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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effect of humidity on the composition and yield of isoprene photooxidation secondary organic aerosol" *by* T. B. Nguyen et al.

Anonymous Referee #1

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The authors demonstrate the chemical composition of SOA changes substantially under high or low RH but that the SOA mass, and therefore SOA yield (using conventional definition) did not change. These results are consistent with Dommen et al., who studied the RH effect on SOA formed during isoprene + OH oxidation extensively and found no effect on the SOA mass/SOA yield. The finding that the chemical composition of the SOA changes though, is quite important. There are implications for CCN suitability as well as other physio-chemical properties. The discussion at the end of the paper is important, but the introduction of new ideas and a new framing of the findings at the end of the manuscript is a bit awkward. There are some experimental details that require better justification. My main concern is that the use of signal-to-noise (S/N) ratio seems not well defended. The S/N could change due to changes in the signal (potentially due to increases in concentration) or due to changes in the noise (potentially due to higher

variability). Usually, the signal is used to make statements regarding concentrations. Provided the authors can defend the use of S/N and provide explanation or clarification to the comments below, this paper is suitable for publication in Atmospheric Chemistry and Physics.

Abstract, Line 7: Is the word significant used colloquially or does it imply a statistical test?

Page 9218, line 4: I think the proper notation for 2 author works would make it Finlayson-Pitts and Pitts, not Finlayson-Pitts et al. That book has very useful descriptions of the gas phase oxidation products of isoprene + OH, but there is no discussion regarding subsequent SOA formation.

Page 9219, line 4: mechanism of SOA formation might be better wording

Page 9219, line 7: the presence of liquid water not only affects chemical reactions. I think of partitioning as a physical process.

Page 9219, Line 10: This volume is sufficient to dissolve the most water soluble compounds. Is this volume not sufficient to accommodate water soluble compounds that are not the most soluble?

Page 9219, Line 20: increased LWC can affect the rate of uptake, but the volatility of the VOC (or its oxidation products) does not change. The thermodynamics remain constant, just the kinetics change. This sentence should be re-worded.

Page 9219, Line 24: This a curious sentence/reference mix. It is true that methylglyoxal hydrates in the condensed phase, but the first Reference is about glyoxal. There is mention of the word “methyl glyoxal” in references cited but only in the introductions/discussions. . .not in the experiment. If the authors want to talk about methylglyoxal specifically (and not glyoxal, as I agree and think they should) a Reference that studies methylglyoxal specifically might be more appropriate (a recent example that comes up in my isi search is Tan et al., (2010) AE 44(9):5218-5226).

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Page 9220, Line 4: can chemical reactions in the aqueous phase produce compounds that are not water soluble?

Page 9220, Line 16 due to increased LWC

Page 9220, Line 16/17 I think the correct term is keto-enol tautomerism

Page 9221, Experimental section: The authors provide uncertainty estimates for temperature and relative humidity. How about for O3, NO/NOx/NOy?

Page 9221, Experimental section: The authors state the RH was initially ~90% for "humid" experiments, but during the experiment RH was "somewhat" lower due to a slight rise in temperature (5 deg. C). A quick estimation of potential RH change for the system suggests changes of more than 20%, which is substantial (e.g., 90% RH vs. 70%). The authors do not need to be qualitative, they can quantitatively explain what the RH values were.

Experimental section: The reference provided for the 1.2 g cm-3 density assumption is from a study investigating a-pinene ozonlysis. The VOC was different and ozonlysis is an inherently different oxidation process with very different products. Can the authors defend their selection for particle density a little more robustly? How sensitive are the findings to this assumption?

Page 9224, Line 22 typo with lacetaldehyde?

Page 9225, Line 9/10: The fact that the conventional definition of SOA yield is insufficient is an important point. They might also want to mention this in the caption for Figure 2. The authors should state explicitly the formula they used.

Page 9226: Changes in the S/N could be due to higher variability or less mass at a particular m/z correct?

Page 9228 Line 18, increase in the gas phase would be surprising, but not in the condensed phase.

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Atmospheric Implications: The discussion of chain length, solubility, viscosity all seems very important. However it is awkward to introduce new ideas at the end of the paper. Perhaps the authors can find a better spot in the manuscript to begin this discussion?

Page 9234, starting at line 19: the hygroscopicity of SOA formed from a-pinene ozonlysis? Are the chemical properties of compounds formed via ozonlysis similar enough to isoprene + OH oxidation to say that the results are expected or in agreement? I think we can say they are not inconsistent with each other. The sulfate/CCN sentences as written seem non-sequitor. The work here also does not investigate CCN activity.

Table 1: caption. . .number of carbons in the in the monomer unit. Why not call "kmax" what it is, that is "homologous units" or homologues? Kmax wet and dry should be identified in the column heading similar to "total signal".

Figure 2. The secondary y-axis in (a) shouldn't go to 50. It seems there are important differences at t$\sim$30 minutes for 3MF, if the axis went to $\sim$10, readers would be able to see this better. Figure 2 is small and difficult to read in general.

Figure 3. Why not show the arbitrary units of the signal. The S/N can change due to changes in signal or in the variability (noise). The signal could actually be higher, suggesting more mass, but with more variability there could be a lower S/N. There seems to be a higher potential for mis-interpretation.

Figure S-1: the caption states "the S/N of the peaks in the 10 ug/m3 sample is noticeably smaller than for the 40 ug/m3 sample". This is apparent for the m/z of $\sim$125, but not elsewhere in the spectrum.

Editorial: define VOC and m/z at first use 4 digit zip code for Roach affiliation

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 9217, 2011.