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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Density changes of aerosol particles as a result of chemical reaction" *by* Y. Katrib et al.

**Y. Katrib et al.**

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*** We appreciate the important feedback and interest shown for the submitted manuscript.

Reviewer #1 ———— General comments: This manuscript introduces an innovative laboratory measurement system for studying heterogeneous surface reactions on coated aerosol particles. As a demonstration of the performance of this system, it is further applied to investigate the density changes of particles covered by oleic acid as a result of their exposure to ozone. The manuscript is well written and sufficiently well structured, in addition to which it clearly fulfills the necessary scientific criteria.

In summary, I strongly recommend the publication of this paper in Atmospheric Chem-

istry and Physics. Only a couple of very minor comments for the authors to consider for further improvement of the paper is given below.

Specific comments: First, the authors should explain more explicitly why they have chosen oleic acid as the reaction surface. This may be clear for people investigating heterogeneous reactions but not necessarily for a broader scientific community. Especially, the atmospheric relevance of these reactions should be brought up in the text.

*** (1) Oleic acid is widely employed as a marker compound for meat smoke in source apportionment studies. In those studies, it is assumed to be an inert compound. Ozonolysis, however, degrades this compound, and the kinetics of this reaction are important for understanding associated uncertainties in source apportionment (Schauer, J. J.; Rogge, W. F.; Hildemann, L. M.; Mazurek, M. A.; Cass, G. R., Source apportionment of airborne particulate matter using organic compounds as tracers, Atmos. Environ., 1996, 30, 3837-3855.)

*** (2) In the last three years, oleic acid ozonolysis has emerged as a test system in heterogeneous laboratory kinetics. This system has been employed by over eight research groups.

*** Points (1) and (2) are not specifically added to the revised manuscript because we wish to emphasize our study in the context of the literature on aerosol particle density, rather than in the context of oleic acid ozonolysis.

Second, the work performed in the paper can be approached from two perspectives: 1) heterogeneous aerosol chemistry in the atmosphere, or 2) particle density and its changes as a result of atmospheric chemistry. The latter perspective has been raised as a general motivation for conducting this study (see text in “Introduction”), while the latter perspective has been discussed briefly in “Conclusions”. I would like to see the authors to build a little bit more concrete bridge between these two perspectives, as they clearly are linked to each other.



**ACPD**

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Interactive Comment

*** The reviewer has thoughtfully identified the flow and intentions of the manuscript. The reviewer has also rightfully identified the necessary vision in this field of research, which is to build mechanistic bridges from point #2 to point #1. In the current manuscript, however, we believe we have done our job, which is to report on our laboratory studies. We believe the full development of mechanistic bridges is more appropriate for a broader consideration of literature, as for example through a review article, book chapter, or integrative modeling study.

Reviewer #2 ————— This is a valuable paper, providing a thorough examination of how to relate aerodynamic particle diameter to mobility diameter, through a combination of aerosol mass spectrometry and differential mobility analysis techniques. In particular, particle density can be derived from such simultaneous measurements. The paper illustrates how these two diameters are not equivalent, and how one may increase and one decrease due to a chemical reaction, if the density of the particle changes. I have no significant comments on the content of the paper. The authors appear to have been careful in their experiments and analysis. However, I do suggest that, in some way, the approach could be put into a more "user friendly" manner. It took this reviewer considerable effort going through the paper and for it to be useful to a broader community, perhaps a short summary section (or Figure) that codifies the approach could be included.

*** We carefully reviewed the manuscript and tweaked aspects throughout so as to make the material more readily accessible to the reader.

Short Comment by P. DeCarlo ————————————- *** Please see original comment on website where equations are shown.

*** The essence of DeCarlo's comment #1 is that the original formulation of the manuscript assumes that the DMA is operated in the continuum regime instead of the transition regime. In fact, Pd = 10 inside the DMA, which corresponds to the transition regime. Therefore, we have revised the text and equations accordingly. (The numerical



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analysis is not affected because we have a shape factor of 1.0.)

*** Comment #2 is addressed by the changes made in the manuscript as a result of comment #1.

Page 6437 lines 23-25 seems to imply that the critical orifice is the location at which particles are imparted a size dependant velocity, inversely related to the aerodynamic diameter. Particles are actually imparted a size dependant velocity at the nozzle expansion at the end of the aerodynamic lens system.

*** The wording is clarified.

Page 6438, line 2. It is stated here that the chopper used has a duty cycle of 0.05%. This is an extremely small duty cycle compared to what is typically used in the AMS (we are not aware of the use choppers smaller than 0.5%) and would be very difficult to fabricate.

*** This typo is corrected. The duty cycle is 0.5%.

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