

Interactive comment on “A data assimilation method of the Ensemble Kalman Filter for use in severe dust storm forecasts over China” by C. Lin et al.

Anonymous Referee #2

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The paper introduces the use of the ensemble Kalman filter (EnKF) for forecasting dust storms in China, based on a dynamical dust storm model and observations of PM₁₀. The results are validated against three discarded stations and independent measurements (LIDAR observations). The potential of the method and the limitations are briefly discussed.

The paper first suffers from poor grammar and vocabulary, but it would be unfair to judge the article on this aspect only, since the scientific content is good. The authors ought to get hold of a fluent english speaker and improve the language to reach their audience.

The work by Lin and co-authors is well structured, concise and the validation looks rather good for a first attempt in applying the method. However, the discussion of the results should be strengthened.

In conclusion I would suggest the paper is published with minor revisions as pointed below.

Specific comments:

Analogous works in atmospheric chemistry could be referred to, on particular Hanea et al. (MWR 2007) and Blond and Vautard (JGR-D, 2004) The prior on the initial error as uncertain location of the clouds makes sense a priori, but still the ensemble spread is insufficient. The implications of filter convergence are a serious for all ensemble methods and the authors should suggest more satisfactory means to overcome this limitation. For example there are no explicit model errors in the system as if the model dynamics were perfect. Is that a realistic assumption? Why not include errors in meteorological fields?

There is one minor error the initial ensemble P6, l. 10: $1 + \delta$ may take negative values, you should consider another distribution than the normal for this parameter (an exponential for example) and be aware of possible biases.

The authors show in Figure 9 that the differences between Qinghuandao and Dalian can be large at times especially in periods when concentrations are high. The two points are merely separated by one grid cell and clearly points to insufficient model resolution. Why arguing for denser observations, then? It seems useless to have observations denser than the model grid.

It is puzzling that the method performs better at validation points (Figure 4) than at the assimilation points (Figure 5). I conclude that the three validation points are located in areas where the errors are lower and not representative. Please comment on that.

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The discussion of results is too often qualitative, for example in Figure 6, the improvement of the vertical structure must be supported by the RMS errors and correlation, given as a function of H.

In Figure 7, the (negative) correlations with the visibility time series should be given.

In Figure 11 and 12, it is uneasy to compare the colours with the dots sizes. please put the dots in colour for clarity.

Technical corrections:

Define the acronyms like PM10, TSP.

Among many other language errors:

- P1, l. 17: use "correct" instead of "afford".
- P2, l. 8: "in order to" instead of "for the special ability"
- P4, l. 6: "irregular sampling" instead of the "scale influence"
- P4, l. 24: 45 grid cells
- P4, l. 26: "reached" instead of "invaded"
- P7, l. 6: "give" instead of "convey".
- P7, l. 24: "whole" instead of "wide".
- P7, l. 28: the sole assimilation of surface PM10...
- P8, l. 21: I would use "comparable to" instead of "almost better"
- P8, l. 24: Use "spatial variations" instead of "locality", same at l. 26.
- P9, l. 1: "bias" instead of "debases"

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- P9, l. 12: "compensates for" instead of "makes up".
- Figure 11 and 12: (a)s and (b)s are absent.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 17511, 2007.

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