We thank the two reviewers for their insightful comments on our manuscript. Our responses to each of the reviewer's comments are provided below in italics.

Reviewer 1:
The authors worked hard on the new version of the manuscript considering comments from all reviewers. I believe the manuscript is in much better shape now. Most of my remaining concerns are related to smoke height evaluation, more specifically about the model variable that's used to evaluate against observations and regarding addition of analysis and discussion of the results. More details below.

Comments by line (line numbers based om tracked changes doc):

288. The sentence "The simulated plume heights were interpolated to the MISR observation MISR pixels using the nearest neighbor approach." is not clear. Does the closest prediction of injection height is mapped to the MISR/CALIPSO data? Or are smoke heights calculated from the model 3D fields of aerosols within the model used to calculate smoke heights for the model gridcells that are closest to the retrievals (for instance, like in Ye et al., 2021)? Since you are including large fires that could span multiple model grid cells it is recommended that smoke height is derived from the model 3D fields as the result will be a combination of the injection from different grid cells, and not necessarily from the closest one. Also, does this approach takes into consideration the 1.5 factor applied to the heights inside CMAQ? This would be naturally taken into consideration when using the 3D smoke fields to derive height. Also, even though fresh plumes are analyzed here, they are considered up to 100 km away from the fire, so these injections would correspond to those happening on earlier hours. Again, using 3D fields would naturally take care of this.

Thanks for the comment. It is the smoke height from the model 3D fields. The original line 288 has been changed to:

"The smoke heights from model 3-dimensional fields were interpolated to the MISR observation pixels using the nearest neighbour approach."

289. The fire size formulation in Archer-Nicholls et al., (2015) is a function of land use cover. Is this the case for the formulation included here? If so, that would make the Freitas scheme the only one to use land use info in this work (based on the info provided in the paper). If so this could be a reason why this scheme tends to behave a bit different than the other two in this work in the study cases of section 3.1. So it would be informative to add land use cover % for the fires contributing to the injection height to assess, it might explain some of the differences.

Thanks for the comments. Equation 6 in Archer-Nicholls et al. (2015) calculates the fire size for different land use types in one grid cell. The fire size we used from RAP-Chem is the total fire size for all land use types in one grid cell. So, the land use type does not affect the results.

302-304. I think the authors could do a much better job at discussing these results. Reference pictures of the fires and PBL height were added to Figure 3 (which are greatly appreciated) but they are not included in the discussion. MISR often shows a large spread likely due to a combination of smoke that doesn't escape the boundary layer and free-tropospheric injections. Note that WRF has been found to often underpredict PBL height so when MISR heights get close then is likely that those are within the PBLH in reality. If injection heights predictions are used in this comparison (and not smoke heights, see my comments above) then it would be hard for the model to capture the boundary layer smoke height that can naturally occur at the same time as free-tropospheric injections in a given scene given lower winds and larger lateral dispersion occurring in the boundary layer. The visible images are instructive to show different features, like in a) the plume that's further away and is less thick is likely within the PBL, or in b,f) the deep plume within 20-30 km away from the fire seems to be associated to the easterly plume rather than the westerly plume, and in d) the most of the low values close by the fire are due to background smoke in the PBL as the elevated plume is very thin as seen in the visible image in c). I think another important point to take from Figure 3 is that modeled injections are always into the free-troposphere, which doesn't seem to be always the case for MISR. The article below was recently published and talks about this issue

Thapa, L. H., Ye, X., Hair, J. W., Fenn, M. A., Shingler, T., Kondragunta, S., Ichoku, C., Dominguez, R., Ellison, L., Soja, A. J., Gargulinski, E., Ahmadov, R., James, E., Grell, G. A., Freitas, S. R., Pereira, G., and Saide, P. E.: Heat flux assumptions contribute to overestimation of wildfire smoke injection into the free troposphere, Communications Earth & Environment, 3, 236, 10.1038/s43247-022-00563-x, 2022.

Thanks for the comments. The plume height shown in Figure 3 is not the calculated plume injection height but is smoke height. The following discussion is added to line 264. The above paper was cited and added to the reference list.

"The simulated PBL heights were displayed in Figure 3 as a reference. When the fire injection height is lower than the PBL height, the pollution could become confined in the PBL (Sofiev et al. 2021; Thapa et al, 2002). However, when the plume height is higher than PBL, the fire smoke can be dispersed into the free troposphere where wind speeds are stronger, leading to a wider range of pollution dispersion. In all four cases analyzed in Figure 3, the simulated plume heights from the three schemes surpassed the model PBL."

