Geosci. Model Dev. Discuss., 5, C679–C686, 2012
www.geosci-model-dev-discuss.net/5/C679/2012/

Geoscientific
Model Development
Discussions

Interactive comment on "A simulation study of the ensemble-based data assimilation of satellite-borne lidar aerosol observations" *by* T. T. Sekiyama et al.

Anonymous Referee #2

Received and published: 17 September 2012

General comments

This paper applies one of ensemble data assimilation systems, 4D-LETKF, to analyzing sulfate and dust aerosols and to estimating dust aerosol emission under the Observing System Simulation Experiments (OSSEs). I have no doubt of an importance of the topic that this paper was motivated by. Authors have spent lots of effort to achieve a realism of OSSEs and have tried many sensitivity experiments to explore better configurations of the ensemble data assimilation system.

Based on those experiments, the authors clearly show an improvement of the sulfate and dust aerosol analysis through assimilating CALIPSO data, although the result of

flux emission does not look successfully improved. Here, the authors should mention about the impact of transport errors. Because all the experiments here use the meteorological reanalysis with a nudging, background ensembles of dust aerosol cannot represent the transport error well, which do not enable the ensemble of atmospheric aerosol variables to contain major uncertainty induced by the weather. The attenuation of information in a link between the dust emission and the atmospheric dust aerosol makes the flux estimation difficult. Thus, this issue should be explained more within your experimental setting. Also, the authors try the MODE (Method for Object-based Diagnostic Evaluation) for the verification tool with a strong motivation, and thus it shows promising performance partially. Results in this paper indicate that some measure of the MODE (e.g. centroid distance) works well as the authors expected. However, the outcome of 75th percentile intensity ratio does not tell one from another among most of experiments, which tarnishes the motivation of the MODE. Thus, I suggest that the authors also give an attention to explore and to describe the shortcoming of the MODE metrics in the manuscript.

This referee thinks that it is section 2.1 that the authors should modify most significantly. After the introduction describing the motivation of this research well, Section 2.1 is followed for addressing a data assimilation system, and hence readers would expect an explanation for how the authors implemented the EnKF system for an analysis of the aerosols and emission variables. However, the authors spend too much space to describe a history of EnKF and general information about data assimilation methods, which can be found in many books and papers (e.g. Evensen 1994, Kalnay 2003, Kalnay et al., 2007, Bouttier and Courtier, 1999, etc, as the authors also cited). For example, the first two paragraphs in Section 2.1.1 are neither novel nor interesting, but may make readers bored and distracted. The purpose of this paper is an application of one existing EnKF method to very interesting new targets so that the authors does not need to explain this much background of the EnKF and data assimilation methods. I strongly suggest the authors drop too general introduction of data assimilation or EnKF, but that the authors explain your own data assimilation system using the formulation of

EnKF in terms of your system's characteristic features. Section 2.1.3 can be a good example, which describes how to update the variables actually concerned in this work.

Lastly, it is obscure why the analyses of sulfate aerosol are so insensitive to most of parameters in EnKF system (e.g. inflation parameters, observation errors, ensemble size, observation density) although the authors show a significant improvement on its analysis compared to the free-run. The improvement would result from corrections to background, and the corrections (can be interpreted as observational increments) should be provided by good observation. It means that the analysis system represents a background uncertainty to some extent so that the better information from the observations is allowed to correct the background. However, results in section 5 present that changing observation errors, observation density, inflation factor for the background do not change the result of sulfate aerosol analysis. It seems like the case that the background uncertainty of sulfate aerosol is very small so that the analysis system always reflects the background state mostly but no observations. For example, whatever the observations become more accurate or worse, the analysis system does not differently consider the observations at all because the analysis system is confident of the background. Obviously, this is a contradictory if one recalls the improvement of sulfate aerosol analysis in time and space, which is possible when the analysis system has reasonable estimation between the uncertainties of observations and the background state. The authors' explanation "because this data assimilation is constrained by the meteorological field nudged to the reanalysis, so the degree of the freedom to control the aerosol variables is limited in the data assimilation" is not enough to elucidate these ambiguous results. This may be just because of inefficiency of the MODE verification. In any case, this referee severely recommends more careful and profounder examination of this issue.

