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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Development of the Ensemble Navy Aerosol Analysis Prediction System (ENAAPS) and its application of the Data Assimilation Research Testbed (DART) in support of aerosol forecasting" *by* J. I. Rubin et al.

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General comments

This paper carefully examines the performance of a global ensemble aerosol data assimilation system for operational applications. The ensemble data assimilation is a state-of-the-art technology that is promising but unmatured in the field of atmospheric chemistry. The new findings of this paper definitely contribute to the technological progress of ensemble analysis and data assimilation for atmospheric chemistry. This

paper is really interesting for data assimilation researchers.

This paper is well structured and written with elaborate experiments and diagnoses. It is very comprehensible although its description is a little redundant. I believe this manuscript can be accepted as an ACP paper only with a few minor technical corrections; however, the consideration of issues I pointed out as below will make the quality of this paper (or the next paper of the authors) much higher.

Specific comments

(1)

I was surprised that the adaptive inflation worked well for aerosol in this study because my adaptive inflation failed and diverged when I used a method other than Anderson 2009. I have thought that the adaptive inflation for aerosol is unstable due to a large uncertainty of aerosol modeling compared to NWP. What do you think?

(2) Page 28080, Line 5, Page 28088, Line 21, Page 28091, Line 29, etc.:

It was not described in this paper how (and how much) the observation errors were estimated. Even though it is described in the references, the estimation method and size of observation errors are crucial for data assimilation. It is better to show the validity of the method (and error size) in the manuscript, if possible. Generally speaking, "observation errors" are underestimated because it is difficult to estimate spatial representativeness and remote-sensing bias. I am afraid that observation errors are unnaturally underestimated in this paper too.

(3) Page 28089, Line 28, Page 28091, Line 14, etc.:

All the emissions in this study were perturbed using the same factor for a given ensemble member. Actually, it is not a good way as the authors mentioned. Instead of that, there are some alternative techniques to reduce correlations between independent sources. For example, 1) make perturbation factors one-by-one randomly each grid-point, 2) smooth out the distribution of the factors using a 3-dimensional smooth-

ing filter, 3) and use the smoothed factors to perturb emission sources. Usually, the ensemble mean of the perturbed emission flux is not very shifted by this method.

(4) Page 28092, Line 16:

The authors are using a maximum inflation limit, but I am afraid that the maximum value (=1.30) is too small because adaptive inflation factors more than 2 or 3 are acceptable for NWP without any problem.

(5) Page 28092, Line 21:

The authors say, "the ensemble isn't fully representing the distribution with an excess of observations occurring of low ranks," but when the rank histogram shows a one-side peak, it is only certain that the ensemble members have a large bias. With only the information of "one-side peak," we don't know whether the ensemble spread is small or not.

(6) Page 28092, Line 27∼29:

If AOT values are small, it's no wonder AOT observational errors are relatively large because the error of remote sensing is almost independent from the AOT (=retrieved) value. On the other hand, when AOT values are small, it's impossible to make a large ensemble spread. This is a disadvantage of ensemble data assimilation.

(7) Page 28096, Line 18∼20:

Do the authors mean that there is a large difference between meteorological analyses since there are few meteorological observations in the Southern Ocean? If so, this sentence (Line 18∼20) is a little confusing.

(8) Page 28097, Line 29, Page 28099, Line 6, Page 28100, Line 18, etc.:

The authors often use the term "variational" (assimilation, system, initial condition, etc.) as an inferior method to the EAKF, but the "variational" method is the 2D Var in this paper. We have another variational method, the 4D Var, which is comparable or superior

to the EAKF. It is better to always use the term "2D Var" in this paper to avoid confusion.

(9) Page 28098, Line 28:

I could not understand the meaning of "the optimal combined meteorology and source ensemble". What is optimal?

(10) Page 28099, Lines 7∼9, etc.:

In this study, the EAKF system captures sharp gradients while the 2D Var smooths plume distributions. However, the EAKF and 2D Var have similar RMSE and bias. That means, probably, although the EAKF result looks realistic, the plume location is slightly shifted from the real one. It is difficult to judge which is better "sharp but slightly-shifted plumes" or "blunt but broadly-covering plumes" as operational prediction/warning, I think.

(11) Page 28099, Lines 19∼23:

Are these RMSE global?

(12) Page 28101, Line 26:

The authors say, "the observational error may be too large for small AOT values, which could also contribute to the positive bias", but I don't think so. Generally speaking, it is extremely difficult to assimilate zero or almost zero values like small AOT. It is because a population that contains a lot of zeros (or almost zeros) and is not allowed to be negative values (e.g., radar-measured precipitation) is not Gaussian-distributed. Fundamentally, it is nonsense to quantify the error of non-Gaussian-distributed values using a standard deviation. However, data assimilation assumes everything Gaussian. It is the reason why zero-value assimilation is difficult. The positive bias observed in smoke regions may be relevant to non-Gaussian AOT distribution and irrelevant to the size of AOT observational error.

(13) Figure 5:

I am very interested in NH Pacific Ocean, Arctic, and Antarctic.

(14) Figure 15:

Why did the authors plot MODIS AOT that was not quality-controlled? I would like to see the comparison between assimilated observations (= quality-controlled AOT) and assimilation results.

(15) Figure 15:

I am very interested in why the AOT over the Sahara is largely changed by the 2D Var although there is almost no observation over the Sahara. The influence radius of observations in 2D Var is only 250 km or so, right?

Technical comments and corrections

(1) Page 28080, Line 16:

The description “over 13 land regions” is actually “over 15 land regions”?

(2) Page 28085, Line 11:

There are two spellings “blow-up” and “blowup” in this manuscript. Choose either one.

(3) Page 28090, Line 18:

Is it necessary after “large” to put a comma?

(4) Page 28092, Line 22:

isn’t -> is not

(5) Page 28093, Lines 10 and 12, etc.:

There are two expressions “meteorology ensemble” and “NOGAPS ensemble” in this manuscript. Choose either one.

(6) Page 28093, Line 29:

There are two spellings “source-perturbed” and “source perturbed” in this manuscript. Choose either one.

(7) Page 28094, Line 27:

Putting “(Table 2)” at the end of this sentence, it becomes easy understandable.

(8) Figure 4:

The characters “a” $\sim$ “h” in the figure panels are too small and extremely unreadable.

(9) Figure 6:

It is very difficult to find a “point”, especially in (b) panels.

(10) Figure 11:

Some of the AOT observation plots are illegible, especially on 22 August.

(11) Caption of Figures 11 and 12:

The “analysis” is plotted here, I think. But the caption says, “predicted total AOT”.

(12) Figure 16:

It is hard to find the area where the MODIS plot indicates, at a glance.

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