

Interactive comment on “The potential contribution of organic salts to new particle growth” by K. C. Barsanti et al.

Anonymous Referee #2

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The manuscript by Barsanti, McMurry, and Smith describes theoretical calculations addressing the potential impact of aminium salts (formed from low molecular weight amines and organic acids) on aerosol growth. Both concentrated and dilute systems of amines and organic acids are explored. The analysis presented is straightforward and well thought out. The topic matter is one of significant interest given recent experimental evidence of amines in the particulate phase, and because most previous theoretical work has focused on salt formation from amines and inorganic acids. The quantitative results for dilute systems of amine, ammonia, and organic acids are highly relevant and will serve to emphasize the potential importance of particle growth by aminium salt formation. The most important aspect of the results from the concentrated system is a sensitivity analysis aimed at identifying which parameters most critically need to be

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measured experimentally. The authors conclude that the activity coefficients for these types of systems are quite uncertain and have a significant impact on the composition of both the particulate and gas phases. I highly recommend the article for publication in ACP, but there are a few areas in which I feel the manuscript could be improved, these are outlined below.

1.) It is unclear to this reviewer why both 100:1 and 1000:10 NH₃:DMA gas-phase ratios are cited in Table 2 given that the results are, as expected, are identical. If both columns are to be included, there should be an explanation in the text of why they are both there.

2.) In Figure 2 the concentration ratios of NH₃:Amine range from 1:1 to 1:100. Given that the atmospherically relevant concentrations are probably less than 1:1, it would be very informative if the authors could add additional data points at NH₃:Amine ratios of 10:1 and 100:1. While it is mentioned on line 14 of page 20732 that, "The same would be true when NH₃ greatly exceeds DMA in the gas phase. In such cases, *c_i* becomes the dominant driving force for organic salt formation." it would be good to see this effect quantitatively on the graph.

3.) The fraction of dimethylamine in the gas phase is said (both in the text and in the figure caption) to be shown in Figure 2, but there is no indication of gas-phase species in Figure 2. The reader is left to assume that the fraction in the gas phase is negligibly small. If this is the case, it should be clearly noted on the figure and in the text.

4.) Given the importance of activity coefficient estimates to the results presented and given that terminology changes from one text to another, it would seem reasonable to explicitly write the Davies equation in the text and to give a brief explanation of how and why the values of *C* for this work were chosen. I realize this is not the focus of the work, but it is a bit frustrating to see multiple references to the parameter *C* without having the functional form of its relationship to activity written out in the text.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 20723, 2008.

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