340-343. Please be more quantitative in the evaluation. Measures of bias, error and correlation might be reasonable to use here.

Thanks for the comment. The mean bias of each scheme was added to the paper:

"The mean bias for the three schemes is -0.60 for B69, -0.67 for F07, and 0.13 for S12."

Figure 4. Similarly to the MISR analysis, it would be great to have an idea of the dates, times, fires and distance from the fires (and any other useful information like the fuel type, see previous comments) that were used in this analysis. I think a table could be included as an additional panel to this figure with this information. This could help with the interpretation of these results that currently mostly describe what's happening but it's not clear why these results are occurring. For instance, these results are very different compared to those for the Milepost 21 where the Freitas scheme had deeper injections.

Thanks for the comment. The main focus of this study is to evaluate the impact of plume injection heights on the near source and downwind air quality. More details about the comparison of the three plume rise schemes go beyond the scope of this paper. The main objective of our study is to evaluate the impact of plume injection heights on near-source and downwind air quality, and not to compare or evaluate different plume rise schemes. The CALIPSO plume injection height is from the automatic detection method not from any detailed case study. Therefore, the detailed information for each case is quite unknow to us at this stage. We agree that a more comprehensive comparison of the simulated plume height would be an interesting avenue for future research, but it falls beyond the scope of our current study. Thank you again for your valuable feedback.

Section 3.1-3.2. I think sections 3.1 and 3.2 are a bit disconnected, which is understandable as it goes from case studies to monthly means, but I think a simple plot could be added to better connect them. The authors could include box and whisker plots of the Aug-Sept 2020 mean of modeled injection heights for the different schemes. That could allow to see how representative the behavior of the case studies are compared to the means, and will also allow to verify the behavior described on section 3.2 regarding how B69 tends to always have deep injections but the other two schemes have more variability with a lower average but often very high injections for extreme cases.

Thanks for the comments. New figure 5 and the following discussion have been added to section 3.2:

"Figure 5 shows the vertical profile of the two-month average $PM_{2.5}$ concentration for the three experiments. Over the two months, B69 simulated a higher average plume height and injected more $PM_{2.5}$ in the free troposphere than F07 and S12. Meanwhile, F07 simulated a lower average plume height and therefore keep more $PM_{2.5}$ in the boundary layer than B69 and S12.

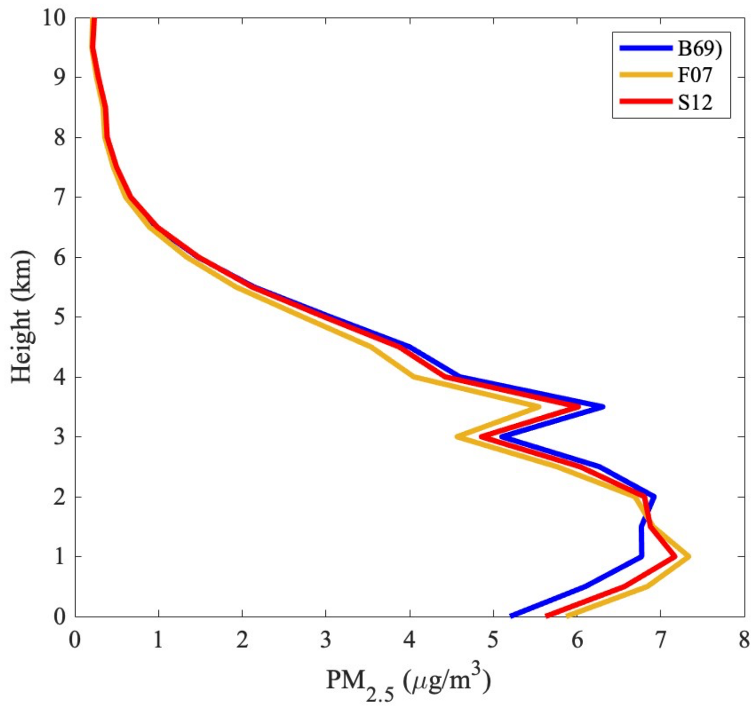

Figure 5: Vertical profile of two-month average PM2.5 concentration for B69, F07, and S12 in the CONUS domain."

419-422. AOD underprediction by models is commonly found for large wildfire events which is consistent with the result on this study, so might want to cite some of these articles here.

