Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-298-RC1, 2018

Geoscientific Model Development Discussions
Open Access
EGU

Interactive comment on "Data assimilation cycle length and observation impact in mesoscale ocean forecasting" *by* Paul Sandery

Anonymous Referee #1

Received and published: 3 May 2018

General comments

This paper investigates the impact of data assimilation window length on a short range ocean forecasting system. Current operational ocean forecasting systems use a range of different assimilation windows from 6 hours to 10 days, and yet there has been little work to look at the impact of different assimilation windows in one systems. This makes the topic of the paper novel and relevant for the ocean modelling community. As we move towards coupled ocean-atmosphere data assimilations the length of the time windows used in the ocean are likely to reduce to be consistent with atmosphere assimilation windows. This study may therefore be of particular interest to those developing coupled data assimilation systems. However, the results in the study would be more significant if the experiments were not using a synchronous data

Printer-friendly version

Discussion paper

assimilation method. The author should address this explicitly earlier in the paper. It's not clear that you couldn't just achieve similar improvements through asynchronous data assimilation.

The innovation statistics (Figure 6) from the 7 day forecasts are a significant result and strongly support the use of a 1 day window over a 3 day window in this system. However, the paper over emphasises the results from the mean and absolute mean increments. As alluded to in the background section, it is difficult to draw conclusions from comparisons of mean increments alone and the current organisation of the paper puts too much weight on the increment results. It would strengthen the interpretation of the assimilation increments if they were discussed within the context of the forecast statistics. I think that the paper could be substantially improved by presenting the innovation statistics first, as the key result, and providing the increments as supplementary evidence.

Throughout the paper the author states that the differences in mean absolute increments suggest observations are having a greater impact with a one day window. I think that you need to be careful with how this statement is used. A reader may mis-interpret this as meaning that larger increments automatically lead to an improved system. Presenting this result within the context of improved forecast innovations would make the statement more robust. The author should also clarify that larger increments do not necessarily mean a better data assimilation system.

In places the paper seems to lack details or justification. For example, the choice of forecast period for assessment or the choice of assimilation windows for the experiments. And is some places the paper seems to make contradictory conclusions about the results (particularly in relation to the mean increments). The paper should be modified to give a clearer narrative.

I think that there are some errors in the interpretation and description of the results. More details are given in the specific comments.

Specific comments

Abstract:
Page 1, line 4-5. The mean increments look to be approximately 1/3 smaller in the 1-day experiment, which is what you would expect for linear error growth. I don't think that you can make any statements about bias here without consideration of the error growth throughout the assimilation window. This statement is also inconsistent with your discussion of the mean increment results on page 4, line 8.

Page 1, line 9. I don't remember seeing any statistics which showed that the biggest improvements were in the Western Boundary currents.

Background:
Page 1, line 21-22. Over fitting is not just a problem for long data assimilation windows. In fact a long data assimilation window with good super obing or thining could produce smoother increments and be less influence by noise in the observations.

Page 2, lines 11. This paragraph is a bit confusing. It seems to argue that the mean increments are not a good indicator of bias, which contradicts your result on line 5 in the abstract. I didn't really understand what the purpose of this paragraph was. To justify the use of mean absolute increments?

Page 2, line 29. What is the forecast range of OFAM3? This might give more context to your choice of forecasts.

Page 2, line 30. Have you specified the horizontal resolution anywhere? This is important since the focus of the paper is mesoscale forecasting.

Page 3, line 10. "EnKF-C (Sakov, 2014) with Ensemble Optimal Interpolation (EnOI)" is not general terminology for a data assimilation scheme. This name is too

specific to be used without context. In reality, I think you are actually using EnOI?

Page 3, line 14. More details about the observation operator would be useful. You should also define the linear observation operator in equation 1.

Page 3, line 15. More details could be given on the data assimilation system, e.g clarifying that this is a synchronous data assimilation scheme, defining when in the time window the increments are applied (presumably the middle).

Page 3, line 23. You discuss the impact of super obing before introducing that you have used super obing. I think the order should be switched round.

Results:
Page 4, line 8. Seems contradictory to the abstract (page1, line 4-5)

Page 4, line 11. The MAI from the 2 experiments are only directly comparable if the forecast error growth is linear. It is worth discussing this here. Your results in Figure 7 should give some indication of the forecast error growth. From these figures it looks like the forecast error growth in the first day is a bit larger than subsequent days.

Page 4. It could also be useful to consider the variability in the increments.

Page 4, line 13-14. Would you expect the fact that you are assimilating more

Printer-friendly version

Discussion paper

observation in the 1-day experiment to also impact on the magnitude of the increments?

Page 4, line 16. What is the temporal resolution of the kinetic energy outputs in Figure 7.

Page 4, line 27. But also the model is only free running for 1 day before the next increments is applied, so less time to drift.

Page 4, line 28. Wouldn't the eddy kinetic energy be a better representation of the mesoscale energy?

Page 4, line 29. If you are going to claim that the model kinetic energy is closer to the observations, you should also show the observation kinetic energy. Comparing the results to the observations would also give more context for the difference between the two data-assimilation experiments. From the current Figure it's not clear how significant this increase in Kinetic Energy is.

Page 5, line 10. "mean forecast bias is more significantly reduced for SLA, SST and sub-surface temperature" - the mean forecast bias for SLA is actually slightly larger in the 1-day experiment in Table 3.

Page 5, line 13. Are the increments applied in the middle of the time window? Could you clarify this.

Page 5, line 10-11. Are the MAD statistics in Figure 6 calculated in the same way (using the same forecasts) as those presented in Table 2? Why do look so different? For example, the subsurface temperature MAD at day 7 looks to have a value of approximately 0.625, but in table 2 it's given as 0.603.

Conclusion:
Page 5, line 25-26. "Further 1 year runs of the two systems with an improved model using renanalysis bulk flux forcing have confirmed (not shown) that the 1-day cycle provides improvements in forecasting the mesoscale circulation in the western boundary current regions." Is this the evidence for the statement in the abstract that the biggest improvements are in the western boundary current region? You should show this result if it forms part of your main conclusions. It would, in general, be good to see more results focused on the mesoscale region given that the focus of the paper is mesoscale forecasting.

Technical Corrections

Page 9, Figure 1. There is quite a lot of irrelevant information on this plot which makes the key information difficult to see. The current vectors make the figure appear noisy in print, and they are not discussed anywhere in the paper. It would be best if they were removed.

Page 10, Figure 4. It would be better to use a sequential colour bar for Mean Absolute Increments.

Page 11, Figure 6 caption. typo throughout.
Page 11, Figure 7, (d). It's very difficult to see the results from the 3-day experiment for salinity.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2017-298,

2018.