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***Interactive comment on “The oleic acid-ozone heterogeneous reaction system: products, kinetics, secondary chemistry, and atmospheric implications of a model system – a review” by J. Zahardis and G. A. Petrucci***

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Zahardis and Petrucci provide an informative, timely, and important account of recent literature. As I am not an official reviewer, I will comment only on a few points I found particularly worth further discussion. Points 1 to 3 are of more interest than the points that follow afterwards. Point 1 may attract comments more broadly from other readers, too.

Point 1 — The atmospheric chemistry literature in recent years has suggested that

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azelaic and nonanoic acids can form by isomerization of their precursor Criegee intermediates. Based upon my recent reading, I believe this suggestion is an unfortunate error that has occurred as gas-phase chemists have moved into the area of liquid-phase chemistry. In the gas phase, the excited Criegee intermediate can indeed isomerize to form acids (e.g., final chapter of Bailey, vol. II). In the liquid phase, however, the excited Criegee intermediate stabilizes and does not isomerize. I find, for example, no reference to liquid-phase isomerization of Criegee intermediates to form carboxylic acid groups anywhere in Bailey, vol. I or II).

Zahardis and Petrucci seem to hint at this error on the top of page 11107 and the bottom of page 11108. They should be commended for this initial note in the discussion. However, I believe the review article should be more thorough in its presentation of this problem in recent literature, perhaps warranting a complete paragraph as a point of emphasis, including some citations to Bailey and any related, informative literature.

On pages 11108 and 11111, however, I think the suggestion that the stabilized Criegee intermediate has a long enough lifetime to be detected must be wrong. Detection of Criegee intermediates has been a long and difficult quest, so I doubt it is so easy. I believe the Criegee intermediates have very short lifetimes and would not be detected by mass spectrometry of aerosol particles.

Point 2 — Page 11149. Studies at elevated relative humidity must be a priority for the future. A comment noting that all but very few of the laboratory studies have been done at 0% RH is worthwhile.

Point 3 — Pages 11138-9 erroneously suggest, by comparison to a bimolecular gas-phase rate constant, that the gamma value of  $10^{-3}$  indicates a bimolecular rate constant corresponding to 1 reaction in 1000 collisions. Moise and Rudich (2002) and Morris et al. (2002) make the point by directly comparing the bimolecular rate constants of liquid and vapor phase reactions. Those treatments also indicate that multiple terms contribute to gamma (e.g., it would be possible to get the same gamma

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value by adjusting D and k in inverse proportion).

Point 4 — Although the authors must make a decision where to draw the line in terms of a review of current literature, a recent paper co-authored by me of Mochida et al. provides additional evidence on several points brought up by the authors and that would be of interest to readers (e.g., results of mixed myristic acid and oleic acid in the discussion of the mechanism of -COOH attack on page 11113 and again on page 11115).

Mochida, M, Y. Katrib, J.T. Jayne, D.R. Worsnop, and S.T. Martin, "The Relative Importance of Competing Pathways for the Formation of High-Molecular-Weight Peroxides in the Ozonolysis of Organic Aerosol Particles," *Atmospheric Chemistry and Physics*, 2006, 6, 4851.

Point 5 — Pages 11113 and 11115. In further support of the reaction of -COOH groups are the results of Katrib et al. showing that a large fraction of Criegee intermediates react with -COOH groups present. The authors might want to include a description of these results here.

Point 6 — Page 11113. The authors have introduced the abbreviations ACOAH and ALOAH. It might be useful for the community to stick with acronyms introduced in Ziemann (2005) and adopted by others of, for example, "AAHP". This would encourage commonality in language at an early stage of research in this field.

Point 7 — Section 4.4.2. I believe "not" is omitted as in "should initially NOT be compared".

Point 8 — Page 11147. Unlikely to be true: "Troposphere OA may be a SIGNIFICANT sink of ozone" given the relative mass loadings of OA and ozone and the regeneration of ozone.

I certainly benefited from Zahardis and Petrucci's article, and I hope that the above points can be used towards the end of strengthening an already excellent document.

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Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11093, 2006.

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