

Interactive comment on “Interaction of aerosol particles composed of protein and salts with water vapor: hygroscopic growth and microstructural rearrangement” by E. Mikhailov et al.

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Received and published: 7 November 2003

Comments to the Manuscript "Interaction of aerosol particles composed of protein and salts with water vapor: hygroscopic growth and microstructural rearrangement" by Mikhailov et al.

This is a highly interesting manuscript presenting experimental results about the hygroscopic properties of protein and inorganic salts. The water associated with organics is a current topic of a research field, where many question are still open. This work was done with care and should be published. I recommend this manuscript for publication after the authors have addressed the comments listed below.

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Specific comments:

P4759: "Proteins account for up to about 10% of WSOC of PM_{2.5}". What is the source of these proteins in the atmospheric aerosol? Is the same quantity also found in the ultrafine size range or do they arise from bioaerosols with diameters > 1 μm ? Please provide more information.

P4760, line 25: change to "18.2 M Ohm times cm"

P4761, line 24: change to "0.1°C"

P4762, line 24: It is assumed that in the DMA (where the particles "see" the different sheath air humidity, RH3) the particles attain their new equilibrium size nearly instantaneously (i.e. within a timescale much smaller than the particle residence time in the DMA drift zone). This assumption is not necessarily valid for particle that undergo a phase transition or when surface-active organic substances are present on the particles. In these cases the determined sizes may therefore differ from the final equilibrium size. This should be mentioned and discussed.

P4763, line 1: "modal diameter (local maxima) of number size distributions" How was the modal diameter exactly determined? Did you fit a log normal distribution to the data? If yes, what is the standard deviation (or spread) of the distribution? Did you observe a change of the standard deviation with changing RH? Please give also more information about the units of the number size distribution. Did you parameterise the raw CPC counts or did you use processed data (such as $dN/d\log D$)? What is the SMPS upscan time? Did you observe smearing effects?

P4763, line 8: "1% precision in diameter measurement". I am wondering how this high precision could be achieved over several hours with rotameters, needle valves and tube clips. This is a brilliant achievement.

P4764, line 10: Throughout the text I would change mass equivalent diameter to volume equivalent diameter. In this context it is less misleading and more common to use

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the volume equivalent diameter.

P4768, line 17: did you mean eq. (7)?

P4774, eq 30: use a more general expression and replace 133 nm with 2 times lamda, where lamda is the mean free path of air.

P4775, Fig.3: The discussion is not satisfying. I attribute the fact that the growth factors measured during dehydration are larger than the one measured during hydration to the instrumental artefact described above (P4762, line 24). During dehydration the supersaturated droplets crystallize in the upper part of the DMA and therefore the measured sizes are larger than their final size. Maybe one could estimate a crystallization time from these differences... An analysis of the standard deviations of the SMPS size distributions could give more insights. See Gysel et al., Atmos. Chem. Phys. Discuss., 3, 4879-4925, 2003

P4776, discussion Fig.4: Again, using the second DMA simultaneously as a dryer as well as a sizer is problematic. I fully agree with the discussion of the observed microstructural rearrangement at $RH < 75\%$. But the stepwise increase at deliquescence RH might be a consequence of the instrumental artefact (see above). This should be mentioned.

P4786, line 9: Figure 6C1 instead of figure 5.

P4788, line 3: Figure 13e should be 13d and e.

P4788, line 17: Which location do you exactly mean with "inlet of DMA 2"? Is it the mechanical inlet or the zone inside the electrostatic field where the electrostatic sizing starts and the sheath air RH is mixed by diffusion with the aerosol RH? I assume that you mean the zone inside the DMA and I would therefore suggest to avoid the term "inlet" in this context. I have the impression that in this paragraph the authors assume that inside the DMA the particle already experience the higher sheath air RH BEFORE their electrostatic sizing starts. This assumption is NOT true.

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P4789, line 6 and 9: These conclusions cannot be drawn from these measurements!

P4817, Figure 1: add RM - rotameter

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 4755, 2003.

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