



Interactive
Comment

Interactive comment on “Spectral diagonal ensemble Kalman filters” by I. Kasanický et al.

Anonymous Referee #1

Received and published: 17 February 2015

This manuscript is unusually clear in its presentation of methodology. The method described is shown to be more accurate for certain types of applications. My only concern was that the particular experiments used to demonstrate the method’s capabilities seemed to be for unusual parameter regimes, and I would like to understand better why the authors chose these cases. I also note a few minor grammatical errors. I think that the manuscript is acceptable pending these minor amendments.

Minor concerns: 1. The authors begin their results section with Lorenz96. This non-dimensional model requires an ad hoc mapping to a dimensional time and there is a standard in the literature for doing this. Normally, experiments assimilate at frequencies that are roughly similar to those in numerical weather prediction when using the standard time definition. For instance, assimilating once an hour would be deemed to be high frequency while every 6 or 12 hours would be common. The authors here in section 7.1 state that they are assimilating once every second after assigning 0.01s

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



for the timestep. It is important that the authors clearly describe how they are defining dimensional time and highly preferable that they use the standard definition. It still seems to me that they are assimilating very frequently compared to most applications in the literature, and if so, this should be motivated. Similarly, the observational error variance of 0.01 is very small compared to most published applications for L96. This error variance is a tiny fraction of the “climatological” variance of L96, and doesn’t look like a very reasonable analogy for the types of error variances found in real geophysical applications. Again, the authors should clearly state why they used such a small value and comment on the relation to the more common values in the geophysical literature.

Similarly, the assimilation frequency for the shallow water example seemed odd. Assimilating every second is very frequent compared to the evolution of the dynamics. Some readers may become suspicious that high frequency assimilation was chosen as a case for which the new method is particularly competitive. Again, the authors should clearly state why they picked such frequent assimilation and how they picked the observational error variance.

2. Not required, but would be a nice addition: There is limited discussion of how the method extends to non-identity forward operators and none about nonlinear forward operators. A paragraph in the conclusions would be a nice addition if something simple can be said.

Minor stuff: 1. p. 6, line 11: $N \ll n$. Real issue is whether $N \ll q$, where q is the rank of the covariance matrix of the Kalman filter solution. This is an important issue because this mistake has repeatedly confused things in the geophysical ensemble literature.

2. p. 6, line 15: ‘tranform’ to ‘transforms’

3. p. 7, line 16: ‘Eq. 5’ should be ‘Eq. 7’

4. p. 8, line 20: It would be better to put the equation that starts at the end of this page on its own line.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

5. p. 10, line 14: ‘mean IS known’
6. p. 11, line 3: It might be clearer to say “only one gridded variable”. I’ve had previous experience with saying ‘one variable’ and having readers interpret that as a scalar.
7. P. 12, line 5: ‘is THE one by’
8. P. 12, line 8: ‘is THE matrix’
9. P. 12, line 13: ‘by A call to’
10. P. 13, line 5: ‘one for each data point’
11. P. 13, line 15: Unclear to me why these must be contiguous. Couldn’t this work for any subset of variables? If not, you might add a sentence to make it clear why (not even clear what ‘contiguous’ means for a grid).
12. P. 14, line 16: ‘state consistS’
13. P. 14, line 19: ‘minimalizes’ to ‘minimizes’
14. P. 14, line 20: Doesn’t the KF minimize the expected RMSE for linear Gaussian?
15. P. 15, line 7: ‘timestep of THE model’
16. P. 15, line 14: ‘but THE spectral’
17. P. 15, line 22: ‘decreaseS the RMS’
18. P. 16, line 7: Aren’t u and v normally described as velocity components, rather than momentum?
19. P. 16, line 10: ‘where’ to ‘were’
20. P. 17, lines 6-14: Could part of this be coordinated with last paragraph on p. 16?
21. P. 17, line 23: ‘continuous’ to ‘contiguous’?
22. P. 18, line 1: ‘error THAN the sample’

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

23. Figures: Since you ran multiple realizations, you might want to mention what the error bars would look like (including them seems a bit much).

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 115, 2015.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

