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Interactive comment on “RAMS-MLEF Atmosphere-Aerosol Coupled Data Assimilation: A Case Study of A Dust Event over the Arabian Peninsula on 4 August 2016”

Anonymous Referee #1

The paper presents a first-time application of the Regional Atmospheric Modelling System (RAMS) couple with the Maximum Likelihood Ensemble Kalman Filter with the aim at improving the aerosol model prediction by assimilating aerosol optical depth retrievals from the Moderate Resolution Imaging Spectroradiometer (MODIS) instrument. The manuscript is divided into a technical description of the assimilation system and a part dedicated to the experimental design and results from a case study.

Indeed, we presented a first-time application of the assimilation of aerosols into RAMS.

In order to prevent a reader from getting the impression that data assimilation necessarily improves a forecast, we have provided a reader with text explicitly stating the goal of this study is to improve the analysis field, a.k.a. initial state for RAMS, and to provide a published example of where an improved initial state did not lead to an improved forecast of HWRF.

While the paper reads quite well and the topic is relevant and appropriate for ACP, I am left with some reservations in regards to its publication as I believe it still needs substantial work. My main comment is that unfortunately the case chosen for the study does not do justice to the advanced assimilation system, due to the lack of observations both for assimilation and for independent verification of the results. The reader is left without sufficient information regarding the performance of the system.

The search for an optimal case is optimistic; that is, a forecaster cannot pick his/her weather to forecast. A forecaster forecasts regardless of the weather. This case was chosen mainly because this case represents a group effort on unraveling the mystery of a missing plume advected off shore of UAE to the central portion of Persian Gulf which was missed by the InfraRed-based dust enhancement algorithms as discussed in Miller et al. (2019). We chose this case and we have decided to move forward with it.

We are a little bit at odds with your comment, “the case chosen does not do justice to the advanced assimilation system”, unfortunately because we are unsure what you mean by “do justice”.

As the first step of assimilating aerosols into RAMS via MLEF, our goal is to convey to the readers that the current effort is to be interpreted as a proof-of-concept. We have provided text in the revised manuscript to reinforce this statement. In addition, we have also added text to explicitly state that this is a part of an ACP/AMT special issue that summarize a group effort to study the missing plume near Persian Gulf.

I would encourage the authors to choose a different case with more observations available. In particular, the authors mention the use of a Coastal Water retrieval but in their study no emphasis is given to how this novel data performs in the assimilation and whether it helps improve the aerosol forecasts.

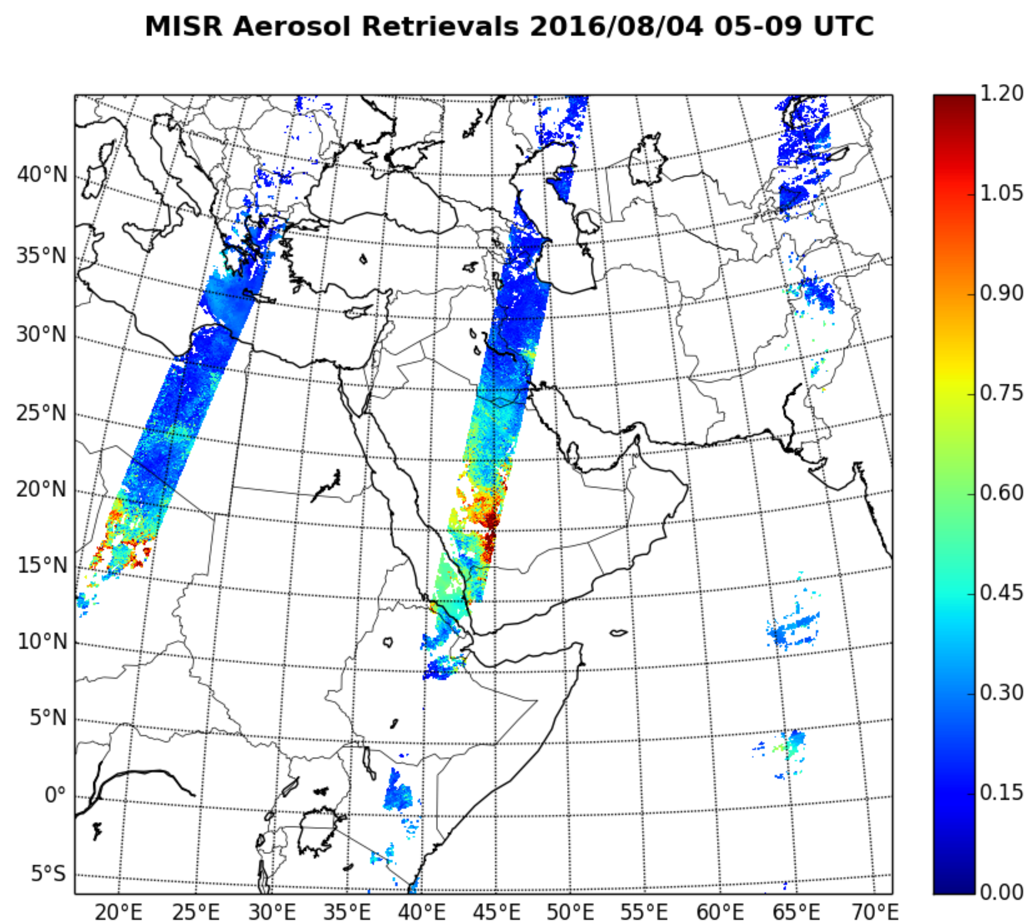
CW retrieval was combined with the Dark Target retrievals, therefore, it is challenging to speak to how CW retrieval perform relative to the Dark Target retrievals.

