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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Alkene ozonolysis SOA: inferences of composition and droplet growth kinetics from Köhler theory analysis" *by* A. Asa-Awuku et al.

Anonymous Referee #3

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Review

The manuscript presents a combination of laboratory and modeling results of CCN properties of ozonolysis products from three different alkenes. Due to the highly complex composition that cannot be resolved by analytical techniques, approaches to infer cloud nucleating properties of secondary organic aerosol (SOA) is important. I have some concerns detailed below that mainly address the usefulness of defining molecular weight as a fitting parameter for inferring composition information while many more parameters (e.g, density, OM/OC, etc) are associated with errors. I strongly suggest a comparison to results from previous on similar topics [Petters and Kreidenweis, 2006;

Wex et al., 2007] in order to clarify the extent to which the data derived for the three specific alkene/ozone SOA systems fit into these more general frameworks. Such a comparison would add to the importance of the current study as it will show the extent to which SOA from individual systems agree with our current understanding and representation.

General comments

There are two major other studies that attempt a similar procedure, namely inferring CCN properties from growth characteristics, for similar organic species. These studies define 'kappa' or 'ionic density', respectively, in order to characterize CCN ability of SOA [Petters and Kreidenweis, 2006; Wex et al., 2007] and, thus, apply a more general approach as the assumption of individual properties, such as density, van't Hoff factor, etc is not necessary.

How does the surface tension that is listed in Table 5 change if you vary the input data you assume for (i) OM/OC ratio (note: the OM/OC ratio for the individual species in Table 1 are all greater than 2), (ii) density, and (iii) van't Hoff factor?

p. 8993, l. 10-19: What is concentration (e.g., in terms of water/solute mass) at the activation point? Can you reword the last sentence? It is not clear if you state that the products listed in Table 1 affect surface tension more than HULIS.

I think from your measurements you cannot exclude that HULIS are indeed formed because (i) the measurements of the surface tension you show in Fig. 4, is very similar to the results that have been determined in another study [Tuckermann and Cammenga, 2005]. In addition, the 'molar volumes' that are presented in Table 3 agree well with the data range that has been suggested for HULIS (1/4.85e-3 - 1/5.55e-3, [Wex et al., 2007]).

Minor comments

p. 8986, l. 21: Are the inorganic ions due to an insufficient deionization of the used

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water?

p. 8988, l. 23/25: Define what to you mean with ‘initial’ and ‘critical micelle concentration’ or give appropriate reference.

Technical comments

Check the whole manuscript, including figure legends and tables, for spelling of ‘1-methylcycloheptene’ (at several places, it is ‘1-methylcylcoheptene’)

Table 6, Footnote: Dissociation

Additional references

Petters, M. D., and S. M. Kreidenweis (2006), A single parameter representation of hygroscopic growth and cloud condensation nuclei, Atmos. Chem . Phys. Discuss., 6, 8435-8456.

Tuckermann, R., and H. K. Cammenga (2005), The surface tension of aqueous solutions of some atmospheric water-soluble organic compounds, Atmos. Environ., 38, 6135-6138.

Wex, H., T. Hennig, I. Salma, R. Ocskay, A. Kiselev, S. Henning, A. Massling, A. Wiedensohler, and F. Stratmann (2007), Hygroscopic growth and measured and modeled critical supersaturations of an atmospheric HULIS sample, Geophys. Res. Lett„ 34, doi: 10.102/2006GL028260.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 8983, 2007.

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