

## ***Interactive comment on “Tight coupling of particle size and composition in atmospheric cloud droplet activation” by D. Topping and G. McFiggans***

### **Anonymous Referee #1**

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In the present manuscript the equilibrium size of aerosol particles in the presence of semivolatile organic compounds is studied. It is shown that co-condensation of semivolatile (organic) substances with water could potentially increase the particle size and affect the ability of particles to act as cloud condensation nuclei. It is also discussed that due to partitioning of semivolatiles the current methods to measure aerosol hygroscopicity might be erroneous in some conditions. The manuscript includes interesting results and the results are mostly presented in a concise way. The topic is highly relevant within the scope of ACP. I have only few, mainly minor comments that should be addressed before accepting for publication.

You did not give the conditions represented in Figure 1 clearly enough. What is the amount of organics and how does it compare with different atmospheric conditions?

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What is the number concentration of particles? Without those it is difficult to estimate how realistic conditions you are using, so a more detailed description is needed. The same for the results presented in Figure 2. Is the number concentration of particles changing with changing size keeping the total mass constant? It should also be highlighted that you are not solving equilibrium between different sized particles, but separately for different sized particles.

It would be interesting to see the effect of nitric acid (with and without ammonia) in the figure 1 with realistic atmospheric concentration. Now the reader can have an idea that the effect of organics is larger than the effect of ammonium nitrate and chloride on CCN potential. This might be the case but some support is needed. Maybe you could also have a figure showing the amount of organics in the particles as a function of saturation ratio compared to the amount of sulphate and thus give some idea how close to the activation point organics are actually condensed from the gas phase. It would be nice to see similar results with the model solving the condensation dynamics, but I guess it is outside the topic of this manuscript.

Equation 1:  $T$  is temperature.

Page 25164, line 7: Is 150kJ/mol a typical value for enthalpy or how did you choose that?

Appendix A: It is said that iteratively solving the equilibrium equations for every single component enables construction of a new Köhler curve, thus cloud activation predictions. Could you then say which particles would activate to cloud droplets at some supersaturation with realistic aerosol size distribution? I guess not. As far as I see Köhler theory cannot actually be used to predict CCN properties of aerosol in the presence of semivolatiles and different sized aerosol particles. It just gives the lowest possible critical saturation when only single type and monodisperse aerosol exist in the system with the known amount of semivolatiles. In the page 25162, lines 23-24 you acknowledge this, but this would hold only in the case of single particle size and not during

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“cloud activation” that is usually understood to be something actually happening in the atmosphere. Little bit more discussion in the main text should be included on what would happen in the case of aerosol size distributions when the difficulty is not only the partitioning of semivolatiles between the gas and particle phase, but also partitioning between different sized particles. Then the equilibrium partitioning would not give the maximum effect on the cloud droplet formation as due to Kelvin effect big particles would grow even bigger.

“ $P_{i0}$  represents the partial pressure of  $i$  above a flat solution containing only a sub-cooled form of itself” is quite complicated way of describing saturation vapor pressure. Should “flat solution” be “flat solution surface”. Also can solution be composed of single component only?

There are several references at the end that are not used in the manuscript: Albrecht 1989, Facchini et al 1999, Solomon et al. 2007, Twomey 1977, Warner 1968, Wex et al. 2009.

Donahue et al. 2010 is missing. Might be a wrong year.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 25155, 2011.

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