As we have stated earlier, one must be cautious the improved initial condition may or may not improve the forecast.

They also use mainly MODIS Dark Target data which are not available over the area of interest for their research, i.e. the desert of the Arabian Peninsula. I am left wondering why MODIS Deep Blue data or MISR data were not used in this case. Also the author state that there were no ground-based data for the verification and use MERRA-2 as their verifying dataset. While it is fine to use independent reanalysis to verify model forecasts, again I am wondering if they could have picked a better case in which some (even limited) ground-based observations were available for independent verification. Satellite products other than MODIS could have also been chosen for verification, for example MISR data (if available for this case).

Dark Target retrievals were chosen because of their higher spatial resolution compared to the Deep Blue retrievals. The spatial resolution of Dark Target retrievals is 3 km while the spatial resolution of the Deep Blue product is only available at 10 km. In addition, the size of 3 km x 3 km (10x10 km) are nominal pixel sizes at nadir and they can increase to 14.4 km x 6 km (48 km x 20 km) at edges. The pixel size of 48 km x 20 km is much larger than the size of the grid point (15 km) used in this study. As a consequence, the Dark Target retrievals are used.

Similar to MODIS, MISR aboard NASA's Terra satellite is another instrument that provides aerosol information. However, the availability of MISR aerosol retrievals is relatively low compared to MODIS due to its narrower swath width (MISR ~360 km; MODIS ~2330 km). An example of a MISR swath over the region of interest is shown below. There appears to be no coincident scenes over and near the Persian Gulf where the interested plume was located.



One final point is that the paper would also benefit from a more in-depth analysis of the meteorological analysis. The authors claim that there is an impact of assimilating aerosol data on the other control variables, for example horizontal winds, but they limit themselves to showing the analysis increments which in themselves do no prove whether this impact is real or an artifact of the coupled system. The information content analysis by itself is not very convincing.

Following your comment, section 5.3 has been completely rewritten to include a new figure and new discussions to provide an analysis of a few meteorological fields that were altered during the data assimilation. In particular, we used the integrated water vapor retrievals from the NOAA Unique Combined Atmospheric Processing System (NUCAPS) to support our findings.

For these reasons I would not recommend this paper for publication in this present stage, but I would encourage the authors to develop it further and resubmit it at a later stage with additional case studies and a more in-depth analysis of the results. Further comments and suggestions are made directly into the manuscript pdf (in attachment).

Supplemental Comments:

Page 2, Lines 1-5: Indeed, there are several operational systems that provide aerosol forecasts on a daily basis and include aerosol assimilation (NAAPS, CAMS, GEOS-5). Please mention them, specifying that they are operational systems.

