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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud droplet activation of mixed organic-sulfate particles produced by the photooxidation of isoprene" *by* S. M. King et al.

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The authors thank the reviewer for the helpful comments, which are pasted in the following text and preceded by the symbol **. Author responses to each comment are preceded by the symbol ».

**General comments The authors present a laboratory study of CCN activation of SOA generated from isoprene. The isoprene SOA was initiated by OH reaction and in the presence of different NOx levels and ammonium sulfate seed particulates. Kohler theory effectively models the cloud activation of the mixed organic/inorganic particles, and no significant change in the CCN activation was observed as the VOC:NOx ratio was changed. Furthermore, a thermodenuder was used to volatilize a portion of the SOA, and the residual organic volume as a function of temperature did not change for the

range of VOC:NOx ratios. This implies the chemical composition of SOA is similar at a range a VOC:NOx ratios. Lastly, the authors show that the cloud properties of isoprene/NOx/ammonium SOA is consistent with the cloud properties of aerosol observed during the AMAZE-08 study.

Specific Comments The authors were careful to note the relevant references that address the effect of residence time on the measured volatility of SOA, and they found no difference in the volatilization of isoprene/NOx SOA when measured at 0.7 and 1.8 s. An et al. showed that volatilization of alpha-pinene/ozone SOA was nearly complete (98%)when the residence time was increased to 10 s, indicating that volatilization is kinetically limited (at least for alpha-pinene SOA). If this finding applies to isoprene/NOx SOA, the organic fraction that remains on the ammonium sulfate core after the thermodenuder treatment represents the kinetically slower material to evaporate. How is the CCN activity of this material relevant to atmospheric particles, where kinetics are not limited?

»We thank the reviewer for this good question. The question in fact goes to the heart of some discussions that have recently appeared in the literature and some additional ones that will soon be submitted for publication to the literature on the behavior of ambient and synthetic organic materials inside thermodenuders of varying geometry and operating parameters. For our study, we should first state that we studied a subset of organic material (i.e., compared to the range occurring in the atmosphere), and this subset of organic material was composed of the products of isoprene photooxidation that condensed into the particle phase for the conditions of our study. Our main goal was to provide an organic material that would have some common characteristics with organic material of the Amazon Basin. For the secondary organic material of our study, the control studies conducted by us for 0.7 and 1.8 s showed no difference in results. We think that a 100% difference in residence time represents a sufficient and reasonable control test in a laboratory setting. The prediction is then that a 10 s residence time within the thermodenuder we employed and for the secondary organic material

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we studied would not alter our observations within an experimental uncertainty. We do not want to speculate beyond what our experimental results have shown.

**Technical Comments Table 1: I recommend that "<MDL" in column 4 is replaced with the value for the instrument's minimum detection limit. This enables the calculation of the lower limit of the VOC/NOx ratio, which may be helpful to future authors for comparison purposes.

»The suggested change has been made. "MDL" is replaced with "0.4", and the explanation in the caption is modified accordingly.

**Table 1: The last footnote indicates that ozone concentrations are an upper limit due to H2O2 interference. I recommend that an estimate the contribution of H2O2 to the systematic error of the ozone concentration is included (a range, even), by estimating the H2O2 concentration after photolysis, and using the absorption cross sections for H2O2 and ozone (see, for example, "JPL publication 06-2 "Chemical Kinetics and Photochemical Data for Use in Stratospheric Modeling").

»The reviewer's recommendation is well understood, and we did in the course of the study try to do this kind of analysis. However, several complications hindered our efforts in calculating steady-state H2O2 concentration after photolysis. For example, there was an additional UV lamp through which the H2O2 flow is passed before entering the Teflon bag. Additional loss pathways for H2O2 aside from photolysis to OH were also probable. So, although we share in the spirit of the reviewer's recommendation, the calculation could not be carried out accurately by us. Nevertheless, because we were aware of this limitation when interpreting the data, the absence of this information does not affect the conclusions stated in the manuscript (it did limit us from being able to make statements about other conclusions, and those omitted conclusions do not appear in the submitted manuscript).

**Figure caption S2: Please indicate the symbols for 25, 60, and 100 oC.

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»These symbols are indicated in the figure legend (in panel 1). For further clarity, “room temperature” is now replaced by “25°C”.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 213, 2010.