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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of data assimilation on the prediction of Asian desert dust using an operational 4D-Var system" *by* Angela Benedetti et al.

Anonymous Referee #2

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This paper assesses the impact of assimilating satellite AOD on aerosol forecast of AOD and surface PM10 with the ECMWF/CAMS global system. This is done by evaluating results from three model experiments with ground-based network measurements of AOD and PM10 for March 2013: (1) free model run (CONTROL), (2) model assimilation of MODIS Dark Target AOD (DT), and (3) model assimilation of MODIS Dark Target and Deep Blue (DTDB).

The paper is well-written and well organized and the information is interesting. However, I have several major concerns/comments that should be addressed in the revision.

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Discussion paper

1. The title of this paper explicitly indicates that the paper is to address the impact of data assimilation on the prediction of Asian dust. However, after reading the entire paper, I feel that the paper is not particularly focused on dust forecast, and only one dust episode is shown. The evaluation of AOD with AERONET is done within an extensive region and large fraction of the area has limited influence from dust. If choosing 1020 nm AOD is for its better representation of dust, it should be clearly stated in the paper. 2. In the abstract it is said that the model experiments were run to understand the relative contribution of Asian dust to air quality over China, but there is no any results or discussion on this topic in the paper. 3. There is not enough data used in the paper (only one month) to generate robust statistics. It is stated in the introduction that the model experiments were run for one year, but only March results are used in the paper. We know that the dust season in China lasts more than just a month in March, why not using multiple months to have more data for statistics? I also noted the statement that "ECMWF is providing twice-daily forecasts of atmospheric composition (including desert dust) up to 5 days ahead", so potentially there is a lot of results to use. 4. The impact of assimilation to surface PM10 should be much better and more quantitatively evaluated. From Figure 9, it is clear that PM10 from the three model experiments are nearly identical and the satellite AOD assimilation brings little improvements of PM10 prediction. Although DTDB is seen to be a little closer to the observations at some time steps, the so-called "improvements" are practically negligible and do not change the forecast skill at all. Please provide quantitative evaluation in this case, including peak values and timing, bias, correlation, etc. that can show the difference among the three model experiments and between model runs and observations to really understand the magnitude of "better agreement". 5. Actually, the PM10 case is a very interesting one that warrants a more in-depth analysis. In the three-day simulations shown in Figure 9, what are the AOD time series look like, compared with AERONET (and/or CARSNET) AOD in the Beijing municipal area? Does AOD and PM10 vary together or not? Can you explain the AOD-PM10 relationship in terms of aerosol vertical profile, composition, and other factors (e.g., hygroscopic growth of aerosols)? What do

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the results tell us about model characteristics and the effectiveness of AOD assimilation for PM10 forecast? 6. The assessment needs to be more objective, more robust, and more quantitative. For example, within the year of 2013, how many days of heavy dust episodes the CONTROL experiment would miss but DT or DTDB would capture? How significant improvements the assimilation brings in heavy dust (or pollution) cases and in background cases?

Minor comments:

Page 2, line 3: Add Taklimakan as a desert of dust source.

Page 2, line 18: Typo and incomplete sentence “since 2005ch is .”

Page 4, line 6: Is the prescribed dry deposition velocity particle size dependent? Does it depend on seasons and locations?

Page 4, line 7, sedimentation: This is strange - you could argue that the errors might be insignificant for the two smaller size groups from ignoring sedimentation, but using a fixed settling velocity is not justified, since the air density and viscosity changes spatially and temporally.

Page 4, line 10: “bulk parameterization” is for particle size, right?

Page 4, line 12-13, “Removal processes include sedimentation of all particles”: This sentence directly contradicting with the sentence in line 7 that “sedimentation is applied only to the largest dust bin”.

Page 4, line 14: How is sulphate formation from SO2 is dealt with in the model?

Page 4, line 23: What “atmospheric composition variables” are assimilated that are relevant to this study?

Page 4, line 24-26: How do you deal with the aerosol hygroscopic growth? How do you factor that in when you redistribute the aerosol mixing ratio at the end of minimization?

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Page 4, line 19-30, vertical profile: Please make it clear that the vertical profiles are all from the model; no data assimilation for aerosol vertical profiles.