Your comment has been included in the introduction. It reads as follows:

Operational numerical weather prediction centres, such as the U. S. Navy Fleet Numerical Meteorology and Oceanography Center and the European Centres for Medium-Range Weather Forecasts, have included aerosols in their assimilation systems to provided global aerosol prediction over the past several years. In addition, the NASA Global Modeling and Assimilation Office also assimilates aerosols.

Page 2, Line 28: What is happening for the first time? The use of the MLEF in RAMS or its application to the aerosol problem. Please clarify.

Both the use of MLEF with RAMS and the application of the RAMS-MLEF system to the aerosol problem are for the first time.

Page 2, Lines 30-31: Following your definition of strongly coupled system above, I assume that the cross-component elements forecast error covariance matrix are used in the assimilation. Please confirm.

Yes, you are correct.

Page 3, Line 6: This is not clear, please rephrase. More details on the CW would also be welcome.

The description of CW algorithm has been revised to the following:

Nevertheless, retrievals of AOD over turbid costal water has been challenging due to large variability in the ocean color or water-leaving radiance in the green to red wavelength. Wang et al., (2017) developed a new algorithm that uses 2.1 μm Top of Atmosphere (TOA) reflectance to retrieve AOD over turbid costal water; this technique is referred to as Costal Water algorithm. Given that the penetration depth (at which light attenuation is 90%) of pure water is only 0.001 m at 2.1 μm (Li et al., 2003), water-leaving radiance is negligible regardless of water turbidity. Since only one wavelength TOA reflectance is used in the CW *algorithm, the aerosol single scattering properties over turbid water are assumed to be the same as those over adjacent open ocean scenes.*

Page 4, Line 19: Please use H (capital h) for consistency of notation with other papers.

Revised.

Page 5, Line 1: Do you mean "similar to what is done in the operational approach"?

No, we mean to follow the same way how it is done in the operations.

Page 5, Line 4: Add "the" between "for" and "RAMS aerosol module"

Revised.

Page 5, Line 11: Please use the Greek letter sigma for the extinction coefficient for consistency with other literature.

On one hand, we are not sure which Greek sigma letters you mean (σ or Σ). On the other hand, we followed Liu et al., (2011) and Pagowski et al., (2014) and used $E_{ext.}$

Page 5, Line 12: The extinction coefficient is function of the size of the aerosol particles. When it's integrated over a particular size distribution, for which an effective radius can be defined, then you can parameterize it as a function of r_eff. I would mention it, for clarity.

We did not develop a functional mapping between effective radius and mass extinction coefficient since this would require additional computation; rather, we generated a look-up table of mass extinction coefficient and different effective radii.

Page 5, Line 17: Replace "calculation of" with "total"

Revised.

Page 5, Line 29: Remove "in general"

Revised.

Page 6, Lines 2 and 3: Add "the" before "central portion of the Persian Gulf" and "interior portion of Saudi Arabia"

Revised.

Page 6, Line 17 and 31: Remove extra coma in reference

Revised.

Page 7, Lines 3-4: For the case chosen, this is probably fine. It's a pity however to have such a complex coupled system and not use it to look also at aerosol-cloud interactions.

As a first step, we are excluding the microphysics, which presents a more complex problem. We have chosen to demonstrate skill in a dry atmosphere.

Page 7, Lines 11-12 and 17-19: Again, this is fine, but wouldn't it have been easier to allow for cloud formation?

Please see previous reply.

Page 7, Lines 29-30: "In order to prevent both temporal solutions from diverging, a methodology is used in MLEF that keeps the difference between the two time solutions close". I am not sure I understand this, please explain more.

Text in this line has been modified to address your comment. The revised text now reads as follows:

In order to prevent both temporal solutions from diverging, MLEF stores the differences between the two time solutions before assimilation, and uses the saved differences along with the altered solution to update the other solution. As a consequence, the differences between the two time solutions stay the same before and after data assimilation yet both time solutions are changed.

Page 8, Lines 18-19: Yes, were deep blue data not available? The choice of the aerosol datasets is discussed in the introduction, but I believe it's important to mention it again. Also what is the added value of the CW product? The use of that product is a point of novelty but it gets a bit lost.

