

Interactive comment on “Vapor pressures of substituted polycarboxylic acids are much lower than previously reported” by A. J. Huisman et al.

Anonymous Referee #2

Received and published: 8 March 2013

This manuscript describes measurements of subcooled liquid vapor pressure of substituted polycarboxylic acids by probing the evaporation of levitated droplets of supersaturated aqueous solutions of these compounds. The manuscript is well-written, clear and contains interesting results. However, I think there are few relatively minor issues that need to be addressed before the manuscript can be considered for publication in ACP:

1. Although the structures of the investigated compounds are given in multiple places elsewhere, I think giving them in a figure here too would increase the readability of the manuscript.
2. I have one issue related to the terminology that I would like to raise: Now the authors refer to saturation or equilibrium vapor pressures as just "vapor pressures". I

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find this terminology confusing, as the term "vapor pressure" could be mixed up with a partial (potentially non-equilibrium) vapor pressure of the species. I therefore would recommend the authors to use either equilibrium vapor pressure or saturation vapor pressure when they refer to thermodynamic equilibrium.

3. There is a continuous flow of N₂ in the chamber. It is well known that a flow influences the evaporation/condensational mass fluxes (see e.g. Smolik et al., Journal of Aerosol Science 32, 739-748, 2001). Have the authors thought about this and how large is the effect expected to be in their setup?

4. The authors report very low saturation vapor pressures for the investigated compounds, which naturally means very slow evaporation rates / changes in the particle size. Although the authors are using relatively large samples and purified compounds, I was wondering have the authors thought about the maximum possible contamination (e.g. through gas phase ammonia or other kind of impurity) in their system and whether that could have any detectable effect on the results?

5. In the supplementary material the authors investigate the possible effect of gas phase dimerization of the organic molecules. I would imagine it would be relatively straightforward to calculate the kinetic minimum time scale for dimerization (i.e. the collision timescale) for the given organic concentrations, and compare that to the timescale of diffusion. I would think that this way the authors' argumentation about the dimerization would be even more convincing.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 1133, 2013.