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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.

R. K. Pathak et al.

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(1) 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?

We did our best to include all the data that were available in the literature for this system. Studies that were excluded had either incomplete published datasets for use in the parameterization (Fick et al., 2003; Jang and Kamens , 1998) or were at conditions

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different from those relevant to our study (e.g., high concentrations of acidic seed particles) (Iinuma et al., Atmos. Environ., 38, 761-773, 2004; Czoschke and Jang, Atmos. Environ., 40, 4370-4380). This point is now explained in the revised paper.

(2) 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.

We have added several sentences discussing the results shown in Figures 6-8 focusing on the smaller variability of the yields in the presence of organic aerosol seed, the lack of a threshold for formation of SOA when organic aerosol is present, etc.

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).

A brief discussion of these efforts plus a few more recent 3D Chemical Transport Modeling studies has been added in the introduction of the paper.

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.

We believe that the Aerosol Mass Fraction (AMF) is a better term than the yield and we are using throughout the paper. The point of the reviewer is well taken and a definition of the AMF and the normalized AMF has been added together with brief comment on

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its relationship to the “yield”.

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

Definitions of these regimes have been added in Section 2 of the revised paper.

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.

This figure is useful because it shows the goodness of the fit of the parameterization compared to all the available data and also shows the small sensitivity of the fit to the use of 4 or 7 products. These points are now stressed in the text. With these changes in the paper, we think that this figure justifies the space that it occupies and we would prefer to keep it.

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 percent not 25 percent. What category were the high NOx experiments at the elevated temperature, but lower RH (25 percent) placed into?

This is a good point. The RH was measured during the experiment. The discrepancy was due to the addition of water vapor during the experiment. This is now clarified in the paper.

Technical Corrections and Specific Comments:

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

Done.

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

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The definitions have been added.

8. Page 1944, line 15 delete parentheses around SOA.

Done.

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

We have added in parenthesis the corresponding mole fractions at 298 K.

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

The missing reference to Figure 2 has been added (before the reference to Figure 3).

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

We have changed the fonts in these two figures.

12. Please list the experimental conditions for Figure 2.

The corresponding conditions have been added to the figure caption. Please note that these are also shown in Table 1.

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

The reference has been added.

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

For reasons that are not clear to us these interesting results have not been published elsewhere. The corresponding conference proceedings are widely available through Elsevier.

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

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This publication to the best of our knowledge exists only as a discussion paper.

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

We have corrected the typo.

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