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Nonlinear Processes in Geophysics
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Interactive comment on "Expanding the validity of the ensemble Kalman filter without the intrinsic need for inflation" *by* M. Bocquet et al.

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We would like to thank the reviewer for his/her time, his/her input and valuable suggestions. Please find below our answers to your questions and how we have handled your suggestions.

- *In my opinion, the authors would do a service if they could supply a little bit more information about the expected computational cost in both time and storage to apply the EnKF-N with the various hyperprior assumptions as a function of the number of degrees of freedom in the dynamical model. A comparison to the cost of the EnKF algorithm would be enlightening.*

  The additional numerical cost is usually negligible compared to the EnKF but depends on the implementation (primal or dual) and the type of estimation method (filtering or smoothing). We have added a discussion on the matter in the numerical illustration section of the revised manuscript. Thank you for the suggestion.

- *1. P. 1092, Line 26: Localization can be required even if not rank deficient for EnKFs or in presence of non-gaussian/non-linear effects.*

  We are not sure about *required* in the rank-sufficient case. But we definitely agree that it is useful in this case of the presence of non-Gaussian/non-linear effects even in the rank-sufficient case. We have changed the sentence accordingly.

- *2. P. 1093, Line 18: Traditional methods only make assumptions about first two moments, not all moments as implied here.*

  Indeed. Thanks for pointing out to this confusing statement. This has been corrected in the revised manuscript.

- *3. P. 1094, line 10: I don't know what the authors mean by doing the analysis in "ensemble" space. This should be clarified here before the subsequent use. I am aware of terminologies like "model" and "observation" space, but don't know how this relates.*

  By ensemble space, we mean the affine space spanned by the ensemble, or to a large extend equivalently in the vector space where the coefficient vector $\mathbf{w}$ is defined (i.e. $\mathbb{R}^N$). We are now more explicit in this outline.

- *4. P. 1094, line 16: Not clear why the posterior should relax to the prior for "quasi-linear".*

  We agree that this sentence is confusing and potentially wrong out of a more precise context. Hence, the sentence has been changed into: "...we discuss caveats of the method in regimes where the posterior ensemble is drawn to the prior ensemble."

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- *5. P. 1095, line 6: Don't see why non-linear dynamics has to be partially responsible. Could this not all be due (at least for second moments) to a degenerate ensemble?*

  This is the point of section 4, where it is explained why non-linear dynamics is a sufficient cause for using inflation, although not a necessary cause.

- *6. P. 1099, line 20: This assumption is only justified in the case that the ensemble is approximately non-degenerate. Is that ever the case in any real applications you have in mind?*

  This assumption remains valid with high-dimensional models using local analysis. In that case the assumption that $\mathbf{x}$ is to be found in the ensemble subspace is only local and not global.

- *7. P. 1106, line2: "Not as performant: : :" You should probably say a little bit more about what you mean by this statement and what experiments you performed to explore it. In particular, it seems that the EnKF-N corresponds to a single inflation value for the whole ensemble, while some of the other methods allow different values for different variables. In a nonlinear model system, the latter could clearly have advantages.*

  To avoid any confusion that would endow the EnKF-N more than what is meant, we have clarified the sentence. "Nevertheless, for the experiments described in Section 5, they are not as performant with the specific goal of accounting for sampling errors as the EnKF-N". Note that used in conjunction with localization, the EnKF-N yields local inflation factors. Hence, it can be adaptive in time and space.

- *8. P. 1107, paragraph starting at line 14. This paragraph was unclear. Obviously violation of the EnKF assumptions can come either from nonlinearity or degeneracy, and which dominates (or even exists) depends on the application.*

Here, you seem to be saying that nonlinearity is somehow the generic cause. As an aside, what happens if you apply the EnKF-N in a case where the EnKF is sufficient (linear, Gaussian, ensemble big enough to span growing and neutral directions)?

We assume that by degeneracy you mean that the ensemble does not span the full, true, range of uncertainty. That said, under the assumption that the model is perfect and that the ensemble is big enough to span the unstable subspace, then nonlinearity is indeed the cause of sampling errors, not degeneracy (our claim). In order to back this claim, we have mentioned the linear Gaussian case where inflation is unnecessary. So we believe you understood this paragraph well. We have tried to make the paragraph clearer and have insisted a bit more on the perfect model assumption, since model error would drastically change this picture.

As for your second question, the original EnKF-N formulation leads to unsatisfying suboptimal (if not divergent for some models) performance in the regime where the EnKF is sufficient. This issue and how to amend the EnKF-N are the objects of Section 6. With the correction, the EnKF-N diagnoses an inflation that goes to 1 when the nonlinearity is made insignificant.

