

Interactive comment on “Impact of NO_x on secondary organic aerosol (SOA) formation from α -pinene and β -pinene photo-oxidation: the role of highly oxygenated organic nitrates” by Iida Pullinen et al.

Anonymous Referee #2

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The goal of this work is to explore the observed and documented suppression of SOA formation in the presence of NO_x from previous studies. The authors hypothesized the observed decrease in SOA mass could be due to HOM termination reactions, in particular the reactions that form gas-phase accretion products or HOM organic nitrates. To test their hypothesis, they performed a series of chamber experiments at the Jülich Plant Atmosphere Chamber, a well characterized environment. They primarily used a Chemical Ionization Mass Spectrometer with NO₃⁻ ionization which has been repeatedly shown to be useful in measuring HOM, and studied the photooxidation of α -

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and β -pinene. They found that HOM accretion products were much more heavily suppressed than non-nitrate monomer products in the presence of NO_x. They determine this loss of accretion products is a possible and even likely reason for SOA mass suppression in the presence of NO_x. Section 4.4 discusses the suppression of accretion products but notably lacking is the possibility of precursor decomposition as a result of RO₂ + NO (R7, page 3 line 24) forming an alkoxy which can rapidly decompose, resulting in smaller RO₂ building blocks for accretion chemistry. This is brought up briefly on page 11 line 23, but is not worked into the discussion in section 4.4. It is however noted in the last sentence of the manuscript, only to say that it will be discussed in a further manuscript. This seems warranted to be discussed within this manuscript, and as-is I find it to be a major deficit of this work that should be discussed somewhere, potentially section 4.4. Additionally, the grammatical errors throughout impede understanding of the manuscript and need significant improvement before publication. All in all, this work is novel and of interest to the readers of ACP, so after addressing these major concerns and the other minor technical edits detailed below this manuscript should be suitable for publication.

Page 1, line 33-35: This could also be due to the higher vapor pressure of HOM-ON relative to similar non-nitrates as you reference on page 17 line 26-27. Consider rewording to make clear you're basing this statement off the results in this paper.

Page 2, line 28: “the absence of particles in the presence of NO_x”: consider rewording. The presence of NO_x doesn't mean there's no particles around.

Page 5 line 28 – Page 6 line 7: It would be helpful to include the average or typical concentration of OH more clearly. It is state on Page 6 line 3 but it feels a bit buried and is a single value as opposed to the range stated for O₃ and shown for OH in Fig. 1. Furthermore, the range of O₃ stated on page 5 line 13 does not match that on page 6 line 2. I would suggest adding the O₃ and OH concentrations to Table 1.

Page 8, line 10: please justify ignoring wall loss by providing a reference for particle

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wall loss rates on similar chambers, or a loss rate estimate from your measurements.

Page 8, line 25: what is meant by “molecules with finite vapor pressures”? Isn’t the vapor pressure finite with a given structure at a given temperature and pressure?

Page 10, line 7-9: is S_p the surface area of the particles? Sentence starting with “Varying $L_p(\text{HOM})$ ” is confusing, isn’t that relationship linear by definition? Consider rewording to replace the verb “led”, similar to the sentence on page 13, lines 11-12.

Page 11 line 16-17: the comment about the endo versus exocyclic bonds should either be explained and/or cited here, or be moved to the discussion section. Fig 2: Should note what these were normalized to as in Figure 4 caption.

Page 13 line 11-13: is this based on assumptions or was new particle formation observed? Consider rewording for clarity. Does loss to particles include loss to NPF in the correction?

Fig 6: why does the $\text{C}_{10}\text{H}_{16}\text{O}_7$ -10 vary so much from the $\text{C}_{10}\text{H}_{14}\text{O}_7$ -10 & $\text{C}_{10}\text{H}_{16}\text{O}_x\text{NO}_2$ homologues in that oxygen range? Is it possible that the peaks are mis-assigned or contain multiple compounds? On page 13 line 22 it is stated that: “the potential to condense on particles was about the same for HOM-PP and HOM-ON”, however this is not entirely consistent with $\text{C}_{10}\text{H}_{16}\text{O}_7$ -10 in Figure 6?

Page 16 paragraph starting line 15: Where does the factor of 1/5 come from? Also, I find the distinction, or lack thereof, of OrgNO_3 and ON confusing throughout the manuscript but particularly in this paragraph. Please clearly define the difference somewhere or use the same acronym throughout.

Page 17 line 4-5 & 7-8: would be helpful to define HOM with respect to # of Oxygens or to define “few O-atoms” with a number. This could be defined on page 3, lines 13-14 or lines 33-34.

Page 18, line 9-10: do you see this higher SOA mass for NO_x experiments relative to no- NO_x ? Has it been reported elsewhere?

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Page 18, 1st paragraph section 4.3: can you describe better what Zhao did and do a more complete comparison? As-is, it seems like you’re in perfect agreement that 10-11% of the OrgNO_3 is in the particle phase, but then lines 18-19 says you’re not in agreement?

Section 4.4: language could be formalized more throughout this section. “Mass loss” is a somewhat misleading phrase to mean converting potentially condensing species to non-condensable, not a direct loss of SOA mass due to, for example, evaporation. Please rework to be clearer and more streamlined.

page 21 line 12-13: please provide citations

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