Thanks for the comments. The AOD prediction performances for different models are different. Everything depends on emission factors, whether FRP or burnt area are used as proxies, and whether the satellite manages to see the dense parts of the plume without misinterpreting them as clouds. Sometimes AOD is underestimated. For instance, this work:
Toll, V., Reis, K., Ots, R., Kaasik, M., Männik, A., Prank, M., and Sofiev, M.: SILAM and MACC reanalysis aerosol data used for simulating the aerosol direct radiative effect with the NWP model HARMONIE for summer 2010 wildfire case in Russia, Atmospheric Environment, 121, 75–85, https://doi.org/10.1016/j.atmosenv.2015.06.007, 2015.
So, we decide not to cite papers here.

Figure 8. A reference map of the photolysis rate reduction by smoke for one of the schemes (B69 might be convenient as is being used as reference) would be helpful to interpret results. Adding 2 panels to this figure (for absolute and relative differences) might work the best. The differences in photolysis rates due to smoke can then be compared to the other studies cited to check if they are within the same ranges or not. Since the current plots are differences within scheme then this comparison to other studies is not possible.

Thanks for the comment. The maps of the absolute and relative photolysis rate reduction by smoke for B69 have been added to new figure 9. The following discussions have been added to line 405:

"Figures 9 a and d show the photolysis rate difference and difference ratio between B69 and the NoFire experiments. The photolysis rate results in the B69 were lower than the NoFire simulation, which proves that fire smoke led to the reduction of the photolysis rate, consistent with the findings of previous studies. The photolysis rate reduction caused by the fire smoke was found in the whole domain, both in the near-source region and the downwind region. Near the fire source, the photolysis rate reduction was more than 50%. Figures b, c, e, and f show the photolysis rate difference and difference ratio between the three experiments with different plume rise schemes. Near the source region where F07 and S12 simulate a higher AOD than B69 (Figure 8), the NO2_IUPAC10 is reduced. Meanwhile, in the downwind region, where F07 and S12 simulate a lower AOD, the photolysis rate is higher than B69. Therefore, the difference in the plume injection height would affect the fire-induced photolysis rate reduction.