- *9. P. 1109, bottom. You need to give a little bit more details about these comparison experiments, in particular stating that the ensemble sizes were the same, and possibly commenting on the relative computational cost.*

  Yes, the ensemble sizes were the same which we have written explicitly in the revised manuscript. We have added the suggested discussion on the computational cost of the EnKF-N in this section.

- *10. P. 1110, line 14: Nonlinearity is the "profound cause" only because the ensemble size of 20 is larger than the number of positive local Lyapunov exponents?*

We have made this statement more precise in the revised manuscript. However, note that the (global) EnKF-N does not correct for degeneracy assuming Jeffreys' hyperprior is used. No inflation can do that. Hence, we do not believe that "profound cause" is exaggerated.

- *11. P. 1110, line 24: You need to give a bit more information about the 2D barotropic model.*

  We have removed "and a 2D-barotropic model" but expanded on its description in the beginning of the section which, moreover, refers to Bocquet and Sakov (2014). This was a standard forced 2D and homogeneous turbulence model which is governed by the barotropic vorticity advection equation. However, it was only fully tested with a global EnKF-N (this model may have a limited number of unstable modes depending on the number of vortices and hence on the forcing power spectrum). Evaluation of a local EnKF-N with this model has been performed, but only in specific regimes.

- *12. Section 6, line 1: This barely nonlinear regime is truly problematic for deterministic ensemble filters, but is known to be a problem for EnKFs in low-order models (see for instance Anderson 2010 on nongaussian filter updates). However, the problem there generally goes away with larger models. Is that anticipated for the issue here?*

  The issue was even more pathological in the original EnKF-N since it did not even behave as a deterministic EnKF in the very weakly nonlinear regime.

  We do not have any clear anticipation. A global EnKF-N with a larger model requires a larger ensemble (we experimented with the already mentioned 2D barotropic vorticity equation but also a 2D shallow water model), so that the pathology ($N/(N-1) \longrightarrow 1$) indeed decreases. It is true that in those experiments, we never encountered such problem. But, on the other hand with a local EnKF-N, we found that that the problem could re-emerge.

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Interactive Comment

- *13. Section 6.2, start: This notion of "relaxing to prior" seemed confusing to me. The observations have no information, so the posterior is the prior. I'm not sure what you are "relaxing" from in this discussion. Thank you for the suggestion.*

  We agree that "'relaxing to prior" could be confusing. Following your remark, we have change the terminology throughout the revised manuscript. Thank you for the suggestion.

- *14. P. 1115, line 20: I think that this use of "climatological" is misleading. The required statistics do not come from the climate of the model as this terminology would normally imply. Instead, they are statistics from the "climatology" of the prediction system including the assimilation. To sample them, one would need to run a high-quality assimilation system (large ensemble, well-tuned) and sample the statistics from that.*

  The term "climatological" is sometimes used in the hybrid/EnVar literature where the authors rightfully meant a time-independent representation of the error statistics of the data assimilation system. Nonetheless, we do agree with you: this term can be a source of confusion. We have modified the whole manuscript to account for your remark. Thank you for the suggestion.

- *15. P. 1124, first paragraph. The practical use of the methodology is to avoid the need to tune multiplicative inflation for perfect model experiments. This avoids the cost of doing multiple runs to tune the inflation. However, given that this is a major result of the paper, there needs to be a little bit more discussion of the computational cost (time and storage) to implement the EnKF-N compared to a basic EnKF. Some of the adaptive inflation methods already in the literature that are referenced in the paper are generally able to produce smaller RMSE that the best tuned single inflation value for the dynamical systems examined here. These methods have very small incremental cost compared to the base EnKF. It would also be important to indicate the expected scaling of the computational cost for*

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the EnKF-N versus the EnKF as the model size grows.

We have added a discussion in Section 5 on the numerical cost of the EnKF-N (with the Jefrreys' hyperprior). In all the experiments and models we tested the EnKF-N with, the numerical cost was negligible (but they are indeed low-dimensional models).

With imperfect and/or inhomogeneous model scenarios (i.e. realistic cases) we do agree that adaptive inflation schemes such in Anderson (2007) are potentially superior, but not necessarily in the perfect-model and homogeneous scenarios we experimented upon. Note that the EnKF-N can sometimes outperform a run with the best tuned single inflation value (a very significant effect with the Lorenz 63 since the EnKF-N better diagnoses the change of lobes of the attractor). More generally, the framework of the EnKF-N allows to discriminate inflation used to counter sampling errors and inflation used to counter model errors. But, in its basic form, it cannot be seen as a competitive method in a realistic EnKF experiment with a significantly imperfect model.

Anderson, J. L.: An adaptive covariance inflation error correction algorithm for ensemble filters, Tellus A, 59, 210–224, 2007.

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