

Interactive comment on “Transformation of logwood combustion emissions in a smog chamber: formation of secondary organic aerosol and changes in the primary organic aerosol upon daytime and nighttime aging” by P. Tiitta et al.

Anonymous Referee #1

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The manuscript by Tiitta et al. “Transformation of logwood combustion emissions in a smog chamber: formation of secondary organic aerosol and changes in the primary organic aerosol upon daytime and nighttime aging” is important since 1) there is a lack of knowledge on the SOA formation from biomass emissions and 2) biomass combustion emissions are extremely heterogeneous in their properties. A highlight is the importance of dark aging on POA transformation and SOA formation and the quantification of fresh and aged organic nitrates. The use of PMF is an important tool that here is exploited to a more sophisticated degree than in previous papers on biomass SOA. An impressive array of highly suitable scientific methods have been used in the study and

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the presentation is scientifically sound. The paper is suitable for publication in ACP after major revision according to the points below.

Abstract: At 496 words the abstract is not optimized to capture the interested reader. I would suggest to reduce the abstract to 300 words to more efficiently convey the main findings.

P4:R28-: The authors chose to investigate cold start (kindling) emissions using a very dry fuel. Since this a very important study the reader needs to know about the generalizability of the chosen combustion variables. How would the findings transform to fuel addition on glowing embers (as I would suspect is more representative for ambient biomass PM) and fuel with higher moisture content?

For example P6R4: The authors make several notes about the relevance for Chinese conditions. I agree biomass combustion emissions are very important for air quality in China. However, for the logic to make sense please comment in the paper if the chosen combustion system (and fuels) are of relevance for Chinese biomass combustion emissions.

P6R30: Dual or Tungsten vaporizer? Which was used in the presented results beyond the rBC data? Please state. Please check that the 60 s is enough to get a stable signal without being sensitive to transients when laser goes on-off. Was there any transients observed or were the results “perfect square waves”?

P7R8: As I understand it a reduced CE (0.6) was needed compared to the mIE calibration with Regal Black (RB). Thus the sensitivity of the SP-AMS to biomass rBC was lower than for RB. Note that this is opposite to the results by Willis et al. (2014) who found that the sensitivity increased upon coating RB. I do not argue against the CE of 0.6 for biomass compared to RB, it is similar to published data for wood stoves (Martinsson et al. 2015). However, it should be motivated based on the TEOM comparison or biomass data in literature and not based on the Willis et al. reference.

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P7R21: So the CE of rBC did not change much upon aging? Please clearly state if this is correct or not. I assume it makes sense since the rBC particles are only thinly coated by aging.

P9R16: It is very good that it is pointed out that the SP-AMS measurement of K is highly uncertain. However, also the quantification of Cl and SO₄ requires caution as their chemical state is quite different from ambient observations. Their quantification from SP-AMS data is new for me in biomass combustion emissions. Please add to the methods section how this was done (vaporizer mode, CE, RIE etc). As is stated K salts may be present as external mixtures. If so what would make the rBC free particles vaporize in the W and dual vaporization modes respectively, given their absorption cross sections and vapor pressures? Was there any filter samples to validate the new method to quantify Chl and SO₄ in biomass combustion data?

Figure 2: The small and capital letters a, b, c risks the reader will find it hard to quickly understand the figure. Please change the denotation for the different stages of each experiment to something that is more helpful to the reader.

Figure 2b: If the SO₄ increase after HONO addition is most likely an artefact then this should be mentioned in the figure caption. However, it is very good that it is mentioned and not sorted away as may sometimes be the case.

Table 4: double legend, please remove

Table 4: It is very good the age is calculated for OH exposure. Could relevant numbers be derived also for the dark aging?

P10R26: If ozone was added in the "UV-only" experiments, could you then conclude that OH chemistry and not O₃ chemistry is responsible for the SOA formation in this case?

P11R5: Was the laser vaporizer engaged when calculating the elemental ratios? Please describe in the text. This is critical since the laser may produce a relatively

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strong CO₂⁺ signal which have been hypothesized to origin from surface oxides on the soot (Corbin et al. 2015). It is not clear to me if this signal should be attributed to the rBC core or the OA coating. Thus, one may speculate that the O:C would be significantly higher for dual vaporisers than W-vaporiser. Please comment in the text the O:C for fresh emissions with dual and W vaporizer, respectively.

Figure 5: Please change the names of the POA and SOA factors to represent what they are tracing, it would make it much easier to convey the messages to the reader. Add info about fast and slow ignition to the figure, this is currently missing.

P14R2: Yes possibly toluene, but Xylenes commonly have a much higher f₄₃/f₄₄ ratio than found here. What about Naphthalene? Another group of important SOA-precursors are phenols. Please comment about their potential contribution to the different factors based on their reactivity towards different oxidants. An important paper on the precursors responsible for SOA formation was recently published by Bruns et al. (2016). Please add this reference and compare your conclusions with their findings.

Figure 6: It would be important to include the rBC emission factors in this figure (on a separate axis) so it becomes clear for the reader that these aerosols are always rBC dominated.

P14R9: POA seems to contain very low PAH contribution, why would then PAH degradation by UV be a major course of the reduction of this factor with UV?

References:

Bruns, Emily A., et al. "Identification of significant precursor gases of secondary organic aerosols from residential wood combustion." *Scientific Reports* 6 (2016).

Corbin, J. C., et al. "Black carbon surface oxidation and organic composition of beechwood soot aerosols." *Atmospheric Chemistry and Physics* 15.20 (2015): 11885-11907.

Martinsson, Johan, et al. "Impacts of combustion conditions and photochemical processing on the light absorption of biomass combustion aerosol." *Environmental science*

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& technology 49.24 (2015): 14663-14671.

Willis, M. D., Lee, A. K. Y., Onasch, T. B., Fortner, E. C., Williams, L. R., Lambe, A. T., ... & Abbatt, J. P. D. (2014). Collection efficiency of the Soot-Particle Aerosol Mass Spectrometer (SP-AMS) for internally mixed particulate black carbon. *Atmospheric Measurement Techniques*, 7(12), 4507-4516.

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