Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-686-RC1, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Molecular composition and volatility of isoprene photochemical oxidation secondary organic aerosol under low and high NO_x conditions" *by* Emma L. D'Ambro et al.

Anonymous Referee #1

Received and published: 14 September 2016

General Comments

In general, this is a very well written manuscript focused on experiments that investigated secondary organic aerosol formation from isoprene oxidation under low humidity scenarios in which nitrogen oxide levels were varied. It includes an assessment of the molecular formulas of the constituents, as well as their volatility. A comparison is made to volatility (vapor pressure) prediction methodologies, as well as to recent field results. It is certainly a topic of much current interest in the atmospheric chemistry community and is appropriate for ACP. Scientifically, there are no major faults with the paper, as it is standard methodologies (chamber at PNNL, for example – please do not misinterpret, as I recognize the difficulty of doing chamber measurements) along side those

that are considered cutting edge (FIGAERO, for example) but evaluated and tested.

However, at times, I felt as if I were reading two papers that had been combined into one – that is, there could have been one manuscript that focused on these experiments with a comparison to field results; there are certainly sufficient data to do so. A second manuscript could have focused on the comparison of the volatility measurements to vapor pressure predictive capabilities. While the inability to predict vapor pressures of SOA constituents is not a new finding, the depth to which this area can be probed with the FIGAERO measurements is new and novel – perhaps warranting a second manuscript? This would certainly improve the readability of this manuscript. There were not significant issues with the writing, but the length and density of the manuscript made it slightly difficult to read.

Overall, I have no trouble recommending publication (following the authors addressing relatively minor specific comments below) based on its timeliness, topic, and high quality science. However, I would encourage both the editor and the authors to consider whether the manuscript could be split, simply to improve readability and perhaps to increase impact.

Specific Comments

It might be helpful to include a short figure that shows the chemical structure of specific molecules that are included in the discussion (ISOPOOH, ISOP(OOH)2, IEPOX, etc.) for the individuals who might not think about this on a daily basis.

Typo, p. 6, line 144, no need for the word "the"

Typo, p.7, line 168, "were" should be "was" (suite is singular)

Typo, p. 8, line 192, programmed has two m's

In Figure 1, it might be beneficial to show subsets that allow for seeing shorter periods of time. The length of time included/compressed on the x-axis makes seeing detail nearly impossible.

On page 10, lines 227 and 231. What is the basis for the factor of 1.5 to correct for wall losses?

Typo, p. 16, line 365, data suggest, not suggests

P. 19, third sentence of paragraph beginning on line 435 could be re-written for clarity.

On page 20, with the discussion of use of the measured Tmax to predict the Fp for comparison to measured Fp: Perhaps I am missing something, but this seems like a circular argument. It should be the case that a measured parameter that is related to Fp should do a better job predicting Fp than something unrelated like the vapor pressure estimation technique. I don't think that this adds very much to the paper. I do find the information that allows an investigation of predicted Fp as a function of molecular formula to be quite interesting and useful though.

On page 21, lines 473-475. Please reword for clarity. Which models predicted higher C*? And aren't group-contribution methods models? Perhaps word choice needs to be adjusted (or explain what the difference is).

Page 23, line 525. Does the change in vicosity imply a change in chemical composition? Do the measurements reflect that somehow?

On Figure 5, for predicted Fp (based on equation 1 and an estimated C* from group contribution methods), a COA is needed. What value was used here?

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-686, 2016.