

This work represents a significant contribution to the understanding of SOA production from biomass burning emissions. Fig S6 shows that reported SOA production are consistent with what has been previously reported, with the principal difference being the use of a new parameterization for AMS collection efficiency. As such, the new CE parameterization should be more rigorously verified. I will respond to comments in-line below.

Referee #1: 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.)

Author response: SP2 and SP-AMS black carbon mass loadings generally agree within a factor of two, with SP2 mass loadings consistently higher than the measurements from the SP-AMS and a reasonably linear relationship ($r^2 = 0.9$). BC measurements from the SP2 are those reported in the manuscript. The SP2 was operated with time-varying flow rates to account for the large dynamic range in the BC concentrations over the course of an experiment. In this manner, coincidence and under-counting (saturation) issues were avoided. The SP2 was calibrated with size-selected fullerene soot, and mass concentrations were corrected for “missing mass” outside of the SP2 detection window via multi-modal log-normal fitting. However we note that SP-AMS BC measurements were used only to determine which experiments to filter out of the analysis (due to enhanced wall loss), and so any CE-related errors will not affect the results presented. Black carbon measurements and other details about primary emissions will be discussed further in a future publication (Cappa et al., in preparation).

Please clarify which BC measurements are used when in the manuscript.

Referee #1: 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.

Author response: While an SO₂ monitor was present, we observed that the measurements were unreliable due to, most likely, strong interferences from large concentrations of PAH's and other molecules, despite the internal scrubber in the SO₂ monitor. Further, it is not entirely clear to us how sulfur balance (gas + particle) would provide clear insights into the particle collection efficiency; we did not have an independent particulate sulfate measurement, but a gas-phase sulfur measurement. Given that the SO₂ measurements were compromised by interferences, we have now removed them from the list of measurements that were made.

If any of your SO₂ measurements were not contaminated, you could calculate the mass of SO₂ oxidized. That SO₂ would likely condense onto the particles as (NH₄)₂SO₄, which can be measured by your AMS. This would give you another way to verify that you understand your collection efficiency.

While we can use the SEMS-determined MFR, it is not clear how this would lead to a “direct” correction for the CE. It would provide an alternative approach, but with a complication that the MFR from the SEMS would include contributions from non-refractory material and thus does not directly address the issue of how the organic component of the particles responded to temperature changes.

To clarify, I am not proposing you use the SEMS-determined MFR. Rather, I question whether your parameterization of AMS CE is an improvement over using the SEMS-measured volume and AMS measured mass to calculate CE. It appears to me that given the degree of uncertainty in your CE parameterization (especially for low MFR), the contribution of rBC to SEMS-measured particle volume may be small in comparison. For most experiments, you would overestimate OA mass by less than 10%, 20% at the most for your selected experiments. That is assuming that you don't account for the measured BC mass (Slowik et al. 2004). I would also posit that it may not be necessary to fit a Gaussian to the particle size distributions, depending on how much variability you see in the calculated OA density.

This should be compared to the uncertainty in OA mass introduced by applying your parameterization. I do think your parameterization has merit and should be published, but it needs to be validated more rigorously and I'm not convinced it is the best way to calculate OA mass given this suite of instruments.

I would also be curious to know if the scatter in Fig S3 is due to precision of the measurement, or if it is due to different experiments having slightly different relationships between CE and MFR.

Jay G. Slowik, K. Stainken, Paul Davidovits, L. R. Williams, J. T. Jayne, C. E. Kolb, Douglas R. Worsnop, Y. Rudich, Peter F. DeCarlo & Jose L. Jimenez (2004) Particle Morphology and Density Characterization by Combined Mobility and Aerodynamic Diameter Measurements. Part 2: Application to Combustion-Generated Soot Aerosols as a Function of Fuel Equivalence Ratio, *Aerosol Science and Technology*, 38:12, 1206-1222, DOI: 10.1080/027868290903916