Atmos. Chem. Phys. Discuss., 5, S304–S307, 2005
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European Geosciences Union

Atmospheric
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Discussions

Interactive comment on "Water activity and activation diameters from hygroscopicity data – Part I: Theory and application to inorganic salts" *by* S. M. Kreidenweis et al.

Anonymous Referee #1

Received and published: 4 April 2005

General comments

I strongly support the idea that HTDMA data can be used to predict critical supersaturations and thus also CCN concentrations, for both laboratory and ambient aerosols. This paper would help to further promote this way of thinking. I would therefore like to see this paper published in ACP.

Having said that, it deserves to be noted that this concept is not new. It has been used in several previous studies. Recently, it has been extensively elaborated upon along the same principles as outlined in the submitted paper in Rissler et al. (2004) for

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ambient aerosols, and in Svenningsson et al. (2005) for laboratory aerosols. Nearly all equations have been derived and thoroughly discussed elsewhere, also of course in some of the references that are already cited.

Equation (17) appears to be very useful in order to provide simple parameterizations of the hygroscopic behaviour of both single compound aerosol particles and ambient aerosols. Since it can be derived from the Köhler equation (as shown in this paper) it is more appealling than some other parameterizations previously used, such as the “gamma" function, where GF(RH)=(1-RH/100)) raised to “gamma", or similar. “Gamma" is then fitted to the HTDMA data. Equation (17) has been used previously, as mentioned, but deserves more attention.

The parameterization of critical supersaturation (section 5) is not new. Equation (30) is identical to equation 15.34 in the text book of Seinfeld and Pandis (1998). As noted in Rissler et al. (2004), this relationship can be used to estimate critical supersaturation from HTDMA data. Anyway, I believe it can be a very useful way to link HTDMA data to critical supersaturation, and also deserves more attention. The important parameter when predicting the critical supersaturation is (apart from the surface tension) the number of ions or molecules in solution in the activating particle, and this quantity can be successfully estimated from HTDMA data. (It is basically the parameter “a" in Equation (17) multiplied by rhow/MWw, parameters “b" and “c" take care of all non-ideal complications such as low solubility and ion-ion interactions).

Specific comments

Abstract:

The H-TDMA is most often referred to as “Hygroscopic" TDMA, not “Humidified". I would prefer to leave out the reference to Part II.

Introduction:

A 2% uncertainty in RH is not very good, at least not for laboratory conditions. Dew point hygrometers are more precise. The problem is then knowing the temperature inside the second DMA.

Section 2.1:

It is good to be reminded once in a while about the origin of the Köhler equation, and that it is actually the partial molar volume of water that enters the equation. But since the assumptions normally made are quite valid at the point of activation, it is perhaps not necessary to note this.

Section 2.2:

The use of SI units is recommended (not dyne/cm).

Following equation (20), the authors note that one should use known surface tensions, but these are not easy to get for ambient aerosols, although they can be measured.

Section 2.3:

The definition of shape factor also includes the Cunningham slip correction factor, so equation (25) does not comply with the normal definition of the dynamic shape factor. In this section, I would also rather see that the authors do not use “true diameter" or “dry diameter", but use “volume equivalent diameter" and clearly distinguishes this from the measured dry mobility diameter. For the wetted particle, the measured mobility diameter equals the volume equivalent diameter since the shape factor is unity.

Section 3:

Reference to Fig. 3 should be Fig. 2.

I wonder if it is really necessary to carry out such an elaborate sensitivity study for two of the most common inorganic salts. These are often used to calibrate CCN counters, since it is believed that their activation can be modelled accurately. It is well known to most scientists in the field that water activity should use instead of RH, and also that the shape factor of NaCl need to be considered. I would agree that the AIM model is probably the best reference for water activities.

Section 5:

Reference to Equation (28) should be (27).

Before equation (31) it is stated that the only thing that is not a constant in the equation is “beta". The surface tension (rhow) should actually be the surface tension of the solution at the point of activation, which may actually differ significantly from that of pure water.

Finally, I would like to apologize to the authors that my referee report was somewhat delayed. It was certainly not from lack of interest in the paper.

References

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 287, 2005.