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.

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 $PM_{2.5}$ measurements or the ECMWF products. Keeping these errors and typos will hurt the credibility of the journal.

1. For the response to my major issue 1, the response is not describing how the column AOD is related to $PM_{2.5}$. 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 $PM_{2.5}$ because usually the vertical $PM_{2.5}$ profile does not significantly change, and the surface source of $PM_{2.5}$ 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.

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.

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

   Liang, F., Xiao, Q., Huang, K., Yang, X., Liu, F., Li, J., Lu, X., Liu, Y. and Gu, D., 2020. The 17-y spatiotemporal trend of PM2. 5 and its mortality burden in China. Proceedings of the National Academy of Sciences, 117(41), pp.25601-25608.
   Xiao, Q., Chang, H.H., Geng, G. and Liu, Y., 2018. An ensemble machine-learning model to predict historical PM2. 5 concentrations in China from satellite data. Environmental science & technology, 52(22), pp.13260-13269.
   Geng, G., Meng, X., He, K. and Liu, Y., 2020. Random forest models for PM2. 5 speciation concentrations using MISR fractional AODs. Environmental Research Letters, 15(3), p.034056.

   I am not listing all, there are a lot of $PM_{2.5}$ estimations from AOD in the US and around the world. The authors need a full literature review to estimate the advantage and disadvantage of the model methodology, the model performance compared to previous studies, and the performance applying in fire.

   Also, previous studies of fire $PM_{2.5}$ estimates such as Geng et al (2018) and else also need to be discussed.

   Geng, G., Murray, N.L., Tong, D., Fu, J.S., Hu, X., Lee, P., Meng, X., Chang, H.H. and Liu, Y., 2018. Satellite-Based Daily PM2. 5 Estimates During Fire Seasons in Colorado. Journal of Geophysical Research: Atmospheres, 123(15), pp.8159-8171.

The authors well addressed the other comment in the last review.