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Ocean Science Discussions



# *Interactive comment on* "Obstacles and benefits of the implementation of a reduced rank smoother with a high resolution model of the Atlantic ocean" *by* N. Freychet et al.

**Anonymous Referee #2**

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Review of the manuscript osd-9-1187-2012 "Obstacles and benefits of the implementation of a reduced rank smoother with a high resolution model of the Atlantic ocean" by N. Freychet, E. Cosme, P. Brasseur, J.-M. Brankart and E. Kpemlie.

1. The scheme.

The scheme of the smoother detailed in Sec. seems to be equivalent to the EnKS (Evensen and van Leeuwen 2000, Evensen 2003, Evensen 2009). It can be summarised as applying the ensemble transforms obtained with the EnKF back in time. This is a generic, scheme independent, formulation.

If the above is correct, then I suggest that the authors drop most or the whole Section 2



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and concentrate on the oceanographic aspects of the study. Otherwise, it is necessary to detail the differences with the existing approaches.

2. Experiment settings.

In contrary to the suggestions that could be drawn from the title, the study concentrates on results of a specific experiment with very lenient settings, rather than trying to replicate the conditions encountered in practice. The FALSE and REF runs are conducted with identical forcing; both the model and observations are perfect; and the observation network is dense and on a regular grid.

The RMS difference between the free run and assimilated runs seems to be stable in time, which points either to a non-chaotic system or to nonlinear saturation. Assuming that it is a result of nonlinear saturation, the system perhaps needs more time for stabilisation than it is given in the experiment. Analysing results obtained with the smoother on day 2 after the beginning of assimilation is likely to be premature, as the system is probably still in the transient and/or nonlinear regime.

3. Observations.

The study uses perfect observations, without any justification. This is completely unrealistic and unnecessary for the goals of the study.

4. Dynamical consistency of the analysis.

The claim that the smoother is able to produce analysis "more consistent with the dynamics" seems to be one of the main results of the study. This indeed can only be possible in a nonlinear system, as in a linear system a smoothed and filtered solutions that assimilate the same observations do coincide. This is an interesting observation, but it is based on a single experiment, and this is absolutely not sufficient to justify the general conclusion, particularly when there are no theoretical arguments presented to support it.

5. The smoother based on a static filter (section 6).



OSD

9, C336–C338, 2012





This is an interesting scheme that deserves a more thorough investigation. In particular, it would be valuable to compare it performance in experiments with small models, both linear and nonlinear, as well as to get some theoretical insight on expectations of performance of such system. I do not think that this material in its present form is ready for publication.

Conclusion

In my view the paper can not be published in its present form. The importance of the theoretical part is not clear, the experimental settings are doubtful, and the conclusion about better dynamic consistency of the smoother compared to the filter is not substantiated. I recommend rejection.

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9, C336–C338, 2012