

Interactive comment on “Introducing the concept of Potential Aerosol Mass (PAM)” by E. Kang et al.

Anonymous Referee #3

Received and published: 10 August 2007

The previous referees have nicely summarized the goals of this work. This new concept eventually should be published. The current paper suffers from trying to include too much information, while glossing over several important points that must be addressed for this concept to be truly useful. Also, the present version of the paper is not well written; it is, in places, poorly organized and rambling. Section 3.4 is not really needed, and Section 3.5 is premature. The performance of the technique needs to be firmly established in the laboratory before it can be applied to ambient data. I suggest that these two sections and their associated table and figure be removed. (Figure 3 is also not necessary.) The following points need to be addressed:

1. What is the effect on the chemistry of the 185 nm and 254 nm radiation? Photodegradation of VOC reaction products may occur at these wavelengths while not at those >300 nm. The authors do establish that this radiation is not affecting α -pinene chemistry; this may not be the case for other systems in which products are more

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

photolabile.

2. What is the sources of OH and HO₂ under dry conditions? Wouldn't one expect virtually no production of these radicals at RH less than 1%? This led to problems in the results in Section 3.2.5.

3. How does one know that SOA formation is really complete given the short residence time? SOA formation can occur as a result of second-generation reactions. Do these have enough time in the tube?

4. In judging the NO_x regime, the important ratio is that of HO_x to NO (page 9946). The authors need to carefully compare the conditions in their system with those in large chambers.

5. The lack of agreement of the aromatic SOA yields with those of Ng et al. (2007) is troubling. The presence of seed aerosol cannot explain this discrepancy. Are the NO_x conditions really the same? (See comment above)

6. In a-pinene + O₃, why is it necessary to add up to 12 ppm of O₃ to achieve maximum yield? Do we really understand the mechanism occurring in the system?

7. Section 3.2.3. Why is the a-pinene SOA yield 5 times higher in light vs. dark. Known OH yields do not seem to be able to explain this.

In summary, this is an intriguing new idea, but the work, as presented here, is premature. The authors have tried to do too much, yet have left fundamental issues in atmospheric chemistry unanswered.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 9925, 2007.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper