

Interactive comment on “Establishing the Impact of Model Surfactants on Cloud Condensation Nuclei Activity of Sea Spray Aerosols” by Sara D. Forestieri et al.

Anonymous Referee #1

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The authors present a comprehensive study on the potential role of a subset of surfactant molecules in dictating aerosol activation behavior. This is an issue that waxes and wanes in the literature, with no consistent theoretical framework or set of standards for comparisons. I appreciate the brief summary of studies presented and I enjoyed digesting the results. In many occasions I found myself deleting a specific point to raise that was answered further in the document. Indeed, the small number of questions raised below reflects this. The authors have considered multiple angles and it is certainly worthy of publication in ACP. I would appreciate that, prior to publication, a number of general issues are clarified. For studies with relatively low number of compounds, theoretical results are more sensitive to variations in pure component and

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mixture properties. These issues might have arisen from a confused interpretation on my part, but are important to consider given the varying prescribed importance of this area of study.

Section 2.2 Kappa values are not constant with changing water content due to non-ideal mixing. A value of 1.33 assumes ideality, presumably, whilst Kappa values for Sodium Chloride, theoretically, stride from ~ 1.5 to 1.23 as RH increases from 90 to 99%RH. Does this affect the methodology for RH calibration? Would this change figure 5B

Equation 2, as used from the Petters and Kreidenweis 2007 paper, is referenced as being valid in that original source when $\kappa > 0.2$. However, in this study you presented derived Kappa values < 0.02 even for the oxidised test case. I would like to see presentation of why this assumption is not important.

More generally, I'm unsure if the role of activity coefficients in dilute solutions might somehow affect any re-partitioning, assumptions of ideal mixing in equation 4, or raise the potential for LLE in the systems studied here. Could the authors comment?

Section 2.5. Perhaps I've interpreted this section incorrectly, but how are variables used in the fitting process constrained?

Section 3.3 For such high mass fractions, how would any solubility limitations affect results from assuming an ideal kappa-mixing rule?

Section 3.4 Following the previous comments, for the oxidation experiments the authors note that all of the organic mass could be accounted for by three compounds [86+7+7]. All of these compounds still have low kappa values, the highest < 0.02 . Multiple studies rely on the potential for LLE to explain any variation in observed hygroscopicity. Given the often high mass fractions of total organic in these studies, I re-iterate if the authors have considered any contribution from a possible LLE, with unknown morphology, that might introduce errors associated with the approach presented here.

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Minor comments: Page 11, line 7: Please correct 'The surface tension is constrained by always be larger than some minimum value.'

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