

Interactive comment on “Improving PM_{2.5} retrievals in the San Joaquin Valley using A-Train Multi-Satellite Observations” by A. W. Strawa et al.

Anonymous Referee #1

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In this paper, the authors reported new method to improve PM_{2.5} retrieval in San Joaquin Valley using A-Train Multi-Sensor Observations. This is a well written paper, yet it lacks in scientific discussion and innovative ideas. Paper also makes several conclusive statements without proper analysis or reference, which leads to miss representation of the data. Some of the analyses performed in the paper demonstrate the limited knowledge of the authors on data being used in this study. For example, trying to correlate MODIS AODs at 447 nm and 550 nm (table 2 and text on page 10), which does not show any new thing or make any sense due to the fact that MODIS dark target algorithm reports AOD at 550 nm by interpolating AODs at 447 nm and 667 nm. In other words AOD at 550 nm is calculated using AOD at 447nm; therefore they must show high degree of correlation. Also, it is surprising that why AOD 667 nm has not been used in similar sense? In an attempt to use only satellite data for

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PM_{2.5} estimation, authors have thrown all available satellite products into statistical model ignoring the actual meaning of predicting parameters. For examples, the use of AODs at same wavelength from two MODIS algorithms does not make sense. The arguments behind the use of two AODs are their poor correlation (Authors claim this is interesting – I would say it is not interesting but big reason to worry). If the two algorithms are performing well then AODs should be very close (or exactly same) to each other. Poor correlation between two AODs clearly shows quality of at least one AOD is very poor and should not be used in the model. Inter comparison with AERONET AOD can be performed to determine the quality of AOD data. The poor correlation in AODs may also arise due to the fact that only on rare occasion both deep blue and dark target algorithms retrieve AOD values for the same pixel due to limitation by surface on each algorithm. Therefore, it is possible that two AODs represent two different geographical areas, which is not visible as data is averaged over 5x5 pixels in this study. The study also reports results of six different GAM models (table 4), which varies in terms of number of parameters used to estimate PM_{2.5} mass concentration. In order to see the impact of adding a specific parameter to GAM, it is important to keep the number of observation same (keep the sample same). If different set of observations have been used in different GAM model then it is difficult to say that improvement in correlation is occurred due to the inclusion of specific parameters in the GAM model. I would recommend redoing the analysis while keeping same sample in all six models and just changing the number of parameters. There are several other arguments made throughout the paper, which are subject to concern.

1. To improve the number of data points, quality flags associated with satellite data are ignored (page 8) – this condition force study to use poor (or unknown) quality of the data.

2. OMI pixel is large enough therefore single pixel is selected (page 8) –OMI pixel size is 13x24 km whereas MODIS is 10x10 km. So if you are averaging 5x5 MODIS pixels, which covers approximately 50x50 km area then the use of single OMI pixel cannot be

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justified.

3. Meteorological effects are represented with a seasonality parameter – local meteorological is one of the most important (after emission), which controls the PM_{2.5} at surface and which cannot be captured just by seasons. Several studies in past have shown this [Tai et al., 2011, Atmos. Chem. Phys. Discuss., 11, 31031-31066, 2011, Liu et al., 2004, 2005, 2006].

4. OMI NO₂ does not typically correlate well with surface measurements because surface measurement is a point measurement while satellite data represents a more distributed value (page 14). If this is the case then why AOD from satellite should match with surface point measurement? Is it possible that OMI derived NO₂ is not sensitive enough to boundary layer NO₂ concentration due to its use of UV channel and low signal to noise ratio.

5. Figure 4 – not clear what is sensitivity means?

Similar to these there are too many technical small and large problems throughout the paper for me to cite. I would recommend reanalysis of the data, which may require serious effort and therefore, I recommend that the authors revise and resubmit the paper. The paper in current format is not acceptable for the publication.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 30563, 2011.

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