

Interactive comment on “A new technique for the selective measurement of atmospheric peroxy radical concentrations of HO₂ and RO₂ using denuding method” by K. Miyazaki et al.

Anonymous Referee #1

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General comments

This material in this paper is within the scope of AMT. It reports on a laboratory study to quantify the reported difference in heterogeneous radical loss rates for HO₂ and RO₂ radicals and use it to design a radical amplifier to separate the signals due to these radicals. My concern is that this is a laboratory study and that no ambient measurements are reported. This would have been a huge addition to the paper.

Specific comments

Page 3294 line 15. There is no evidence that the photolysis of acetone at 185 nm only
C1080

produces only methyl radicals. This path is less than 10% at 248nm (Rajakumar et al. 2008 J Photochem. Photobiol. A 199 336). Also here is no indication if the lamp is filtered, in which case photolysis of acetone can occur at 254 nm where the absorption cross section is much higher than at 185 nm. If acetyl and methyl radicals are both produced then the interpretation of the results is not so clear.

Page 3300 Eq 8–9. Figure 4 shows that α and β are functions of concentration. The authors should indicate how to use these equations to determine radical concentrations if these parameters are not constant.

Technical corrections

Page 3293 line 9. I think a reference to the original Cantrell and Stedman work should still be included.

Page 3294 line 19. In reality there is no blank “cell”.

Page 3295. Do not mix metric and imperial units

Page 3296 line 6. The laser is different than that in Fig 1

Page 3297 Eq 2. The use of I_{dec} is confusing. Since I_{blank} refers to the blank path, then it should be called $I_{removal}$ to be consistent.

Page 3297 line 5. Remove (I_{HO_2})

Page 3300 Eq6. The casual reader would not realize that C_{humid} is independent of the radical measured. This should be clarified.

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 3291, 2009.