Review of Xue et al., Satellite-based Estimation of the Impacts of Summertime Wildfires on Particulate Matter Air Quality in United States

This study uses the GWR method to predict surface PM2.5 concentrations in the US based on satellite AOD and meteorological variables. The statistic method is robust and is well referenced from previous studies, and the prediction results show good agreement with the in-situ measurements. However, the study is still lack of adequate scientific expansion from the results, and the conclusions are similar to the studies on satellite AOD products or ground measurements only, making this study less meaningful.

Before the consideration of publishing, the authors need to further explore the prediction results, make good findings or quantifications that simple AOD and scattering ground measurements cannot show. The authors also need to clean up the minor typos, formats, the potential figure-caption disagreements and misleading journal names in the page head.

We thank the reviewer for the insightful comments, and we have incorporated changes to reflect most of the suggestions provided by the reviewer.

The major difference between the science of AOD and surface PM2.5 is that, AOD is showing the vertical column conditions instead of surface only. Even not considering the aerosol chemistry and secondary formation in clouds, the convection conditions, atmospheric stability or vertical profiles of other meteorological conditions should contribute very much to the difference of AOD and surface PM. Especially for fire plumes, the long-term transport of fire smoke can be at a high altitude, and the vertical pattern of PM will be very different from the no-fire patterns. However, in the GWR model used in this study, only near-surface data are used. Also, noticing the AOD coefficient is much higher that all the other predictors (Table 4). It is doubtful how good the model is, compared to the agreement between AOD and surface PM. Therefore, the authors need to:

1. Show the improvement of the model from using AOD as the only factor, and discuss how the model predict the surface PM out of a column variable.

We added a paragraph describing how the column AOD is related to surface PM2.5:

AOD which represents the total column aerosol mass loading is related to surface $PM_{2.5}$ as a function of aerosol vertical properties and physical properties (Koelemeijer et al., 2006):

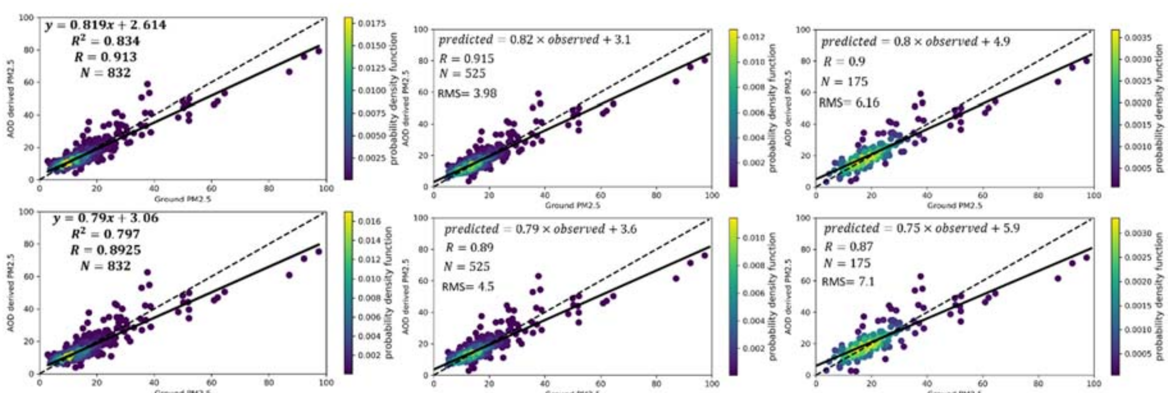
$$AOD = PM_{2.5}\, H\, f(RH)\, \frac{3Q_{ext,dry}}{4\rho\, r_{eff}} = PM_{2.5}\, H\, S \qquad (1)$$

Where H is the aerosol layer height, f(RH) is the ratio of ambient and dry extinction coefficients, $Q_{ext,dry}$ is the extinction efficiency under dry conditions, r_{eff} is the particle effective radius, ρ is the aerosol mass density and S is the specific extinction efficiency (m^2 g^{-1}) of the aerosol at ambient conditions. Therefore, AOD usually has a strong positive correlation with $PM_{2.5}$ but the relationship varies depending on other meteorological variables. BLH is used to represent for H by assuming boundary layer is well-mixed under most conditions, and RH related to f(RH) in the equation can adjust the ratio between PM2.5 and AOD through hygroscopic growth of particles. Other meteorological such as surface temperature, wind speed and surface pressure mainly affect the aerosol mass density through different processes which are introduced in the 2.3 section of the paper.

