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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigating the assimilation of CALIPSO global aerosol vertical observations using Four-Dimensional Ensemble Kalman Filter" *by* Yueming Cheng et al.

Anonymous Referee #2

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General comments:

The authors present the result of an aerosol analysis comparison between CALIOP/CALIPSO data assimilation and NRL-MODIS data assimilation using a 4-dimensional ensemble Kalman filter with a global aerosol model to demonstrate the effects of the vertical profile information on the analysis performance. The manuscript looks well written and organized in its structure. Some interesting results are included for data assimilation researchers. However, it is impugnable that the "hourly" aerosol vertical analysis is really scientifically plausible, although the authors claim the hourly analysis as the novelty of this paper. In addition, some of the validation processes the

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Discussion paper

authors used were not suitable to validate data assimilation results. Therefore, I do not recommend to publish this manuscript as an article of EGU's high-IF journals.

Specific comments:

(1)

The authors insist that this is the first study to conduct the hourly aerosol analysis using a four-dimensional data assimilation method. That is because an hourly data assimilation with a 24-hour assimilation window is mathematically inadequate, and thus, nobody has conducted it.

If I am not mistaken, the authors assimilate one observation again and again (probably 25 times) because the data assimilation is performed every hour and each time window is set 24 hours. In contrast, if the authors define the 1, 2, ... 23-hour forecasts as the analysis, which means the data assimilation is done once per day, the authors' method is completely the same as the previous studies.

Theoretically, observations should be used strictly only once in the data assimilation process. Of course, in reality, a multiple use of one observation sometimes yields a good analysis result. However, those practical methods are often controversial. At least, it is not scientifically ingenious. The authors did not discuss this controversial issue at all in the manuscript. For example, Eugenia Kalnay's "Running in Place (RIP)" scheme (https://doi.org/10.1002/qj.652), which is slightly similar to the authors' method, assimilates one observation repeatedly. Therefore, Kalnay et al. carefully discussed the RIP scheme and its application range in their papers. Despite that, the RIP scheme has been controversial.

If the authors like to generate hourly aerosol analyses with hourly observations, a three-dimensional ensemble Kalman filter should be used to avoid a multiple use of one observation, or very narrow time localization should be applied to a four-dimensional ensemble Kalman filter to reduce the influence of temporally remote observations. In

that case, a 24-hour assimilation window is too long.

(2) Sections 2.2 and 3.2

Although the MODIS-Aqua DT/DB dataset is described in Section 2.2, the NRL MODIS dataset is not. Although it is described in Section 3.2 how to make super observations of CALIOP/CALIPSO, those of NRL MODIS are not described. Please clarify them. The NRL MODIS dataset is probably 6-hourly. If so, how did you make the hourly analysis of DA-NRL?

(3) Lines 167-168

Because the 4D-LETKF is not a smoother, the analysis has only one timeslot during its time window, which can be arbitrarily specified. When is the analysis timeslot during the 24-hour window in this study?

The control variables are fine/coarse mass mixing ratios only? The authors did not separately control all the prognostic variables of NICAM-SPRINTARS? Please describe it in detail.

(4) Line 181

The horizontal and vertical localization scales are described here, but the temporal localization scale is not. If the temporal localization is not applied, please specify it.

(5) Lines 241-246

The authors say "... the DA-CALIPSO experiment achieves more higher correlations and lower RMS differences than those with the MODIS assimilation. This indicates that ..., while the CALIOP assimilation can also improve the temporal and spatial variations of the aerosol extinctions," but I do not agree. The authors validated the two experiments with the CALIOP data that were assimilated only for the DA-CALIPSO experiment. The reason of the high correlation and lower RMSD of the DA-CALIPSO is the self-verification between the DA-CALIPSO results and the CALIOP data, which

should be strongly refrained for data assimilation validation. What the authors did in Section 4.1 was sanity or internal checks, not validation.

