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Interactive comment on "Joint Measurements of $PM_{2.5}$ and light-absorptive PM in woodsmoke-dominated ambient and plume environments" *by* K. Max Zhang et al.

Anonymous Referee #1

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The authors present a compilation of interesting field campaigns in two winters in the NE US in environments where wood burning is an important source of air pollution. PM2.5 and Black Carbon (BC) have been measured and the delta-C (DC) parameter identified as a good (semi)quantitative indicator for the presence of wood burning contribution to PM2.5.

The manuscript is an analysis of local particular air pollution. The following changes and additions will greatly contribute to its scientific significance and quality and will facilitate the understanding by an average reader.

The authors are interested in quantification of the contribution of woodsmoke to PM2.5. Source apportionment always depends on the assumptions of the method employed

to determine the sources. The authors make the statement that PM2.5 is dominated by woodsmoke on p. 5 (lines 12-13) and p. 10 (line 14). This should be the result of the work, not an initial assumption, which cannot be tested without additional chemical methods being applied to the same samples. The starting assumptions need to be checked and a "proof" that PM2.5 is exclusively due to woodburning needs to be produced. Alternatively, a reference to a monitoring agency report or previous publications would be advantageous.

BC and PM2.5 have similar diurnal variation. We can see from Figures 3 and 4 that the BC/PM ratio is not constant during the day. This means that the composition of PM is changing. The authors correctly point out that unless UV-absorbing species feature a constant absorption cross section, DC cannot be considered quantitative. The change in composition implies the change in the absorption cross section. This part of section 3.3 needs to be expanded and arguments provided. Comparison to other source apportionment methods using similar methods would help (Sandradewi et al., 2008).

Finally, the manuscript ignores the production of SOA. The change in composition of PM is already evident from the diurnal variation of the BC/PM ratio. SOA and the applicability of the method described in the manuscript need to be discussed in terms of primary and secondary PM. SOA can dominate PM in places where PM is heavily impacted by wood burning – the sites presented in the articles are such places.

I agree with Reviewer 2 that the presentation of the "PAH" as measured in this campaign is weak. I would recommend to either remove this subsection or significantly redact it and expand the discussion on the PAH measurement method. If the authors expand the section, they should switch the PM2.5 and PAH axes. They argue that PAH is not as good an indicator as DC, hence the plot should be made into PAH as a constituent of PM2.5 by switching the axes.

Minor comments:

Dates need to be changed so that the European readers will find them unambiguous (Figure 3).

Figure 7: Why just report this for Rutland?

References

Sandradewi, J., Prevot, A. S. H., Szidat, S., Perron, N., Alfarra, M. R., Lanz, V. A., Weingartner, E., and Baltensperger, U.: Using aerosol light absorption measurements for the quantitative determination of wood burning and traffic emission contributions to particulate matter, Environ. Sci. Technol., 42, 3316-3323, doi: 10.1021/es702253m, 2008.

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