The improvement of the model (GWR) from using AOD as the only factor has been investigated in other studies (Jiang et al., 2017), which shows improvements of R^2 from 0.69 to 0.78 and RMSE from 7.25 to 6.18 by adding 4 meteorological parameters in summer in easter China. For our model, R^2 increases from 0.79 to 0.83 and RMSE decreases from 3.8 to 3.4 from the AOD only model. R^2 and RMSE has larger improvements for smaller AOD values than AOD larger than 35 µg m^{-3}: R^2 increases 0.9 from AOD only model for regions with AOD less than 35 (0.6 to 0.69), while R^2 increases 0.05 for areas with AOD larger than 35. RMSE decreases 12% and 7% for AOD less and larger than 35 conditions respectively. Overall, the meteorological factors have larger improvements for low polluted areas.

2. Estimate the model performance only looking at fire region, compare to Figure 3, and discuss the performance and potential bias.

The figures below show the model fitting and validation results for the whole US (left), smoke regions (middle) and NW US (right). The left figure is the original figure 3 in the paper, the middle figure is the results for performing GWR model only for regions with smoke flag larger than 0 and the right figure is for NW US bounded by 35~50°N and 105~130°W. the model performances at fire regions are relative stable and shows similar results compared with the model for whole US region. The R^2 have very little variances among the three models, only the RMSE varies due to the higher $PM_{2.5}$ values close to the fire. This is the benefit of using GWR model to simulate spatially varying factors. The model itself already considered spatial variances, so the model performance at different regions should be the same.

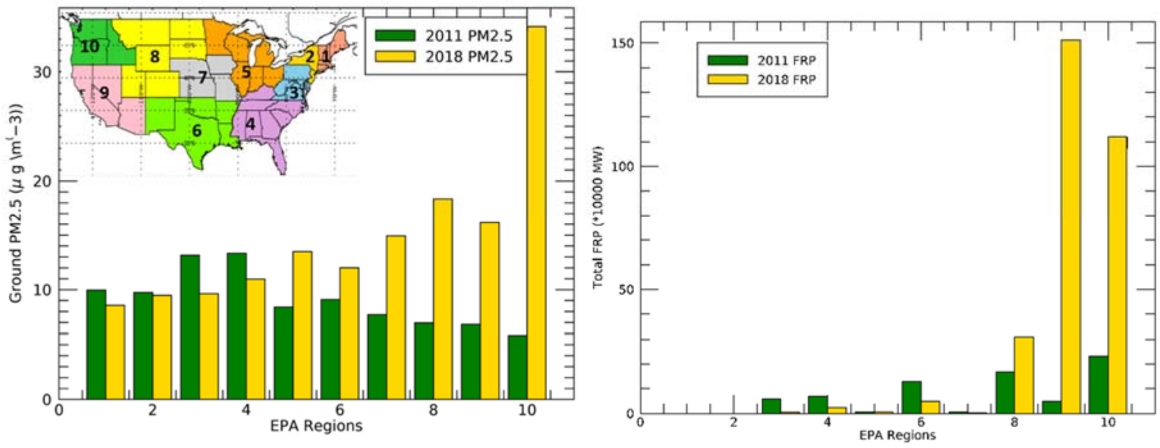


Except for the main concerns, there are some minor suggestions and questions listed below:

1. Line 141-143: Since all the regions in the US are evaluated (in Figure 6), FRP in the other regions should also be verified, to make sure the 2011-2018 difference over the regions other than NW US is not affected by regional fire.

The figures show the PM2.5 difference (left) and FRP difference (right) of the two years in different EPA regions. NW US (EPA region 8~10) has larger FRP in 2018 than in 2011 while all other regions has no fire or has slightly larger FRP in 2011 than in 2018. Therefore, the PM2.5 increase in region 6 due to 2018 fires could be underestimated, and the PM2.5 decrease in region

3~4 is partly due to the fires in 2011. By quantifying the total FRP in different EPA regions, we add some relevant uncertainties in the 4.6 section.

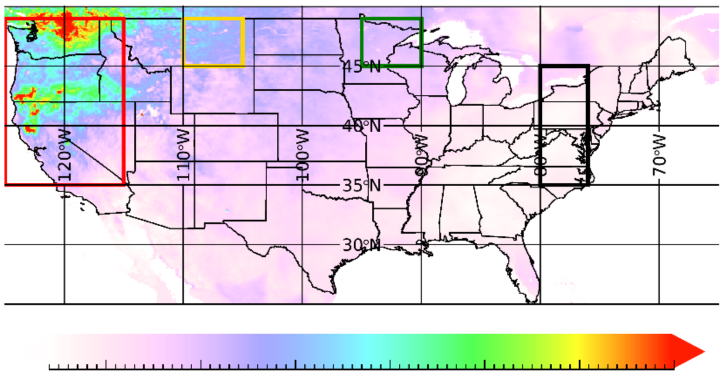


2. Line 269-270: as discussed in 1.2, further discussions e.g. calculation R2 for high PM values may be useful.

As suggested by the reviewer, we have added some discussions for R^2 for both high and low PM2.5 values in section 4.2.

