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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ensemble filter based estimation of spatially distributed parameters in a mesoscale dust model: experiments with simulated and real data" *by* V. M. Khade et al.

Anonymous Referee #1

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Ensemble filter based estimation of spatially distributed parameters in a mesoscale dust model: experiments with simulated and real data

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In this paper, the authors attempt to estimate dust erodibility maps for North Africa and the Arabian peninsula, based on observations and model calculations. Their methodology is based on Bayesian inference, in particular an ensemble Kalman filter. The authors explain methodology, test it in controlled experiments (Observing System Simulation Experiments) and finally apply their method to real (MODIS) observations of AOT. Independent forecasts using the new erodibility maps are validated against MODIS

AOT observations.

The paper tackles an important issue in aerosol modelling, namely the parametrisation of dust emission. It uses a relatively recent technique (ensemble Kalman filters) for which the authors have found a new application. The authors also attempt to give the reader a 'peek under the engine's hood' by describing in more detail than previous papers on similar topics the application of an ensemble Kalman filter for parameter estimation. As such the paper is entirely appropriate for publication in ACP.

Yet, the presentation of the material could be better organised. Also, the authors describe the working of the system in great detail but do not address several science questions that naturally result from their work (and which should be addressed, I feel). In particular: 1. How certain are the authors that the MODIS AOT (used in this paper) is determined by dust? This may be an incorrect assumption that can potentially seriously impact your results. Stating, as the authors do, that they are only considering the dust season is not sufficient. 2. Why assume that erodibility is wrong but the threshold windspeed is not? In all likelihood, both are wrong and the newly estimated erodibility maps will be affected by this (correcting for incorrect threshholds where possibly erodibility is reasonably estimated) 3. The major assumption here is that erodibility is constant in time (at least over several weeks). This raises at least three issues: a. Is the technique (a filter with an assimilation cycle of 24 hours) appropriate? Possibly longer cycles and time-averaged observations should be used. b. Do the erodibility maps derived from real observations truly converge with time for the filter used in this paper? c. If the erodibility is not constant (this possibility is hinted at in Section 7), what shall be the real application of the technique developed in this paper?

General comments 28840,15: A few more references to aerosol modelling and its uncertainties would be welcome, e.g. AEROCOM results. Also, both Huneeus et al 2012 (ACP) and Schutgens et al 2012 (Remote Sensing) developed Kalman filters/smoothers for estimation of aerosol emissions. Hunees et al. contains a long list of papers discussing emission uncertainties. There are other papers that attempt to estimate aerosol emissions, a topic very close to your own (e.g. Dubovik et al. 2008), using different techniques.

28841,5: The paragraph after this list seems to belong to other sections: methodology, results and summary

28842,13: What is the highest atmospheric level in this model?

28842: It is not clear what happens to the meteorological variables. Obviously they are calculated by the model but are they also nudged to any meteorology? Section 3 seems to suggest so "The OSSE is run using the meteorology of June/July 2009" 28846,21.

28843,10: I find the explanation of erodibility a bit confusing. If erodibility is only a surface weighting function, why is it called erodibility? If it is related to a surface's ability to emit dust , why is it's value between 0 and 1? The definition of erodibility is not clear (but should be as it is central to the paper). As far as k (Eq 1) is concerned: if it is determined from a fit to observations, assumed values of erodibility must have been used. What values were used and how certain are they?

28844,2: AOD is not a measure of the amount of dust, it is merely an indication of the amount of dust over regions where dust dominates AOD

AOD is often used but an odd term nevertheless. i suggest AOT: Thickness refers to the complete aerosol layer. Depth actually refers to a level within that layer.

28844,5: Do you have a reference to the source of this value of extinction? If it is based on e.g. Mie calculations, what sort of particle (distribution) was assumed? I gather the mean size is 2mu?

28844,6: Am I right in assuming that dust is represented by a single size bin (or mode with fixed size and width) in the model? Please discuss this. How realistic is this (various deposition processes vary in efficiency a lot from 0.5 to 10 micron). There is no feedback of dust on the meteorology? Finally, what is the impact of other aerosol?

Your model does not include those? They nevertheless exist in real life.

28845,5: Eq (2) and before seem to ignore dust emitted in previous time steps but not advected. If this dust is included in the transport term, then please explicitly mention this.

28845,10-20: I suggest to move this to where you initially define and discuss erodibility.

28846, section 3: the text is confusing because the authors hop from topic to topic within one paragraph. I would suggest separate paragraphs in the following order: 1) ensemble DA and estimation of erodibility. give Kalman filter eq, discuss generation of ensemble (erodibility), discuss spin-up and assimilation cycles 2) treatment of boundary conditions in ensemble DA framework, including boundary conditions for aerosol 3) OSSE, assumptions on erodibility and threshhold windspeed (values, perturbations), generation of synthetic observations.

28846,9: It is a bit confusing that the set-up of the OSSE is discussed at the same time as the treatment of the meteorological boundary conditions. They are really different topics, please deal with them in different paragraphs. By the way, where do the different realizations of boundary conditions come from? NOGAPS is not an ensemble DA as far as I know. I suggest a separate paragraph (sub-section?) to explain the treatment of boundary conditions in an ensemble DA context, not just the meteorology but also the aerosol itself.