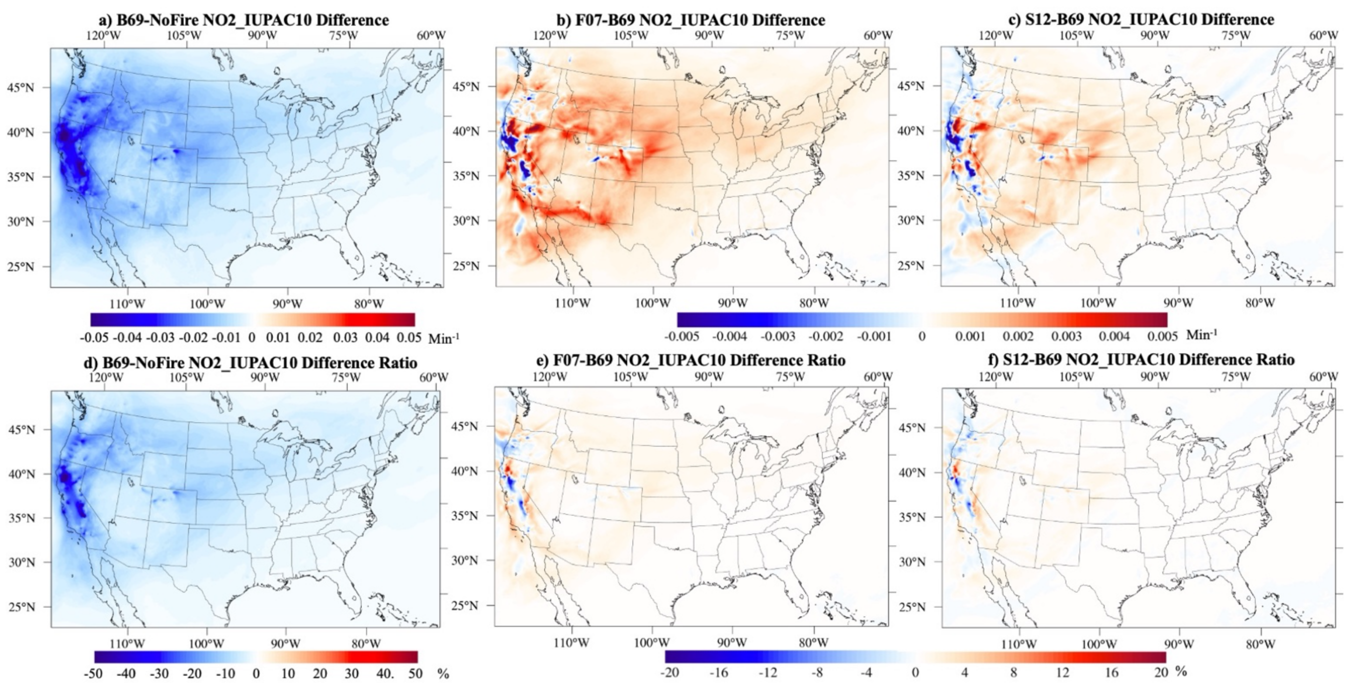

Figure 9: The average photolysis rate NO2_IUPAC10 differences between B69 and NoFire (a), between F07 and B69 (b), and between S12 and B69 (c) from August 1st, 2020 to September 30th, 2020; and the average photolysis rate NO2_IUPAC10 difference ratio B69 and NoFire (d), between F07 and B69 (e), and between S12 and B69 (f) during the same period."

632. Not sure why "humidity" is mentioned here if it was not discussed before

We mention humidity in section 3.1. We found that the higher F07 plume rise on August 15 may be related to high humidity on that day.

Minor Edits

28 Add CALIPSO

Thanks for the comments. "Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations (CALIPSO)" has been added to line 28.

35. Since you are not running in forecasting mode I would replace "forecasts" by "predictions"
Thanks for the comment. "forecast' has been changed to "prediction" in line 36.

105-107. Section numbers need to be updated
Thanks for the comment. The section numbers have been updated.

331-338. This whole explanation belongs to the methods
Thanks for the comment. Line 331-338 is moved to section 2.4.1.

"CALIPSO is an Earth Science observation mission that was launched on 28 April 2006 and flies in a nominal orbital altitude of 705 km and an inclination of 98 degrees as part of a constellation of Earth-observing satellites. CALIPSO's lidar instrument, the Cloud-Aerosol Lidar with Orthogonal Polarization (CALIOP), provides high-resolution vertical profiles of aerosol and cloud attenuated backscatter signals at 532 nm and 1064 nm (Winker et al., 2007). The footprint of the lidar beam has a 100 m cross-section with an overpass around 1:30 p.m. local time. The CALIPSO smoke injection heights are directly calculated from Level 1 attenuated backscatter profiles at 532 nm following Amiridis et al. (2010). There are several steps involved in this process. First, GBBEPx FRP data were used to locate the fire location along the CALIPSO swath. Then, a slope method (Pal et al., 1992) is applied to each profile to smooth out the original Level 1 532 nm attenuated backscatter coefficient profiles at each fire point. Next, we calculate the steep gradient in the attenuated backscatter profiles. The height of the minimum gradient value is selected as the smoke injection height."

283. Add citation for this algorithm, I believe is the following:
Kondragunta, S.; Laszlo, I.; Ma, L. JPSS Program Office (2017): NOAA JPSS Visible Infrared Imaging Radiometer Suite (VIIRS) Aerosol Optical Depth and Aerosol Particle Size Distribution Environmental Data Record (EDR) from NDE. [NOAA-20 dataset]. NOAA National Centers for Environmental Information. NOAA Natl. Cent. Environ. Inf 2017.

Thanks for the comment. The above paper is added to section 2.4.3 and the reference list.

332. Can you clarify that only daytime retrievals were used?
Thanks for the comment. Original line 332 (new 286) has been changed to:
"The vertical profiles of CMAQ simulated $PM_{2.5}$ are also compared to the CALIPSO daytime aerosol vertical profile."

394. I think this should be "Fig 3e" instead of 3a.
Thanks for the comment. "Fig 3a" has been changed to "Fig 3e" in the original line 394 (new 353).

630. add CALIPSO
Thanks for the comment. Line original line 630 (new 494) was changed to:
"The plume heights simulated by all three schemes are comparable to MISR and CALIPSO observations of aerosol height."

668. I think the NO_2 analysis was removed so I would remove this sentence.
Thanks for the comment. The original line 668 has been removed.

Reviewer 2:
The revised manuscript has addressed the reviewers' comments and provided additional analysis to improve the demonstration. Further comments and discussion to the revised manuscript is provided below:
Minor revision suggestions:
Fig. 6
Fig 6 shows the zonal averaged concentrations throughout the two-month period to illustrate how different plume rise models affect the spatial and vertical distribution of the studied atmospheric species, and species' fractional contribution in PM2.5.
The occurrence of wildfires in the US is quite sporadic. The zonal averaging is appropriate to highlight the dominant pattern of the fire activities in the US. What is the latitude range you've applied for the zonal averaging? Are you using a constant averaging range (from the west to the east) or a changing averaging range as the smoke dispersed?

