

Responses to the Comments of Reviewer 1

(1) Jorga and coworkers have revised the manuscript “Night-time chemistry of biomass burning emissions in urban areas: A dual mobile chamber study” along the comments of the reviewers of the first submission. Most of my comments have been reasonably addressed and resolved by the responses from the authors. However, minor revisions are needed before the publication.

We do appreciate the effort of the reviewer to help us improve the paper. We have done our best to address his/her comments. Our responses and corresponding changes to the paper (in black) follow each comment (in blue).

Specific comments

(2) I think merging into one section is a good way to show the results of different chamber experiments. However, you must clarify why the experiment 1 is chosen as the representative in the beginning of the section 3.

We have added in the beginning of Section 3 a brief explanation of the reasons for our choice of Exp. 1.

(3) Even if you are adopting results from an unpublished work, you should cite the work in the same way as the published articles. Please add a citation at the end of the sentence in Line 236 of the change tracked revision.

We have added the requested citation to the work of Kaltsonoudis et al.

(4) Please clarify if the comparison is conducted with respect to daytime oxidation results from your own experiments or from previous studies in Line 371-373 of change tracked revision.

We have rewritten this sentence to clarify that this is a comparison between the spectra of the produced SOA in this work (processing of ambient air) and the laboratory experiments with photochemical processing of biomass burning of Kodros et al. (2020).

(5) From the Figure 8, I only see 5 data points, which means 6 out of the 11 experiments do not have NO_3 measurements. Also in the figure, it seems like R^2 can be biased as there are only 2 data points where the organic nitrate concentration is higher than $1 \mu\text{g}/\text{m}^3$ and the rest 3 data points are around $0 \mu\text{g}/\text{m}^3$. I think it would be better to add a comment in the manuscript about the limitations that the authors discussed in their response here; that there are only small numbers of experiments that NO_3 results are available. Further, I suggest adding that the details of organic nitrate estimation can be found in SI at Line 268-271 of the change tracked revision. To add, are there any dependency of organic nitrate concentration (or mass fraction, yield) on VOC speciation? If not, please clarify this in the revised manuscript, too.

The discrepancy is due to the fact that the NO_3 level in two of the experiments (Exps. 3 and 10) was below the detection level of 2-3 ppt. So it was measured (we know that it was less than 2-3 ppt), but was not included in Figure 8 due to the corresponding uncertainty. This is now mentioned in the revised paper. We have also added a comment about the limitations of this analysis as it relies on a few experiments for its conclusions. We also now mention that the details of the organic nitrate estimation can be found in the SI. We did not find any links between the organic nitrate concentration and the VOC speciation, something that is now mentioned in the text.

Technical comments

(6) Please add the information about the difference between with and without the neutralizer in the manuscript (or SI) when you describe the particle wall loss.

We have added this information to the SI.

(7) Please also change organonitrates to organic nitrate in Line 383 of the tracked revision.

We changed organonitrate to organic nitrate.

(8) Again, I still see minor grammatical problems (e.g., missing commas, misuse of plural and singular forms, etc.) in the revised manuscript. Please carefully edit the manuscript for language and proofread thoroughly before the re-submission.

We corrected a few more of the typos. The ACP editing should do the last careful editing of the manuscript.