

## ***Interactive comment on “Comparison of secondary organic aerosol formation from toluene on initially wet and dry ammonium sulfate particles” by Tengyu Liu et al.***

### **Anonymous Referee #1**

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This manuscript describes laboratory experiments aimed at measuring the yield and composition of SOA formed from the OH oxidation of toluene, in the presence of either initially deliquesced ("wet") or effloresced ("dry") ammonium sulfate (AS) seeds. This work extends the study by Faust et al. (2017), by examining the effects of toluene SOA yield/composition at different OH exposures. The authors certainly highlights an important point that SOA formed onto AS seed particles will lower the deliquescence RH (for initially dry AS) or contribute to additional aerosol liquid water (for initially wet AS). However, my main concern is that the authors only attributed the decrease in relative SOA yield (between dry and wet AS) with increasing OH exposures to the effects of aerosol liquid water, while the experiment design/data presented preclude the isolation

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of this effect. In particular, from the composition data presented, this trend could have also arisen due to enhanced contribution of later generation products from the gas-phase oxidation of toluene to the final SOA formed. These later generation products, which the authors mentioned, are generally of smaller molecular weights, would certainly result in less SOA mass, for the same number of toluene molecules reacted. Additionally, the heterogeneous OH oxidation of the formed SOA is important at higher OH exposures, and could have lead to enhanced loss of SOA mass due to fragmentation for wet AS due to the effects of liquid water on viscosity. A greater discussion is warranted; can other possible explanations for the observed trend in relative SOA yield be ruled out?

Also, the authors only noted that the photo-oxidation of toluene was conducted under low/no NO<sub>x</sub> conditions in Figure 1 - this is a critical point that needs to be highlighted (especially in the abstract) and warrant a discussion.

Specific comments:

Line 94: The experiment approach is similar (if not identical) to that used in Wong et al. (2015) and Faust et al. (2017) and should be referenced, especially since the introduction of the paper highlights the limitations of these previous studies as motivation of the current work.

Line 114-118: Are the surface area distributions of the "initially" dry and wet AS seed particles? Also, are the total seed particle surface areas significantly high enough that homogeneous nucleation was suppressed?

Line 138: The extent to which the toluene + OH reaction perturbed the OH exposures may be estimated, given that the concentration of toluene added is known.

Line 152: What is the final mixing ratio of toluene in the oxidation flow reactor?

Line 154-155: Were the experiments at different OH exposures conducted separated or in a step-wise manner (e.g. increasing the OH exposures throughout an experiment)?

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Line 166-168: Given that fragmentation reactions can lead to the formation of CO<sub>2</sub> gas, the filter correction factor should change throughout the experiments.

Line 171-172: It is not clear how the results from Matthews et al. (2008) is comparable to that of the current study.

Line 231-232: It is not clear what is driving the uncertainties in the calculated SOA yields.

Line 246-248: This statement assumes that the wall loss of dry and wet AS particles are identical - is this true?

Lines 257-259: Given that the suppression in the DRH of AS is dependent on the fraction of organics, for the current study, what was the fraction(s) of organics as a function of OH exposure?

Lines 321-322: Do the standard deviations reflect the variability from multiple experiments?

Technical corrections:

Line 81: It is not clear to whom "their" is referring to in this sentence.

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