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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol effects on deep convective clouds: impact of changes in aerosol size distribution and aerosol activation parameterization" *by* A. M. L. Ekman et al.

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We would like to thank the reviewer for providing thoughtful comments that have lead to an improvement of the manuscript. In the following section, we give our responses to each of the comments or questions in a format that lists the comments in order followed by our corresponding answer

Reviewer #1

1. *The study uses a crude bulk microphysics two moments scheme, which is*

able to slow the autoconversion in response to enhanced concentrations of CCN aerosols,but inherently unable to simulate correctly the cloud and hydrometeor particle size distributions. This is critically important, because most processes, and with particular importance here scavenging and evaporation, depend strongly on these distributions.The model is also run in the rather crude horizontal resolution of 2 km and vertical resolution of 400 m, which cannot resolve but the largest convective elements. Therefore,such a model would fit to test very broadly the hypothesis that adding aerosols invigorates convective clouds due to delaying the autoconversion. It does so successfully by showing generally more invigoration with greater number of aerosol concentrations. This has been shown already by many previous similar studies. However, when convolving the crude processes of the cloud microphysics with the fine processes of the aerosols the inevitable outcome must be crude results, which render almost meaningless the admirable attempts of the authors to investigate the impacts of the aerosol properties and processes. This level of refinement with the aerosols has to be done with a spectral bin microphysics model, or not at all. For example, one of the major "findings" where the simulation results deviate from the conceptual model of Rosenfeld et al. (2008) is the decreasing rain in greater amount of aerosols in some of the variants, explained by enhanced evaporation (e.g., first paragraph of page 6355). Evaporation rate of rain is critically dependent on its drop size distribution. It has been documented that rain drops are much larger in more microphysically continental (i.e., with more cloud drop number concentration) clouds (Rosenfeld and Ulbrich, 2003). Because the shape of the rain drop size distribution is prescribed, this effect cannot be taken into account in the 2-moment scheme, and hence all processes that depend on evaporation rate are very dubious, and respectively the conclusions that are based on them.

Reply: Firstly, we would like to point out that in the paper by Rosenfeld et al. (2008), their conceptual model is to some extent supported by/based on results from cloud-resolving model simulations. Some of these cloud-resolving models do apply two-moment microphysical schemes (e.g. van Den Heever et al., 2006 and Wang, 2005). Secondly, Seifert et al. (2006) examined the characteristics of a maritime deep convective cloud and the sensitivity of the cloud to varying CCN concentrations using both a bin microphysics model and a two-moment bulk scheme. They found that the vertical structure of the deep convective cloud, the updraft velocity and the surface precipitation was similar when the bulk and bin methods were applied. More importantly, the sensitivity to varying CCN concentration was also found to be similar when using the two different microphysical methods. Seifert et al. pointed out that in order to get a good agreement between the bulk scheme and the bin method, an accurate representation of the warm phase autoconversion was of crucial importance. As noted by the reviewer, this process appears to be realistically simulated by our model. We therefore believe that the sensitivity to varying CCN concentrations is simulated in a realistic manner as well. Our results are also consistent with the study by Fan et al. (2009) in which a bin microphysical model is applied. We do acknowledge that there are certain limitations of applying a bulk microphysics model. However, in a corresponding way, we do believe that a simplified aerosol model will provide certain limitations and this is one of the main points of the present study. Our intent is not to say that the conceptual model by Rosenfeld et al. (2008) is wrong. Rather, we would like to encourage more studies, in particular using bin microphysics models, but these studies should carefully consider how they treat the aerosol population dynamics. This is now stated in the conclusions. The reference to Seifert et al. (2006) has also been added in the model description. Regarding the vertical and horizontal resolution, the model has been evaluated versus observations in e.g. Barth et al. (2007) and showed good agreement with observations in terms of e.g. peak updraft and radar reflectivity.

2. *The authors state that they test the conceptual model of Rosenfeld et al. (2008) in the sensitivity study by varying CCN concentrations between a maximum of about 700 to a minimum of 700/4 cm-3 (see Fig. 1). However, the conceptual model of Rosenfeld et al. (2008) and the observational studies referenced there show that much larger*

range of concentrations should be used for capturing the range of sensitivity.)

Reply: For the present study, it is not our intent to simulate the whole range of aerosol-induced sensitivity. Our intent is only to simulate a range where aerosols according to the theory should enhance the convection. Figure 4 in the paper by Rosenfeld et al. (2008) displays an almost linear increase in release of convective energy vs. aerosol concentration for aerosol concentrations between approx. 200 and 700 cm-3. We therefore believe that this range of aerosol concentrations should be sufficient for the purpose of our study.

3. *P6343 L21-23 The text reads: "smaller cloud droplets also imply a shift of the homogeneous freezing level to colder temperatures". The authors misinterpret here Rosenfeld et al. (2008), according which smaller drops causes heterogeneous ice nucleation to occur at colder temperatures, and in the extreme case that is caused by cold cloud base temperature and high aerosols can be delayed to the homogeneous ice nucleation temperature of -38C.*

Reply: As pointed out by the reviewer, this was a misunderstanding of the article by Rosenfeld et al. (2008) and the formulation has been changed in the new version of the manuscript.

3. *P6344 L23-26: The text reads: "we constrain our study and do not include the effect of droplet size on homogeneous Freezing". The statement here is problematic for the same reason as for the previous comment. The authors probably mean that the droplet size does not affect the temperature or rate of heterogeneous freezing. With this respect, the model should still have implied dependence of heterogeneous freezing on drop size for the following reasons: The autoconversion rate depends strongly on*

cloud drop size. I suppose that warm rain is allowed to be formed in the model in supercooled temperatures. If so supercooled rain should freezes heterogeneously when colliding with ice crystals, snow or graupel. Hence, heterogeneous freezing occurs faster with larger drop size even in this model. If so, the conclusions that are based on the assumption that it is not so should be changed.

Reply: The point is well taken and manuscript has been changed according to the suggestion by the reviewer.

Reference: Seifert, A., Khain, A., Pokrovsky, A. and Beheng, K. D., 2006, A comparison of spectral bin and two-moment bulk mixed-phase cloud mircophysics. Atmospheric Research, 80, 46-66.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 6341, 2010.