

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

Anonymous Referee #2

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This approach is truly very innovative and might help to elucidate the SOA formation in new ways. In some sense it is similar to the POCP (photochemical ozone creation potential) rather than the total OH reactivity as pointed out by one of the other reviewers. I think that the paper is long enough and includes a thorough discussion. I hope that the chemical characterisation using aerosol mass spectrometers will follow soon to get a more complete picture of the potential of this technique. If there are some measurements available, the authors might like to comment on the results already in this manuscript but refer to a future publication showing this in more detail. I think that this is a very interesting contribution and surely should be published. I would like to see a more detailed discussion on the main points below:

Main points:

- p.9929 “ \dot{E} should occur as they do in the atmosphere” This should be stated a bit

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more carefully. The night-time chemistry including NO₃ oxidation will not be modelled by the chamber. Another issue are possible reactions in the condensed phase that might take a bit longer than the PAM chamber can simulate. Oligomerisation reactions have shown to be important (Kalberer et al., 2004, Science, 303, 1659-1662). The time evolution of this oligomerisation process is detailed in Gross et al. (2006, Anal. Chem., 78, 2130-2137). In this context the use of both commercially available aerosol mass spectrometers should be used for the chemical characterisation of the PAM chamber in future.

- P9930 As I understand, the air including the particles is exposed to the lights emitting at 185nm and 254 nm. I am wondering if the processing concerning photolysis of e.g. aldehydes can be considered to reflect what is happening in the atmosphere. How much processing is happening due to photolysis compared to OH and ozone chemistry if one thinks for example about aldehydes in both the gas and aerosol phase.

- p. 9938: small wall loss of SO₂: SO₂ is not a very sticky component. If I think about some oxygenated VOCs or the VOCs of intermediate volatility as suggested by Robinson et al. (2007, Science, 315, 1259-1262) the losses might really be important. I suggest that the authors might mention that future experiments comparing heated with unheated inlets might be beneficial.

Minor points:

- p. 9927 Concerning high fraction of SOA, I would include the paper of Lanz et al. (Atmos. Chem. Phys., 7, 1503-1522, 2007)

- p. 9931: NO_x : Fraction of NO and NO₂?!

- p. 9934 comparison of TEOM and aerosol mass spectrometer. What was the assumed collection efficiency (with rather high uncertainty) to provide the comparison of 10%?

- p.9934 I would not state that ozone is independent of RH. The dependence is rather

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significant.

- p. 9935: Main loss of OH is the reaction by HO₂: Is this also true for really high NO₂ or VOC concentrations

- p. 9945 NO_x was added Ě : please specify in which form (NO or NO₂ or both (in which percentage)?)

- p. 9946 [VOC]/[NO_x] (ppbC/ppb) < 15 : One should be careful with such interpretation of this ratio regardin NO_x or VOC sensitivity of the ozone production as this is strongly dependent on the VOCĚ please add a sentence..

- p 9949 Volkhamer should read Volkamer. I suggest to include the paper of Robinson et al. in this discussion. The higher yield might not be due to mixture of very volatile VOCs but by these intermediate volatility organic compounds.

- in general (maybe it was mentioned ..): What material was the inlet made of. There should be a trade-off of maybe loosing particles versus loosing sticky gases.

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

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