

Interactive comment on “High formation of secondary organic aerosol from the photo-oxidation of toluene” by L. Hildebrandt et al.

Anonymous Referee #2

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The authors present new experimental studies addressing the ability of toluene to form secondary organic aerosols. Whereas this chemical system has been studied in the past in many previous studies, they produced a large range of predicted SOA yields whose application in models introduced considerable uncertainty. The present study identifies three main factors that might have caused this large scatter in previously-reported results, namely differences in UV intensity, the analytical method that is used to quantify formed organic mass (SMPS vs AMS) and the deposition loss of organic vapors to the walls. The paper is clearly written and certainly falls in the scope of Atmospheric Chemistry and Physics and I recommend publication after the consideration of my comments below.

p. 695, l. 20 ff. and also in the discussion of your experiments: It is not the absolute

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NO_x level but the ratio of RO₂/NO that determines the branching ratio in the gas phase. Could you report this ratio - or in a first approximation the ratio of toluene/NO - that has been used in previous studies and in comparison to your experimental conditions?

p. 696, l. 26/27: This sentence does not seem correct as it is since recent work has shown that organic aerosol mass does increase with increasing relative humidity and/or liquid water content (e.g., Volkamer et al., ACPD, 2007; Hennigan et al., GRL, 2007). To be more correct you should rephrase saying 'Aerosol formation by semivolatile compounds seem to be dependent'.

p. 698, l. 23: What structure can you ascribe to $m/z = 39$?

Experimental section: Have all experiments been carried out at dry conditions? If not, at what RH?

p. 707, l. 15: Is the time scale of ageing the same under atmospheric conditions or is it slowed down due to smaller oxidant concentrations? How important is the 'several generation' ageing under atmospheric conditions?

p. 708, l. 15; l. 21: The term 'internally mixed particle' seems misleading since it might imply that organics and sulfate are homogeneously mixed whereas a coating suggests rather a heterogeneously mixed particle. Clarify the use of 'internal mixing'; here.

p. 711, l. 25: (i) What are the OH levels in the chambers? Are they comparable to atmospheric levels? (ii) In the atmosphere there are many more sinks for OH than just toluene. Thus, does the photolysis rate of 0.18 min⁻¹ result in the same OH levels as in the atmosphere?

p. 712, l. 27 and Fig. 7: It looks like a very steep increase of the %44 signal in the first few minutes. (The first point seems to be lower than the following ones). If this is real, could you comment on it and quantify the time scale of that step increase?

p. 713, l. 27: How do your measured %44 percentages compare to ambient aerosol

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samples?

p. 714, l. 12: What are the reasons for the changing product distribution? Is it because reaction rates are enhanced at higher temperatures that convert species into products with different %44 content or is it rather evaporation of 'high %44' species and leaving behind 'low %44' species?

p. 715, l. 10: The fact that in your system all NO is quickly converted into NO₂ does not resemble atmospheric conditions where NO can be a significant fraction of NO_x. Thus, it seems to me that under those conditions the yields reported by Ng et al. are ore appropriate.

Conclusions: It would be very useful if you could add in the conclusions some quantification on how much each of the three factors (UV light conditions, AMS vs. SMPS, and wall losses) has changed the yield as compared to previous results. In addition, some error estimate would be useful as well.

Technical comments l. 717, l. 9: 'Worsnop' misspelled.

l. 717, l. 12 : 'measurement' misspelled.

l. 718, l. 10: 'secondary' misspelled.

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