

Interactive comment on “Laboratory investigation of photochemical oxidation of organic aerosol from wood fires – Part 1: Measurement and simulation of organic aerosol evolution” by A. P. Grieshop et al.

Anonymous Referee #3

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This paper present experimental results from controlled burning of a variety of solid pieces of wood in a stove and investigates the properties of the smoke aerosol as it ages, including volatility and formation of secondary organic aerosol (SOA). The topic is important and the paper carefully and precisely describes these interesting experiments. The paper was very well organized and well written. Below are some topics that could be clarified.

I believe it is very important that the authors indicate early in the paper (eg, possibly in the abstract) how representative these experiments are compared to the various

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forms in which biomass is burned naturally or as a fuel. For example, adding a line describing that these experiments involved burning wood in a wood stove would clarify at the very beginning what type of fire smoke was investigated. The paper tends to imply that the results are widely representative, including being applicable to burning in natural environments. Considerable and very worthwhile discussion is given to the comparison of concentrations of a number of smoke compounds (eg, OC etc) between these experiments and ambient observations. However these experiments involved burning various varieties of small pieces of wood in a small stove (page 15703 lines 5 8211; 10), which seems highly unlikely to represent ambient biomass burning where fuel mixtures (wood, duff, canopy leaves or needles etc), range of moisture contents, etc, are much more complex. Furthermore, in natural fires heating of non-combusting material (eg, forest litter and canopy) by the fire could produce an additional variety of VOCs along with those generated in the burning.

Along with noting early in the paper the specific burning method used, a larger discussion later in the paper on the limitations of broadly applying these results to biomass burning in general would also be of value. For example, can the results of the OA formation observed in this paper be applied broadly to natural fires, and under what conditions would the results be applicable.

In the Discussion and Conclusions, the OA production in the experiments are in some ways contrasted to ambient results, three studies being cited. Here again, the comparison between ambient and these experimental results should be qualified by including why there could be significant differences and what the implications are. For example, it is noted that Lee et al suggested isoprenoid species generated from heating (canopy etc) as a possible source for OA production, but not surprisingly, considering the burning method, these experiments observed that they were too low by 2 orders of magnitude. It would also be worthy discussing that little O₃ was generated in these experiments, whereas in ambient studies O₃ production can be substantial in biomass burning plumes (eg, Lee et al.)

In the section on Basis Set Modeling, pg 15715 lines 20 to 25. If I understand this correctly the distribution of measured OC on the front and back quartz filters are used to infer the partitioning of SVOC between the particle and gas phases (ie, front filter equals particle concentration, back filter equals SVOC concentration). This should be explained more clearly in the text, although I may have it wrong. If this is what is used, what is the justification for assuming that OC volatility relative to air flowing through a baked quartz filter is the same relative to smoke particles?

On page 15720, lines 3 to 6 it is suggested that the results here of SOA production in smoke may explain the modern carbon observed in urban areas by other investigators. Do these cited papers report that biomass burning was a significant source of the urban OC? This must be the case for this statement to be justified. More details are need if this point is to remain in the paper.

Minor comments.

Page 15701 line 16: 8220;SOA is formed when 8230;8221; This statement should be qualified noting that this is the 8220;traditional8221; SOA model, see Volkamer et al 2008 (Atmos. Chem. Phys. Dis, 8, 14841-14892) for an alternative theory.

Page 15705, lines 3 to 10. The use of wavelengths 880 and 590nm as indicators of brown carbon seems highly suspect since many references show little light absorption by brown carbon at these wavelengths, see for example Fig 1 in Sun et al, Geophys Res Lett, VOL. 34, L17813, doi:10.1029/2007GL029797, 2007. This would explain why no wavelength dependence was observed with SOA formation; it may not be detectable at these wavelengths.

Page 15705 line 17, what about the AMS vs filter OC magnitude (slope) comparison, only r^2 is given, whereas both slope and correlation is given for BC vs EC.

Page 15711 line 5, I do not believe this sentence should be referring to Table 1, maybe Figure 1

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Page 15713, line 5, typo, 80

Table 1. Define MCE somewhere on the table (footnote?).

Fig 7, no letter labels on individual plots. Also, in Fig 7 caption, change they to the in second last line.

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