Nonlin. Processes Geophys. Discuss.,
https://doi.org/10.5194/npg-2018-6-AC1, 2018

Nonlinear Processes in Geophysics
Discussions
Open Access
EGU

Interactive comment on "Ensemble Variational Assimilation as a Probabilistic Estimator. Part II: The fully non-linear case" *by* Mohamed Jardak and Olivier Talagrand

Mohamed Jardak and Olivier Talagrand

mohjardak@gmail.com

Received and published: 9 April 2018

We thank M. Bonavita for his comments and suggestions. We give below a first response to them.

The paper in subject is the second part of a study of the characteristics of the EnsVAR ensemble data assimilation as a probabilistic estimator. This second part aims to extend the results of the first part to the fully nonlinear case. While the basic methodology follows the one used in the first part of the study, the robustness of some the results presented in this second part appears more questionable and some issues need to be explored further, at least to the mind of this Reviewer, in order for the paper to be

Printer-friendly version

Discussion paper

acceptable for publication. In the following I detail these concerns.

1) In Sect. 3 the Authors compare results of the QSVA EnsVAR, EnKF, PF. Not surprisingly, QSVA EnsVAR shows better results as a probabilistic estimator and also for more standard resolution measures. This is unsurprising, to my mind, because this comparison is not fair. As the Authors noted, the costly QSVA extension is needed to keep EnsVAR assimilation in an approx. linear error evolution regime and thus guarantee good behaviour in this long-window assimilation set-up. To compare apples with apples the Authors should directly compare results of the standard EnsVAR algorithm at the end of the window with those of EnKF and PF. Additionally, it would also be of interest to compare results of QSVA EnsVAR with those of an EnKF whose assimilation is run on shorter assimilation windows, to guarantee linear behaviour, and then cycled.

We do not fully understand what the referee means. We consider our comparison is fair in the sense that we compare three algorithms that have used the same information (same model, same observations and same statistics as to the associated error; that is only what matters in the last resort). As for standard EnsVAR algorithm (i.e., without QSVA), it fails over a 10-day window, as shown by Fig. 1. And, from what we understand, the referee suggests to 'cycle' EnKF. To us, the latter, which is sequential, is cycled by construction.

2) In Sect. 4 on weak-constraint assimilation, I understand that the model error perturbations are drawn from the same error distribution whose covariance is used in the 4D-Var cost function. If this is correct, this is a significant limitation on the potential applicability of the results, as the difficulty in obtaining realistic characterizations of Q is probably the most important cause of the limited success of weak-constraint 4D-Var in realistic applications.

The referee raises a very important question, but it goes well beyond the scope of our present papers. An assimilation algorithm must first be evaluated in conditions where the errors affecting the data follow the same statistics (first- and second-order

moments) that are used in the assimilation. That is what we have done. Skipping that step would make the interpretation of results much more difficult. The same question arises concerning observation errors. We will stress that our twin experiments are fully 'consistent' concerning the errors on the data.

3) In the last paragraph of Sect. 4, the Authors explain that the performance of EnsVAR, EnKF, PF in the weak-constraint case appears in terms of reliability measures (e.g., rank histograms). This could depend on localization used in the EnKF, for example. Have the Authors explored this parameter space?

This remark is appropriate. But, no (except for basic elementary checks at the start), we have not done any comparison or tuning on the parameters of both EnKF and PF. This is actually mentioned ll. 308-310. A similar question has been raised by referee 2's comments on our first paper (see his comment 36 and our response).

4) Lines 310-311: " ...many possibilities exist for the reducing the cost of EnsVAR, through simple parallelization or ...". I am puzzled on how parallelization can reduce the computational cost of EnsVAR. Maybe the Authors meant clock time?

Yes, you're right. Thanks for the remark.

5) Lines 319-320: "On the other hand, EnsVAR is largely empirical, with the consequence that, should difficulties arise, conceptual guidelines may be missing to solve these difficulties." I struggle to see what these difficulties might be. In the linear case, EnsVAR (aka EDA) is constructed so as to be a consistent statistical estimators assuming the input data errors are correctly sampled. In the nonlinear case, its behaviour will depend on the amount of nonlinearity and the ability to track the true global solution. In this respect, EnsVAR is as empirical as the EnKF.

Yes, we do not know either at this stage what theses difficulties might be. But one must always be ready to encounter unexpected difficulties. And, yes, EnKF is also empirical, and the remark we make about EnsVAR also applies to EnKF.

6) Lines 339-340: “EnsVAR has been implemented here on a small dimension system. It has to be implemented on larger dimension, physically more realistic models.”. I suspect the Authors mean QSVA-EnsVAR in this context. Standard EnsVAR has been running at ECMWF and MeteoFrance for a number of years.

No, we did not necessarily mean QSVA-EnsVAR (although that of course may be part of the work to be done). We meant systematic assessment of EnsVAR as a probabilistic estimator. We will rephrase our statement.

Interactive comment on Nonlin. Processes Geophys. Discuss., https://doi.org/10.5194/npg-2018-6, 2018.

Printer-friendly version

Discussion paper