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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comprehensively accounting for the effect of giant CCN in cloud droplet activation parameterizations" *by* D. Barahona et al.

Anonymous Referee #2

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This paper addresses the issue of how to include in parameterisations of aerosol activation in cloud the effect that arises from very large particles of relatively high water solubility that do not have sufficient time to reach their critical diameter of activation; salt particles being a good example. The paper is well written and the outcome is potentially useful, but the explanation of the phenomenon needs more detail, the authors have overlooked previous work directly related to this issue and the concept of entrainment is incorrect and irrelevant to this application. Detailed comments follow.

1. The abstract states: "Cloud droplet activation parameterizations used in aerosol indirect effect assessments often assume that droplet growth after activation is much

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greater than their equilibrium size close to cloud base." What does it matter if it occurs "after activation", unless LWC is unconstrained?

2. Also in the abstract, you state: "If a large fraction of the aerosol is composed of such particles (such as regions with large fractions of dust particles and seasalt), neglecting such kinetic limitations in cloud droplet activation parameterizations leads to an underestimation of droplet surface area during cloud formation, hence overestimation of maximum supersaturation and cloud droplet number." And accordingly, in the introduction you state: "Incorrectly accounting for this surface area can underestimate the condensation rate of water vapor, which leads to overestimation in maximum supersaturation, Smax, and droplet number." I understand the logic of this conclusion as being that larger droplets are oversized by existing parameterisations (models) and therefore smaller droplets with more of the surface area are undersized. Your inherent assumption (?) is that the smaller droplets have more surface area, and of course we assume that the models adequately conserve LWC. But if this assumption is not always true, then the model will overestimate the surface area associated with these kinetically limited particles/droplets, not underestimate it. To me, this point is not so obvious and requires a better explanation. For example, Phinney et al (Limitations of using an equilibrium approximation in a cloud droplet activation parameterization, J. Geophys. Res., 108, 4371-4380, 2003.) compared the effect of treating this phenomenon in the ARG aerosol-cloud droplet parameterization with a parcel model and appears to have found the opposite to your statements (see their Figure 4).

3. The introductory statement "For a single-mode aerosol, an overestimation of Smax may not lead to substantial errors in droplet number given that most CCN would activate." Does this mean for all modal sizes and spreads, for all updraft speeds as well as for all compositions? This statement can only be true in a very limited number of situations.

4. There can be subtleties associated with applying a parcel model to this issue of kinetic limitation. When you initiate your parcel model, how do you define the sizes of

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the kinetically limited particles and at what position w.r.t cloud base? Does it make a difference?

5. Section 2, equation 1 etc – There is no plausible physical reasoning behind the parameterisation of entrainment in an adiabatic parcel model to study cloud base activation. It is misleading because its application says that there are no adiabatic parcels in cloud. How then did the cloud form? It is only a method of matching the parcel model results with the means of observations. It is good that the authors have refrained from actually using it in their current application, but it is inappropriate to bring it into this discussion at all.

6. The authors need to explain their results in relation to those of Phinney et al. (2003) and any other appropriate study.

7. The colour coding of updraft speed appears to be incorrectly labelled in Figure 1.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 24717, 2009.