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.

Specific comments

(3) *As the manuscript discusses the difference between two experimental conditions (section 3 (typical) and section 4 (others)), there should be a description of the difference in experimental conditions.*

It appears that our separation of the result of our experiments into sections (3 and 4) is causing some confusion. Our intention is to present the results of one typical experiment (Exp. 1) in detail and then summarize the results of the rest (Exp. 2-13) without repeating the same details. There is no difference in the experimental conditions, just on the level of detail of the presentation of the corresponding results. This is why the results of all conducted experiments are grouped together in Table 1. To avoid this problem, we have merged the two sections describing results into one and also explain in the beginning of this section that all experiments were conducted in similar conditions. We also explain that we discuss the details of only one and summarize the major results of the rest.

Reviewer response: 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.

(4) *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.*

This is a valid point. The dual chamber experiments took place together with a traditional month long field campaign, in the city of Patras Greece. The PMF results of the ambient OA measurements are described in a companion paper that is ready for submission. We have added a paragraph summarizing the details of the PMF application and the major results of this second paper together with a reference to a presentation of these results.

Reviewer response: Even if you are adopting results from an unpublished work, you should cite the work in the same way as the published articles.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.

(9) *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.*

We have added some information about the bbOA contribution to the OA in the field studies that are mentioned in the paper and are used for comparison with our results. For example, in the study of Ge et al. (2012) for Fresno, California biomass burning accounted on average

for 16 % of OA, while in the Florou et al. (2017) study the bbOA contributed 40-60% of the OA. The corresponding references can be found in our paper.

We have followed the suggestion of the reviewer and added a brief discussion of the comparison of the AMS spectrum of the SOA produced in this study with the spectra of the SOA produced during daytime oxidation. There are notable differences, with theta angles approaching 30 degrees. This has also been noted by Kodros et al. (2020).

Reviewer response: 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.

(13) *Line 324: NO_3 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 NO_3, but also reports higher VOCs than the rest of experiments which NO_3 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.*

Please note that NO_3 measurements were not available for only 4 out of the 11 experiments discussed in the paper. These are challenging measurements so there were some unavoidable instrument problems. In the two experiments (Exps 3 and 10) NO_3 radicals were not detected probably because their concentration was below the detection limit of the instrument. To the best of our knowledge it is the first time that NO_3 measurements are available in ambient chamber experiments of this nature. Although, the available data are limited the good correlation with the organic nitrate concentration formed is encouraging and can help strengthen the point of NO_3 reactions in the perturbed chamber. We do believe that the information provided by the NO_3 measurements (despite the gaps) is a major strength of the present work.

Reviewer response: From the Figure 8, I only see 5 data points, which means 6 out of the 11 experiments do not have NO3 measurements. Also in the figure, it seems like R2 can be biased as there are only 2 data points where the organic nitrate concentration is higher than 1ug/m3 and the rest 3 data points are around 0ug/m3. 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 NO3 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.

Technical comments

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

The ammonium sulfate seeds after the atomizer passed through a diffusion dryer and then were injected in the chambers. We did not use a neutralizer after the atomizer. In past experiments we have seen little difference in the measured deposition rate constants with or without the use of a neutralizer. This information has been added to the paper.

Reviewer response: 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.

(33) *Line 277: be consistent with either organic nitrate or organonitrate.*

We changed the organonitrates to organic nitrate.

Reviewer response: Please also change organonitrates to organic nitrate in Line 383 of the tracked revision.

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.