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Nonlinear Processes in Geophysics
Discussions

Interactive comment on "Non-Gaussian statistics in global atmospheric dynamics: a study with a 10 240-member ensemble Kalman filter using an intermediate AGCM" *by* Keiichi Kondo and Takemasa Miyoshi

Anonymous Referee #2

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This manuscript presents some fascinating and novel analysis of the distributions generated by an ensemble assimilation system and a low-order atmospheric model using ensembles of unprecedented size. It is generally well-written and presents a series of results that expose a number of novel aspects of the problem. It is a great addition to the ensemble literature and motivates the need for non-gaussian DA procedures rather than just using ever larger Kalman-class ensemble filters. The only weakness is that, in some places, the authors may be a bit too bold in extrapolating their results. What they have documented is the distributions from their system using a simple model and an

LETKF. It is unclear whether the non-gaussian aspects of the resulting distributions are fundamental to the observing system and model, or if they might be very different with a more general data assimilation system. As an example, it is not clear if a non-gaussian DA system would result in more (or less) gaussian distributions if applied with the same nature run and observations. The authors do provide hints that the low-order model may not be a very good proxy by showing a few results with a more realistic model which look very different from the results for the SPEEDY model used here. Looks like lots of room for additional interesting studies in the future.

Specific comments:

1. Line 29: "disappear 'naturally"' Not clear what it means to be 'natural'. Do you just mean that they disappear after a while? Are there any places where things disappear 'unnaturally' in contrast?

2. Line 30: "1000 ensemble members may be necessary. . ." This is a tricky argument. Maybe it takes many fewer if the DA system isn't assuming and enforcing (to some extent) gaussianity for increments. Or maybe there is a whole bunch more 'detailed' structure out there in that case.

3. Line 34: "find the optimal initial state" Definition is too limited. Goal is to find some representation of a pdf. A subcase would be an optimal state.

4. Line 53: Actually Anderson shows that outliers occur in WEAKLY nonlinear situations, not in strongly nonlinear cases.

5. Line 62: "non-Gaussianity will degrade the analysis." Compared to what?

6. Line 82: There is an issue with your definition of kurtosis throughout. The standard definition of kurtosis (check wolfram, Wikipedia) has a value of 3 for a normal distribution and the term beta_2 is usually used for this. The excess Kurtosis is this value minus 3 and has a value of 0 for normal distribution. You should make sure that both your symbols and definition are clear throughout.

7. Line 91: “the PDF is considered to be non-Gaussian”. This is a magic number here. Give some insight on why you picked it or make it clear that this will be discussed below.

8. Line 125: k = 20 is another magic number. Give some insight into why you picked it.

9. Line 157: “No localization was applied, yielding the best analysis accuracy.” Do you know this to be the case? If so, state explicitly what localizations you tried (in the previous work I assume). It is surprising that no localization would be optimal since localization can also protect against nonlinear relations which are certainly occurring in the presence of non-gaussian marginal distributions that you are examining.

10. Line 181: “extremely large” Compared to what?

11. Line 230: “their instability is mitigated in the model.” I had trouble with this sentence which seemed to say that the model generated instability and mitigated it at the same time. Just needs clarification.

12. Line 250 and elsewhere: “propagates” I think that this may be a poor word choice for how marginal non-gaussianity is generated.

13. Line 286: “Gaussian filters cannot produce accurate analysis when significant non-Gaussianity exists.” I am uncomfortable with this statement although I hope it is qualitatively accurate. Still, I don’t think you have documented it. Kalman filters are still optimal in certain senses even for non-gaussian priors. You have provided no evidence that a non-gaussian filter would produce significantly different analyses in cases with significant non-gaussian priors. I think this statement should have a lot of caveats and suggest the need for more research.

14. Line 288: “non-gaussianity of the atmosphere” First, the atmosphere doesn’t have a PDF, so it can’t be non-gaussian (caveat classical physics). Second, you have used such a simple model that it is difficult to say too much about more realistic models with similar observing systems, so be careful to not be too strong here.

15. Line 294: At least in some cases, “non-Gaussianity is explained by the convective instability.” However, your results from the more realistic model suggest it is not the only cause, and your study here only looks at a few points in detail so cannot provide strong conclusions. In addition, it is easy to demonstrate that any advective problem will generate non-gaussian distributions when the advecting flow is uncertain and the quantity being advected has gradients. This is certainly introducing non-gaussianity in all atmospheric models. See also line 302 which I think is too strong of a statement.

16. Line 339: I believe these results are actually consistent with the results in Anderson (2010). The last section of that paper looks at results in a low-order atmospheric model, similar to SPEEDY but dry, and finds that outliers occur at local points (not for all state variables), that they form and then normally quickly disappear, and that sometimes multiple outliers can occur at the same point. Even for Lorenz-96, outliers do not form for all variables simultaneously. For instance, ensemble member 1 may be an outlier for state variable 2, while member 10 is an outlier for state variable 12.

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