Specific comments

p. 1878, lines 14-15: Dust emission estimation shown here is not improved successfully. Please correct this point.

p. 1878, lines 21-23: I strongly agree with the authors in terms of the importance of coupling meteorological variables with the aerosol variables in the analysis system.

p. 1881, lines 12-13: Since the current system does not reflect uncertainties of aerosols caused by weather, any resultant parameters of inflation or localization in this paper are unlikely to be useful in practice. Background uncertainty in the atmospheric aerosol concentration due to the transport error would be remarkable in reality.

p. 1881, line 19: "Other aerosols, however, such as sulfate and smoke, cannot be measured and…". Here, "cannot" sounds too deterministic. Is it impossible forevermore? It would be good to change "have not".

p. 1882, lines 19-20: I do not think that the authors can use the same parameter settings given by this study for the real data because the background uncertainty of the current system does not represent realistic one due to the nudged meteorological variables. To me, the aim of this paper described above (lines 13-15) looks strong enough.

p. 1883, lines 11-26 and p. 1884, lines 1-25 should be shortened drastically, and be filled with the actual features related with this paper (e.g. state variables that this work concerns about, and how to update those variables during the analysis with 4D-LETKF system).

p. 1885, line 4: It is obscure. Do you mean that an ensemble size cannot increase infinitely in practice?

p. 1885, lines 7-8: What do you mean by this sentence? Is this a unique feature of LETKF, which other EnKF or Var does not have?

p. 1885, line 20: What do you mean by "the diversity of the ensemble members"? Is it degrees of freedom that ensembles span, or just spread of ensembles?

p. 1885, lines 24-28 and p. 1886, lines 1-19: This is also a part of purely theoretical introduction about 4D-LETKF that can be replaced just by a brief description with a citation of several papers.

p. 1886, lines 20-28: This is what the authors should elaborate in this paper, which is distinctive and/or novel among other standard EnKF or LETKF systems.

p. 1887, lines 16-17: Since the authors do not mention what are “the atmospheric state variables” before, this sentence becomes less efficient. These state variables in your analysis system should be specified before.

p. 1888, line 2: “inverse analysis” may sound neither comfortable nor familiar to general readers. It should be defined first.

p. 1888, lines 9-10: However, your localization covers the negative correlation regions either because the localization is determined by a fixed physical distance from the analysis grid point. This is another source to cause sampling errors into the analysis system.

p. 1888, lines 17-18: Although the dust emission has a persistent forecast, the error information of the dust emission would be propagated during the forward forecasting of dust aerosol in the atmosphere. I know that the spread of dust emission would not be changed during the forecast step. This should be clarified.

p. 1892, lines 708: This is very good to point out.

p. 1892, lines 10-11: Finally, the authors mention the state variables in the analysis system here! This referee believes that the first part of Section 2.4 can be merged with Section 2.1.1 and Section 2.1.2 effectively.

p. 1893, lines 4-7: This referee has experienced that using random Gaussian noise to the initial field/additive perturbation for the inflation is not a good strategy in EnKF system. There is also a paper by Zupanski et al 2006, Tellus. In addition, do you think that you can have enough spread of your ensemble just by adding random Gaussian noises while the meteorological variables do not provide enough uncertainties into the ensemble of the aerosol variables? The referee is not convinced of whether a spread

of your ensembles would be reasonable.

p. 1893, lines 12-15: Isn’t there any bias caused by this artificial condition given by the analysis system?

p. 1895: lines 24-25: The sentence “The Nature Run provides the “truth” of the atmosphere” is repeated too many times.

p. 1895, line 28: It’s not necessary. Nature run model can have “significantly” different climatology from the model used for the data assimilation.