Deep Blue retrievals were available. Please see above.

We have re-stated the choice of aerosol datasets in the revised manuscript.

The use of CW product was meant to supplement the Dark Target products as opposed to a standalone product. Unfortunately, the CW retrieval was combined with the Dark Target retrievals, therefore, it is challenging to speak to how CW retrieval performs relative to the Dark Target retrievals.

Page 8, Line 21: Well, that was surely a pity, particularly because the Saudi plume had a larger spatial extent. It seems to me that you did not choose an easy case for your assimilation test.

Please see above.

Page 8, Line 27: I agree. Perhaps this reinforces the notion that a different case should have been chosen.

Please see above.

Page 9, Lines 22-24: Yes, were you expecting this? Besides looking at the impact of the AOD observations, should the impact of the meteorological observations be assessed? It seems to me that this DFS is too small. Of course, I am not an expert in this, but as you observe this is a small number compared to the number of the ensemble members. You mention significant numbers...what is your measure of "significance"? Please comment. Typo: "Both of which are not significant numbers". I don't think you can say that from the DFS analysis only.

Yes. Our previous experiences suggest that a more realistic value for DFS in an optimal case (the availability and the spatial distribution of observations are idealized) would be smaller or close to $N_{ens}/2$. As stated in Section 5.2, DFS can be written in terms of eigenvalues of the information matrix Z (Eqs. 5-6), which is a combination of both observations and the background error covariance. As a consequence, DFS measures information from both the observations and the background error covariance. Given the scarcity of the meteorological observations, which are mostly limited to ground, and the availability and form of aerosol observations (e.g., vertically integrated aerosol optical depth), there is a lack of vertical information from assimilated observations. With that, we would not expect DFS to be anywhere near the $N_{ens}/2$ value.

We have also removed the word "significant" as it can be subjective. Instead, we provided two references for the value of DFS from previous studies with MLEF along with possible causes of a low DFS value.

Page 10, Lines 5-6: Please, add here that since the model is run "dry" this variable is equivalent to potential temperature.

Revised.

Page 10, Line 14: Once again, why was this case chosen?

Please see above.

Page 10, Lines 13-16: What is the mechanism for which this happens? Are the dust aerosols radiatively interactive? Please elaborate.

This is a consequence of the ensemble background error covariance that is meant to spread observation innovation (observation minus model equivalent first guess) spatially and among control variables.

After a second view of the manuscript, we decided to remove the discussion about analysis increment. Instead, Section 5.3 has been completely re-written to discuss the analysis field by comparing the total precipitable water field from the two experiments with the NUCAPS retrievals.

Page 10, Lines 23-24: Is AERONET AOD assimilated in MERRA-2? In this case, you mention that there aren't any AERONET observations, so that would not matter.

Yes, AERONET AOD was assimilated in MEERA-2. This sentence serves to provide a brief summary of the MERRA-2 product for the benefit of the readers.

Page 10, Lines 30-31: Well, actually in the case of the Saudi plume the ATMAOD run performs much worse than the ATMONLY.

Text has been modified to highlight the focus of this study is to improve the representation of the Persian Plume.

Page 11, Line 2: It's clear that this situation is poorly represented even in the ATMAOD run due to the lack of observations. Could you re-run with more data? For example Deep Blue or even MISR? This is really not showing the system capabilities at its best. Also, I would encourage you to pick a case with at least some independent ground-based observations available.

Please see above.

Page 11, Lines 5-6: I am not sure this has been demonstrated satisfactorily.

Text has been modified to address that our goal was to focus on improving the initial conditions for aerosol in the hopes that the forecast will also be improved.

Page 11, Line 24: This was not very convincing either.

In addition to DFS, we have rewritten section 5.3 to further examine a few meteorological variables that were altered during data assimilation, and compare the updated state (i.e., the analysis) with independent observation from NUCAPS retrievals.

Page 12, Line 1: Yes, but again marginally better.

As stated in the revised abstract and the introduction, this is a first-time effort to apply the newly developed RAMS-MLEF system to the assimilation of aerosol. We believe this paper should be viewed as a proof-of-concept rather than an in-depth overview of the performance of the system.

