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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Cloud droplet activation of black carbon particles coated with organic compounds of varying solubility" *by* Maryam Dalirian et al.

Anonymous Referee #2

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By reviewing the manuscript on the CCN activation properties of BC particles coated with a few organic species I feel myself rather uncomfortable. The study is a carefully planned and executed combination of experimental work and theoretical calculations aiming at providing new insights into the CCN behavior of atmospheric BC particles. However, the entire approach looks as a textbook-like routine exercise that contains no traces of novelty and innovation that would have been required by a high-standard journal like Atmospheric Chemistry and Physics. Any laboratory study in itself is free to use virtually any combinations of agents and conditions, yet it would be expected to be a quasi-realistic model of physical reality. The basic concept of the present study does not fulfil this fundamental requirement. What sort of real-life BC particles do Regal

black stand for? Aged diesel soot particles or BC particles from flaming biomass combustion? Levoglucosan, which is an abundant pyrolysis product of wood combustion, is not a semi-volatile species that is available for adsorption or condensation in the global atmosphere such as PAHs or n-alkanes. It is always present internally mixed with smoke particles, not as a gaseous species. Oleic acid is also a primary tracer which–unlike levoglucosan–is present in the gas phase but on a very limited spatial scale near its sources. I would doubt that this photochemically reactive species can make it to the free troposphere to participate in cloud nucleation. I wonder if anybody has ever detected oleic acid in cloud water or precipitation. In spite of these serious limitations oleic acid experiments at least yielded some unexpected results which are not fully exploited in the manuscript. Perhaps a molecular adsorption modelling approach would have helped explain the observations. Glutaric acid does exist as SOA product in the atmosphere, though at far lower concentrations than smaller dicarboxylic acids or other SVOC species. In addition, a very similar study was published for the CCN effect of adipic acid. I suspect that upon releasing fresh BC particles from any source there is a plethora of co-emitted semi-volatile species that are ready to be adsorbed onto their surfaces. It is strange that in the experimental section no temperature values are given for the coating procedure. These temperatures would also indicate that the atmospheric occurrence of such processes is unlikely. For lack of originality, the manuscript just declares plain trivialities such as on Page 9 Line 5-6 "As expected, the critical supersaturation is generally higher for pure BC particles than for the particles with organic coating and the pure organic particles have the lowest critical supersaturation". Overall, this manuscript presents a lab-based approach in combination with a well-established theoretical approach that has little if any atmospheric relevance. It is a textbook-like repetition of previous studies and completely lacks originality and innovation.

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