

Reply to the Editor

Dear Authors,

Due to the unexpected difficulty to obtain a second independent review, I will provide my evaluation of the paper as Editor / Reviewer. Such delay doesn't occur often and I should apologize for the time needed to close the discussion of your paper. Thanks for your patience. I have read carefully the paper and, in agreement with reviewer 1, I find it highly interesting, well written and with a high pertinence to ACP scopes.

Dear Editor,

We would like to thank you for your effort in seeing this paper reviewed and evaluated for possible publication, including serving as a reviewer. Please find our answers to your comments below.

I have nevertheless major issues that relates to the methodology of evaluation.

(1) The comparison is limited to the month of March 2013 while MODIS data shows that April and May 2013 have high levels of AOD (dust) in the region. So it would be highly desirable to extend the period of analysis and provide a more structured presentation of the events that are included in the analysis period in addition to the snapshot of figure 1. This may improve the quality of the analysis as outlined below.

The period of analysis has been extended to three months (March-May 2013) which covers the whole Asian dust season. Moreover the spatial area has been restricted to central/northern China to be able to focus better on the dust monitoring.

(2) Moreover, a weakness is the demonstration of the improvement of forecast that is central to your analysis. I am convinced that limited additional work may improve the quality of the paper and its potential impact.

The evidence for an impact in the forecast has been added (new figure showing both bias and FGE as a function of forecast range). However, the title was changed to refocus the paper. It now reads: "The value of satellite observations in the analysis and short-range prediction of Asian dust" which describes better the intention of the paper and does not arise unmet expectations in the reader.

Few specific points:

-Figure 4 aims at showing a bias reduction. This is qualitatively discernible from color scale. Nevertheless, it would be more convincing to propose a quantitative table in addition to the time series of figures 6-7. A comment on why the spatial distribution of the improvement is desirable.

-While comparison with AERONET provides convincing outcomes, the one with CARSNET, as also stated by the authors is less discernible. Especially the expected increase in AOD related to the March 9th event that is almost not visible in the observations of figure 7. It is difficult to say whether improvement is achieved or not (e.g. look for instance at Tahzong site)

Due to the change in the study area from the wider South-East Asia (23S-50N,65E-180E) initially evaluated to the current area in the revised version of the paper (Central-northern China, 30N-45N, 75E-135E), the comparison with CARSNET station data appears now clearer in the bias plots. For the station data, what is shown is now a longer time-period and it's possible to monitor the behaviour of the model over the whole spring season. In particular for Tahzong, although the agreement is far from perfect, it is possible to discern that the run with assimilation of Dark Target and Deep blue MODIS data generally outperforms the run without any aerosol data.

-A set of model maps from three experiments may be desirable here to evaluate where and how assimilation improves / modify the results.

Unfortunately, the model plots do not show very well the changes. These are better captured in the maps shown in figures 4 and 5.

-The method of comparison with the independent data may be better outlined. It is mentioned that all stations in a single grid box are considered. Are they aggregated ? averaged ?

This sentence was added in the text:

“Model data was bilinearly interpolated to the AERONET/CARSNET site locations and then averaged over 24 hour periods (from T+3 to T+24). AERONET data is similarly averaged, with each data value receiving a weight proportional to the time difference between the data values before and after it, up to a maximum of three hours. The CARSNET data used was already in the form of daily averages and no further averaging was done.”

- The effect of assimilation in forecasts, as expected, is less evident and limited in this analysis to histograms of figure 8. This is just partly evident from comparing fractional bias in the bottom line. A more quantitative statement would be desirable here. Moreover, also FGE would be interesting. It would also be useful to have time series. As said above, it would be much beneficial to extend the period of analysis.

This has been done and a new figure was included in the paper to show the evolution of the fractional bias and FGE over the forecast range.

- The improvement described in figure 9 for the Beijing area is not particularly striking. The authors states that higher values (less than 10%) of PM10 for experiment Modis DT+DB are due to the benefit of assimilating additional data.

This section has completely been reworded. In particular, the value of the global model analysis and forecasts for PM10 have been criticized and put into perspective.

- Conclusions may report a more through discussion on the system skills and limitations before stating in the last sentence about the “usability” of AOD forecasts.

This has been done.

Minor (editorial) issues:

Page 2 line 18:

“2005ch is” → Typo ?

Corrected

Page 3 line 23: please detail what CEPA is

China Environmental Protection Agency, it was in the abstract but it is now repeated in the main body.

Page 4 line 20: add the CAMS website where data are freely available. You may state it also in the acknowledgements

Website added.

Page 5, 15: A sentence to clarify why the experiments were carried out at 80 km instead of 40 km and an assessment of the validity of results for both resolutions would be useful here.

The resolution of the CAMS system in 2013 was 80km, and the experiments were run with that configuration. To present a comparison with the current resolution (40km) would involve running more experiments which at this point is not feasible for this study. The impact of the change in horizontal resolution on the aerosol forecasts is nevertheless an interesting point which was addressed only internally and should be the subject of a separate article.

Page 9, 14: Sentence on Users and forecast time is not clear (it seems obvious that an improvement at T0+48 is useful) - Clarify – extend or skip it.

A reference with a concrete example was included to clarify the statement about the usefulness of the 48h forecasts.

Page 9, Section 4.2: I guess the discussion refers to figure 8 – please refer to it.

Reference to the figure has been added.

Figure 7: please, use the same terminology for experiments in the caption

Figure 9: same as above

The terminology has been homogenized.