

Atmos. Chem. Phys. Discuss., referee comment RC1
<https://doi.org/10.5194/acp-2021-284-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on acp-2021-284

Anonymous Referee #1

Referee comment on "Nighttime chemistry of biomass burning emissions in urban areas: A dual mobile chamber study" by Spiro D. Jorga et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-284-RC1>, 2021

Jorga and coworkers investigate nighttime secondary organic aerosol (SOA) formation in urban ambient air using a dual chamber; one is used for SOA experiments and the other is used as a control reactor. This measurement approach allows for improved understanding of the oxidation chemistry and the secondary pollutant formation in the actual ambient conditions. Specifically, this work is conducted at night during the period when high biomass burning impact is observed. As nighttime oxidation of biomass burning emissions and its impact on secondary organic aerosol formation is relatively less studied compared to the photochemical oxidation, the results in Jorga et al. can potentially help improve our understanding in secondary organic aerosol formation from biomass burning emissions, especially their interaction with urban air during nighttime.

One of the major findings in this study is that the ambient chamber experiment reproduced the mass spectra of biomass burning oxygenated organic aerosol (BBOOA) reported in Kodros et al. (2020), where the AMS mass spectra of BBOOA was retrieved from flame chamber experiments and parameterized for simulation application. Further, this study quantified the secondary organic nitrate formation from biomass burning, which is less understood compared to organic nitrate generated from the other sources such as biogenic emissions.

However, there are number of drawbacks in this study that need to be improved. The major comment that the referee would like to make is that the discussion is insufficient. The discussion in the current manuscript solely focuses on comparison of mass spectra with previous studies, providing very limited new scientific insights beyond those reported in previous studies. The role and importance of different oxidants and oxidation pathways (ozone, OH, NO₃) need to be better justified and discussed. While the manuscript is focusing of OA formation from nighttime chemistry of biomass burning emissions, only total OA enhancement is reported. As the ambient air is not pure biomass burning, what are potential contributions from oxidation of non-BB VOCs and/or further non-BB related particle-phase chemistry to the observed OA enhancement? Furthermore, the criteria applied for mass spectra comparison is not consistent throughout the manuscript.

Specifically, $\theta = 9-18^\circ$ is considered as "similar" in Line 267 whereas $\theta = 19^\circ$ is considered as "significant difference" in Line 232. This makes it difficult to evaluate if the results here are consistent or not with previous studies.

Overall, since this manuscript is not submitted as a Measurement Report, major revisions (e.g., to include more in-depth scientific findings and discussions) are needed before the manuscript can be accepted for publication.

Specific comments:

- Experimental description and chamber characterization
 - As the manuscript discusses the difference between two experimental condition (section 3 (typical) and section 4 (others)), there should be a description of the difference in experimental conditions.
 - PMF results are used for the discussion in the manuscript (Line 189-196, Line 263), but there is no description about the application, or the literature from which this study adopted the results from.
 - Line 243: does this mean that the wall-loss is aerosol composition dependent? How about particle size or other parameters?
- Line 243-261: What did the authors achieve from the comparison? Does the similarity mean that the authors observe BBOOA from this experiment? Please elaborate. Although the initial OA is mostly BBOA, it would have been better if the authors not just report the OA enhancement during the experiments but also quantify the possible BBOOA formation. Separating BBOOA from the OOA would be more informative.
- Line 249-250: what do the authors want to address from Figure 4 and Figure 5? Neither the description nor discussion of these figures can be found in the manuscript. Also, this sentence would be better as in: "Figure 4 and Figure 5 show the estimated produced OA and corresponding AMS mass spectrum in Exp. 1, respectively."
- Line 249: Δ OA enhancement is estimated here. Does the Δ OA enhancement corresponds to the expected SOA amount (based on the yield of individual VOCs)? Also, how do the authors evaluate the loss of OA between evaporation loss vs. wall loss in the chamber? Were there any changes in specific compounds from either gas-phase or particle-phase composition before the oxidation began (other than O/C mentioned in the manuscript)? Since there is a recent study that proposed the evaporated gases from BBPOA can be the major source of BBSOA, it would be better to discuss thoroughly about other possible effect that can affect the Δ OA enhancement.
- Line 263-270: When compared with previous studies, it is difficult to tell from the manuscript how the results here are different or similar to the others. The authors should have provided more background (e.g., citing relevant literature) specifically how these cities are affected by biomass burning. Further, if available either as in literature or experiment results, it would be interesting if the mass spectra of this study are compared and discussed with the mass spectra evolution of daytime oxidation of biomass burning emissions.
- In line 295, OH is reported to be 20-40% of the daytime ambient level when O3 is injected in perturbation chamber. The authors comment that this is not significant and

