Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-1004-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Carboxylic acids from limonene oxidation by ozone and OH radicals: Insights into mechanisms derived using a FIGAERO-CIMS" *by* Julia Hammes et al.

Anonymous Referee #2

Received and published: 19 December 2018

General comments

The authors present results from 33 limonene oxidation experiments performed in a flow reactor at 20°C under different RH and precursor conditions. Using an acetate-FIGAERO-CIMs, they focus their analysis on carboxylic acids in the gas and particle phase. Molecular formulae and signal contributions of the most abundant acids are identified, also as a function of experimental conditions (humidity, OH scavenging, ozone level). Reaction mechanisms to explain inconsistencies between measurements and predictions based on the MCM are suggested.

The paper presents an interesting topic, and the dataset is very promising. However,

Printer-friendly version

Discussion paper

there are some issues with the analysis, and more importantly, the presentation of results, that need to be addressed before the paper can be considered for publication.

1. The motivation of the study could be carved out better. The authors introduce limonene as an important indoor VOC, but do not further go into detail about quantities etc. They then switch immediately to limonene in ambient air, but it is not clear at all whether indoor or outdoor limonene was the motivation for the study, and whether precursor conditions were tuned to simulate indoor or outdoor conditions (I assume, outdoor). In addition, the authors do not discuss any numbers, and only make qualitative statements in the introduction. This makes it hard to gauge the significance of the results presented here in the context of previous research.

2. A related point is to be made for the materials and methods section. A short discussion on precursor concentrations, and how they compare to atmospheric conditions, or other lab studies, would help placing the study in context within previous/ongoing research.

3. Generally, choices for both analysis and figures should be better motivated (scientifically). Why was this particular mass spectrum chosen in Figure 2? Why is the analysis focused on acids with 7 – 9 carbon atoms? Why were experiments with 1000 ppb of ozone and 150 ppb limonene only chosen for Figure 3? Again, the importance of the results presented here is hard to gauge without a clear scientific reasoning. In addition, e.g. Figure2 and Figure 3 would be more interesting if they represented average and diversity of mass spectra for different experiments, and not examples.

4. The discussion on the different effects on observed spectra is interesting, but somewhat hard to follow. It would be beneficial if the authors could provide figures that support their claims, and visualize the most important statements/relationships.

Specific comments

P. 1, l. 19: This reference is used rather often throughout the manuscript. It is with-

Printer-friendly version

Discussion paper

out any doubt an important reference for the study. However, there might be certain subtopics of SOA that have seen some progress and update in the last decade, and it might be worth finding these.

P. 1, l. 19: Are number, size, and chemical composition “particle properties”? This sentence should be clarified.

P.2, l. 16: Why was only the anti – CI* pathway chosen for Figure 1, and not the syn – CI* pathway? Please motivate.

P. 2, l. 29: The switch to RO2 radicals is rather sudden. Try to better introduce that paragraph.

P. 4, l. 2: Table S1 should be moved into the main manuscript. It would greatly help in following the results.

P. 4, l. 17: Is it possible that dilution may have influenced the gas-particle equilibrium as resulting from G-FROST in your sampling line? From Figure S1 it looks like the flow to the SMPS was diluted as well (same inlet)?

P. 4, l. 32: Please motivate why you used Spearman correlation for your analysis.

P. 5, l. 16 – 17: Is the statement of the influence of water based on the numbers in Fig. 3? Are the differences significant?

P. 5, l. 23 – 24: Can the authors explain why the product distribution of the gas phase in pure ozonolysis experiments is more diverse?

Technical corrections

P. 1, l. 18 – p. 3, l. 32: The introduction should be divided into paragraphs, separated by line breaks.

P. 2, l. 16 – 17: “[...] where the oxygen atoms contribute to the formation of [. . .]”

P. 4, l. 4: Unfinished sentence

Caption Figure 2: Spell out OH-S

P. 9, l. 23: Should read “fig. 1”, not “fig. 11”?

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-1004, 2018.