Page 12, Lines 7-12: I agree, but I think this should be addressed in this paper rather than postponed to future work. Can you run an additional case where more observations were available?

Please see above.

Figure 3: Please provide URL.

Fig. 3a DEBRA: http://rammb.cira.colostate.edu/ramsdis/online/msg-3.asp

Fig. 3b MODIS Aqua Reflectance:

https://worldview.earthdata.nasa.gov/?p=geographic&l=MODIS_Aqua_CorrectedReflectance_TrueColor,Graticule,Reference_Labels(hidden),Reference_Features(hidden),Coastlines&t=2016-08-04-T00%3A00%3A00Z&z=3&v=44.22744414882004,21.24767787383792,59.208137478945034,28.16631631996292

Anonymous Referee #2

The focus of this paper is to use coupled atmosphere-aerosol data assimilation to improve forecast initial conditions using ensemble data assimilation (Maximum Likelihood Ensemble Filter). The focus is on a dust case study to demonstrate the use of coupled data assimilation in the difficult to predict littoral zone. Coupled aerosol-atmosphere data assimilation is very new, so I think this work is a useful contribution. However, I believe for it to be published additional information and substantially more analysis needs to be provided. There are several comments that I think should be addressed:

1) In the introduction (page 2, line 31-32), it is mentioned that the goal of this study is to improve representation and forecast of aerosol in the littoral zone. This is the only place in the whole paper that this is mentioned. If this is a main goal of this work and part of what makes this analysis unique from previous aerosol-atmosphere coupled data assimilation studies, it would be beneficial to expand on this in the introduction and mention what the results mean for this goal in the conclusions. It would be useful to address why coupled data assimilation is the right strategy for this problem and why you chose this case study with the goal in mind.

   In order to prevent a reader from getting the impression that data assimilation necessarily improves a forecast, we have modified text to explicitly state that the goal of this study is to improve the analysis field, a.k.a. initial state for RAMS, and to provide a published example of where an improved initial state did not lead to an improved forecast of HWRF.

   We have also provided text both in the abstract and introduction to emphasize the motivation of this study was the missing of Persian Plume by the InfraRed-based dust enhancement algorithms. In addition, as the first-time efforts of applying RAMS-MLEF system to assimilate aerosol, our goal is to convey to the readers that the current effort is to be interpreted as a proof-of-concept.

2) One of your main conclusions is that the assimilation doesn't improve the representation of the Saudi Arabian plume because of lack of observations in the interior of the plume. In your introduction, you discuss various MODIS retrieval algorithms for AOD, including the Deep Blue retrieval over land. The Deep Blue MODIS retrieval would provide the over land observations for the Saudi Arabian plume. Why did you not assimilate this dataset as well? It might be worth rerunning the experiment with these observations included.

   We apologize for not making it clear in the first submission that this study focuses on the Persian Plume. Similar to our reply to your comment 1, we have added text to reinforce this statement.

   Dark Target retrievals were chosen because of their higher spatial resolution compared to the Deep Blue retrievals. The spatial resolution of Dark Target retrievals is 3 km while the spatial resolution of the Deep Blue product is only available at 10 km. In addition, the size of 3 km x 3 km (10x10 km) are nominal pixel sizes at nadir and they can increase to 14.4 km x 6 km (48 km x 20 km) at edges. The pixel size of 48 km x 20 km is much larger than the size of the grid point (15 km) used in this study. As a consequence, the Dark Target retrievals are used.

   Similar to MODIS, MISR aboard NASA's Terra satellite is another instrument that provides aerosol information. However, the availability of MISR aerosol retrievals is relatively low compared to MODIS due to its narrower swath width (MISR ~360 km; MODIS ~2330 km).

   Please also see our reply to reviewer #1's comments.

3) Can you describe in more detail how the ensembles are formed? Do the ensembles account for differences in boundary conditions for both the atmosphere and aerosol components? Along this same line, I think you should include some analysis of what the ensemble spread looks like for your experiment since this is important in determining the kind of impact the observations will have. Do you need some spin-up time for your experiments to generate sufficient spread? Did you do any analysis to determine if the spread is enough? This is a very short experiment, so more details to see what is produced in 6 DA cycles would be useful.

