

## ***Interactive comment on “Organic acids as cloud condensation nuclei: Laboratory studies of highly soluble and insoluble species” by P. Pradeep Kumar et al.***

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### SPECIFIC

1. In the list of questions on pages 952 and 953 (particularly, question 3), is it possible that in a mixed system in which smaller soluble acids (oxalic, for example) and larger insoluble acids are present, the smaller soluble acids will be absorbed into the insoluble ones leading to activation of a second, separate phase (as opposed to just the soluble acid alone)?

This is a good suggestion and has been added to the paper.

2. On page 957, is it at all possible that any of the particles of interest activated yet stayed below the lower cutoff of the aerodynamic particle sizer (0.5 microns). I tend to

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doubt it, but did the authors attempt to confirm this in any way?

For all of our experiments, the data shown in Figure 3 were representative of the size spectrum of the activated particles, i.e. no indication of significant number density at sizes smaller than the lower size cutoff of the aerodynamic sizer. Given that only a single mode of particles was being supplied to the chamber, there is no reason to believe that a second, smaller mode would appear in the activated particle size spectrum. Thus, we always assumed that we detected all the activated particles.

3. On page 961, it would be helpful if the authors discussed exactly how Equations 2 and 3 are altered when non-ideality is considered. This would clarify how the calculated diameter would change for the readers that are not part of the physical chemistry or aerosol communities.

A full discussion of non-ideality is given in *Microphysics of Clouds and Precipitation*, Pruppacher and Klett (1980), Section 4.6. In particular, it is described in detail how a quantity called the Osmotic Coefficient is related to the water activity of a solution (Equation 4.69 and Table 4.2, Pruppacher and Klett). This reference is now added to this section of the text. The manner by which we calculated activities for the non-ideal case was not to modify Equations 2 or 3 but to use the overall equation, Equation 1, that describes the water vapour pressure over a droplet. In particular, using osmotic coefficients from the Clegg et al. reference we calculated water activities (and water supersaturations) for specific sizes of dry particle. By trial and error we varied the size of the dry particle to calculate the supersaturation at which the experiment was performed. This is a good point, and the text now specifies that Equation 1 was used in this regard.

4. On page 963, it would be extremely informative if more detail (at least some) were given on how the van't Hoff factor for oxalic acid was calculated.

A description of this is now given in the text.

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5. For organics, why was no activity corrected diameter calculated? On comparing Figures 5 and 9, the amount of scatter seems to be comparable and this correction was made for the inorganic salt shown in Figure 5. The routine UNIFAC could be used for such calculations.

Although the amount of scatter is comparable between the ammonium sulfate and dicarboxylic acid data sets, the former systematically differed from the predictions assuming ideal solutions whereas the organic results did not. Indeed, the reason that we did not pursue more elaborate calculations for the di-acids was that the agreement with the ideal Kohler theory predictions was superb, to within 2 or 3 nm, i.e. within the experimental uncertainty of the technique. This is very reasonable behaviour because, as already mentioned in the text, ionic solutions are considerably less ideal than neutral ones. For example, see Figure 4.1 in Pruppacher and Klett (1980) which shows that in the limit of dilute solutions sucrose is considerably more ideal than ionic solutions. Thus, we felt there was no need to invoke non-ideality in the model to explain the data.

6. It might be helpful (for visualization purposes) to rotate Figure 2 so that it is horizontal (since the flow is horizontal).

When we said in our ACPD paper that the chamber was oriented horizontally, what we meant to indicate was that the two copper plates were mounted horizontally, i.e. each was parallel to the ground. Thus, it would not help visualize the experiment if we were to rotate the schematic drawing in Figure 2. Instead, we have re-worded the text in the Experimental section to make the orientation clearer.

7. It should be noted in the caption of Figure 7 that the data shown is for oxalic acid dihydrate.

This comment is correct, and a change to the figure caption has been made in the paper.

TECHNICAL

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1. An umlaut should be used in Kohler throughout the text.

This change has been made in the paper.

2 Units of some equations would be helpful (particularly Equation 1).

Given that there is no single set of correct units, we do not want to specify units for equation 1. All that matters is that the units used are consistent with each other. However, we point out that typical quantities used in the calculations are given later in the paper.

2. On page 954, define SLM.

This has been done.

3. On page 964, Figure 10 is cited before Figure 9. The order of the figures should be switched.

Actually, Figure 9 is cited earlier than this, in Section 3.2 (page 962).

4. In the list of references, the Clegg et al. reference has some numbers in a chemical formula that need to be subscripted.

This has been done.

5. In Table 1, the a in the title should be superscripted. The word depending in footnote c is spelled incorrectly.

These changes have been made.

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Interactive comment on Atmos. Chem. Phys. Discuss., 3, 949, 2003.

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