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Ocean Science Discussions

Interactive comment on "Dynamically constrained ensemble perturbations – application to tides on the West Florida Shelf" *by* A. Barth et al.

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In this paper, the authors investigate the problem of constructing appropriate random perturbations of ocean model input data (initial condition, forcing, parameters) for use in ensemble Kalman filters. The perturbations are drawn from a Gaussian probability distribution, with zero mean and given covariance matrix. The purpose of the paper is to propose ways of constructing appropriate covariance structures, which include a set of weak dynamical constraints and/or the anisotropy of the land boundaries. As an example, they show how a certain type of homogeneous and isotropic covariance structure can be made anisotropic by including appropriate boundary conditions on the land boundaries, and how this covariance structure can be further constrained by linearized shallow-water equations. As an application, the method is then used to compute perturbations of the tidal motion in a ROMS model configuration of the West Florida Shelf, showing that linearized shallow-water constraints (rather than geostrophy) is appropriate to produced balanced perturbations and to avoid spurious barotropic waves.

As the authors state, this problem is not only important for data assimilation but also, more basically, to get a better understanding of the various sources of model uncertainty. Their application is useful because the design of a consistent assimilation system with a model resolving tides is still an open question (mainly for the reason that they discuss). Moreover, although none of the ingredients of the method are really novel, they are put together in an unprecedented way (to my knowledge). For these reasons, I believe that this paper is suitable for publication in Ocean Science. I only make a few comments that the authors may find useful to improve the clarity of the paper.

1) It would be helpful to mention in the introduction that the perturbations are drawn from a Gaussian distribution, since it is an essential assumption in the development of the method. In the present version of the introdutcion, this is only implicit in the 1st sentence of the 2nd paragraph. It is indeed because of the Gaussian assumption that uncertainty can be completely described by the covariance.

2) It would also be useful to make a clear distinction between the method that is proposed for constructing an appropriate covariance structure (which is the core of the paper) and the method for drawing a perturbation from a Gaussian probability distribution with given covariance (which is anecdotic in terms of method, even if certainly essential in terms of numerical cost, see next comments).

3) The main information that is lacking in the paper is certainly an information about the numerical cost of the method. As it is explained in the paper, the method requires explicit square root decomposition of the resulting covariance matrix, a computation which quickly becomes excessively expensive for large size systems. Please add an explicit mention of the cost of every elements of the algorithm as a function of the size of the perturbation vectors, the number of perturbations to draw,...

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4) Concerning the drawing of random perturbations from a Gaussian distribution with given covariance, please note that any square root of the covariance matrix can be used, as indicated for instance in the appendix of the paper: "Fukumori, I., A partitioned Kalman Filter and Smoother, Monthly Weather Review, 130, 1370-1383, 2002". Cholesky decomposition for instance would provide a cheaper square root.

5) Concerning the construction of the covariance matrix, you explain in detail why the dynamical constraint term is an essential component of the inverse covariance matrix (for instance, in equation 3), but you give little explanation as to why the smoothness component provides an appropriate reference covariance structure. This is the kind of parameterization that is assumed when little is known about the covariance structure (except a correlation length scale). A short explanation for this particular choice (which certainly does not fit any kind of purpose) would be welcome.

6) In the "combined cost function" (equation 3), it is necessary that the subspaces defined by the dominant eigenvectors of the individual components (the longer principal axes of their respective covariance ellipsoids) are not orthogonal to each other. If they were, the method would only produce small perturbations not satisfying any of the required properties (because such perturbations would not exist). In the examples, the method works correctly because there exist perturbations which are both dynamically constrained and smooth (approximately at least). Maybe this is worth a word of caution.

7) In the construction of an anisotropic covariance structure constrained by the land boundaries (section 3), little information is given about the numerical computation of the D matrix. How is it computed in practice? How are the boundary conditions applied? Moreover, the disucssion would be more logical if you describe first the isotropic and homogeneous solution, and afterwards how this can be modified by taking into account the presence of land boundaries.

8) In the description of the model in section 4, the name of the region is missing (you only use the undefined acronym WFS): in the present version, it is only given in the title

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and at the end of the introduction. You could also give a few more elements about the model behaviour to help understanding the results of section 6.

9) The conclusion looks like an abstract of the paper, without any intrepretations or perspectives. I think that it would be worthwhile to rewrite it carefully to give a better view of the output of this paper.

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