

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

Anonymous Referee #5

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General comments

Overall, this is a clearly written manuscript describing a new concept and measurement focused on secondary organic aerosol. The PAM measurement seems to be a corollary to the total reactivity with respect to OH when discussing VOCs. This is clearly a topic appropriate to ACP(D). For the most part, it cites references appropriately. The length, figures, and abstract are all acceptable. However, there are several issues, the most important of which is omission of chemical characterization data, that prevent me from recommending publication of this manuscript in ACP at this time.

Specific comments

Page 9926, last line. It is inappropriate to use Seinfeld and Pandis as a blanket citation for such things. The papers showing things like the health effects of aerosols should be cited directly.

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Page 9934, the uncertainty of the TEOM is quite large compared to the scale of the measurements being made. Should a different measurement for aerosol mass be used? Or has one?

Page 9934, why was an additional port not incorporated into the instrument to allow for measurement of OH and HO₂ during the actual experiments. Now, these values must be assumed based on a limited subset of previous experiments.

Page 9934, with respect to Figure 2. What is going on with the points at approximately 35% RH? They all seem to look like fliers from the trends of the rest of the data as a function of RH. Also, the text states that the O₃ level could be reach up to 30 ppmv, yet the figure y-axis exceeds this value. Does the unexpected behavior at the 35% RH point mean that they can not say with any certainty what the oxidant level in the PAM chamber is?

Page 9935, the values used as typical seem somewhat large. Does this affect their comparison to the PAM chamber values?

Page 9937, I find Figure 3 unnecessary.

Page 9938, is it really true that other SOA precursors react more rapidly with OH than alpha-pinene? I thought that aromatics reacted more slowly.

Page 9939, what is going on here with the results showing an initial maximum followed by a lower plateau in the aerosol mass concentration? This has not been observed with these precursors in the literature. Because this paper is focused on the PAM measurement and its initial application, this observation needs to be explained to a much greater degree. The authors speculate that reactions of SVOC mass in the gas-phase cause repartitioning. What if the oxidation of these SVOCs leads to less volatile products? Then their explanation is clearly incorrect.

Page 9940, their finding that continued reaction between ozone and alpha-pinene reaction products does not occur is not surprising given that the parent VOC only has one

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internal double bond, which should be oxidized completely in the first reaction. This is discussed in Ng et al. with respect to first- vs. second-generation products contributing to SOA.

Page 9941, the authors should use a better source for reaction rate coefficients than the MCM website (see earlier comment about use of Seinfeld and Pandis as a reference). In addition, the authors do not know a priori that the increase of yield associated with OH is due unequivocally to alpha-pinene/OH reactions. Could it not also be that OH oxidizes the reaction products of alpha-pinene/O₃?

Page 9942, I would caution the authors to say that RH does not affect SOA formation from the ozonolysis of alpha-pinene. It does not appear to affect it in this case because of the lack of a seed aerosol (see Cocker et al. papers from 2001).

Page 9943, is there a difference in wavelengths between the PAM and the Presto studies that could be leading to these discrepancies?

Page 9947, the seed used by Ng et al. was neutral, so there should be no seed effect as hypothesized by the authors.

Pages 9948 and 9949, the 'room air addition studies' do not provide much useful information to this work.

Sections 3.4 and 3.5 should be removed from this paper. All section 3.4 does is verify the hypothesis of Odum et al. regarding how one can calculate SOA yields of mixtures from the data from individual VOC experiments. Section 3.5 describes the first field deployment. I believe that the first PAM paper should focus on the concept and laboratory study/validation. Future papers should focus on field use.

Page 9954, the authors, in my opinion, absolutely must present the chemical characterization data of laboratory-generated aerosol in this paper. Because this paper is the first to describe PAM, the instrument needs to be validated completely, including chemically. This is by far the biggest weakness of the manuscript and needs to be

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addressed prior to publication on ACP. With the deletion of sections 3.4 and 3.5, the authors can add this material without making an overly long paper.

Page 9954, the end of the conclusion seems like a sales pitch and can probably be reduced.

In Figure 11, yield fitted curves should go through the origin.

Technical corrections

The caption on Figure 4 refers to measured and expected sulfate aerosol, while the data show measured sulfate and SO₂.

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

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