p. 1897-1900: This paper deserves to be complimented on an effort to have a realism of the simulated observations.

p. 1899, line 27: Gray lines in Figure 8 do not look analogous with each other.

p. 1900, lines 17-18: It does not look only due to the averaging data. The location of maximum value in OSSE seems different from that in real data. I know that the simulated observations do not need to have the same values as the real observations. However, the authors also do not need to say something obscure. Thus, this sentence should be modified. Or, you may try averaging out the real data to model grid to prove this statement.

p. 1901, line 23: Do you mean a synchronous detection of aerosol plumes?

p. 1903, line 23: The authors should define a meaning of each quantity precisely, because they are not that intuitive to general readers.

p. 1904, line 4: It would be good to explain more about how to determine the weight.

p. 1904, line 10: It seems to me that J can be too subjective according to the way to distribute the weight to each attribute.

p. 1905, lines 23-26: This referee is not convinced of this statement.

p. 1905, lines 27-28, and p. 1905, lines 1 4: Here, RMS error works better than the 75th percentile intensity ratio. Besides, I do not know why the authors repeat the presentation of the MODE values in Tables 5 and 6 after showing them in Figures 12 and 13. Maybe Tables 5 and 6 can be dropped.

p. 1908: How are the MODE scores for the flux estimation against the truth? It would be helpful to show the quantitative comparison using the MODE as the authors have done for the aerosol analyses. Difference between FmR and Exp-Std does not look obvious in Figure 16.

p. 1908, lines 11-13: This can be a clue implying that the flux inversion suffers from transport errors. The system works fine with a flux budget estimation over somewhat broad area, although every grid point estimation is contaminated by transport error that the current system has not resolved yet.

p. 1909, lines 19-20: It is very good to point out this shortcoming in the current system, which can give a good direction for the further improvement of your system.

p. 1909, lines 27-28: This is very important point that the authors should think about for the future work.

p. 1910, lines 20-22: In order to have a reasonable uncertainty of aerosol variables, coupling uncertainties of aerosol variables along with the meteorological variables is essential. Thus, a sensitivity experiment to the ensemble size is likely to have a different result from what the authors have obtained here. That is, the EnKF parameters that the authors obtain as the best in these OSSEs (e.g. ensemble size, inflation parameters, localization scale, observation errors and density) may not work well with a real data.

p. 1911, lines 5-7: Interpretation of the results is not clear. To me, Exp-Inf05 does not look bad as the authors describe.

p. 1911, lines 25-27: In general, the results show extremely insensitive analysis of sulfate aerosol to most of EnKF experimental settings (in addition to the observation

error). This makes me puzzled as mentioned in the general comments. While the sulfate aerosol analysis is significantly better than the FmR, how can its analysis be that insensitive to these different EnKF environments? The authors may need to check the result more carefully, or to suspect the robustness of the MODE metrics.

p. 1911, line 19: Tables 7 and 8 prove that the 75th percentile intensity ratio does not work as a metric at all.

p. 1913, line 10: I understand that the authors would like to focus on analyzing aerosols over Asia and that the authors have noted the motivation of this area of interest. However, the authors can add at least one-paragraph description about the performance of the system over the globe, because your system has spent a remarkable computational cost and a disk space for the global aerosol analysis.

p. 1914, lines 6-7: I do not understand what the authors mean by this sentence.

p. 1914, lines 18-21: This is not enough. If the system does not have enough degrees of freedom, then it becomes vague to explain how the authors get that remarkable improvement shown in Figure 17 and 18 with the current system with the nudged meteorological field. This referee strongly suggests that the authors take a look into this carefully. This is very important for a sound conclusion in this paper.

p. 1915, lines 13-14: What are the many controversies?

Technical corrections

p. 1883, line 12-13: The sentence includes the same meaning twice. Maybe “computationally” should be dropped.

p. 1936, Figure 7: It would be good to notify what the colors indicate. I can guess what it means without lots of difficulties, but it is good to be specified in the caption

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