Atmospheric Chemistry and Physics Discussions

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Comment on acp-2021-1080

Anonymous Referee #3

Referee comment on "Chamber investigation of the formation and transformation of secondary organic aerosol in mixtures of biogenic and anthropogenic volatile organic compounds" by Aristeidis Voliotis et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-1080-RC3, 2022

Voliotis et al. describe a comprehensive set of chamber experiments of the secondary organic aerosol (SOA) formation from individual VOCs as well as VOC mixtures including both biogenic VOCs (isoprene and α-pinene) and anthropogenic VOC (o-cresol). By novel experimental design of VOCs with comparable reactivity, it nicely compared the SOA form signal precursors with that from their binary and ternary mixtures, revealed very interesting results, as well as the complexity in the experimental design and evaluation. It provides valuable information and discussion for our understanding of chamber studies of VOC mixture systems, and also guidance and inspiration for future study of such essential topic. I would recommend the publication of this solid and excellent work. However, there are a few comments I would like the authors to address.

General comments

This paper is very long. I suggest restructuring and shortening it. I understand that there are too many things that have to be considered and also worth to be mentioned in experimental design and evaluation. However, I suggest focusing on what you did rather than what you did/could not do (and the reason behind), clearly separating method, results, and discussion, and helping readers to quickly get the most important results and information from the paper.

I appreciate that this paper also offers the chemical or physical properties measured by both online and offline instruments, which could be used to explain the interactions found in VOC mixtures experiments. However, I was a little bit disappointed to see some very general introduction/results, but not a close link to the most interesting findings, e.g. shown in Fig. 5. Figures like Fig. 13, in my opinion, are not necessary to put there and

make the paper too long. It could be mentioned e.g. in the discussion.

Specific comments

Line 26 to 28: It is difficult to get "½ initial reactivity" in the abstract. Please add more explanation.

Line 30: If it is too small, could you say "a suppression of the SOA yield from o-cresol is not found when it is mixed similarly with isoprene"?

Line 43: The reference could be updated.

Line 224: It was already mentioned in line 196.

Line 338: Missing references for FIGAERO-CIMS and Orbitrap.

Line 390: Please specify the terms involved in the equation.

Line 404, Figure 2:

- the resolution of the figure is low (the same as most other figures).
- Line 404: b) is o-cresol and c) is isoprene.
- The description of colors leads to confusion and it is enough with the legend.
- Why in (b) the line of ternary experiments doesn't have shaded areas?
- It is not clear, that whether the decay rate of signal VOC comes from all full reactivity, half reactivity, etc. or only one experiment.

Line 429, Figure 3: Honestly, it is a very interesting figure, but difficult to read.

Line 456-457: Need reference.

Line 461, Figure 4: it is not clear how these data points were calculated

Line 489: Please specify the terms in equation 4.

Line 580: It is not clear how the normalization was performed. As I- signal should be very high, the normalized signal could not as high as 0.01or even 0.1.

Line 586, Figure 8: It is better not to cut the highest signal. Also, please mention the mass spectra were from which experiments.

Line 591 – 598: As both the FIGAERO-CIM and the LC-Orbitrap were used for particle-phase chemical composition, a general comparison or information of how many compounds were detected/identified for both will be interesting, and how to combine the molecules detected in positive and negative modes with the LC-Orbitrap.

Line 600: It is not clear what is "in -ve 600 ionisation mode 57% of the signal in 48 individual peaks with nC > 10 and in +ve mode 60% in 115".

Line 614: It is again not clear how the normalization was performed.

Line 636: The method of the calculation of C* should be mentioned earlier.

Line 842: Please mark Shao et al. in prep.

Line 846: The contributions of different isomers are not very different. I doubt whether it could be used to explain the different time trends measured by FIGAERO-CIMS.

Line 852: Similarly, please mark it Du et al. in prep.

Line 876: Wang et al.?