

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

### **Anonymous Referee #2**

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Review of Manuscript acp-2017-679 Title: Highly-controlled, reproducible measurements of aerosol emissions from African biomass combustion Authors: Sophie L. Haslett, J. Chris Thomas, William T. Morgan, Rory Hadden, Dantong Liu, James D. Allan, Paul I. Williams, Keïta Sekou, Cathy Lioussse and Hugh Coe

Overview: I find this manuscript to be well written and logically organized. The manuscript describes 8 biomass burning measurements of organic aerosol (OA) and refractory black carbon (rBC) from one type of rubberwood material in a highly controlled combustion system. The combustion system was designed through international standards for measuring combustion properties of different fuels for industrial

C1

and insurance purposes. This is (at least) one of the first studies that start to bridge the gap between industrial standard combustion practices and ambient environmental studies. I applaud the authors for taking this approach. As these highly detailed measurements are important and there are few such measurements reported in current literature, this manuscript is both timely and appropriate material for ACP. The manuscript should be published with attention paid to the following minor issues.

General comments:

- 1.) “. . .from African biomass combustion” in the title implies a broader appeal than the actual use of 32 pieces from one specific type of rubberwood tree. Perhaps, “. . . from combustion of a common African biofuel source”.
- 2.) What was the mass of the combusted fuel? The only details given are one dimension (100 mm), a picture, and ~7% moisture value.
- 3.) Interestingly, the authors do not emphasize the use of an industrial standard equipment for controlling the combustion process. This is a ready mechanism for others in the field to conduct similar experiments, such that a bit more detail might help move the whole field further along.
- 4.) In the discussion of the OA mass spectra, is there a reason why PMF was not done on the current data set to see how well it reproduces the average spectra for the two conditions (phase 1 and 3)?
- 5.) In the discussion of Figure 6, placing either PMF factors or phase 1/3 weighted means on the plots would help in interpreting the discussions of the relative abundances of f44 vs f43 and f44 vs f60. For example, line 395 “On average, the f44 contribution increased as combustion developed” is stated without an average value on the plot to assess. Attempting to discern “averages” of data points on a plot is difficult for readers, especially when the markers overlap.
- 6.) Differences in the PMF factors from Young et al. showed very nice correlations

C2

with the differences in the mass spectra from phase 1 and 3. How do the actual PMF factors compare with the phase 1/3 mass spectra? While the differences are important, the actual connection in the text is that the Young et al. SFOA1 and SFOA2 may well represent phases 1/3 in ambient, which is a comparison of the absolute individual spectra, rather than just differences.

7.) During the discussion of ambient measurements (around lines 505 and 560+), the authors cite Zhou et al (2017) and Brito et al (2014), but appear to have missed Collier et al (2016), which showed a very strong correlation between OA/(CO<sub>2</sub>+CO) (which is a type of ER or emission ratio, rather than EF or emission factor, given the lack of information on fuel mass) and MCE in ambient wildland fires in the western US. The current lab results and these ambient results are in strong agreement about how OA emissions are governed in large part by combustion conditions for similar fuel types. They may differ by the pyrolysis emissions and/or effects of heat. The implications emphasized herein and noted in Collier et al. are important from both ambient and lab viewpoints and could readily be included.

8.) Lines 483-485. The authors appear to give a median values of m/z 46:30 as 0.084±0.15 and linear correlation with a slope of 0.29. Are these values from the same distributions, suggesting that the m/z 46:30 distribution is highly skewed? What does a skewed distribution say about the results?

9.) The authors rightly emphasize the potential impact of pyrolysis OA emissions in the underlying emission factor variations observed with MCE (especially lab studies); however, the authors thereby downplay differences in heat load on fuels for a given fire. Here, the authors include this parameter explicitly (perhaps for the first time for ambient-related measurements). The heat load can vary depending upon whether the combustion occurs inside a stove (i.e., solid fuel combustion) or open air (wild land combustion). It can also vary by how much fuel is available (both in lab and ambient) and how well packed. Might this have a large an impact on measured OA emissions per MCE, similar to that suggested for pyrolysis?

C3

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C4