Page 5, line 32: Change “1” to “Figure 1”.

Page 6, line 1 and Figure 1 and 2: The different spatial domains between Figure 1 and 2 makes it hard to visually relate the dust plume locations. I suggest make these two figures for the same geographic area or mark the Figure 2 area on Figure 1.

Page 6, line 3: From Figure 1, it looks that the dust storm originated in Taklimakan.

Page 6, line 4: transported to southeast, instead of southwest?

Page 6, line 7: Are the observed values from AERONET and CARSNET? What is the reason for using AOD at 1020 nm instead of 550 nm MODIS retrieved?

Page 6, line 8-9, SE Asia are: This is a large area. The stations within this area must have quite different aerosol composition. How many of them are surely being impacted by dust in your analysis?

Page 6, line 11-12: Four-digit after the decimal seems an over kill and means little. The differences are small: R = 0.74, 0.75, and 0.76. To what degree it matters? What are the RMSEs for these cases?

Page 7, Figure 3: What do the different colors represent?

Page 7, line 6-7: In the case of dust storms or episodes the “outliers” are probably the most critical ones for measuring the model skill.

Page 8, line 8-9: Can you quantify the model agreement with CARSNET and AEROENT separately? Is there any collocated CARSNET and AERONET stations to compare the differences? Do they use the same type of instrument? What are the known uncertainties of their instruments? Any calibration issues?

Page 9, line 5: Change “AD” to “AOD”

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Page 9, line 10: Where is the summary given? Figure 8?

Page 9, Section 4.2: It would be informative to know after how long the benefit of data assimilation disappears, and what does it tell us about the importance of the quality of the model itself.

Page 10, line 5-8: Too many subjective statements here. How much off is the timing that is "slight wrong"? What is the standard for "good agreement" (e.g., within x%)? What is the measure of the model skill that warrants the achievement of "a good degree of skill"? The evaluation is too descriptive and not quantitative.

Page 10, line 8-10: "the experiments with assimilated satellite data draw closer to the observations": How much closer? 1%, 5%, or 50%? The three lines in Figure 9 are nearly identical and I am not sure what matrix you use to benchmark the improvements? Clearly, quantitative assessment is needed. Can you use R, FB, and FGE for assess the results of PM10 here, similar to what you did for AOD, in order to quantitatively measure the effectiveness of assimilating satellite AOD on predicting PM10 concentrations?

Page 10, line 10 (continued on Page 12 line 1), the "spurious secondary peak of March 10": DTDB is about 370 ug/m3, which is probably 20 ug/m3 lower than CONTROL, but still more than 300 ug/m3 higher than the observation! It is hard to mark it as improvement.

Relative question regarding Figure 9: It would be helpful to indicate the MODIS overpass time that the data are ingested in the assimilation system. Clearly, the nearly identical time series of the three model runs reflect the fundamental characteristics of the model processes, of which the satellite data assimilation is not able to change. The opposite diurnal variations between data and model do not change at all, the more than 2x over estimation from late Mar 08 to mid Mar 09 remains the same magnitude among the three model runs, and the model behavior in late mar 09 to Mar 10 does not change at all from CONTROL to DTDB after the strong dust episode in Mar 9. So

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what have we learned from it? To me, the figure has told me that the assimilation of satellite AOD (1 or 2 time/day?) in this case helps make small adjustment of PM10 but is unable to change the quality of forecast.

Page 11, Figure 7: Please show statistics of the comparisons at each site. What are "gmsy", "goij", and "goik" in the legend?

Also, a general comment on the color scheme: model runs of CONTROL, DT, and DTDB are represented in green, red, and blue in Figure 6, but green, red, and gray (dashed line) in Figure 7, and yet, they are red, green, and blue in Figure 9! Please keep the color scheme and style consistent.

Page 12, Figure 8 caption: There are only two rows in Figure 8 and there is no "middle" row.

Page 12, line 9-10: As I mentioned again and again, the effectiveness of assimilating satellite data needs to be quantitatively assessed. The assessment of the impact on daily AOD (not just for dust) forecast is more quantitatively done, but the assessment of the impact of diurnal variation of PM10 is mostly addressed by visual impression and subjective.

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