Atmospheric Chemistry and Physics Discussions

Interactive comment on **"Calibration and measurement uncertainties of a continuous-flow cloud condensation nuclei counter (DMT-CCNC): CCN activation of ammonium sulfate and sodium chloride aerosol particles in theory and experiment"** *by* D. Rose et al.

Anonymous Referee #2

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The manuscript describes extensively the calibration and the stability of the DMT CCN spectrometer, an instrument that is going to be used by an increaing number of groups. Understanding of CCN activity of atmospheric aerosols as a function of composition and size is one important key to better quantify the indirect effect. The authors provided a very important and helpful peace of work, which I read with great interest. The article can be published in ACP as it is.

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If I have to critize something, then I would strongly support the comments by David Topping. There should be only one Koehler Equation and the authors tested several routes and levels of approximations, not different Koehler equations. (If that could be changed ....?) Since Köhler equation is rigorous thermodynamics, the laboratory measurements of well defined systems give the chance to test and to develop thermodynamic approaches and to leave the older approaches behind, which were often pragmatical and owed to the limited resources at that time. The Pitzer Simonson Clegg (PSC) model, which is molefraction based as the Koehler equation itself as given in eq.3ff, is to my knowledge the most promising approach. The molity based Pitzer Model (OS in the manuscript) is build on similar principles and I would bet that replacing the polynomial representation of aw by Tang and Munekelwitz(1994) by the PSC model approach will bring the AP1.1 close to the Pitzer OS results. The advantage of the semi-empirical PSC model over all purly empirical approaches, especially the Van't Hoff approximations: the extra- and interpolations are based more rigorously on statistical thermodynamic fundamentals. This I see independent of the need to parametrize the Koehler equation in context with large scale modelling.

I have two small remarks : Why do you reference Kreidenweis/ Koehler for eq 3 and Mikhailov et al. for assumption of volume additivity? Isn't that generalized knowledge ?

p8208 line 12: the sentence introducing eq 13 reads a little strange, I think it missing a bracket or "according to".

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 8193, 2007.

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