The initial ensemble was formed by utilizing a time-lagged methodology, which involves running a single deterministic forecast centered at the initial time (t = 0) of data assimilation, i.e., from t = -*T* to t = *T*, where *T* is a specified assimilation window, which in this study is 6 hours. During this deterministic forecast, model is configured to generate output at every *2T/N* steps and thus creating *N+1* outputs, where N denotes the size of ensemble (*N* = 32 in this study). The one output that is valid at t=0 is denoted xc, which serves as the control member. The other N outputs are used to define ensemble perturbations, p_i, i = 1, *N*, at t = 0 by $p_i = \frac{1}{\sqrt{N}}(x_i - x_c)$.

No, lateral boundary conditions (LBC) were not accounted for. Yes, there is a spin-up of 6-12 hours (2-3 DA cycles). Our general experience with MLEF suggest that after that initial time period there is a negligible impact of the initial spread. There is also an ensemble covariance inflation at the end of each DA cycle, following Whitaker and Hamill (2012) and Zhang et al., (2004). In particular, we use a linear combination of the two methods, with 0.9 weight given to the method proposed by Whitaker and Hamill (2012) and 0.1 weight given to the method described by Zhang et al. (2004).

We have expanded this particular portion of Section 4 to provide more detailed information on the configuration of the MLEF part of the experiments.

4) You cite an observation error from Liu et al. 2011, which just refers to the observation error from Remer et al. 2005. I would recommend citing Remer et al. 2005 as well since this is the source of the observation error that you are using. Also, the observation error that they use is actually 0.03 + 0.05 *AOT over water and 0.05+0.15*AOT over land. There are more recent error evaluations for collection 6 MODIS products (Levy et al 2013; Sayer et al 2013) There are also MODIS error evaluations specifically focused on aerosol assimilation that you should consider for your work (Zhang and Reid, 2006; Hyer et al 2011; Shi et al 2011).

We have included Remer et al., (2005) in the revised manuscript. We used 0.1 + 0.05 * AOD for AOD over ocean and 0.1 + 0.15 * AOD for AOD over land, but we made a typo in our initial submission. The typo has been corrected in the revised manuscript.

Thank you for providing the literature of the observation error assignment for MODIS retrievals. We wish to include them in our future work.

5) In Figure 6, it would be helpful to include the analysis of both experiments (ATMONLY and ATMAOD) since you are trying to compare the two experiment. If you don't show it because the difference in AOD was minimal between first guess and analysis for ATMONLY experiment, then it would be worth mentioning this. How do the analyses from ATMONLY and ATMAOD compare throughout the experiment?

We have included text in section 5.1 to address your comment. The added text reads as follows:

Since the differences of AOD field between the first guess and analysis of ATMONLY experiment are minimal, the analysis of ATMONLY is not shown for brevity.

We have also provided a total precipitable water (TPW) field from the analysis of both experiments in Section 5.3. In addition to AOD, the TPW field in ATMAOD experiment also appears to be supported by the NUCAPS retrieval. In general, the representation of the Persian Plume in the ATMAOD analysis, as opposed to that in ATMONLY analysis, is more consistent with the NUCAPS TPW pattern.

6) The conclusion that including AOD in the assimilation brings valuable information is not surprising since it's directly related to aerosol and has been shown to be of value in many other data assimilation papers, including some papers that were cited. It would be useful to get a better understanding from your work of the impact of the meteorological observation on aerosol. It doesn't seem like they are doing much for this case? Would you then just recommend weakly coupled data assimilation? Does the aerosol have any feedback on the meteorology? I think more discussion/analysis along these lines are needed since this is the important part of this work.

   Please see our reply above regarding the new Section 5.3.

7) For the analysis increment, you only show results from level 11. Why did you choose this level to look at? Is most of the aerosol contained in this model level? Are the increments similar throughout the model profile? I think you should expand on this analysis. I think it would also be useful to show the first guess, in addition to the increments, to get a better sense of how large of an impact the observations have.

   After a second view, we have decided to remove the discussion about analysis increment. Instead, Section 5.3 has been completely rewritten to include further examination of the analysis field using an independent observation from a NUCAPS retrieval.

