

Interactive comment on “Water activity and activation diameters from hygroscopicity data – Part II: Application to organic species” by K. A. Koehler et al.

Anonymous Referee #1

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The paper by Koehler et al. is a detailed treatment of the hygroscopic behavior of a range of relatively small organic constituents of tropospheric aerosol. The experimental data consist of hygroscopic growth measurements conducted in a standard hygroscopic tandem-DMA approach. Considerable emphasis is given to relating the hygroscopic behavior measured at sub-saturated conditions to the CCN activity of the material that has been measured (by other workers) under supersaturated conditions. A strength of this paper is that a systematic comparison is made (see Table 2, alpha and beta terms) between the predictions of the relationship between particle dry mass and critical supersaturation performed using straightforward Kohler theory and by the

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HTDMA measurements. The relatively close agreement between these two quantities gives some confidence in using either the Kohler model or sub-saturated hygroscopic growth measurements to predict organic CCN activity. Alternatively, it provides some measure of the level of uncertainties that prevail in these predictions. Lastly, the paper illustrates using an adiabatic parcel model the sensitivity of the predicted number of cloud droplets to the chemical character of the activating particles. I recommend publication, largely based on the quantitative connection that is made between the hygroscopic measurements and the CCN activity for a class of molecules that is now recognized of having some prevalence in tropospheric aerosol.

A few points should be addressed prior to publication:

1. What is the uncertainty that arises from the fact that the hygroscopic growth curves are referenced to particles at low RH that have not effloresced and still contain some residual water? As pointed out on page 10885, molecules such as malonic acid effloresce at low relative humidities but the growth measurements (Figure 1) illustrate that insufficient drying occurred to make the particles effloresce under the experimental conditions.
2. On this topic, it is important that the residence times in the experiment and the manner by which the drying was performed should be given in the paper. What were the sheath/sample flows in the DMA? If extra drying between DMA1 and DMA2 is occurring, why was not an extra mixing volume added to the experiment prior to DMA1 to increase the residence time at low RH to allow the drying to proceed fully?
3. One of the novel aspects of this work is that evaporation of the organics is accounted for in the data analysis. It would be valuable to provide a back-of-the-envelope calculation to confirm that it is indeed organic evaporation that is proceeding between DMA1 and DMA2. This can be readily done by assuming an organic vapour pressure, residence time, particle number density, etc. This calculation could confirm in an order-of-magnitude sense whether or not it is indeed particle evaporation that is proceeding,

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or whether some other factor like structural rearrangement leading to a change in mobility diameter is occurring instead. The reason I bring this up is that I am surprised that molecules that have such low vapor pressures (e.g. $10e-5$ torr or lower, see Table 1) are evaporating on the timescales of seconds.

4. In Table 1, there are a number of small errors: What is meaning of “z” in column 7? What is the meaning of the caption of the top of column 9? The stoichiometric coefficient “2” is missing from chemical formula for the dihydrate of oxalic acid (column 2).

5. In Table 2, there seem to be too many significant figures in the values for alpha and beta.

6. In Table 3, it would be useful to state what the parameter “k” refers to.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 10881, 2005.

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