**Atmospheric
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Discussions**

Interactive
Comment

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

A. Asa-Awuku et al.

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We would like to thank the referee for the thorough comments; our responses follow.

General comments

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.

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Done. Comparisons are done however considering that the material analyzed here corresponds to the soluble fraction extracted from filters. That implies the soluble fraction in the analysis here is unity, which can be different from the parent SOA (e.g., Asa-Awuku et al., 2009; Englehart et al., 2008).

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.

Ionic density, kappa-Kohler theory and Kohler theory analysis (KTA) are all manifestations of the same theory – Kohler theory. Indeed the former two require fewer parameters, and thus are uniquely placed for parameterization of organic impacts in large scale models. KTA is *complementary* to the latter two methods, in that it aims to understand, to the extent allowed by theory, the potential sources of hygrsocopicity, (i.e., deconvoluting surface tension effects from average molar volume). KTA has been successful in numerous cases (now cited in the text), yielding average molar volumes and surface tension depression that are consistent with measurements. There are limitations to KTA, but we have been straightforward in stating them (for example, see Padró et al., 2007 and the response to reviewer 1).

With this said, all methods mentioned above are based on Kohler theory, and are subject to uncertainty whenever the latter is not a full description of CCN activity.

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?

We have included sensitivity studies to assess the variability in molar volume on the van't Hoff factor (and other parameters) and the molar mass to the density.

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.

It refers to the carbon concentration based on the amount of carbon in the dry aerosol, devided with the liquid water available at the critical wet diameter of the CCN, following the procedure of Padró et al., (2007) and Moore et al., (2008).

Last sentence is reworded.

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])

Excellent points. The text has been revised accordingly and cited references included.

Specific comments

Page 10310, line 15 Here the authors describe that both PM1 and PM2.5 were measured. Later on page 10312, it is mentioned that PM10, PM1.3, and PM1.3-10 for PTFE and PM1 and PM10 for quartz filter samples were analyzed. Are these different measurements?

The filters that were collected were: i) PM1 and PM10 Quartz filters, and, *ii*) PM1.3, PM1.3-10 and PM10 (their sum) PTFE filters. We apologize for this confusion; the manuscript is now corrected.

Minor comments

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

No. Ultrapure 18Mohm water was used. The amount of ions in the water was negligible, and included measurements of it in the text to show that. The text has been modified to make this clear.

p. 8986 L23: "Table 2 provides a summary of the offline WSOC chemical composition measurements and nominal anion concentrations (less than $2.55 \times 10\text{-}5$ mg L-1) in the extracted samples"

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

Done.

Technical comments

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

Corrected :P8993 L23, Table 2, Table 3, Figure 3 Caption, Figure 4, Figure 5.

Table 6, Footnote

Done.

Appropriate references were also added throughout the text.

References

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