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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Hygroscopic properties of ultrafine aerosol particles in the boreal forest: diurnal variation, solubility and the influence of sulfuric acid" *by* M. Ehn et al.

**Anonymous Referee #1**

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General comments:

Overall this is a very good paper and well suited to ACP. The results are novel and interesting. The conclusions are a bit vague; this makes the paper a bit hard to understand. The reason seems to be that it is not clear what is really going on. The results provide some tantalizing clues, but are not conclusive. I do not think that is a big problem since this area is not well enough developed to be able to expect more. I do have one serious concern, addressed first under specific comments, that may affect the validity and/or interpretation of the results.

Specific comments:



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Discussion Paper

(1) I am concerned about a possible systematic error associated with the method of drying particles within the first DMA. On page 9940, line 24, the authors state that the water in the particles evaporates very quickly once they enter the DMA. I doubt that this is so. Water will certainly evaporate quickly once the particles enter dry air, but when they first enter the DMA they are still surrounded by moist air. They only encounter dry air after they are transported some distance radially in the DMA. Furthermore, because of the diffusion of water vapor, the transition to dry air will not be sharp. So the particles will be changing size as they pass through the DMA and the average particle size, determined by the DMA voltage, will be somewhat greater than the size of the dry particles exiting the DMA. This would cause the growth factors to be systematically somewhat small. One might hope that the effect is not significant, but the authors provide no evidence of that.

If this systematic error were always the same, it would not be of much concern since it would have no effect on the major results of the paper. But variations in the RH of the aerosol flow could lead to variations in the magnitude of this effect. I am especially concerned that this might produce a diurnal variation as the humidity increases under the nocturnal inversion and decreases during boundary layer growth. That would directly impact one of the most interesting results: the diurnal variation in growth factor. The authors might have the data needed to address this point. Figure 1 shows a humidity sensor on the excess air output of DMA1. How constant were those readings? Did they show a diurnal variation? Of course, any variation in sample air humidity would be greatly damped at that point.

(2) Although the authors state that the sheath air in DMA1 was dried, they do not give the range of RH values. This data is essential to assess whether drying was complete.

(3) With respect to equation 2 (page 9944), I think there now is quite a bit of data showing that the organic component of atmospheric particles does pick up water. This should at least be acknowledged since it would alter the calculated soluble fractions. It would not alter the major conclusions, which are based on trends in the soluble fraction.



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(4) A frustrating shortcoming in the paper is the lack of any error estimates. Are the differences in growth factors discussed on page 9948, lines 3-12, actually significant? Although the scatter in figure 8 gives some idea of uncertainty, error bars would be helpful in figure 9. On page 9950, line 10, the authors refer to soluble fractions greater than 1. Are they really greater than 1, or just near 1 with some scatter? The latter seems more likely to me.

(5) Are the trends in Figure 6 statistically significant? Are the correlation coefficients R, or Rˆ2?

Technical corrections:

(1) The first three sentences of the abstract would be appropriate for an introduction, or perhaps a conference abstract, but they don't belong in the abstract to a paper.

(2)The size range of sampled particles should be given in the abstract.

(3) In figures 2&4, I found the top panels illegible when viewed on screen. The black and white printed versions were actually much more readable. I don't know if this a problem is somehow specific to my computer. But Figure 7 is fine.

(4) In discussing Figure 8, the text refers to specific particle sizes but not to the deviations from the mode. So in the figure, different symbols should be used for the former with the different colors for the later. Then the text could be followed with printed black and white figures.

(5) Different symbols should be used in Figure 9 so that they may be distinguished when printed in black and white.

(6) Page 9945, lines 20-22. It would be helpful to the reader to say explicitly that the purpose is to ensure that the samples are near the mode maximum.

(7) Page 9950, line 8. "The large variations ...". This caused me some confusion since all sizes seem to have similar variations. Also, it is not clear if the variations referred to



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are the trends during each day or the differences from one day to another (I think it is the latter).

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Interactive comment on Atmos. Chem. Phys. Discuss., 6, 9937, 2006.