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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Data assimilation experiments using the diffusive back and forth nudging for the NEMO ocean model" *by* G. A. Ruggiero et al.

Anonymous Referee #2

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General Comments:

The manuscript "Data Assimilation experiments..." by Ruggiero et al. is in my opinion not suitable for publication in the present form. The topic is surely interesting and my feeling is that their results could be correct and interesting but the paper is very unclear. I read it also with the help of another researcher expert in the field of DA and several points prevent us to understand the correctness of the approach used.

After the authors clarify the points we raise the paper could be reconsidered for publication.

Specific Comments:

Here is a list of the main issues:

a) the main point regards Eq.(6) and (7). The authors only cite a personal communication that should explain why after an infinite number of iterations their algorithm should converge to a trajectory calculated without the diffusive and the nudging term. We think that this point is important in driving the reader in the comprehension of the results presented. So the authors should give some theoretical justifications and verify it in their results.

b) It is not clear the behavior of the diffusive term in the backward integration. We understood that this term eliminates the small scale structures both in forward and in backward integration. The sign indicated in (4) suggests this intepretation but some sentences at pag. 1080, line 15 and following let the reader quite confused.

c) It is completely unclear what are the different kinds of K's used. At Pag. 1080 it seems (we use latex notation) that K =k HˆT Rˆ{-1}, then the authors speak about a "K based on the PLS regression model", somewhere else (e.g. Pag. 1083) it seems that after the DBFN the PLS regression is used. We strongly suggest the authors to make the technical details of the different experiments of their method clear.

d) We agree with the other referee that the relative error is not a good measure of the difference between two stetes. We suggest the use of the RMS or of the RMS normalized by the standard deviation.

e) This point regards the DFBN technique: the authors state the in absence of observations the iterations converge to an homogeneous state. This means that after several iterations the analysis is completely independent of the dynamics equation (F(x) in Eq.(2)). We think that with no diffusive term, after several iterations, the model is in some sense "forced" to become equal to the observations in the observed points. Reading the manuscript we have understood that the authors think that with a balance of the diffusive and nudging terms, the trajectory should converge to an actual trajectory of the model without diffusion. If this is correct the authors should better clarify

and also prove that this behavior holds, at least in the model under examination.

We found very difficult to follow the results section. We still think that the paper could bear interesting results but it should be made clearer by the authors.

Techical corrections:

In some figures (e.g. Figure 15) there is a label "BFN". Do you mean DBFN or is another algorithm without diffusion? If yes please clarify.

Interactive comment on Nonlin. Processes Geophys. Discuss., 1, 1073, 2014.