3. Line 274-278: Is there any logic about the box selecting? For example, how to decide the size of the box? Can the box be larger? For each type of region, the authors can also show a regional mean with standard deviations of each coefficient. Also, can the box/region selection be more quantified, for example, by classifying using the background PM concentrations or FRP?

We thank the reviewer for pointing this out. The box selecting is based on the smoke transport path and surface PM2.5 concentrations. The red box is selected to include all the fire sources in US (as mentioned in the paper, major fire sources are in 3 states: Washington, Oregon and California), the yellow box is selected based on the estimation map of surface PM2.5 because this region has relative high pollution concentration and no fire sources which means that the smoke is transported from other places. The green box is selected of the same reason as the yellow one but with longer distances from the fire source region which represents for different vertical distribution from the yellow box. the black box is the farthest region from the fires in which the pollution sources should mainly from anthropogenic pollution but not wildfires. The size of red box is the largest so it can cover 3 states and all the fires, while the yellow and green box are the same size large enough to include PM2.5 within certain ranges and the black box size is chosen to include the minimum change regions according to the difference map. Within each of the box, we also select samples according to the estimated PM2.5 and FRP values to calculate the mean and standard deviation of different coefficients.



D-PM2.5 map between 2018 and 2011 August

Mean coefficients	sample selection	N	AOD	smoke flag	PBL	T2M	RH	U	SP
box1(red)	FRP>1000	213	91.94	-0.14	-2.25	0.33	0.08	-2	-0.06
box2(gold)	PM2.5>30	362	60.1	0.013	-2.9	0.23	-0.08	-1.6	-0.03
box3(green)	PM2.5>17	278	6.2	0.05	0.2	0.2	0.014	-0.3	-0.02
box4(black)	17>PM2.5>10	938	7.1	-0.02	-1.2	0.22	-0.035	0.06	-0.005
whole US region	~	106352	28.1	0.024	-0.9	0.06	-0.04	-0.7	-0.002

Standard deviation	AOD	smoke flag	PBL	T2M	RH	U	SP
box1(red)	6.5	0.04	0.5	0.3	0.15	0.45	0.03
box2(gold)	6.1	0.02	0.11	0.07	0.02	0.07	0.004
box3(green)	0.11	0.002	0.05	0.02	0.002	0.02	0.001
box4(black)	1.45	0.01	0.22	0.17	0.03	0.16	0.006
whole US region	29.3	0.08	1.01	0.31	0.1	0.66	0.03

4. Line 612-614: The caption of Figure 4 seems not agree with the figure it self. "*PM2.5 values equal or larger than 30 μg m-3 are shown as the same color (red)…*", but the color label is ~-5 to 60 ug/m3.

Corrected.

The "Satellite-based Estimation of the Impacts of Summertime Wildfires on Particulate Matter Air Quality in United States" presents an interesting work, although it requires a major revision before it is suitable for publication in the esteemed journal ACP.

We thank the reviewer for the helpful suggestions and we have revised the paper to address the concerns.

a. The term "Particulate Matter Air Quality" is very confusing here. I have checked the manuscript and I did not see the authors discuss PM10 or other Particulate Matter. Meanwhile, "Air quality" can make people think of "Air quality index". Actually, the authors should specifically emphasize this research is concerning the Impacts of Summertime Wildfires on PM2.5 concentrations. The revised title is more precise and more consistent with the abstract and the manuscript.

Thank you for pointing this out, we have changed the title to PM2.5.

b. Given many relevant papers concerning PM2.5 estimation I read and reviewed, PM2.5-Meteorology interactions are very important factors to be considered and well explained. For the introduction part, the reviewer should briefly state that different meteorological factors influence pm2.5 concentrations through different mechanisms and introduce some relevant studies to present readers a clear background. In the method part, when the authors explain what meteorological factors were included in the research, they should also briefly introduce why or introduce some relevant studies.
Generally, PM2.5-Meteorology relationship is closely related to this research and should definitely be introduced more in details in the manuscript. Authors could refer to some recent references for more information.

As suggested by the reviewer, we have added a paragraph explaining the interactive processes between PM2.5 and meteorological parameters in section 2.3.

c. The format and language of this manuscript should be carefully checked, as there are many unnecessary typos. For instance, this manuscript was not converted to a specific ACP template. Instead, it confusingly shows" for review in remote sensing of environment". And, throughout the manuscript, 2.5 in "PM2.5" is not presented in a subscript format, which should be completely checked and revised. And other language issues should also be addressed through a more careful proof-reading.

We have corrected all the typos pointed out by the reviewer.