Ocean Sci. Discuss., 6, C106–C108, 2009
www.ocean-sci-discuss.net/6/C106/2009/

Ocean Science Discussions

Interactive comment on "Application of a hybrid EnKF-OI to ocean forecasting" *by* F. Counillon et al.

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Received and published: 18 May 2009

Review of "Application of a hybrid EnKF-OI to ocean forecasting", by Counillon et al.

The authors present a description of a hybrid EnOI - EnKF data assimilation system. They include a description of the theory behind the approach, a series of results using a small quasi-geostrophic model and a realistic general circulation model. The authors clearly demonstrate the benefits of the hybrid approach. Furthermore, the authors explain why the hybrid system works better - with reference to the ensemble-based covariance structures, and the increase the in sub-space of the ensemble. I've included some comments below that the authors might address before this paper is finalised, but I recommend that the paper be accepted. It's a good contribution, with important implications for both operational oceanography and Numerical Weather Prediction.

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Interactive Discussion

Discussion Paper

Comments:

Not enough is said about how the static ensemble is generated. On page 657, line 19, the authors state that a "static ensemble may contain . . .", but I can't see where they describe how they generated the static ensemble for the cases presented in this paper. This is important.

The authors need to be clear about the meaning of the term "diverging". I think they use it to refer to the errors - that is, the errors diverge, or grow (see line 26 of pg 661 for eg). But for an ensemble, it could mean the ensemble diverges, or for the application, it could mean that the circulation diverges. I suggest the authors adopt a different phrase. They tend to say "the method diverges". Perhaps they simply mean the system fails.

re: equation 4 - did you test what happens if you just augment the stationery ensemble with a few dynamic members? This would expand the sub-space of the ensemble (following the arguments on pg 656, following line 18), but would eliminate the need for the "blending factor" beta. Also, assuming the number of dynamic members is always small, c_d will always suffer from sampling error - but augmenting the static ensemble should only act to improve sampling error due to finite ensemble size. I suspect the ratio of the number of static members (n_s) to the number of static + dynamic members (n_s+n_d), (n_s)/(n_s+n_d) $\sim$ optimal beta. Note the the "optimal" beta for the HYCOM case is 0.95. n_d = 10, n_s = 122, so (n_s-n_d)/n_s = 112/112 $\sim$ 0.92 - pretty close to just augmenting the static ensemble with the dynamic members.

The results in Figure 2 are quite unsatisfying. I suggest they be excluded. Figure 3 gives the same story without the uncertainty. The configurations that succeed/fail seem quite random. There's not a very clear trend for configurations that succeed or fail. For example, for EnKF-OI (beta = 0.8) the system works ok when the inflation is <1.2, equals 1.45 or 1.55, but fails if inflation is 1.25-1.4, and fails for 1.5 and 1.6. Perhaps the experiments weren't run for long enough, so the statistics still suffer from sampling error. Figure 3, when more dynamic members are used, doesn't have the

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same problem.

On pg 662, line 18 the authors state that "The flow dependent covariance is expected to be more consistent physically than a static covariance." I don't agree with this. Perhaps the flow dependent covariance could be more optimal, but I'd expect it to be less dynamically consistent. Recall that the static covariance is generated from a long run that is not frequently updated, or initialised. The static ensemble is therefore dynamically consistent. By contrast, because the dynamic ensemble is regularly updated, or initialised, it includes some noise associated with the shock of the initialisation. The dynamic ensemble is therefore probably less dynamically consistent than the static ensemble. I therefore don't accept the explanation in the paragraph following line 18 of pg 662. Perhaps this is why these results contradict those of Wang et al. (2007) - see line 28 of pg 662.

On pg 672, line 15, the authors say that as the size of the dynamic ensemble is increased, the optimal beta (blending coef) decreases - that is the dynamic ensemble is given more weight. But the authors also find that the hybrid system always outperforms the dynamic system. How can these conclusions be reconciled?

Minor comments:

- the statement on pg 656, line 10, that the cost of the assimilation will remain negligible is not accurate. This depends on many factors that vary with each applications - the number of obs assimilated, the localisation radii, the model resolution etc. This statement should be removed. - The captions for Table 1 and 2 need to include the application's they are valid for. ie Table 1 for QG model, Table 2 for HYCOM. - it's meaningless to include labels 5.2, 5.4 etc on fig 7b. The values for the horizontal axis are run numbers. For clarity, you could extend the horizontal range to 4.5-7.5. - is the sentence on pg 672, line 11 correct?

Interactive comment on Ocean Sci. Discuss., 6, 653, 2009.