

Interactive comment on “The role of surfactants in Köhler theory reconsidered” by R. Sorjamaa et al.

R. Sorjamaa et al.

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We would like to thank the Referee 2 for the comments.

General comments:

Comment 1: My primary issue is in the treatment of the Raoult term. It was not clear how the "Raoult" term is calculated. Is it from the bulk concentration of solute (after equilibration with the surface phase)? Or is it calculated from the surface phase concentrations? It seems that the latter should apply, since the surface phase is in equilibrium with the water vapor, and not the bulk. This should be made very clear, as it is the primary focus of this paper.

In equilibrium, all species have constant chemical potentials across all three phases (bulk, surface and vapor), i.e. both the bulk and the surface are in equilibrium with the vapor. The Raoult term describes the vapor pressure decrease of water due to dissolved species in the bulk of the droplet. Thus, we calculate the Raoult term from the bulk concentrations. See also our reply to Referee 3.

Comment 2: The authors lack comparisons with experimental data. Clearly the predicted changes in equilibrium supersaturation can be measured; the authors should therefore seek literature data on measured CCN critical supersaturation that would support their theoretical calculations.

Indeed, Rood and Williams (2001) have presented preliminary laboratory data on the activation of SDS-NaCl particles. We will add a comparison to the revised manuscript. Furthermore, we are currently collaborating with B. Svenningsson and M. Bilde, who have made activation experiments for SDS particles. The results will be added to the revised manuscript.

Concerning pinonic acid, the only experimental work (that we are aware of) (Hegg et al, 2001) used a large expansion chamber and was somewhat problematic in that the authors' cloud model results were very far from the experimental results even with pure ammonium sulfate. Meaningful comparison with our predictions is therefore virtually impossible.

Comment 3: A discussion on the implications for cloud microphysics should be supported with cloud droplet number calculations; a simple parcel model calculation on droplet number change (with and without considering the surface phase) would suffice.

Unfortunately, it is not quite as straightforward to include our equations in a box model as one would think. This is because to solve for the surface-volume partitioning, we have to iterate for several equations simultaneously, and making this efficient in our box model will require some work. We therefore would prefer to leave the possible implications of the current theory as hypotheses to be tested later.

Specific comments:

Comment 1: Please provide a reference for equation 6.

The equation has been given by Laaksonen et al. (1998). We will add the reference to the revised manuscript.

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Comment 2: The authors should discuss the possibility of the organics not completely dissociating (as opposed to what happens to SDS). Would the "buffering" capacity of partial dissociation affect partitioning?

A change from complete to partial dissociation does not have a significant effect on surfactant partitioning (thus not on critical supersaturation either) as far as SDS is concerned. It does, of course, affect the number of bulk moles, but the change in critical supersaturation for example for pure SDS particle (with dry radius of 25 nm) is -0.003

References:

Hegg D.A. et al. (2001), Atmos. Res. 58:155

Laaksonen A. et al. (1998), J. Atmos. Sci. 55:853

Rood M.J and Williams A.L. (2001), J. Atmos. Sci. 58:1468

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