



EGUsphere, referee comment RC3  
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## **Comment on egusphere-2022-713**

Anonymous Referee #3

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Referee comment on "Impacts of estimated plume rise on PM<sub>2.5</sub> exceedance prediction during extreme wildfire events: a comparison of three schemes (Briggs, Freitas, and Sofiev)" by Yunyao Li et al., EGU sphere,  
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Review comment on "Impacts of estimated plume rise on PM<sub>2.5</sub> exceedance prediction during extreme wildfire events: A comparison of three schemes (Briggs, Freitas, and Sofiev)

The authors compared three popular plume rise schemes, namely Briggs 1969, Freitas 2007 and Sofiev 2012, and their impacts on the simulated plume heights, AOD, PM<sub>2.5</sub> and NO<sub>2</sub> photochemistry using the CMAQ model driven by WRF meteorology data for the 2020 western U.S. wildfire season. With global warming, the increasing trend in western U.S. fire activities, and the need to predict hazardous air quality associated with wildfires, the study would make a timely and significant contribution to wildfire and air quality modeling science. So publication is recommended. However, I believe, the presentation can be significantly improved to increase the scientific impact of this study.

Major comments:

The descriptions of each of the plume rise schemes are short. More details of the schemes could be provided to help readers know better of the differences of the schemes (length of description can be doubled or tripled). Also, the authors focus mainly on plume top height, however plume extension (top and bottom of a plume) in the vertical at emission is as important as plume top. Information about plume vertical extension at emission and how emission mass is distributed in the vertical (e.g. evenly or weighted) from the schemes should be provided.

It's not clear how model and MISR plume heights were compared. The model and MISR

observations don't have the same spatial and temporal resolutions, and MISR observations are not continuous in time and space. So some spatiotemporal interpolation is expected. The treatment of the model and MISR data for comparison should be clearly stated.

Section 3.3 and conclusion: Why do F07 and S12, which tend to have lower plume height than B69 near source region, have higher AOD than B69 near source region? What is the column total PM<sub>2.5</sub> differences in the source region for the three schemes? Is the difference small? (You could consider providing total/average PM<sub>2.5</sub> or even better dry mass in Figure 4 for the different regions and schemes) If so, what causes the 20-30% AOD difference in source region? Is it purely because of different vertical distributions of same mass? For example, there could be more aerosols in the lower altitude in F07 and S12 (and RH tends to be higher than higher altitude), so that hygroscopic growth of smoke in the lower layer leads to the higher AOD? Or is it due to different SOA production rate? The authors should be able to provide some discussions through analysis.

Figure 9: This is a case study of PM<sub>2.5</sub> exceedance. Using a color wheel with overlapping colors to represent simulated PM<sub>2.5</sub> exceedance regions from the three schemes is brilliant. I do have a few questions though: Why August 20<sup>th</sup> is chosen as the case? The authors should provide a reason. Since this is a case study, some background of the wild fires and PM transport should be provided. Did you compare the plume heights with MISR (This case was not included in the earlier section or Figure 3)? Why F07-B69 difference in daily surface PM<sub>2.5</sub> is provided, but not S12-B69? The authors should provide the reasoning of leaving this comparison out or making this comparison.

Minor comments:

Line 69: "heigh" should be "height".

Line 77-78: Please define "PM<sub>2.5</sub> exceedance". Is it based on daily-mean or hourly PM data? Also for the 3720 observations, how many sites are the observations based on?

Line 79-80: There is no direct visual link between "hazy" and AOD shown in Figure 1. I would suggest adding a matching VIIRS true color image and/or define "hazy" in terms of AOD and PM values.

Figure 2: What is the data source of this figure and pie chart in the figure. The authors should cite some papers on fire emission chemistry (currently there are none) in the introductory paragraph (line 95-105) of experiment design. Below are a few examples:

Koppmann, R., von Czapiewski, K., and Reid, J. S.: A review of biomass burning emissions, part I: gaseous emissions of carbon monoxide, methane, volatile organic compounds, and nitrogen containing compounds, *Atmos. Chem. Phys. Discuss.*, 5, 10455–10516, <https://doi.org/10.5194/acpd-5-10455-2005>, 2005.

Schlosser, J. S., Braun, R. A., Bradley, T., Dadashazar, H., MacDonald, A. B., Aldhaif, A. A., Aghdam, M. A., Mardi, A. H., Xian, P., and Sorooshian, A.: Analysis of aerosol composition data for western United States wildfires between 2005 and 2015: Dust emissions, chloride depletion, and most enhanced aerosol constituents, *J. Geophys. Res.-Atmos.*, 122, 8951–8966, <https://doi.org/10.1002/2017JD026547>, 2017.

Line 145-146: To be more accurate, the vertical profiles of PM<sub>2.5</sub> would match the “vertical profiles of backscatter” from CALIPSO.

Line 156-157: Not a complete sentence.

Line 183: “70°” instead of “70”.

Line 232: “...which include nitrate formation from both wildfires and anthropogenic emission”. This is confusing. I would expect no anthropogenic influence, as “the impact of other PM<sub>2.5</sub> sources was removed by subtracting the results of NoFire run” from line 224.

It may be worth labeling the longitudes on the upper x axis on the geographical plots (e.g. at least Figure 1), so that readers would know the projection of the maps and where the division lines lie between the regions. This information is currently not straight forward. An alternative is to plot the division lines on the maps.

Line 298-299: “The consumption of NO<sub>2</sub> is slowed down so that the NO<sub>2</sub> concentration is

higher in the high AOD area." I think you meant "so" instead of "so that".

Line 271: You could consider updating the subsection title to include the impact of plume rise on photochemistry besides AOD.

Line 321-325: The description of color scales is already included in the figure caption, which is the right place. It is redundant here in the text.

Figure 8: It would be helpful if a plot of the average surface PM2.5 from B69 overlaid with AirNow measurement is provided, as the difference plots here are based on B69 surface PM2.5. Also the addition of AirNow would provide some kind of evaluation for the model.