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Atmospheric Chemistry and Physics Discussions

Interactive comment on "10 yr spatial and temporal trends of $PM_{2.5}$ concentrations in the southeastern US estimated using high-resolution satellite data" *by* X. Hu et al.

Anonymous Referee #2

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The paper entitled '10 yr spatial and temporal trends of PM2.5 concentrations in the southeastern US estimated using high-resolution satellite data' by Hu et al., demonstrate the capabilities of current earth observations from satellites. Authors have selected very important and critical issue of spatial and temporal trends in surface PM2.5 mass concentration. Although, it is well written paper but authors fails to justify their PM2.5 estimation method and analysis. The methodology is poorly discussed in the paper. Authors do not provide proper reference or analysis on the claims about accuracy of satellite AOD product over urban regions. The paper in the current form is not recommended for the publication and I strongly suggest to resubmit. Followings are

major concerns:

1. Page 4, Line 25: Chudnovsky et al., 2012 shows a correlation of 0.62 and 0.65 for MYD and MAIAC AODs with ground PM2.5. First, it is equivalent to existing 10km product and I would not call 0.65 strong correlations. The authors should reexamine their claims and avoid such statements. I would strongly suggest that author provides inter-comparisons of MAIAC and MYD AODs with AERONET measurements in their study region.

2. In the model (eq 1) there are several input parameters and each have different impacts on PM2.5 fields. I have failed to obtain any information on which of this input parameter is most critical in PM2.5 estimation? After including all non-satellite parameters as input in the model, how much AOD improves the PM2.5 estimation? Can this model works in the areas where satellite AOD retrieval is not possible due to cloud cover.

3. Separate model for each year and then using the estimated data for trend analysis is hard to absorb. In this case it is very important to make sure there is no year to year bias in the estimations. It is not very clear why there is need to develop separate model for each year? Is there any specific relationship, which is changing from year to year? I believe AOD-PM2.5 relationship shows variability with seasons but why it should behave different in 2003 than 2005? Or there are other input parameters, which behaves differently in different years? Figure 7 clearly demonstrate that there is more variability in PM2.5 with different season than different year. I would develop models for seasons rather than years.

4. It is not very clear how the Figure 6 has been generated? Authors report the annual PM2.5 numbers for each year from 2001 to 2010 on Page 13, line 5. It is very clear from these numbers that there was hardly any changes in PM2.5 values from 2001 to 2007, in 2008 there was sudden decrease in PM2.5 values (also visible in Fig 7). Therefore, mapping 2001 to 2010 is miss leading as decreasing trend is not linear. There is hardly

any discussion on ‘why year 2008 has sharp decrease? Probably, difference between mean of 2001-2007 and 2008-2010 will be more meaningful.

5. I would strongly suggest that temporal trends should be analyzed for each season separately. Also, impact of fires on PM2.5 does not fit here and can be left out for separate analysis/paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 25617, 2013.