

Interactive comment on “Photodegradation of secondary organic aerosol generated from limonene oxidation by ozone studied with chemical ionization mass spectrometry” by X. Pan et al.

Anonymous Referee #3

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The authors present a study of the photodegradation of limonene SOA, along the lines of what they had done previously in Mang et al (2008) and Walser et al (2007). The unique aspect of this manuscript is the use of a chemical ionization mass spectrometer to characterize the volatile products, and some lower ozone and limonene concentrations were used. The paper is well-written and should be published in APCD after the following comments and questions are addressed:

1) The authors state that the samples are stored under flowing dry nitrogen, and the photodegradation experiments are also carried out under dry nitrogen. This practice

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must cause the evaporation of all but the least volatile SOA products from the aerosol sample. I presume that the experimental motivation for doing this is to keep the background signal from semi-volatile material low so as not to interfere with the signal from the volatile degradation products. But the authors should discuss how this affects the atmospheric implications of their results.

2) The authors should discuss how the intensity of their light source compares with the actinic flux that a typical aerosol particle will encounter in the atmosphere (this is related to point (3) raised by Anonymous Referee #1) .

3) Do the authors have a sense of the rate of mass loss caused by photodegradation and thus how photodegradation will change the size of the aerosol over its lifetime?

4) How do the mass transfer limitations observed in your experimental setup (p.4738, line 18-19) compare to what would happen for an atmospheric aerosol particle?

5) The authors should do a more thorough job of connecting the discussion of the mechanism on pp. 4743-4744 to the observed mass spectra

Detailed comments:

p. 4731 line 4: Please provide more details regarding the Teflon FEP bags, in particular, what is their volume?

p. 4732 line 5: “300 pm” should be “300 ppm”

p. 4737 line 4-5: “. . . only oxygenated species. . . should be detectable by this instrument.” I believe this statement is incorrect. See, for example, the review of PTRMS by Blake et al (2009).

p.4741 line 6: “a very satisfying cross-validation result” please remove this commentary or use more technical language

p.4742 line 20: please remove the word “kinetically”

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REFERENCES

Blake et al., Chem. Rev. 109(3), 861-896, 2009

Mang et al., J. Phys. Chem. A 112 8337-8344, 2008

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4727, 2009.