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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Hybrid variational-ensemble assimilation of lightning observations in a mesoscale model" *by* K. Apodaca et al.

Anonymous Referee #1

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Journal: Nonlinear Processes in Geophysics (NPG) Title: Hybrid variational-ensemble assimilation of lightning observations in a mesoscale model Author(s): K. Apodaca, M. Zupanski, M. DeMaria, J. A. Knaff, and L.D. Grasso MS No.: npg-2014-40 MS Type: Research Article

GENERAL COMMENTS

The manuscript describes the application of a (previously documented) hybrid ensemble-variational assimilation method to observations of lightning rate from a surface lightning detection network, intended as a proxy of a future satellite observing instrument, which should be able to provide similar data in areas presently uncovered,

such as the oceans.

The assimilation of lightning rate data is applied to a model where convection is parameterised, so it is aimed at improving, rather than the description of convection processes, that of convection "environment", i. e. values of model prognostic variables locally involved in the computation of model-estimated lightning rate, through the estimated vertical velocity (itself not a prognostic variable in this parameterised convection system).

After the definition of the observation operator, described in some detail in the appendix, the application of the assimilation method is straightforward.

The test case is an important event of a front impacting on South-Eastern and Eastern U.S.A., causing many convection episodes associated with heavy rain, wind gusts and tornadoes.

The motivation, context and development of this work appear relevant and well described. However the results appear much weaker than what is suggested in the manuscript title and what is commented in the body of the text and in the conclusions. Indeed it is appropriately shown that, at analysis time, the lightning rate observations are able to provide information and that this is transferred to model state variables. The interpretation of the effect of assimilation on forecast, though, is too optimistic.

My indication is that a major revision is needed.

The authors should revise the manuscript, including the title, and present their results under a much more cautious shade. An improvement in the presentation of results is also desirable to make their interpretation clearer.

SPECIFIC COMMENTS - MAJOR POINTS The manuscript title, as it is now, suggests that the lightning rate assimilation is a completed task, but (as it is appropriately said in the conclusions) the manuscript only describes a preliminary step, in a particular context (the parameterised convection model) and with ambiguous results on forecast.

On the one hand, the authors succeed in showing that, at analysis time, some information is provided by the lightning rate observations, and that this information affects the model state variables through the relations included in the observation operator and the multivariate background error covariances, estimated in a flow-dependent way in a hybrid ensemble-variational scheme.

On the other hand, in spite of what is optimistically said in Section 4.4 and in Section 5, the analysis increment induced on prognostic variables does not show a coherent and positive impact on the 6-h forecast (a short range for a model where convection is parameterised). In particular, Fig. 11 does not show that a forecast error reduction has been achieved, it only leaves the possibility of an improvement open.

The authors should: 1) attenuate the optimistic tone of the manuscript title and of the comments to their results; 2) improve the description of assimilation effects on the forecast; 3) an analytic effort would also be appreciated, to connect forecast improvements (and deteriorations) to the system evolution and to the ability of the observational network to detect relevant sources of error (and of error growth) at different times.

MINOR POINTS AND TECHNICAL CORRECTIONS: * The manuscript contains many annoying misprints.

* P. 923, Sect. 2.2. Please motivate the choice of computing observed lightning rates on a grid different from the model grid.

* P.923, line 24-26. This choice should be synthetically motivated. In areas interested by convection (in either the model or reality) and in locations nearby, observations of zero lightning might in principle represent information about misplaced convection events. Moreover: how does this choice affect the PDF of innovation vectors discussed in Sect. 2.3 ?

* P.924, line 2-4 and Eq. 2. The definitions if h1 and h2 in line 2-4 are not consistent with Eq. 2 and with the equation (line 3) h= h1 h2. Which of the two is the "transformation"?

Which is applied first?

* P.924, lines 8-9. Interpolation FROM observation points (the "grid" used to compute lightning rate as described in Sect. 2.2) TO model grid points? Since the definition of an observation operator goes from model state variables to observation estimates, I expect that the interpolation would go the same way. Did you instead choose to build pseudo-observations at model grid points? In this case this should be said in a more explicit and clear way (while motivating the initial choice of using a different grid, see above).

* P.925 line 3. The abbreviation "CWM" has not been defined.

* P.925 line 4. "...and neighbouring points": why? how many?

* P.925 lines 11 and following. How does the choice of discarding zero observations affect the innovation PDFs?

* P.927 line 10 and line 17: "the number of ensembles". Do you mean "the number of ensemble members"?

* P.928 Sect 3.1. Please convert units, use m sˆ-1 for wind and °C for temperature.

* P.928 Sect 3.1. The event description should be completed with a more detailed discussion of the evolution in time of the front and of the convective activity, and this should be later used in the discussion of results (Sect. 4) at different times, with regard to forecast improvements and deterioration.

* P. 931 line 14. Why at 17:00 UTC? Please make explicit reference to the evolution of the event of Sect. 3.1 (after improving it, see above).

* P 931, Sect.4.1. It would be useful to explicitly give the size of the spatial extension of the signals appearing in Fig. 6, and compare it with the model grid size.

* P932 line 12-13. The agreement is not very good for Cycle 3 (middle panel of Fig. 7). Please acknowledge this. Moreover: the maxima of degrees-of-freedom-for-signal

appear to be somewhat off with respect to the distribution of observed lightning rate. The authors should perhaps discuss how this is related to the effect of observations on model prognostic variables, with regard to the main flow.

* P.932 line 18 and line 19: "the number of ensembles". Do you mean "the number of ensemble members"?

* P 933 Sect.4.3 You should include, in Figure 8 and 10, a panel with the same field (of 8a and 10a) for the NODA experiment. I think that the discussion would benefit from a LIGHT-NODA comparison (here too, not only in Sect. 4.4).

* Sect. 4.4. Please refer the assimilation cycles to time and event evolution

* P 934 lines 6-12. In Fig 11-a, an improvement can be seen in 5 cases, however not for the 6th. Is it possible to investigate possible reasons for that?

* P 934 line 8. From Fig 11-b, no systematic improvement can be seen in the 6-h forecast. What appears here is that there is some improvement in 2 cases out of 6, some deterioration in 1 case, and no change in 3 cases. I don't think this is enough to consider this a "partial improvement".

* P 934 lines 9-12 . Supplementary observations should in principle improve the assimilation results, but they could mask the effect of assimilating lightning rate. Shouldn't this new observational source be useful to compensate for lack of information in under-observed areas?

* P 935 line 14-15. From the results shown, it is not possible to affirm that the analysis improvement is partially retained in the forecast. Omitting in the conclusions that a short range forecast (+6h only) was considered is also misleading.

* P.941, reference list, line 8. Is Rodgers' book title correct? Please check.

* P.941, reference list, line 12. There is a misprint for Zupanski et al. (2007) : delete "bibitem 26"

* Caption of Fig. 7. The agreement is not very good for Cycle 3 (middle panel). Please acknowledge.

* Caption of Fig. 11. It is not correct to comment this Figure saying that the error reduction obtained at analysis time is kept, even partially, in the 6-h forecast.

Interactive comment on Nonlin. Processes Geophys. Discuss., 1, 917, 2014.