(6) Line 252

The DA-CALIPSO is not “clearly” superior to the FR and DA-NRL because in this section the authors merely checked the sanity of the DA-CALIPSO experiment. If the authors like to indicate the superiority of the DA-CALIPSO or DA-NRL results, independent observations should be used. In other way, the forecasts from the data assimilation analyses can be validated by the assimilated observations.

(7) Lines 260-263

Does this mean that the vertical data assimilation breaks the aerosol profile balance? The analysis of the aerosol profile becomes not similar to either the prior or observation information? This is very interesting.

(8) Line 281

The word “improvement” is not appropriate. The authors were simply tested the similarity between the DA-CALIPSO results and the assimilated observations in this section.

(9) Line 296

As the authors say, the MODIS and CALIOP observations are generally inconsistent. Therefore, the authors have to compare the MODIS and CALIOP observations used in this study before the validation of data assimilation results.

(10) Lines 321-322

Nobody can conclude that the CALIOP assimilation is “superior” to the MODIS assimilation before the validation with independent observations. In this section, the authors checked only the similarity between the CALIOP assimilation results and the assimilated CALIOP observations.

(11) Section 4.1

Section 4.1 is too long in comparison to Sections 4.2 and 4.3. The most important result is Section 4.3. Although Section 4.1 is much longer than Sections 4.2 and 4.3, it does not contain the similarity check between the data assimilation results and the NRL-MODIS data.

(12) Line 325

The authors described a comparison with the MODIS Aqua C6.1 AOT products as an independent validation. Probably it is false. The NRL MODIS dataset highly probably contains information very close to the MODIS Aqua C6.1 AOT products. If the authors like to insist that it is an independent validation, the portion originated from MODIS Aqua should be screened out before the NRL MODIS data assimilation.

(13) Line 326-328

I do not understand the necessity to eliminate the potential effects of the sparse CALIPSO observations. I think the authors would rather evaluate the effect of the CALIOP sparseness here.

(14) Line 346 and Line 347

It is not unexpectable that DA-CALIPSO has larger biases and RMSEs compared to FR. Generally, different remote instruments have large biases. Therefore, “deterioration” is a hasty conclusion. A careful evaluation of the MODIS AOT and CALIOP AOT must be done with independent observations other than Figure 9 to say “deterioration”.

(15) Line 362 “The AERONET sites with more than 10 observations during the study period are selected.”

Please describe a criterion for the site selection. In addition, the number of AERONET sites in Figure 10 seems much more than 10.

(16) 5. Discussions

First, the localization scale of 200km seems very small. The authors used a global model with a 223km horizontal resolution, which means that the authors simulated only synoptic scale phenomena and the distance of 200km was just a next grid. The data assimilation performs within only adjoining grids in this situation.

Second, the 400km localization is still too small to evaluate the localization length effect when the model resolution is 223km. If I were one of the authors, I would check the localization scales of 500km, 1000km, and 2000km. In addition, evaluation of the ensemble size might be needed when a large localization length is adopted.

Therefore, it is a hasty conclusion that the assimilation of aerosol vertical observation is “more” sensitive to the vertical localization than the horizontal localization.

(17) Lines 414-415

In this study, the hourly analyses were “validated” by only AERONET AOTs. CALIOP observations and MODIS AOTs merely confirmed the similarity between the assimilation results and the assimilated observations.

(18) Line 423

What does “all the four aerosol regimes” indicate here?

(19) Lines 428-430

If so, why did the authors not additionally conduct a simultaneous data assimilation of MODIS and CALIOP? It is unnatural that the simultaneous assimilation experiment is not at all mentioned in this manuscript.

Technical corrections:

(1) Line 320

“both the CALIOP or MODIS” -> “both the CALIOP and MODIS”

(2) Line 403

“MODIS observations, in” -> “MODIS observations. In”

(3) Caption of Figure 11

(g) CALIPSO derived vertical aerosol sub-types -> (i) CALIPSO derived vertical aerosol sub-types

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