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Nonlinear Processes in Geophysics
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Discussions

Interactive comment on "Hybrid Levenberg–Marquardt and weak constraint ensemble Kalman smoother method" *by* J. Mandel et al.

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Hybrid Levenberg–Marquardt and weak constraint ensemble Kalman smoother method

Mandel et al, submitted to NPGD

Review, July 3 2015

Reviewer: Emmanuel Cosme

This paper presents a new approach to fixed-interval data assimilation, i.e. the data assimilation problem that 4DVar and fixed-interval smoothers try to solve. Similarly to the incremental 4DVar, the proposed algorithm is composed of an outer loop and

an inner loop. In the outer loop, a model trajectory is computed. In the inner loop, an ensemble smoother is run for the increment, instead of explicitly minimizing a cost function as in the incremental 4DVar. The algorithm is implemented with the Lorenz 63 system and a 2-layer quasi-geostrophic model.

Overall, I believe the idea brings a significant increment to the data assimilation science, and the paper must be published. The method is nicely presented, with a surprising conciseness given the technical complexity. However, I think some pieces are missing in the present version of the manuscript. I list them below, along with other comments and suggestions of lesser importance. The backbone of several points below is that the paper does not provide the necessary information to help the reader determine whether the method is the appropriate one for his/her own problem, how difficult the implementation is, etc.

1. About the numerical experiments and results

1.1 For the proposed method to be considered as a serious alternative to others, its performance must be compared to others, like the standard EnKF (or ETKF), the SIR filter, on a systematic basis. See for example the works of Oke, Sakov, Bocquet, to cite only a few names.

1.2 The diagnositcs must be statistically robust. According to the authors cited previously, and also based on my personal experience (Metref et al, NPG 2014), the diagnostics of DA with the Lorenz 63 system are robust if they are based on 100,000 assimilation steps at least, excluding spin-up.

1.3 For the QG experiments, I find it quite reductive to limit the diagnostics to the value of the objective function.

1.4 Several statements are not motivated, unclear, or inaccurate: p.882: why choosing 8 iterations? p.882, lines 23-25: "the objective function decreases with iterations". Not for τ = 0.1 and 0.01 where the function increases at the last iteration. What happens with more iterations? This requires further investigations. p.883, line 8: "for the first iteration, the best decrease in objective function is obtained when τ=1": actually, it increases from 1 to 2. Is the first iteration before that? What is the initial value of the objective function?

2. About the presentation of the method

2.1 More must be said about the computational complexity and the implementation complexity. Of particular importance is the increment of work from an EnKF, for example. An what are the assets of the method, compared with others?

2.2 Algorithm 3: If I understand it well, the algorithm consists in: - compute the full model trajectory from x_0; - Apply an EnKS on z, which dynamics are governed by the model linearized at the previously computed trajectory; - update the trajectory, re-linearize the model (and H), and iterate I understand that the method is proposed as an improvement (or another way to solve the inner loop) of the incremental 4DVar. But I do not see where the "variational" part of the algorithm is, other than in the perturbations z used for the EnKS. Meteorologists and oceanographers are used to speak about variational methods when the objective function is explicitly minimized to reach the solution. Here, it seems that the calculation of the objective function is not essential to solve the problem, but is only used as a diagnostic. To me, it looks more like a hybrid of a "two-step" smoother and an EnKS (see for example Cosme et al, MWR, 2012, but I do not request you to cite my work). Could you clarify that?

2.3 Algorithm 3: Following the previous point, if I am right and if you agree, the name "EnKS-4DVar" should be modified.

2.4 I wrote earlier that the presentation was clear and concise, and I like it. But it requires a significant amount of background in data assimilation to understand the paper. Perhaps the authors could guide the reader toward some appropriate references for his/her self-education if necessary (more than in the present version).

3. Minor comments, typos, etc

p.872, Beginning of section 3: the notation z for the states (line 5) is confusing after notation x in the previous section. Perhaps you can anticipate this by stating shortly why you adopt this notation (a short statement in brackets should be enough). When we understand the rationale of this notation (later in the text) Equation 5 becomes all the more confusing because it involves a nonlinear model. I am sure the authors will find a smart way to make things a bit clearer.

Equation 6, 19, and elsewhere: I know it is common to perturb the observations y, but the usual observation equation ($y=h(x) + \epsilon$) says that $h(x)$ should be perturbed instead, as in the model equation. This reverses the sign of the perturbation. Of course, it is equivalent with centered and symmetric noise as it is here. Since it is (unfortunately) common to present things that way, this changes of sign is not a strong requirement.

Introduction and almost everywhere: Although the Kalman filter is indeed due to Kalman (1960), optimal linear smoothers are not. "Kalman smoothers" should be replaced by "Smoothers based on Kalman's hypotheses". But I agree this is quite cumbersome. If the authors does not find an easy way around this, I do not make it a strong requirement.

Equations 14 and before 11: should the fisrt x be replaced by a z?

p.875, first line: perhaps a reference to Eq. 4 rather than Eq. 2 would be more appropriate.

Figures 4 and 5 are not used in the discussion. They could be removed.

p.887, lines 16-17: "it is capable of handling strongly nonlinear problems". I tend to disagree with this statement. The Lorenz 63 system fully observed every 25 steps is considered "weakly nonlinear" (Sakov et al, MWR 2012; Verlaan and Heemink, 2001; Metref el al, 2014) and the QG model with grid meshes of 300 km and observed with a ratio 1/32 is probably not very nonlinear (I do not have a reference for this).

p.888, lines 2-5: the advantages of using varying τ's are only speculative from the results presented. They are not shown.

p.888, line 7: the QG model is one of the simplest models of the atmospheric circulation. It cannot be considered standard, because it is rarely used for meteorological applications.

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 865, 2015.