NPG MS #2014-10 Authors: G. Wu, X. Zheng, L. Wang, X. Liang, S. Zhang, and X. Zhang

12 May, 2014

Title: *Improving the ensemble transform Kalman filter using a second-order Taylor approximation of the nonlinear observation operator*

Recommendation: Accept subject to **Minor Revision**

The manuscript discusses two novel approximations to deal with nonlinear observation operators in the context of the ETKF. The problem has central relevance in geophysical data assimilation where Earth observations are often derived from nonlinear relation. The proposed approach leads to a new way to compute the inflation factor required to adjust the forecast error covariance matrix in most ensemble-based scheme. To the best of my knowledge the method introduced by the authors is new and it is well outlined in the context of state-of-the-art procedures against which it is compared numerically.

The manuscript is quite well written and relatively easy to follow; it is somehow long in the mathematical derivation but the authors have rightly cut the formalism into appendices in order to make the read of the main text easier.

My overall opinion is therefore positive and I think this study is worth to be published after minor modifications that I list below.

1. Page 545, line 19. Typos: Burgerss $\Rightarrow$ Burgers

2. Page 545, lines 22 - 25. It is unclear why the linear approximation on $\mathcal{H}$ should affect the error covariance evolution and not just the analysis step.

3. Page 548, lines 5 - 10. I suggest the authors to improve notation. In fact while the number in the brackets for the state vector is time, it is the observation in h. Furthermore h is an operator which is applied to the state vector and using the notation $h(1)$ is confusing.

4. Page 554, lines 20 - 24. Along with model error, other factors may affect the consistency between F-RMSE and F-Spread, namely the nonlinearities and the sampling error. The authors mention them later in the text; I suggest them to do it at this point too.

5. Page 556, Eq. (32). The authors state the observations are spatially correlated. From Eq. (32) this is not clear, since the the observation at point k, only depends on the state vector at the same point. Does the spatial correlation of the observation error come from the state vector in (32) ?. Please clarify.

6. Page 557, line 4. I suggest the authors to include also the main equation characterizing the ETKF for consistency with the other listed methods. These equations should be Eq. (12) and (13) I guess.

7. Page 557, lines 4 - 9. The comparison would be more self-consistent if an algorithm having SS in the inflation and nonlinear in the optimization would be at hand. I suggest the authors to either add such an algorithm among those under comparison or at least discussing it in the text.

8. Figure 1 and 2. Please improve the quality of the figures by using colors or thicker lines. Also, I strongly suggest the authors to include a similar figure showing A-RMSE for the algorithms under comparison as a function of α. This will further fortify the result in Fig. 2 and the overall results in general.

9. Page 557, lines 23 - 28. The authors should include some hints on the physical interpretation of L in terms of the state-estimation accuracy. This will help to fully interpret the results in Table 1.

10. Page 558, lines 1 - 2. It is not strictly true that a smaller error corresponds to a smaller value of the objective function L: see SS.

11. Page 560, line 6. I would change ”... may be more appropriate ...” into ”... may also be appropriate ...”. Moreover inflation of the background is also useful when the system is highly chaotic and the ensemble size too small. Multiplicative inflation of the background in fact does not change the range of the matrix which can be desirable if the ensemble members have correctly catch the dynamics instabilities.

12. Page 560, lines 9 - 12. I understand that everything is based on the equality: $< \mathbf{d}\mathbf{d}^T >= \mathbf{H}\mathbf{P}\mathbf{H}^T + \mathbf{R}$, where $\mathbf{d}$ is the innovation vector, $\mathbf{P}$ the forecast error covariance matrix and $< . >$ the expectation operator. If my understanding is correct, I suggest the authors to include this in the text. A good place might be when the objective function is introduced.

13. Page 561, lines 1 - 8. This paragraph is important but not clearly written. Please rephrase the description of the experiments with a fixed-tuned inflation.

14. Page 578, Table 1. I suggest the authors to include the ratio between F-RMSE over A-RMSE. This will help quantifying the relative improvement gained at the analysis and the average error growth during the forecast. For instance, these ratios would reveal the large error reduction obtained by TN at the analysis.