

Interactive comment on “How quickly do cloud droplets form on atmospheric particles?” by C. R. Ruehl et al.

Anonymous Referee #1

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The authors have measured the apparent accommodation coefficient, α_{app} , at four stations in the continental US, and find that an appreciable fraction of the particles in the size range 100 to 200 nm grow more slowly than ammonium sulfate particles of the same diameter.

The results in the paper are intriguing; it could be published “as is.” However, I think the authors should consider the following points.

The authors make a point of stating (at the top of page 14236) that α for pure water should be about 1. But they ratio their results to α for ammonium sulfate, which is ~ 0.01 . Why is the accommodation coefficient for an ammonium sulfate solution 100 times smaller than for water? I find this result quite puzzling, especially when consid-

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ering the fact that the laboratory generated particles are entering the supersaturated region of the instrument after being conditioned at 80% RH. The particles are pretty big at that point, implying that the ammonium sulfate solution is already fairly dilute. (I realize that this is not the central point of the paper, but if you make a point of saying that $\alpha_{water} = 1$, at least comment on why the values for ammonium sulfate solutions are so low.)

Low α' particles tend to show up when trajectories show that air arrived from aloft. The conjecture is that cloud cycling might be responsible for depletion of the particles that grow as fast as ammonium sulfate. Why would this be restricted to air masses arriving from above the boundary layer? Admittedly, the lifetime of particles increases as you go higher into the troposphere, but a lot of that has to do with a lower probability of running through a cloud cycle. Not every cloud rains. Cloud cycling alone won't explain this. I think you need precipitating clouds.

The connection that the authors make between cloud cycling and studies which have found organic aerosol particles in the free troposphere is tenuous. There have also been studies which found organic aerosol in the boundary layer. A chain like this I have no problem with: descending air masses \rightarrow organic material \rightarrow low α' . Cloud cycling could be involved, but I am not convinced that the data presented here supports it.

The authors note that kinetic limitations to growth peaked around noon, then decreased, leading to the supposition that photochemical aging was responsible for the change. In other words, further oxidation led to an increase in solubility. That scenario implies that accumulation mode particles, which presumably have been in the atmosphere for a couple of days at least, were not already oxidized. Why?

Having raised these questions, let me note that I think including the possibilities in the **Discussion** is warranted. I just want to point out some problems in the interpretation as currently presented.

Technical points.

Page 14240, line 12: "optical divergence of the droplet." Is the droplet diverging optically? I'm not sure what that phrase means.

Page 14242, line 9: Equation 2 is referenced. I think you mean Equation 3.

In my opinion, the readability of the paper would be improved by removing the acronyms and simply writing out the station names. BON → Bondville, etc... (I realize this is probably a minority opinion. Feel free to ignore it.)

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 14233, 2007.

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