

Interactive comment on “Secondary organic aerosol formation and primary organic aerosol oxidation from biomass burning smoke in a flow reactor during FLAME-3” by A. M. Ortega et al.

Anonymous Referee #2

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The paper by Ortega et al. summarizes results from measurements of organic aerosol (OA) formed in biomass burning of different fuels. Primary OA (POA) was measured using a high-resolution aerosol mass spectrometer by sampling burn emissions without exposing the smoke to oxidants while secondary OA (SOA) was measured by exposing the primary smoke to elevated OH and O₃ in a PAM reactor flow tube. Gas phase characterization of the unprocessed and oxidized smoke was also performed by using a PIT-MS. The paper discusses in detail amount and characteristics of SOA formed in different burns and under different OH exposure limits. In general, results are very unique in that the conditions in the oxidation experiments reach levels beyond what is

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typically achieved in a smog chamber set up, and therefore the result extend what has previously been studied in smog chamber oxidation of biomass burning smoke. Conclusions of this work are still consistent with previous research and confirm the variability in SOA formation potential of different fuels. The paper is very well written and has a strong technical backbone. I recommend publishing the paper after the following comments are addressed: 1) A general comment I have is regarding the design of the sampling by PAM. Of course the burns themselves are quite dynamic and I believe gas phase (and possible POA) emissions throughout a burn were not constant. Now if different OH exposure levels were tested out at different stages of a burn, doesn't that introduce another angle of variability on the data? How can these two effects be separated in the results presented here? An explanation of this is needed in the paper. 2) The variability in the observed OA mass and characteristics have been discussed only in the context of different fuel types; what is the role of burning condition in the observed variability? Some discussion based on the gas phase measurements (CO, CO₂, NO_x, O₃ etc) is warranted. 3) In Fig. S4, authors compare the OA mass calculated from UMR analysis of full spectra with OA mass from HR resolution of spectra for $m/z < 100$. Why isn't the UMR-based mass or HR-based mass compared together for different lengths of spectra? (i.e., full UMR-based mass vs. UMR-based mass for $m/z < 100$, etc.) Related to this, why isn't HR analysis used for estimating mass at larger fragments? These are details that most probably would not change the conclusions of the paper since OA mass beyond m/z 100 is expected to be small, but still more clarification on the approach is needed. 4) Line 22, p. 13810: clarify that substantial organonitrate or organosulfate mass concentrations may lead to higher estimates of inorganic nitrate or sulfate and therefore misleading the measured ionic balance. 5) Line 15, p. 13815: Clarify which tracers 'these' refer to. 6) Editorial corrections: line 3, p. 13820 should read "...would be a near flat line ..." and last sentence should be "...to other burns where EROA is >1.". 7) It's stated on lines 10-15, p. 13820 that signal at m/z 44 for aged ponderosa pine smoke is dominated by CO₂⁺ and not C₂H₄O⁺, but the opposite is mentioned on lines 8-9, p. 13815. Doesn't Figure S7 indicate compa-

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rable signals at CO₂⁺ and C₂H₄O⁺? 8) Line 11, p. 13821: Please clarify why would higher partitioning of SVOCs at higher OA concentration to the particle phase result in less gas phase oxidation?! 9) Line 3-4, p. 13825: increased O/C factor should be 3.27 10) Line 28, p. 13826: "... as the smoke from each..." 11) Fig. 9: Why not show the sticks in the top mass spectra as % and indicate the average mass for the unprocessed and aged cases? This way it would be easier to see how the composition changes with aging.

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