Thanks for the comments. The zonal average is for the whole CONUS domain, we use the same zonal average region for all the analyses. To make it clear, "over the whole domain" has been added to line 342.

Result analysis of Fig. 6 (Section 3.3)
Authors explained the roles of the observed/modeled plume height on AOD reasonably. Higher plume height will accelerate dilution near the source, as well as enhance the transportation to the downwind regions.
Fig. 3 shows that the modeled plume heights by B69 and S12 are quite close to each other for the four studied cases (different wind scenarios). In Fig. 6, the positive values of S12-B69 near 120o W above 6 km (Fig. 6n) seems to be mainly contributed by organic carbon (Fig. 6b). Given the primary fire emissions among three plume rise models are kept being the same and the plume height modeled by B69 and S12 are quite similar, this "positive-negative-positive" pattern in a bottom-up or top-down direction near 120o W can be also caused by variated sensitivity responses or modeling performances between B69 and S12 models to the fire cases in the zonal averaging region but not included in the fire case studies in the previous sections.

Thanks for the comments. To show the overall plume height simulation of the three schemes, a new figure, Figure 5, and the following discussions have been added to the paper.

". Figure 5 shows the vertical profile of the two-month average $PM_{2.5}$ concentration for the three experiments. Over the two months, B69 simulated a higher average plume height and injected more $PM_{2.5}$ in the free troposphere than F07 and S12. Meanwhile, F07 simulated a lower average plume height and therefore keep more $PM_{2.5}$ in the boundary layer than B69 and S12.

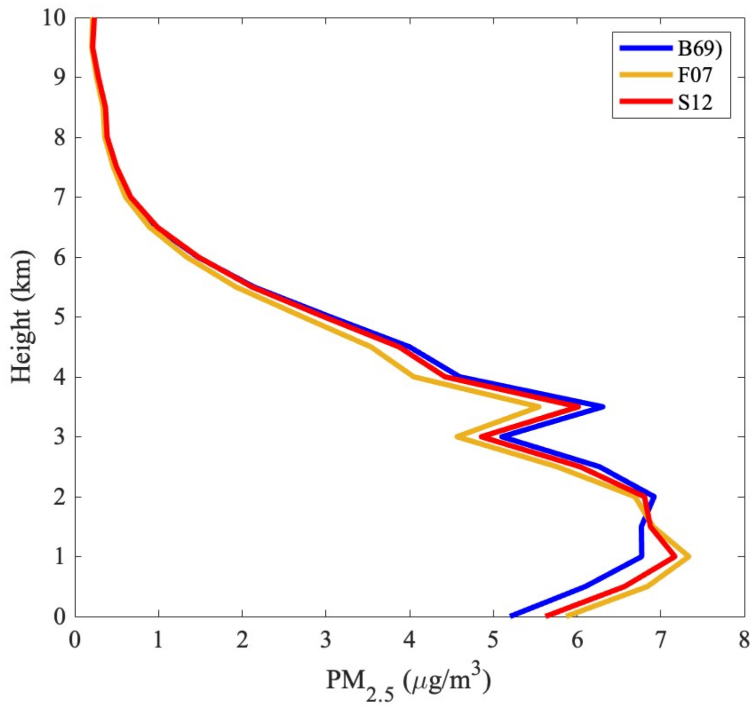

Figure 5: Vertical profile of two-month average PM2.5 concentration for B69, F07, and S12 in the CONUS domain."

Line 336: "When comparing the CMAQ AOD to VIIRS AOD (Figures 7b-d), we applied VIIRS AOD saturation level (5) to CMAQ AOD results."
What does "saturation level (5)" here stand for?

Thanks for the comments. For VIIRS data, it has a saturation level for AOD (e.g., AOD=5 for VIIRS). All points with an AOD higher than this saturation level was changed to the saturation level. To make this clear, original line 336 (new 374) was changed to:

"…we applied VIIRS AOD saturation level (AOD $\leqslant$5) to CMAQ AOD results (any CMAQ AOD values higher than 5 were changed to 5)"

Other revision suggestions:
Author affiliation information 1: space between "VA" and "22030".
Thanks for the comment. Space has been added between "VA" and "22030".

Figure 10: Caption is in blue color.
Thanks for the comment. The caption for figure 10 has been changed to black.