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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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General Comments

This paper describes results from a series of simulation chamber experiments to investigate secondary organic aerosol (SOA) formation from the photooxidation of toluene. Thirteen experiments have been performed under a variety of conditions to study the effects of temperature and NOx concentration on SOA yield and composition, as determined by measurements using an aerosol mass spectrometer. The authors also report a new protocol for the correction of SOA yields due to the deposition of organic compounds to the walls of the chamber.

The article is well written and the results are presented in a clear and logical manner. The experimental data are of high quality and the interpretation and discussion of the

results is generally appropriate. Overall, this is a very good paper which highlights the range of experimental conditions and parameters (e.g. wall loss) that need to be characterized in order to obtain accurate SOA yield data.

I recommend publication following revision of the manuscript in line with the following comments and suggestions.

Specific Comments

1. Page 696: A very recent study by Cao and Jang (2008) provides some new data on the effects of acidity on the yield of SOA from the photooxidation of toluene. It would be appropriate to incorporate these results into the introduction.

2. Pages 697-698: There are a number of important details missing from the experimental section. Was hydrogen peroxide used in all experiments, including those with high NOx levels? How much hydrogen peroxide was added to the chamber? What was the mass concentration of seed aerosol in the experiments? If some of these parameters varied from experiment to experiment, then it may be more appropriate to include them in Table 1.

3. Page 701, line 16: It is assumed that the wall deposition constant of the organic material is the same as that of the inorganic material. Could an experiment be performed to confirm this? For example, the deposition of SOA produced via nucleation in the absence of seed aerosol could be compared against the deposition rate of the pure seed.

4. Page 711, lines 18-28: The authors compare experiments 1 and 2 to show that a higher SOA yield was obtained with higher UV intensity. This effect has also recently been reported for the photooxidation of m-xylene, and has been attributed to higher OH levels which cause a faster decay of the parent hydrocarbon and more rapid SOA formation (Warren et al., 2008). It is therefore somewhat surprising that the authors observed no significant difference in the decay rate of toluene between experiments 1

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and 2. However, closer inspection of the data reported in Table 1 shows that the initial conditions are in fact quite different and one may expect that the OH levels and thus the toluene photooxidation rate may be quite different anyway. Thus it is possible that any difference in the toluene decay rate attributed to the change in UV light intensity may be offset by the change in decay rate due to different levels of toluene and NOx. In my opinion, it is not entirely appropriate that these two experiments be compared directly compared in this manner. Regardless, it is important that the authors address and clarify this matter in the revised manuscript.

5. Page 712, lines 13-22: As reported in other studies, higher SOA yields are obtained under low NOx conditions. However, the recent work of Cao and Jang (2008) showed that, under high NOx conditions, the relative starting concentrations of NO and NO2 have a profound effect on yield, with significantly higher yields obtained when the initial concentration of NO2 was higher than NO. It would therefore be instructive to report initial NO and NO2 concentrations in Table 1 for comparative purposes.

6. Page 715, lines 9-13: It is reported that most of the NO is quickly converted to NO2 within a few minutes of turning the lights on. This is somewhat unusual in high NOx experiments, where the decay of NO generally takes the order of tens of minutes. What is the reason for the very rapid conversion? Is hydrogen peroxide also present in the high NOx system? This produces lots of HO2 which may accelerate the conversion of NO to NO2.

Minor Comments

1. Page 696, lines 1-7: Song et al., (2007) report that, contrary to expectation, propene does not enhance OH levels in chamber studies - it reduces them. This part of the text should be changed accordingly. In addition, "Takekawa" is spelt incorrectly in line 5.

2. Page 697, line 11: End sentence after "oxidation".

3. Page 699, line 19: The company name is "Vaisala".

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4. Page 711, line 7: Remove second "not".

5. Page 717, line 32: The correct representation for ammonium sulfate should be (NH4)2SO4.

6. Page 718, line 4: First author is "de Gouw".

7. Page 718, line 17: Change "nox" to "NOx"

8. Page 719, line 13: Remove capital letters in the title of this paper.

9. Page 727, figure 4: Change "communication" to "interaction" in the figure caption.

10. Page 729, figure 6: Change "communication" to "interaction" in the figure caption.

References

Cao, G. and Jang, M.: Secondary organic aerosol formation from toluene photooxidation under various NOx conditions and particle acidity, Atmos. Chem. Phys. Discuss., 8, 14467-14495, 2008.

Song, C., Na, K., Warren, B., Malloy, Q., and Cocker, D.R.: Impact of propene on secondary organic aerosol formation from m-xylene, Environ. Sci. Technol., 41, 6990-6995, 2007.

Warren, B., Song, C., and Cocker, D.R.: Light intensity and light source influence on secondary organic aerosol for the m-xylene/NOx photooxidation system, Environ. Sci. Technol., 42, 5461-5466, 2008.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 693, 2009.