28848,24: "These values are chosen after experimentation with different values." what does this mean? How were those values chosen? Did you thoroughly examine parameter space or merely consider a few (likely ?) values?

28849,3: Depending on the situation, you adjust either individual members or the mean. I guess the mean in an ensemble system can only be adjusted through its members. A bit more detail here will be appreciated.

28849,23: Instead of calling this alfa-up, why not alfa-posterior? The paper already has an alfa-prior and alfa-up may be thought to be related to something upstream.

228849, Eq 3: this is the Kalman filter equation under this paper's simplifications. Please mention this. Better yet, refer to the full equation which you quote in an earlier section.

28850,1: "error variance is given by var(AODobs) which is set to 10 % of the mean observation" This information should really be in a separate paragraph, in an earlier section where the DA system is discussed. Also, even though you are working with synthetic obs and are free to choose your obs errors, I doubt 10% is a realistic estimate for real errors, certainly for obs over land.

28850,11: "the uncertainty in the AODprior ensemble is due to the uncertainties in local α, local uâĹŮ, upstream α, upstream uâĹŮ and winds". Why do winds differ among ensemble members? Only because of boundary conditions or are there additional reasons as well? How large are the variations across the ensemble? It would be good to discuss this earlier, in section 2 or when discussing the ensemble DA system.

28851,1-7: Again, most of this should have been introduced in a methodology section, where localization is also discussed. Later, one can then simply refer to that section while discussing figure 2.

28851,10: "In this work the mean of the simulated observations is not perturbed. The difference between the observation mean and prior AOD mean is termed the “innovation” " Should have been defined & discussed before.

28851,10-25: In my opinion, this explanation is not needed. It follows from the equations. At the very least, I suggest shortening it.

28852,3: "The covariance between α and AOD at the observed grid point informs what part of the innovation is used in the increment." There are no parts to the innovation. I think I understand what the authors intend, but this sentence needs rephrasing. Also, spurious covariances are discussed once more. It is better to introduce this terminology

early on and later show examples. At present, the explanation is very haphazard.

28853,9: "The observational error is set equal to 10 % of the mean observation, consistent with instrument accuracy". This is NOT consistent with instrument accuracy because AOD error from satellite, especially over land, does not depend on instrument calibration. Rather, retrieval assumptions (cloud free, surface albedo, particle species) determine these errors. Again, I think 10% is too small.

28854: The association of high erodability and high advection noise seems a bit tenuous. Advection noise depends more strongly on friction speed in neighbouring gridpoints. Would it be more instructive to show the correlation between AOD and erodability in contours in Fig 4?

28855,16:"The reason for the bad covariances is a combination of the effect of advective noise and the small size of the ensemble". Ensemble size: yes. Advective noise: no. The advective "noise" is an integral part of the covariance and allows you to use down-stream observations to estimate erodibility. Model errors will cause bad covariances because e.g. transport is poorly represented. That is one likely reason that you need to use a cut-off radius.

28856,18:"then constructing an ensemble-member by ensemble-member weighted average". I would suggest "a spatially-smoothed perturbation for each ensemble member separately".

28857: I find this an interesting result, that an optimal correlation length for erodability perturbations can be found through use of the filter itself. But this begs the question what exactly defines this correlation length. I believe the authors leave this question open. One issue are the synthetic observations. They are sparse but the method for sampling them is not explained clearly. However, I suppose it is really the spatial correlation in the true erodability map that determine this correlation length. Did the authors investigate this?

28858: As a follow-up: how can you be certain that results from tuning experiments for an OSSE can be applied to real data?

28858,15:"This confirms our hypothesis that correlating perturbations leads to decrease in advection noise thereby improving the covariance estimates". Can figures like 2c verify this? I would suggest to verify this hypothesis: is it possible that the advection noise does not decrease but that the reduction of degrees of freedom in your system increases the impact of observations?

28861,12:"After 28 update cycles the mean of tuned α converges to values shown in Fig. 8c.". it would be good to show that α truly converges. Given the number of simplifying assumptions you have made, it is possible that α does not converge but continuously adjusts as dictated by the observations (especially since you keep windspeed threshold constant). Proving that α converges in a large part of the domain, would add considerable strength to your argument. As the estimated α are much smaller than the operational α, care should be taken in defining a convergence criterion.

28861,15: Can your system deal with significant underestimations of the emission flux? As α <=1 by construction, it would appear fortunate that the operational model overestimates dust emission.

28864-28867: The point of this expose eludes me. In any case, a shorter text may be more convincing. I would suggest to show scatter plots of forecast AOT vs observation, for both estimated and operational erodability maps, possibly for individual regions. Combined with Figs 9 and 10, that would give a much better insight in the overall improvement than Fig 12.

Section 7: I would suggest a fairly substantial rewrite of this section. As it is, it mainly discusses technical issues and ignores some of the major points: - what did the authors set out to do? - why is that new and interesting? - what methodology did they use? after this, a brief summary of results as well as possible improvements can be discussed.

Textual comments 28846,6: "An OSSE is cast in as a perfect model experimen". Please rephrase.

28850,17: "Out of the total spread of αprior" I guess this should be AODprior?

28864,6: MAE should be dMAE?

Figures: several figures could do with shorter captions which repeat part of the paper's text anyway.

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