conclude that NO₃ or O₃ is responsible for SOA formation based on the chamber experiment results. However, wouldn't some VOCs be susceptible to OH oxidation in addition to NO₃ and O₃ oxidation? Since additional ozone is making unintended OH as a byproduct of ozonolysis, the authors need to justify how the perturbation experiments in this study are representative of nighttime chemistry. Further, since the "4.2 NO₃ and OH radical levels" is in the "4 Results of other experiments" section, does this mean that the authors did not observe any OH formation in "3 Results of a Typical Perturbation Experiment"? Lastly, please add a brief comment why OH is observed when O₃ was injected in perturbation chamber, but not in control chamber.

- "4.3 Factors affecting the SOA production"
 - Line 311-316: how did the difference in the speciation of initial VOCs affected the results?
 - Line 317: why would low level of initial NO have the lowest SOA production? Low NO corresponds to low VOCs in Figure S2. Please add this information in the discussion.
 - Line 324: NO₃ concentration is not available for more than half of the experiments. Most of the experiments with high VOCs are missing this information. Further, the Exp.8 not only shows higher NO₃, but also reports higher VOCs than the rest of experiments which NO₃ data is available (except Exp. 6, and this is the outlier in Figure 8). It is not reasonable to make a comment with limited data, with missing information from the most important experiments.
- "5. Conclusion": The main takeaway message is unclear. Please provide the novelty of this study and emphasize the main findings. Moving the main discussion sentence to the beginning of each paragraph will help readers to understand the main points that this paper addresses.
 - Line 345: does this mean than the OOA (biomass burning affected) in this study does not show any differences with the urban OOA that are not impacted by biomass burning, but only seasonal change matters?

Technical comments:

Firstly, please edit the manuscript for language and proofread the manuscript thoroughly. Some of comments below pointed out phrases written in an inappropriate manner, but not all grammatical problems have been noted in this review. Many of sentences are written in a colloquial style and need to be modified.

Line 33: OA increase in the abstract is reported as percentage, but organic nitrate is reported as concentration. What is the fraction of organic nitrate formed among total OA?

Line 56-57: please complete the sentence: "..., but reactions of emitted monoterpenes with ozone can also contribute."

Line 152: "BC" appears the first time here, so this should be "black carbon (BC)" (and not

in Line 162.)

Line 164-168: Was the RH of particles controlled using a dryer before the instrument?

Line 177: Was the neutralizer attached to the atomizer during the wall-loss characterization of the chamber?

Line 207: The authors use "Fig" for some figures and "Figure" for others. Please keep it consistent.

Line 221: "organics" to "organic nitrate" for the clarification.

Line 223-224: What percentage change does the $1\mu\text{g}/\text{m}^3$ increase in NH_4 correspond to? What was the 8% change in mass concentration from control experiment? Please keep it consistent. It would be useful to report both concentration change and relative change.

Line 228-232: Please check the grammar of this sentence.

Line 234: the sentence "Figure 2 represents....end of Exp. 1." should be moved to the first part of paragraph. The change in each mass is better to be described afterwards.

Line 236: O : C change from 0.4 to 0.41 is difficult to justify "low but non-zero chemical activity" since the change is too small, should be within the uncertainties of the instrument. Also, it is very difficult to see this difference from Figure 3.

Line 239: not only O_3 , but NO_3 should be also existing in the control chamber, although it would react away faster.

Line 248: should be "pre-existing".

Line 251-262: rearrange the sentences in this paragraph for better readability. Also, no need to describe the conditions in Kodros et al. (2020) repeatedly.

Line 258: add a comma after "Here".

Line 263: OOA appears in Line 193. Please change the previous one to "oxygenated OA (OOA)".

Line 277: be consistent with either organic nitrate or organonitrate.

Line 280: add a comma before "ranging from...".

Line 282: add a comma after "At the same time".

Line 312: what are the authors trying to say in "therefore took place during polluted conditions"?

Line 312: it is recommended to include initial VOC concentration in Table 1.

Line 314-316: it would be better to make a statement after the evaluation of both VOCs and NO₃ level. Then propose which factor is affected the most and is recommended as an indicator for SOA formation.

Line 335: "organics" to "organic nitrate" for the clarification.

Line 338: it would be better to remove "injected" when describing control experiments. It may confuse the readers.

Figure 4: Dot plots are difficult to see. Please change the figure into line plot (or line with markers).