state are present? For example, if the ice extent is systematically underrepresented (which appears to be the case for TOPAZ4 in summer) the error covariances will also highlight uncertainty (as in Fig 2) in the wrong location. A clear demonstration of the impact of the bias correction needs to be provided.
15. Pg 1537, line 6: The agreement can hardly be considered "good" when the innovation standard deviation is only 50% of the RMSD.
16. Pg 1537, line 22: It would be much more insightful to have run the assimilation of both sst products in parallel to isolate the impact of this change.
17. Pg 1538, line 5-10: This argument is not clear. If there is a seasonality in the mixed layer depth, why not evaluate this against Argo-based mixed layer depth products rather than just speculate?
18. Pg 1538, line 12: Section 3.2 does not indicate the date this starts. Why not start this at the beginning of the reanalysis?
19. Pg 1538, line 15: I can't see any impact. Clarify.
20. Pg 1538, line 18: The previous paragraph commented on the changes in ensemble spread and here it says the spread is "relatively constant".
21. Pg 1538, line 19: I don't agree that there's "no tendency" towards ensemble collapse. Prior to the red vertical line in Fig 5 and 6 there seems to be a negative trend.

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22. Pg 1539, line 9: Where is the bias? Given the biases noted in Fig 5,6 and 7, an indication of where the biases are located should be given, especially for sea ice. Given that this analysis system is used for the Arctic MFS in MyOcean I would have expected a focus on evaluating the sea ice cover in the Arctic. Moreover, Fig. 7 suggests some important seasonal biases in sea ice cover. A systematic demonstration of where and how these occur should be included. This is especially relevant given the assertions made in the introduction that the ensemble covariances are “essential” for coupled ice-ocean data assimilation.

23. Pg 1540, line 11. This looks more like tightening of the Gulf Stream rather than shifting.

24. Pg 1541, line 27: This argument is misleading given that the rossby radius is smaller in the Arctic, and thus the system is not eddy-permitting in the Arctic even if it is for the Gulf Stream.

25. Pg 1542, line 14: These results are not systematically related to EVP. There are many examples of EVP models able to produce thick ice along north of Greenland and the Canadian Archipelago.

26. Pg 1542, line 22: Why are these diagnostics not shown? Fig. 11 shows drifters in the Gulf Stream and the text states that the Arctic Ocean is the main focus (Pg 1541, line 27). Moreover, the text states that this is the first system to assimilate ice drift. Should it not then demonstrate the result of this?

27. Pg 1542, Sec 5.4: What about the spatial distribution of sea ice concentration errors? Given that the Arctic is the focus of the system and the claims made regarding the need for ensemble error covariances a systematic demonstration of effects along the ice edge needs to be presented.

28. Pg 1543, Sec 5.5: Again, given the Arctic focus a more detailed analysis of the representation of temperature and salinity in the Arctic needs to be included. With the

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International Polar Year, there are numerous in situ datasets to use, in addition to more basic evaluations of model drift and how assimilation corrects this (or not).

29. Pg 1543, line 20: Showing a single year comparison is hardly sufficient given the large number of changes applied to the system during the reanalysis.

30. Pg 1545, line 16: Given the initialization problem how well can the data assimilation constrain the system? This in itself is an interesting question that could have been addressed here with parallel experiments.

31. Pg 1548, line 13: As noted above, this claim is unsupported by the demonstration provided. Only one example of the effect is given with no systematic evaluation of the impact over time or a comparison with an analysis produced without it.

32. Pg 1550, line 28: The claim regarding the circulation in the Arctic should be revised as nothing is shown of the circulation in the Arctic itself and Fig. 15 suggests the system has difficulty even reproducing climatology.

33. Pg 1550, line 29: What does “almost similar” mean?

Technical Corrections

1. Pg 1531, line 12: should read “spun up”

Interactive comment on Ocean Sci. Discuss., 9, 1519, 2012.

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