Atmos. Chem. Phys. Discuss., 14, C9368–C9371, 2014
www.atmos-chem-phys-discuss.net/14/C9368/2014/

Atmospheric Chemistry and Physics
Discussions
Open Access

Interactive comment on "Characterization of primary and secondary wood combustion products generated under different burner loads" *by* E. A. Bruns et al.

Anonymous Referee #1

Received and published: 20 November 2014

The manuscript entitled "Characterization of primary and secondary wood combustion products generated under different burner loads" by Bruns et al. investigates the effects of fuel loadings on both primary and secondary wood combustion products in a smog chamber, including the composition of both the gas and the particulate phase. Special interest lies on the contribution of polycyclic aromatic hydrocarbons (PAH's) to organic aerosol mass, determined by an Aerosol Mass Spectrometer (AMS) and by GC-MS analysis from filter samples. Within this study, a novel approach for the quantification of these PAH's from AMS data was therefore developed. Main conclusions from the manuscript include that the contributions of PAH's to total organic aerosol

(OA) increase with higher loadings of combusted wood in the burner. The increase of OA concentrations during aging rises with fuel loadings. Furthermore, an increase of functionalized PAH's were observed with aging. The knowledge of the composition of wood burning emissions, especially the contribution of PAH's and their oxidation products is relevant since these compounds are known to have adverse effects on health. Only few publications on the investigations of secondary organic aerosols (SOA) deriving from wood combustions are available. Thus, these results are important for the understanding how gas phase PAH's emissions from this source contribute to the formation of SOA. Overall, the paper is well written and the new data is important to the community. Further, the work is within the scope of work published by ACP. Therefore, I recommend publication once the comments and questions below are addressed.

Major comments and questions:

1- An important point was made in a previous short comment from Glyn Hughes. The authors explain precisely the preparation of the smog chamber but do not point out how the burner was prepared prior to the loading with wood and burning processes. Such a description for the burner should be added.

2- Using a chamber presents a limitation on the number of trials possible which creates a scenario like a field campaign. I assume time limitations prevented repeat experiments and experiments with different fuel loadings. Ideally, those experiments would have been completed to allow statistical analysis. Experiment 1 clearly shows different results than experiments 2 and 3 although the fuel loadings are roughly the same. Are there any reasons for these discrepancies, e.g. the very high OA and PAH mass loadings which were seen in experiment 1?

3- Why did the authors chose a different experimental strategy for experiment 6 than for the other experiments, precisely the second and third injection of burning emissions?

4- Currently, the ions C2H4O2+ (m/z=60) and C3H5O2+ (m/z=73), detected with the AMS, are commonly used as tracers for primary biomass burning emissions in ambient measurements, as they are preferably formed from the fragmentation of levoglucosan within the AMS. The HR spectra in figure 1 seem to show large differences in f60 and f73 ratios between the experiments. The authors should also address this issue.

5- I have some general questions regarding the analysis of the AMS data, especially the novel approach to detect and quantify PAH-peaks:

The fits of the high resolution (HR) peaks of the PAH-ions showed in the supplement look quite nice, as the authors already stated in their publication. I assume that the IGOR-based AMS data evaluation software SQUIRREL (for UMR data) and PIKA (for HR data) was used in this study. It would be helpful to give some information in the supplement, which software versions you used and how the parameters for the m/z-calibration, the peak width and peak shape were determined, e.g. did the authors include these PAH ions for these determinations? Currently, PAH contributions are considered in the SQUIRREL UMR fragmentation table as an additional aerosol subclass, but are not considered quantitatively by default. Did the authors use these entries in a certain way?

Detailed comments:

Page 26046, line 20-21. How did the authors judge if the burning conditions are stable and there were no emissions from the fire starters? Which parameters were used which indicated that the injection into the chamber had to be done at least 15 Minutes after the ignition?

Page 26048, line 12. Was the W-mode of the AMS also used in this study? It would be interesting, how the PAH-peaks at the high m/z look like in this mode. The pictures of the fitted peaks of the V-mode in the supplement show quite high signals, indicating there might be enough signal to get nice peaks also in the W-mode. The use of W-mode data would also enhance the identification and quantification of HR-peaks with sufficient signals and to distinguish not only between PAH and non-PAH related ions at the same nominal mass, but also between several different PAH related ions at the

same nominal mass, if these cases exist. As the authors already stated out, the last two issues are difficult, but important to achieve.

Page 26048, line 20. Was the humidity of the sample air after drying with a Nafion dryer measured to assure that the air was really dry? We well know, that at high humidities, the collection efficiency (CE) of the AMS changes, despite the fact that the CE was already set to 1 in this study.

Page 26057, line 19-21. It is confusing that the fragmentation table is linked also to the inorganic aerosol species at this point, because on page 26053, line 11, it was stated that these species were determined by using HR spectra, where most of the ions are fitted and not assigned by the fragmentation table. This section should be revised.

Minor comments:

Page 26057, line 15. Within “as discussed in previously”, the word “in” has to be deleted.

Page 26061, line 3. Between “measured” and “m/z”, a space character is missing.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 26041, 2014.