

Comment 1

[1] I disagree with the use of "OH concentrations" in the manuscript (e.g. l. 93, l. 189 etc.). The authors do not measure in situ concentrations of OH (which are much lower at any given point in time), but rather use a probe to estimate time-integrated OH production. I suggest to replace "OH concentration" with "OH production" where applicable.

Response:

We thank the Editor for the suggestion. References to "OH concentration" have been modified as noted below.

Figure 1, line 77

"OH ~~concentrations~~ production were was measured for stage III."

Lines 96-97

"Each sample was run in triplicate and final ~~p-HBA~~ concentrations were averaged."

Lines 100-103

"Total ~~Final~~ OH ~~concentrations~~ production during the course of the reaction were was determined by dividing the measured p-HBA ~~concentration~~ by the fraction of OH that forms p-HBA after 16 hours, previously determined to be 0.56 (Runberg and Majestic, 2022)."

Lines 187-188

"For soot slurries that were exposed to light, soot that had been photo-aged for 24 hours previously had the lowest OH ~~concentrations~~ production at $0.38 \pm 0.02 \mu\text{M}$."

Lines 189-194

"Soot that had been dark-aged for 24 hours prior to the photoreaction in water resulted in significantly higher ~~concentrations of~~ OH production, ~~with~~ at $(0.89 \pm 0.05) \mu\text{M}$ (Figure 1) ($p < 0.005$). This is consistent with a recent study reporting a decrease in OH ~~concentrations~~ formation in wildfire smoke plumes downfield from the point source, vs the same plume measured nearer the source (Akherati et al., 2022). This indicates that smoke which has been exposed to sunlight for a longer period of time (i.e. the downfield plume) results in lower ~~concentrations of~~ OH production."

Figure 5, Lines 201-202

"Figure 1 ~~Concentrations~~ Production of OH measured ~~from~~ after 16-hour lighted reactions of soot that had been previously aged for 24 hours in the light (E), aged for 24 hours in the dark (G) and an ultrapure water control."

Comment 2

[2] I suggest to add more disclaimers to the comparison with the work by Akherati et al. (2022). The authors already formulate with care ("... is consistent with ..."), but the brevity of the statement might lead a reader to overinterpret the findings. It could be made clearer that (i) not all gas-phase OH in an air pollution plume comes from photochemistry of soot in aqueous environment, (ii) OH produced in this way might not even enter the gas phase, and (iii) OH concentrations in a biomass burning plume will likely decrease naturally over time due to dilution. Alternatively or in addition, the authors can generalize the statement more, e.g. that a reduced capability of OH production after a period of photo-irradiation is also seen in more complex systems.

We thank the Editor for the insightful suggestion. The following text has been added at line 194-195 to address this.

“While photoaging of airborne soot particles in a much more complex system such as a smoke plume is likely not the only reason for the decline (e.g. there are multiple OH production pathways and the plume is diluted), the same pattern of diminished OH production is reported.”