Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-326-RC1, 2019

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Secondary organic aerosol formation from biomass burning emissions" *by* Christopher Y. Lim et al.

Anonymous Referee #1

Received and published: 7 May 2019

The manuscript "Secondary organic aerosol formation from biomass burning emissions" presents the results of photooxidation studies of during the FIREX 2016 campaign, culminating in two pragmatic parameterizations that can be used to describe the organic aerosol change from aging biomass burning emissions. The first parameterization shows good correlations between the mass of non-methane organic gases to secondary organic aerosol yield for biomass burning emissions for a given OH exposure. This relationship is calculated by Christopher Lim et al. using aerosol mass spectrometer (AMS) organic aerosol mass measurements. These measurements are strongly influenced by the instrument collection efficiency (CE). The authors account for variability in the CE using the relationship between CE and mass fraction remaining after a thermodenuder. The second parameterization is the carbon-based SOA yield.

Both parameterizations will be useful to the community of chemical transport modelers, with some additional modeling for dilution. Given the potential importance and utility of these parameters, it would be prudent to include in the manuscript some additional details.

In the following, I will use P1.5 to indicate page 1, line 5 of the submitted manuscript.

Title: Accurate but not precise, in my opinion. Some reference could be made that this is a lab study and/or utilizes a photooxidation mini-chamber.

P4.20 Where/how is the additional O3 injected? Is there a sufficient jet to induce mixing? Is there any concern about a small area of highly concentrated O3 chemistry in the chamber? I'm curious about the O3/OH reactivity in general in your chamber, but more specifically at the injection port.

P5.2 Were seeded blanks (e.g. ammonium sulfate) ever run to establish a background OA production for this chamber and test the efficacy of the cleaning procedure?

P5.8 Did the black carbon measurements for these two methods agree? I wonder because the SP2 can saturate at high number concentrations, and the SP-AMS CE for BC requires some additional considerations (Ahern et al., 2016; Onasch et al., 2012; Willis et al., 2014.)

P5.26-30 I find your parameterization of CE very interesting and possibly broadly applicable. But why was it necessary at all? If you can calculate MFR, why not use the SEMS-measured size distribution to correct for CE directly? Given the large amount of variability in your FigS3, it is not obvious that using the correlation is an improvement in accuracy or precision over a size distribution correction.

Additionally, I don't agree with the statement that there was no good internal standard available. While I think that your CE parameterization could be very useful, it warrants verification by looking at other measurements more closely. For example, you state that there was an SO2 monitor, which would allow for a sulfur mass balance. Black carbon, measured by SP-AMS or SP2, was present at useful concentrations for some of the experiments.

P5.31 Would you please confirm that the experiments used to calculate your CE were devoid of nucleation, particles grown outside the SEMS or AMS transmission/measurement ranges, and weren't unduly influenced by rBC after thermodenuding? Also, out of curiosity, how frequently was the thermodenuder valve switched, and therefore a new CE able to be calculated?

P6.2-3 Please provide a citation or clarify regarding the relationship between volatility and phase. It might be easier to provide citations that claim that SOA has been observed to be an amorphous solid with low volatility, and therefore is likely to bounce.

P8.5 What are the possible implications for the chamber OA concentration having decreased by two orders of magnitude from the beginning to the end of the experiment (FigS4)? It stands to reason that fewer of the semi-volatile SOA products will condense at low OA concentrations late in the experiment, but that any that do condense will have a larger impact on OSc.

P10.18 How does this approach compare with the measured PTR-MS NMOG concentrations? It's not obvious to me why you compare [calculated VOC reacted]/[measured SOA formed] instead of [measured VOC reacted]/[measured SOA formed]? Or to go backwards, can you use your [measured NMOG reacted]* this calculated SOA yield to predict SOA formation?

P11.19 Given the importance of dilution and volatility on the results presented here, is there information on the volatility of the POA and the SOA from the thermodenuder measurements, that can be compared to past campaigns?

Minor technical corrections: P3.22 "subalpine fir" rather than "subalpine fire" P7.25 Please include Hennigan et al. 2011 P11.20 Missing a period. FigS3 No red exponential fit FigS6 It looks like some plots have multiple y-values for a given time; some

boomerangs at 0-5 days where they should be smooth functions. Is it possible that the d-butanol injection is also being plotted? Does this change the OA enhancement ratios?

Ahern, A. T., Subramanian, R., Saliba, G., Lipsky, E. M., Donahue, N. M., and Sullivan, R. C.: Effect of secondary organic aerosol coating thickness on the real-time detection and characterization of biomass-burning soot by two particle mass spectrometers, Atmos. Meas. Tech., 9, 6117-6137, https://doi.org/10.5194/amt-9-6117-2016, 2016.

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