8) You mention in the analysis increments section of the results that it is difficult to identify relationships between different variables. If you only allowed one additional variable at a time in your data assimilation (ie. aerosol and wind, aerosol and water vapor etc), perhaps you could get a better understanding of what the different cross-correlations are doing.

   A single observation experiment is often used to identify relationships between selected control variables. While it is beyond the scope of the current study to address this matter, a separate paper by Zupanski et al., (2018) in the ACP/AMT special issue titled *Holistic Analysis of Aerosol in Littoral Environments – A Mutidisciplinary University Research Initiative* explores the relationship between selected control variables in a coupled data assimilation via a single observation experiment.

9) You verify your results against the MERRA renanalysis product. You might want to include other products for comparison. The Navy also has an aerosol reanalysis product (NAAPS) and the ICAP multi-model ensemble is also a very useful product for comparison.

   Section 5.4 has been expanded to include the Navy Aerosol Analysis and Prediction System (NAPPS) product and the aerosol Multi-Model Ensemble product from the International Cooperative for Aerosol Prediction (ICAP) in addition to MERRA-2 product.

10) It would be useful to mention in the paper that you are using the ensembles to provide you with the forecast error covariance, including the cross-component elements that are needed for the strongly coupled data assimilation.
    The configuration of MLEF as discussed in Section 4 has been expanded to address your comments.

11) Lat/lon information on some of your figures would be helpful (Figure 3,4) and it is difficult to read the values on the colorbars in Figure 8.

    Revised.

Reference:

Li, R.-R., Kaufman, Y. J., Gao, B.-C. and Davis, C. O.: Remote sensing of suspended sediments and shallow coastal waters, IEEE Trans. Geosci. Remote Sens., 41(3), 559–566, 2003.

Liu, Z., Liu, Q., Lin, H. C., Schwartz, C. S., Lee, Y. H. and Wang, T.: Three-dimensional variational assimilation of MODIS aerosol optical depth: Implementation and application to a dust storm over East Asia, J. Geophys. Res. Atmos., 116(23), 1–19, doi:10.1029/2011JD016159, 2011.

Miller, S. D., Grasso, L. D., Bian, Q., Kreidenweis, S. M., Dostalek, J., Solbrig, J. E., Bukowski, J., Van den Heever, S. C., Wang, J., Walker, A. L., Zupanski, M., Wu, T.-C. and Reid, J. S.: A Tale of Two Dust Storms: Analysis of a Complex Dust Event in the Middle East, Atmos. Chem. Phys., (Sumbitted), 2019.

Pagowski, M., Liu, Z., Grell, G. A., Hu, M., Lin, H. C. and Schwartz, C. S.: Implementation of aerosol assimilation in Gridpoint Statistical Interpolation (v. 3.2) and WRF-Chem (v. 3.4.1), Geosci. Model Dev., 7(4), 1621–1627, doi:10.5194/gmd-7-1621-2014, 2014.

Remer, L. A., Kaufman, Y. J., Tanré, D., Mattoo, S., Chu, D. A., Martins, J. V., Li, R.-R., Ichoku, C., Levy, R. C., Kleidman, R. G., Eck, T. F., Vermote, E. and Holben, B. N.: The MODIS Aerosol Algorithm, Products, and Validation, J. Atmos. Sci., 62(4), 947–973, doi:10.1175/JAS3385.1, 2005.

Whitaker, J. S. and Hamill, T. M.: Evaluating Methods to Account for System Errors in Ensemble Data Assimilation, Mon. Weather Rev., 140, 3078–3089, doi:10.1175/MWR-D-11-00276.1, 2012.

Zhang, F., Snyder, C. and Sun, J.: Impacts of Initial Estimate and Observation Availability on Convective-Scale Data Assimilation with an Ensemble Kalman Filter, Mon. Weather Rev., (132), 1238–1253, 2004.

Zupanski, M., Kliewer, A., Wu, T.-C., Apodaca, K., Bian, Q., Atwood, S., Wang, J., Wang, Y. and Miller, S.: Assimilation of AOD and atmospheric observations using WRF-Chem and strongly-coupled data assimilation system, Atmos. Chem. Phys., (To be Submitted), 2018.