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Interactive Comment

Interactive comment on "The effect of fatty acid surfactants on the uptake of ozone to aqueous halogenide particles" *by* A. Rouvière and M. Ammann

Anonymous Referee #1

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This paper investigates the effect of organic coatings on the ozonation of iodide in aqueous aerosols, which is an interesting topic. However, there are several serious issues which prevent me from recommending that this paper be published in ACP in its current form.

My major problem is with the authors' assertion that a phase change occurs at higher organic surface loadings, forming condensed films at the aerosol surface. To my knowledge, these condensed films have only been observed when monolayers are compressed to increase the surface pressure (e.g. with a Langmuir trough). I am not aware of any studies which show this phase transition occurring simply by increasing surface coverage. Intuitively, I would not expect it to occur; rather, I would simply expect multilayers to form.

Further, I see no evidence of this phase change in the displayed results. If the ozone uptake is decreased due to the formation of a condensed film, I would expect to see a fairly abrupt decrease in uptake at the surface coverage where the phase-change occurs. However, the results show a very smooth decrease in reactive uptake with increasing surface coverage. This seems more consistent with increased physical blocking of ozone by the organic as the surface coverage increases.

Another concern I have is with the assertion that deliquescence has occurred and that monolayers of the fatty acids have formed. The aerosols were formed by exposing solid salt particles covered with fatty acids to water vapour. Organic films are known to inhibit the uptake of water, so it is possible that they could inhibit deliquescence of the salt particles. If the authors have evidence that deliquescence occurred (for example, evidence of significant growth in particle diameter), this should be made explicit. I am also not convinced that monolayers would be formed under the experimental conditions employed. The experiments ranged from 2 to 25 seconds – this is likely not enough time for equilibrium to be established and for monolayers to form at the aerosol surface.

If the authors can show that they did indeed form aqueous aerosols coated with monolayers of organics which formed condensed films at high surface loadings (and if they can more convincingly relate the trend of reactive uptake with increasing hydrocarbon loading to such a phase change), then I would recommend that this paper be published. If they cannot support these assertions, however, then I believe that their results simply show that loading organics on an aqueous surface blocks ozone uptake, and I do not recommend this manuscript for publication.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 15023, 2010.