

Interactive comment on “Assessing uncertainties of a geophysical approach to estimate surface fine particulate matter distributions from satellite observed aerosol optical depth” by Xiaomeng Jin et al.

Anonymous Referee #2

Received and published: 11 November 2018

This is a very well written paper that explores sources of random and systematic bias on estimates of ground-level PM_{2.5} derived from satellite based AOD measurements and the ratio of AOD and PM_{2.5} from a regional air quality model.

The paper provides a review of the literature in this area, and then uses MODIS MA-IAC data and the CMAQ model to make PM_{2.5} estimates. Comparisons are made to Aeronet ground based measurements, and field measurements from the DISCOVER-AQ campaign. They carefully evaluate errors that originate from satellite AOD errors and from the modeled PM₅/AOD relationship.

The methodology, analysis, and data sources are all clearly described. The figures are well formulated and clear. I found the conclusions to be very clearly written and supported by the details in the manuscript.

There is one area where the authors should consider revisions. I think the hygroscopicity is an important element, and perhaps does not come across that way given that the details of the models for RH dependent particle growth are in supplementary material, and the statistics for RH are calculated like all the others. I would argue that factors like MEE and mass can be shown in box and whisker plots, but not the RH. The change of mass and extinction is very non-linear in RH. If the model says the RH is 90% and the field measurements say it is 60%, the situation is very different then if the model says RH is 60% and the observations say it is 30%. Can the analysis focus on the error due to RH errors that lead to substantial errors in the estimated aerosol growth - separate out high RH cases? This error source will be very seasonal and regional. Figure 8 hints at this, but the discussion still treats RH as if it is a factor that can be aggregated and treated like other linear factors, and I disagree.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-962>, 2018.

[Printer-friendly version](#)[Discussion paper](#)