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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Ensemble Variational Assimilation as a Probabilistic Estimator. Part I: The linear and weak non-linear case" *by* Mohamed Jardak and Olivier Talagrand

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We thank M. Bocquet for his comments and suggestions. We give below a first response to some of these. The referee's comments and suggestions are on top of our responses.

The weak points of the paper are:

1. An insufficient discussion in the introduction of the fact that we already know that naive RTO / EDA / EnsVAR is not (perfectly) Bayesian and proper references to Oliver et al. (1996); Bardsley et al. (2014); Liu et al. (2017) should be added or better referred

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to.

Thanks. We will mention these papers and discuss them in the perspective of our two papers.

2. An abstract that – very surprisingly given how well the paper is written – does not faithfully reflect the findings of the paper.

We do not fully understand what the referee means. But see our response below to the reviewer's specific remark 3.

Specific remarks, in connection, or not, to the previous remarks are:

3. Abstract, line 10-14: the emphasis is on the performance (accuracy) of the method compared to, e.g., the EnKF. I do not believe that this is wise in the absence of a proper cycling with which the EnKF could shine. I do not understand why the emphasis is not on the discussion of the Bayesian (or not Bayesian) trait of the method and the quality of the updated ensemble, which is the strong point of this study.

By writing performance, we did not mean specifically numerical accuracy. We meant global performance of the algorithms under comparison, and primarily their performance as Bayesian estimators. We will modify the wording so as to avoid any misunderstanding. As for cycling of EnsVAR, that is certainly an important and interesting point, but we do not think it is directly relevant to our paper, since we have compared the three algorithms (EnsVAR, EnKF and PF) over the same assimilation windows.

8. line 80: "The present work is devoted to the study of that algorithm, and of its properties as a Bayesian estimator..." Precisely! That is why the abstract should reflect this point instead of focusing on the accuracy.

See previous point 3.

9. line 103-104: The connection between RTO and EDA as used in geophysical data assimilation has first been made, put forward and discussed in Liu et al. (2017) (and

much more1). This must be mentioned here. (Incidentally this is how the authors of the present manuscript became aware of Bardsley et al. (2014).) Moreover, Oliver et al. and were the first to discuss this problem in 1996 (Oliver et al., 1996), which is something that Liu et al. (2017) recalled. You must cite this reference as well.

See 'weak point' 1 above.

12. line 125, before section 2: As already discussed (and illustrated) in Oliver et al. (1996); Bardsley et al. (2014); Liu et al. (2017), RTO/EDA (hence called naïve RTO in Liu et al. (2017)) produces a biased nonlinear sampling. This should be briefly mentioned in the introduction as this is an established and published fact, important to your paper.

Again, see 'weak point' 1 above.

13. line 133: "data operator": why invent a new name when there exist "observation /forward / Jacobian / source-Receptor operator" in geophysical data assimilation?

We want to stress here (see ll. 136-140) that the data vector z contains all the information to be used for estimating the state vector x (physical observations, complete or partial background(s) or a priori estimates, 'balance' conditions or dynamical equations to be verified to some degree of accuracy by the final estimate, ...). The expressions suggested by the referee are more restrictive than that. We will stress more strongly what we mean.

15. line 175: "there is of course no reason to think...": again this has been settled in Oliver et al. (1996); Bardsley et al. (2014); Liu et al. (2017). So why not be more straightforward and factual here? Such as: "In general, this procedure does not lead to an unbiased Bayesian estimation, but can nonetheless provide a very useful approximation (Oliver et al., 1996; Bardsley et al., 2014; Liu et al., 2017)".

Again, see 'weak point' 1 above.

34. lines 450-451: "Fair comparison is therefore possible only at the end of the as-

similation window.": yes, but not only. A fair comparison of DA methods would also imply cycling, which is not the case of EnsVAR here. I am very fine about your using the EnKF and particle filter to compare the ensemble qualities; but not really when it comes to comparing RMSE at the end of the window. At the very least this should be briefly discussed.

We do not really understand what the referee means here. What we mean is that it is only at the end of the assimilation window that the three algorithms have used the same amount of information, and that it is only at that time that comparison is fair, in terms of Bayesianity as well as RMSE. Having a form of cycling for EnsVAR within the overall assimilation window would define another algorithm, which could also be compared to what we have obtained. But we do not understand in what that would be 'fairer'.

35. line 459: "...is the one described by Evensen (2003)": which one? G. Evensen's book describes both stochastic and deterministic EnKFs. (Of course I know the answer, you just need to improve the statement.) By the way, you should, from time to time, insists on the fact that you picked up the stochastic EnKF since the deterministic is now more popular. Moreover, choosing the stochastic EnKF makes sense in this study as the EnsVAR is also stochastic. You could mention this as this would strengthen your choice for the stochastic EnKF.

We compare three algorithms which use the same input (data and a priori statistical information about the quality of those data). Whether or not these algorithms are stochastic (in the sense that they use random generators at some stage, if that is what the referee means) is secondary in our mind.

36. line 461: With a fully observed system and an ensemble of size N = 30, you do not need to use localisation. In the present context it could actually be detrimental to the quality of the EnKF ensemble! (e.g., Bocquet and Carrassi, 2017). I would recommend that you do not use localisation here.

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A number of other parameters than localisation could be evaluated there spatio-temporal distribution of the observations, statistical properties of the observation errors, . . .). Our purpose is not to make a thorough study of the many variants that could be implemented for EnKF of PF. It is just to compare EnsVAR with what seems to us to be two 'reasonable' algorithms for both EnKF and PF. And we say clearly that our conclusions on the comparison of the three algorithms cannot be considered as definitive.

37. lines 479-481: It is fine to report these numbers in here, but not allude to them in the abstract, where, out of context, they do not make much sense.

Actually, it was not our intention to allude to these numbers in our abstract (see our response to specific remark 3 above).

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