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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Quasi static ensemble variational data assimilation" *by* Anthony Fillion et al.

Anonymous Referee #2

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———————————- General Comments ———————————-

The aims of this paper is to look at the application of the quasi-static scheme developed by Pires et al (1994) to ensemble variational assimilation algorithms. This extension is useful for large data assimilation windows with chaotic non-linear models, as standard approaches fail to find the global minimum of the cost function.

The paper is restricted to the case of low order models with perfect model assumption. This limitation seems restrictive and this clearly diminishes the scope of the results. Indeed, the original papers on quasi-static variational assimilation (QSVA) were at least partly addressing the case of higher dimensions and model error (e.g., Swanson et al. 1998).

The application of QSVA together with ensemble formulation has received some interest in the community (e.g., Goodlif et al. 2015) but the paper is in my opinion missing a discussion on how the results compare with the one of Goodlif et al. 2015.

The paper is sometimes hard to follow : in a first section, theoretical developments are used to compare the performance of 4D-Var and of the IenKS in a linear and highly simplified context. This is interesting but then 4D-Var is dropped out of the DA schemes that are considered and it is not obvious why. The limitations of the standard IEnKS with increasing DA windows are well illustrated and lead to section 3 with quasi-static versions of the IEnKS compared to standard ones. Here, a novel algorithm is discussed, the MDA. I would recommend to focus on IEnKS only; dropping the 4D-Var and the MDA versions to make the paper more focussed.

Figures are generally clear, with the exception of Fig. 10 and 11 where the third panel (about the number of ensemble propagations) is put on the same "level" as he two other ones (RMSE) which is confusing at first glance. There is yet a general problem with the colours that do not render well in gray scale and thus likely confusing for colour blinded people : the authors may consider using better colour maps for this purpose.

Overall, the paper may be suitable to publication only if those concerns are properly dealt with, which is why I would recommend a major revision.

———————————- Specific Comments ———————————-

[1] I would recommend that the paper is more clear about the limitations of the study. It is definitively in the text but not in the title and in the abstract. I would mention the perfect model assumption in the abstract. Also, the title is too general. I would make it more specific, for instance "Performances of the quasi-static formulation of the iterative Kalman Smoother on low-order models", or "A quasi-static version of the strong constraint iterative Kalman Smoother" for instance. [2] Page 2, line 18. Is it a known fact that the number of local minima increases exponentially with the data assimilation window ? If yes, please provide a proof or quote, if not please be more

vague.

[3] Page 2, lines 20-25. There are other methods that address the convergence of minimization despite the non-linearity of the operators by using globalization methods, even published by the authors (e.g. Preconditioning and globalizing conjugate gradients in dual space for quadratically penalized nonlinear-least squares problems by Gratton et. al.). Please add and comment references with alternative minimization algorithms to address non-linearity.

[3] Page 2, last paragraph. You mention that your paper is designed to be a “more complete analytical and numerical investigation”, but you do not comment on the main results of the paper you are citing. Please provide a better discussion of your paper with the existing literature.

[4] Page 3, line 19. Your paper is about low dimensional and perfect model, such that I would change the sentence to “not meant to improve high-dimensional nor imperfect models”.

[5] Page 6, line 16 : please detail in which sense the inverse square root of the matrix is taken , as it is ambiguous.

[6] Page 11, line 5 : I do not understand the qualitative explanation that is given, please reformulate. , [7] Page 20 : the description of Figs. 8, 9 and 10 is very short, with only a few lines to comment 10 panels. Please consider discussing more the resultts or simplifying the figures by showing only what you tell.

[8] Page 23 and 24 : I do not understand what the number of ensemble propagation is, and the paper is missing an explanation of why in Fig 11 we observe different behaviours between L63 and L95, and also why it has non-monotonic evolution with parameter NQ.

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