Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-223-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Constraining nucleation, condensation, and chemistry in oxidation flow reactors using size-distribution measurements and aerosol microphysical modelling" *by* Anna L. Hodshire et al.

Anonymous Referee #2

Received and published: 21 June 2018

This is a nice modeling study that utilizes measurements of SOA formation potential of ambient air organic mixtures at two separate field locations. The study demonstrates the utility of the OFR combined with a model to understand processes representing oxidation of organic gases, new particle formation, and growth.

I have the following suggestions and questions that need to be addressed before the Manuscript is accepted: The Manuscript would benefit if there are specific discussions about what OFR processes/parameters could be relevant in the atmosphere, and which of these are less applicable. For example: 1. The OFR does not represent high NOx

conditions. But the reality is that NOx is necessary for oxidant cycling. Granted that the OFR by design creates high OH concentrations even at low NOx. This is fine for reacting carbon. But the product and species distributions created this way in the OFR could be very different than those occurring due to NOx-mediated oxidant cycling in the atmosphere, even if the oxidant concentrations are the same. Please provide some discussions along these lines.

2. Due to the same reasoning as (1), please comment on whether the OFR can be used to study actual anthropogenic-biogenic interactions in the atmosphere. Note these interactions are NOx dependent.

3. Due to fast OFR processes, there is significant amount of ELVOC remaining in the gas-phase e.g. in Figure 2b. The authors explain that this is because the production of gas-phase ELVOCs exceeds the timescale of condensation and gas-phase fragmentation in the OFR. But in the real atmosphere, this will not be true since there is enough time for condensation and gas-phase fragmentation. Can the authors throw some light on this OFR-atmosphere timescale difference through any of their sensitivity studies?

4. The authors only consider fragmentation of the lowest ELVOC bin. On page 9, the authors mention fragmentation leads to more non-volatile products. It seems this is an error in the description. Fragmentation should lead to more volatile products. Please correct.

5. The argument that high kELVOC in the ELVOC bin effectively accounts for lack of fragmentation in the higher volatility bins, is not convincing. The mass of vapors in the higher volatility bins is much higher than the ELVOC bin. Also how fragmentation in higher volatility bins affects NP depends on details of oxidation, movement of species across the volatility intervals, the addition of functional groups, and particle phase processes (e.g. diffusion limitations etc.). So I find this statement as a major oversimplification. Please reframe this as a sensitivity study instead.

6. The fact that SIVOCs contribute so much to SOA potential over the Amazon seems

a bit weird. The rainforest is dominated by biogenic VOCs. Is this conclusion only valid for the dry season (where biomass burning is high) and not so much for the wet season?

7. What is the role of SIVOCs from biomass burning in the SOA formation potential over the Amazon?

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-223, 2018.