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To
The Editor
ACP

Dear Editor,

We are submitting a revised version of the manuscript MS No.: acp-2020-1152.

Reviewer 2 suggested accepting the manuscript with minor revisions.

Reviewer 1 suggested that we include the material from the response to his suggestions in the manuscript. We have added material to the revised manuscript and added a supplementary section explaining further details.

We believe that the manuscript is now ready for publication.

We look forward to a positive response.

Sundar Christopher.

Sincerely,

Sundar. A. C.

Sundar A. Christopher
Professor

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

The authors did not address the major issue I pointed out in the first response, and the revised manuscript did not add any information supporting a deeper finding or importance of this work. Some of the questions are well answered, which should be good for the scientific significance, but the corresponding updates did not appear in the revised manuscript. The authors need to further consider this issue and do full discussions in the manuscript.

We thank the reviewer for the insightful comments, and we have been able to incorporate changes to reflect most of the suggestions provided by the reviewer. We have added additional material in a supplement section of the revised manuscript.

Before discussing the old issue: although it may not be a reviewer's responsibility, the font size in Section 4.3 is smaller than the previous test, and the acknowledgment did not show credits to the in-situ PM2.5 measurements or the ECMWF products. Keeping these errors and typos will hurt the credibility of the journal.

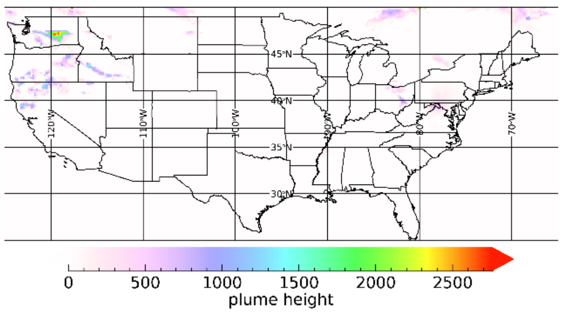
We the reviewer for pointing this out. The font size is corrected, and the acknowledgment is added for both EPA and ECMWF data.

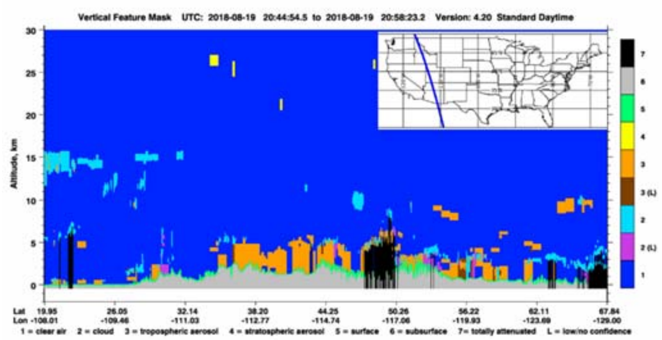
1. For the response to my major issue 1, the response is not describing how the column AOD is related to PM2.5. Here is the reason:
Here shows the equation (1) the authors added:

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

This equation represents the AOD in a specific layer. If 'H' is the height of the column, the 'AOD' is the column AOD; if 'H' is the PBLH, then the 'AOD' on the LHS is just an AOD of the PBL. Thus, when in the response the authors claimed that H represents PBLH, the AOD is not the total column from satellite data. This claim did not appear in the manuscript. Therefore, the main issue pointed out in the last review was not solved. AOD does have high correlation with PM2.5 because usually the vertical PM2.5 profile does not significantly change, and the surface source of PM2.5 is mostly mixed in the BL. It is right in most cases, but not in the fire cases. The average fire plume height is 1-2 km, and sometimes the plume can go to 5 km, or even higher. When this study is focusing on wildfires, this issue cannot be ignored, and the free-troposphere transport of smoke will be a robust bias in the model, because the model did not include any parameters with the vertical information. This probably leads to the underestimation of the prediction of the large values. The authors need to well address and discuss this issue in the manuscript.

We thank the reviewer for pointing this out. we add some explanation in section 2.3 and some discussion in 4.7 about the uncertainties. We tried to use the plume height (smoke injection height in MAIAC product MCD19A2) as one input to the GWR model, and it increased the R value from 9.13 to 9.14 but decrease the R for validation from 0.89 to 0.88. For both smoke regions (smoke flag>0) and NW US region, adding the plume height information leads to prediction accuracy decreases. We also compared the plume height product (below left figure) with the CALIPSO vertical profile (below right figure) on August 19th 2018, and the comparison shows that MAIAC underestimates plume height. therefore, we decide not to include the plume height in the model. It is difficult for passive sensors such as MODIS to calculate plume heights.





The second part of this response discussing the previous studies is good for stating the significance of the model and the study, which also answers another reviewer's question about the correlation with meteorological data, but it seems not in the manuscript. This also helps the discussion in the previous paragraph: when the vertical profile information of PM is missing in the model, the weight of parameters other than AOD should be higher. The authors need to discuss this in the manuscript with Table 4, showing the agreement or disagreement about the AOD weight in this study and previous studies.

We added the discussion in section 4.3. The actual weighting is hard to compare with other studies since the coefficient differs for different cases, so we just use different regions in our own model to illustrate the vertical distribution information can improve the model compared with regions with no vertical information (where aerosol higher than BLH).

2. The figures showing the smoke impact region and NW US addressed my question, but not showing in the manuscript. If the authors prefer a clear main text, I recommend the authors include this in the supplementary materials, because without it, it will be the reader's concern that the low values from regions free from fire smoke may dominates the high R.

We added the contents in the supplementary materials.

3. In addition to the previous review and response, the authors need to compare the method and model performance with previous studies.

We added a section (4.6) to compare our method and results with previous studies.