

Interactive comment on “In situ secondary organic aerosol formation from ambient pine forest air using an oxidation flow reactor” by B. B. Palm et al.

Anonymous Referee #1

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The paper summarizes observations of SOA formation and evolution in the ambient air and oxidized ambient air in a flow tube reactor at a forested site in Colorado. The paper is well thought (especially the extent of corrections performed for possible losses in the sampling lines, etc.) and results are clearly presented. The main conclusion of the paper is that oxidation of the measured VOCs is not enough to explain the formation of SOA upon aging. This is not a new finding, but the value of the paper in my opinion is to attempt a mass closure and determine the necessary SOA formation yields for the measured S/IVOCs to explain the observed increase in OA mass after photochemical aging. The analysis for relevant time scales for a typical OFR operation is also valuable for the community. I suggest publishing the manuscript after my comments below are

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addressed:

P 30412, L20: consider changing to “...functionalized products” and “... fragmented products.”

P 30422, L25: Max SOA is observed over night when in reality oxidation by OH in the ambient air would be low and oxidation by O₃ would be high, but OFR185 doesn't include much of an oxidation by O₃, so how relevant are these observations to understanding the ambient SOA formation at the site? P. 30435, L15-20: authors suggest that ORF185 is more suitable for forested environments. I don't agree with this statement since comparisons could not be carried out for short aging times and since OFR185 does not allow for significant O₃ oxidation whereas MTs have a high reactivity with O₃. Plus, for some long aging times, there is a non-negligible difference between OA enhancements observed in OFR185 and OFR254.

P. 30432, L24: It's unclear to me why SO₄ mass should be scaled by Faer? Is this the correction that's referenced in line 27? Similarly, I don't understand why a correction for LVOC condensation needs to be applied. Is the idea to account for the max possible amount of SOA formed beyond the time scales of residence in the flow tube?

P. 30437, L 20: Are the mentioned SOA yield values the chamber-derived yield values, just scaled accordingly for ~5 ug/m³ of OA? If so, what formulation was used to scale down the yields?

P. 30443, L11-12: Please add the aging times when OFR185 and 254 were compared (comparison wasn't possible at short photochemical times and so a general statement on the similarity of the results is not warranted).

Section S1: Particle loss correction: Adding in particle mass to what the AMS measured based on the volume fraction of different species is valid only if there is no size-dependent composition differences. Is that a valid assumption in this data set?

Figure S3 and S6. I'm confused as to why the right panels that plot AMS or SMPS

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volume added after oxidation show negative values. Please clarify.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 30409, 2015.

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