Ocean Sci. Discuss.,
doi:10.5194/os-2016-35-RC1, 2016

Ocean Science Discussions

Interactive comment on "A Hybrid Variational-Ensemble data assimilation scheme with systematic error correction for limited area ocean model" *by* Paolo Oddo et al.

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Received and published: 27 June 2016

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1 General comments

The manuscript investigates application of a hybrid variational-ensemble approach to a limited area model with sub-mesoscale resolution. So far constraining sub-mesoscale features remains largely out of reach of contemporary ocean forecasting with the standard set of remote and in-situ observations. This study investigates sub-mesoscale

forecasting in situation with dense in-situ observations in a relatively calm region.

The hybrid approaches become popular in atmospheric and ocean forecasting due to increasingly clear understanding of limitations of 4D-Var systems due to lack of mechanisms to carry information forward from previous cycles. In my view, the hybrid approaches, while succeeding in adding some degree of flow dependence to the background covariance, still remain largely empirical and lack consistent formulation. As a consequence, to the best of my knowledge, there still no published experiments with small models that would convincingly demonstrate advantages of hybrid systems over much more simple and consistent EnKF systems.

Below I will list some major and minor issues I see with the manuscript and give recommendation in the conclusion.

2 Major Issues

1. **Equations (6), (7) and (8).**

   One problem with mixing covariance matrices is that there is no good way to incorporate it in a consistent way into the optimisation problem. In particular, the claim that linear mixing (6) can be consistent with the cost function (8) is generally wrong. The framework (6-8) assumes $(\mathrm{x}_c + \mathbf{x}_e)^\mathsf{T} [\alpha\mathbf{B}_c + (1-\alpha)\mathbf{B}_e]^{-1} (\mathbf{x}_c + \mathbf{x}_e) = (\mathbf{x}_c)^\mathsf{T} (\alpha\mathbf{B}_c)^{-1} \mathbf{x}_c + (\mathbf{x}_e)^\mathsf{T} [(1-\alpha)\mathbf{B}_e]^{-1} \mathbf{x}_e$. This implies $\mathrm{x}_c \perp \mathbf{x}_e$, $\mathbf{B}_c \perp \mathbf{B}_e$, which is generally not true and difficult (or even impossible) to impose in practice. (The same applies to Eqs. 4-6 in Wang et al. 2007.)

   If the above is true, then the manuscript must be modified accordingly.

2. **Equation (14).**

   It seems to me that it writes "innovation = innovation error", which is wrong.

3. **Ensemble update.**

   Due to the lack of rigorous formulation most hybrid methods employ empirical approaches for maintaining the ensemble spread. It seems that the manuscript does not tell explicitly how the ensemble members are updated. This is important for understanding the method and should be described.

   Further, on p. 10, l. 12-23 it is stated that the ensemble maintains spread due to observations and otherwise collapses due to the deterministic model forcing. This is somewhat contrary to what might be expected. It seems to me that increasing the number of observations in a consistent DA system should *always* reduce state error, that is always reduce the ensemble spread. Concerning the model forcing, in the context of a mainly stable forcing-driven model it is probably a pre-requisite to perturb forcing for ensemble members to match the corresponding uncertainty.

3 Minor Issues

1. P. 2, l. 1: suggest replacing "not feasible to sample" by "not feasible to observe".
2. P. 2, l. 24: suggest replacing "EnKF" by "traditional EnKF".
3. P. 6, l. 19: suggest replacing "model bias error" by "model bias".
4. P. 7, l. 3: suggest replacing "background error covariances" by "background errors".

4 Conclusion and recommendations

The manuscript addresses a difficult and interesting ocean forecasting problem. Some of the statements and approaches can be viewed as arguable (or, in regard to the

EnKF, outdated), but this is what scientific literature is for. The methods used are in my view largely empirical, and again there is nothing wrong with that, as long this is clearly stated up-front.

Concerning the results of the DA experiment described, they probably leave a lot of space for improvement, and this itself is one of the important outcomes of the manuscript.

One line of statements that I tend to disagree with is that "it is difficult to run full EnKF with a large number of members" (p. 3, l. 14-17). Not in the year 2016, and definitely not with a 240 x 240 x 90 model.

Overall, I believe that the manuscript will be interesting and useful for the ocean modelling and ocean forecasting communities. I recommend publishing it in Ocean Science after fixing the major issues listed above, which probably amounts to a major revision.

References

Wang, X., C. Snyder, and T. M. Hamill, 2007: On the theoretical equivalence of differently proposed ensemble-3DVAR hybrid analysis schemes. *Mon. Wea. Rev.*, **135**, 222–227.