

Interactive comment on “A simple representation of surface active organic aerosol in cloud droplet formation” by N. L. Prisle et al.

Anonymous Referee #1

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General comment:

The manuscript proposes a simple parameterization for the organic fraction in (atmospheric) aerosols assuming the organic fraction is overall composed of (strong) surfactants. The parameterization was successfully tested against droplet activation measurements of mixed surfactant/NaCl particles and compared to explicit thermodynamic model calculations. The manuscript is well written, the material is presented in a very clear way. It was stimulating to think about the consequences and implications if organic fraction of organic fraction would indeed be overall strongly surface active. The paper can be published in ACP as it is.

Specific comments:

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The authors still may consider the following comments, which do not touch the core work.

I don't really understand why total surface partitioning is not affecting the surface tension of the systems. Fig. 1d shows an effect of 5-10%. Or is such an effect regarded as not significant (p. 23611).

In the authors earlier work (Prisle et al. GRL, 2010) they consider phase separation in an organic and an aqueous phase in mixed SOA/inorganic salt particles as one option. This would be in accordance with findings e.g. by Anttila et al. (J. Atm. Chem. 2007), who observed formation of SOA coatings on aqueous particles. Would such particles with two phases / coated particles be covered by the simplified approach ?

The conclusions about the applicability to atmospheric systems it is a bit optimistic. It is likely that the organic fraction in atmospheric aerosols is not be overall composed of surfactants. Then the simplified representation would only work if we would be able to determine the soluble and the surfactant fraction. The problem is then shifted to determine this fraction and for the soluble fraction we are still be left with all problems as before. Nevertheless, this can be investigated in the lab as proposed by the authors. It would be helpful if the authors would comment in the conclusion section if and where they see likely applications for atmospheric particles (marine particles with biological material picked up from the ocean surface?).

p. 23606 Isn't the "solubility product" a constant for a given solute? I think what is meant here is that common ions in the solution lead to exceeding the solubility product of the surfactant.

Fig. 1 Caption b) appears twice.

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