

## ***Interactive comment on “A robust calibration approach for PM<sub>10</sub> prediction from MODIS aerosol optical depth” by X. Q. Yap and M. Hashim***

### **Anonymous Referee #2**

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#### General comments:

Satellite remote sensing has been widely applied for air quality monitoring, and the satellite-based PM research has been used for recent health effect studies. In the sense, this manuscript addressed an important topic and presented a modeling approach which was appropriate for the study region. However, the representation of the methods, particularly model equations, was confusing and sometimes misleading. The authors need to clarify points mentioned in specific comments and thus improve the manuscript before publication in ACP.

#### Specific comments:

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P31485, L4: Consider citing any epidemiological studies (PM2.5 or PM10) done in your study region.

P31485, L9: Revise the second PM10 to PM2.5.

P31485, L22: Check the R2 value again. In the study, CV R2 was 0.92 from a daily comparison between the measured and predicted PM2.5 concentrations. The R2 of 0.62 was from a cross-sectional comparison between the measured and predicted site mean PM2.5 concentrations.

P31486, L8: Clearly mention if you indicate PM10 or PM2.5 throughout the manuscript.

P31487, L2: PM10 monitoring methods (such as instruments, monitoring frequency, and so on) need to be added.

P31487, L7: It is not clear what model equation was used and what parameters were included.

P31488, L3: It may be better to simply write  $\Delta\text{AOD} = \pm 0.05 \pm 0.15\text{AOD}$  over land.

P31488, L7: What percentage of AOD values was negative? If it was a large fraction of AOD values, you could bias your results by removing very clean conditions.

P31489, L9: It is not clear how you estimated the monthly spatial variability of the time-varying parameters. Are you just saying you estimated AOD-PM10 relationships monthly? If so, the texts need to be revised. If you actually used each of time-varying parameters (e.g., relative humidity), the equation (1) needs to be revised.

P31489, L14: You kept mentioning your model as 'statistical model' throughout the manuscript including texts, Table 3, and Figure 3. This is confusing, and it is better to refer the model as a mixed effects model.

P31489, L16: In the equation (1), I do not think the error terms should be related to  $\beta(\text{fix})$ .

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P31489, L25: I do not see the AOD random effects and site random effect in the equation (1). If you meant the AOD random effect as  $\varepsilon(mn)$ , this is misleading and needs to be revised.

P31489, L26/P31490, L4: Although you mentioned Lee et al. (2011) previously, the site bias and site random effect explanations need additional citation of Lee et al. (2011).

P31490, L13: Did you use both CV and three independent sampling sites for model validation? How did you perform CV in this study (e.g., 90%/10% random separation)? Were all the subsequent results (i.e., Table 3 and Figure 3) based on CV?

P31490, L21: In addition to correlation coefficient, you used RMSE in Table 3. RMSE should be explained in this section.

P31491, L9: Descriptive statistics for AOD need to be included in section 3.1. What was the average (SE) AOD in the study region? What about the number of AOD values in the study period? Did each location have relatively same number of daily AOD for estimating monthly AOD?

P31491, L19: Provide the range of the number of monthly sample points.

P31492, L8: Consider moving this equation (2) right after equation (1).  $\varepsilon(\text{fix})$  is a constant but varies by site. Therefore, it should be  $\varepsilon(\text{fix}, n)$ . You need to clearly explain how this equation (2) is related to the equation (1). Also it is not clear how you estimated site-specific constant from the regression.

P31494, L1: Is this R2 (0.77) based on CV?

P31494, L5 and 9 and Table 3: Specify what this p-value is for (slope, intercept, or both).

P31494, L9: The intercept is -5.5 not 5.5 based on Figure 4.

P31494, L16: Lee et al. (2011) used a mixed effects model as you mentioned. The linear regression model was only for comparing with a mixed effects model as you did

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in your manuscript. This can confuse readers, and Lee et al. (2011) should not be included in this line.

P31499, Table 1: Presenting 4 decimal points for mean and SE is overly precise.

P31500, Table 2: Again, you do not need to present 5 or 6 decimal points for mean random error. Also p-value can be simply expressed as  $p < 0.0001$ .

P31501, Table 3: RMSE should be positive by definition.

P31504 and P31505: X- and Y-axis should be revised because same intervals should have same length in both X- and Y-axis.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 31483, 2012.

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