Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1152-RC1, 2021

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Satellite-based Estimation of the Impacts of Summertime Wildfires on Particulate Matter Air Quality in United States" *by* Zhixin Xue et al.

Anonymous Referee #1

Received and published: 4 January 2021

Review of the paper "Satellite-based Estimation of the Impacts of Summertime Wildfires on Particulate Matter Air Quality in United States" by Xue et al. to ACP

This paper uses long-term Aqua and Terra MODIS Fire Radiative Power (FRP), Aerosol Optical Depth (AOD), and surface observations of PM2.5 (particulate matter with median diameter smaller than 2.5 um) to study the impact of smoke from wildfires on surface PM2.5. The authors picked a 2-week time period in 2018 to represent extreme wildfires and 2011 to represent low wildfire activity to compare and contrast and report the impact. While it is true that the human induced pollution levels are going down in the US and the role of natural events such as wildfires and dust storms in influenc-

ing the air quality is increasing, I find this work very rudimentary and without scientific rigor. The paper is well written no doubt but this work is merely an exercise of downloading data from different sources and making figures. Let me explain why I think this study needs a major re-work (scientific scope as well as methodology) and is not ready for publication: âĂć First and foremost, to conduct this study, there is no need to use satellite data because the analysis is done in an aggregate sense, spatially and temporally. There are enough ground monitors in different states influenced by smoke from fires that a study can be designed just around the surface monitors without even bringing in the errors associated with scaling satellite AOD to surface PM2.5; âĂć Second, some of the stations (100s of them) have daily (or every other day) speciation measurements including organic carbon and K+, biomarkers for smoke. The authors have not bothered to analyze the surface data and extract only data for the days or locations influenced by smoke. Yes, there are speciation observations from EPA network as well as interagency network (IMPROVE) in many of the states where smoke originates and many states downwind of smoke; âĂć Third, if one or more ground monitors in a county/state are influenced by upwind smoke from fires, an exceptional events waiver must have been filed with the EPA. Did the authors check to see how many exceptional events waivers were filed for 2018 by the states that were under the smoke influence as reported by the authors? âĂć Given that there are fire observations (ground reports from EPA as well as from satellites) and surface PM2.5 data for two decades, why not conduct or extend the study to all years to understand the nuances of the inter-annual variability and the influence of transport etc. Again, this is why I find this paper very premature because the authors have not even scratched the surface of the problem. It is indeed premature to talk about smoke particles (from satellites as well as ground observations) without bothering to understand if PM2.5 observed is indeed due to smoke or not; âĂć There are many documented algorithms that use satellite data to flag smoke and smoke height including the MAIAC aerosol algorithm used in this study. The authors used AOD but not smoke flag and smoke plume height product generated by the same algorithm. While the smoke plume height product is new, the smoke flag and AOD in the MAIAC algorithm are internally consistent and the authors should have used it in this study. Also there is no discussion on the quality of the MAIAC AOD and its performance. The algorithm performance is reported as 66% of the retrievals are within ± 0.5? I am not exactly sure why this is a good performance? How good is the AOD product in different AOD ranges? Does it report AODs as high as 5 or 7 for these smoke events or smoke is misidentified as cloud? If an aerosol model is used in the algorithm, does the algorithm dynamically (correctly) pick smoke model for this time period? How consistently does it pick the smoke model? If another model is picked, what is the AOD bias for incorrectly picking a non-smoke model? And how does that translate to PM2.5 estimation error?

Regarding the GWR method and surface PM2.5 estimates:

âĂć The authors indicate that the GWR is a proven method that is used by many but I have several questions. (1) Please show a map of regression parameters and demonstrate that the values have physical meaning, (2) give details on why you chose the parameters you chose for the model. Let us talk about population density. Why did you use it? I can understand why it is used if you are developing models for urban/industrial pollution where population density can be a proxy for traffic emissions etc. Here, isn't the focus of the study to understand the influence of long-range transport of smoke from fires on humans and their health. Then how can population density be a predictor? (3) no details given on the influence of different predictors such as boundary layer height on the prediction; âĂć The authors have not shown their assessments on how good the estimated PM2.5 values are outside of one evaluation (scatter plot for the whole US). If you look at the density of the data points, most points are within 0 to 20 μg/m3 or so. When the EPA PM2.5 daily average standard is 35 μg/m3, I would be more interested in knowing the performance of the statistical model for exceedances. Can the authors actually tabulate what percentage of each jurisdiction (e.g., state) violated the daily standard and how many times within the 2-week window in 2018? Without metrics like that, the study really does not provide any value. âĂć This work

also needs other corroborative evidence such as back trajectory cluster analysis to show the source-receptor relationship, analysis of LIDAR data (satellite or ground) to show evidence of transported smoke reaching the surface etc.

In summary, I cannot support the publication of this paper without new work carried out to address this problem in a more comprehensive way. Because this work has profound implications for air quality monitoring and policy, and the rigor is missing in designing a study to address the question of smoke influence on PM2.5, I have to unfortunately reject this paper.

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