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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ozonolysis of α-pinene: parameterization of secondary organic aerosol mass fraction" *by* R. K. Pathak et al.

Anonymous Referee #1

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Summary

In this manuscript, Pathak et al. present parameterizations of SOA formation from the ozonolysis of a-pinene under five broad sets of conditions. Several sets of data were fit using the Donahue-group basis set approach to obtain these updated parameterizations. Their results indicate that the updated parameterizations predict SOA levels that differ from those obtained using parameterizations previously reported in the literature.

The authors make interesting observations and the new parameterizations are relevant to state-of-the-art chemistry models. Their results are particularly interesting in the context of current efforts to reconcile models and observations of SOA formation in the troposphere (Volkamer, et al., 2006; Heald, et al., 2005; Takegawa, et al., 2006;

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deGouw, et al., 2005). The paper is well written and the main points are clear. However, I think the paper could be improved in two areas.

Were all the available yield data used in determining the parameterizations? In the introduction, the authors mention yields from Jang and Kamens and Fick et al., however, these data are not listed in the Table 1. If the work of these authors or others has been excluded for a specific reason, it should be mentioned. The data of Lee et al., Ng et al, Winterhalter et al., and Yu et al. are listed in Table 1, but are not included in Figure 2. If they were included in the fit, they should be included in Figure 2. Are there yield data from any of the other chambers (PSI, EPA, UNC, Riverside, Leipzig, Mainz, etc.) that were not included?

The ideas in the paper are very interesting, but could be better developed. Many of the figures are only discussed in passing. For example, comparison of Figures 6, 7, and 8, makes an intriguing and very relevant point which may not be immediately obvious to all readers. I encourage the authors to develop their line of thinking regarding these figures more in the text.

General Minor Comments:

1. Readers of this paper would be interested in a brief discussion of the current attempts to achieve closure between modeled and measured organic carbon in the atmosphere, especially considering that the primary goal of the paper is to provide updated parameters for use in the models (Volkamer, et al., 2006; Heald, et al., 2005; Takegawa, et al., 2006; deGouw, et al., 2005).

2. The authors should define the term aerosol mass fraction. Is AMF equivalent to "yield" described in the older literature (Odum, et al., 1996)? A brief comment on the relationship between AMF and yield early in the paper will clarify the message of the paper for many readers.

3. The authors should be more explicit in defining high NOx, low NOx, high RH, and low

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RH. When the authors do define these terms, it is unclear whether they are referring to the conditions of one particular data set (eg. p. 1945, line 20) or the entire category. These definitions would fit well on p.1946, lines 6-10.

4. I suggest removing Figure 4 from the manuscript. The conclusion that (p1952, line 9) “the errors are on the order of 15-20%...” isn’t obvious from that figure. A plot of the fit residuals in percent units would be better, but is not necessary.

5. Page 1946, lines 19-21. The authors should check their calculation. If no additional water vapor was added during the experiment, the RH at 40 C is 15% not 25%. What category were the high NOx experiments at the elevated temperature, but lower RH (25 %) placed into?

Technical Corrections and Specific Comments:

6. Page 1944, line 4, replace one “and” with a comma.

7. Page 1944, define the following acronyms: CMU/STI, AER, AEC.

8. Page1944, line15 delete parentheses around SOA.

9. Page 1945, line 2. Specifying the saturation vapor pressure in pressure units in addition to density units would help many readers.

10. Reference the Figures in order of discussion. Figure 3 is referenced in the text before Figure 2.

11. The axis fonts of Figures 1 and 7 look different from the axis fonts of the other figures in the paper.

12. Please list the experimental conditions for Figure 2.

13. Yu et al is cited in Table 1, but is not present in the works cited.

14. Saathoff is cited as an abstract from a conference in 2004 and should be updated.

15. Winterhalter et al. is cited as a discussion paper from 2003 and should be updated.

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16. The Giffin et al. reference reads “J. Geophys. Res. Lett.”

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1941, 2007.