Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-679-RC1, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Highly-controlled, reproducible measurements of aerosol emissions from African biomass combustion" *by* Sophie L. Haslett et al.

Anonymous Referee #1

Received and published: 8 September 2017

This work studied aerosol emissions from burning of wood samples from west Africa in a highly-controlled manner. Parameters that were regulated include the shape and mass of wood samples, airflow and surrounding thermal environment. Measurements of OC and rBC emissions were performed online using a compact-AMS and an SP-2 respectively after $\sim$ 100-time dilution. CO and CO2 concentrations were also measured, thus modified combustion efficiency – a qualitative index for combustion condition – was determined. The highly controlled burns coupled with fast real-time measurements made it possible to characterize the fast changes in aerosol emissions over the course of combustion and relate the changes directly to combustion conditions. The connection of the results of this study and those from an ambient study by Young

et al. is rather interesting and provides an example for using the results of this study to interpret ambient BBOA factors and differentiate BBOAs emitted from different burning conditions. This is a quality work that generated new results on emissions and characteristics of aerosol from burning and pyrolysis of wood. The manuscript is overall well written and the scope of the work fits well within that of ACP. I recommend acceptance after the authors respond to the following comments.

Speaking of the repeatability of combustion events, which is the core of this work, one question is whether there is quantitative information to demonstrate this. For example, were the same experimental conditions repeated and how the emission rates and aerosol characteristics varied for the same condition? How do the emission factors determined in this study compare with the values reported in literature?

It was mentioned at the end of page 3 that the usage of a pilot flame had a negligible influence on CO and CO2 emissions. Are there measurements data to demonstrate?

Line 164, how was fuel moisture content determined?

In the calculation of EF in this study, the loss mass was determined by weighing. But how was the effect of the dryness of the fuel accounted for? It would be interesting to also report EFs based on total carbon burned/emitted, e.g., normalized by total carbon measured in CO and CO2. This may allow more direct comparison with field observations.

Since dilution may change the concentration profile, in Fig. 3, it might be interesting to add the 2nd CO2 measurement data on panel b.

What's the detection limit of SP2 for rBC? How much of the org/(org+rBC) variation under low rBC conditions shown in Fig. 3c was due to noise in the rBC measurement?

Line 291, it would be interesting to provide the range of rOA for flaming combustion or quote the "high" and "low" values (and citations) for the subsequent sentence.

Figure 4 is a somewhat difficult to read, I suggest adding an MCE axis in the 2nd and

3 row. Adding ticks on the MCE axis may also be helpful.

With regard to the paragraph underneath Figure 6: for BBOA, f44 and f43 are not necessarily primarily associated with CO_2^+ and $C_2H_3O^+$ respectively. There can be considerable $C_2H_4O^+$ and $C_3H_7^+$ in the MS of BBOA.

Bottom paragraph on page 19, although CH_2O^+ is typically low compared to other organic peaks, since Org/inorg ratio is high in BB aerosol, it can nevertheless be an important contributor to the signal at m/z 30. Organic contribution to m/z 46 can be even more important. These issues should be more clearly discussed since the c-AMS used in this work can't differentiate ions with the same nominal m/z. Also, what was the 46:30 ratio for ammonium nitrate measured during this study?

Line 505 – 508, in addition to the two studies mentioned here, a recent study by Collier et al. (ES&T, 50, 8613–8622, 10.1021/acs.est.6b01617, 2016) also reported a negative correlation between OA emission factors and MCE for wildfires.

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