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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Are simulated aerosol-induced effects on deep convective clouds strongly dependent on saturation adjustment?" *by* Z. J. Lebo et al.

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I agree with the authors and reviewer #2 that the effect of resolved supersaturations vs saturation adjustement is a very interesting and potentially important question.

Reading the manuscript I am quite a bit puzzled that the authors do not show any plots or statistics of the resolved supersaturation of the bin und bulk-explicit models. How high are the supersaturations? Where is the location of the maximum supersaturation, near cloud base or is there a second peak higher up? Are the resolved supersaturations realistic, i.e., do they compare well with observations? Yes, there are probably very few or no observations of supersaturations in an updraft core of a supercell. Then

why did the authors pick this case? Would a maritime congestus cloud (without ice phase) not be much more appropriate to study the effect of resolved supersaturations vs saturation adjustment?

I would also like to see a discussion in the paper whether a vertical grid spacing of 343 m is actually sufficient to resolve the vertical structure of supersaturations in a convective cloud. Would the supersaturation maybe decrease when you increase the vertical resolution? At least when I look at the schematic Fig. 14 the textbook knowledge would say that those supersaturation occur very close to cloud base (few tens of meters), i.e., the simulations presented in the manuscript cannot resolve this at all. Please put some rough numbers on Fig. 14 and compare them with the simulations.

In my opinion, it is not clear and has not been shown in the manuscript that the supersaturations simulated by the bin and bulk-explicity model are realistic. They could be dominated by the numerics (grid spacing, time step) and they could also be affected by the initialization with an unrealistic warm bubble which makes the initial stage of the cloud development much faster than in a realistic cloud development.

In a previous study (Seifert et al 2006, Atmos. Res.) we have compared a bin microphysics model and a two-moment bulk scheme. The agreement was actually quite good and the fact that the bulk model applied a saturation adjustment was only a second-order effect and is therefore not discussed in the paper. In that paper we used 125 m vertical grid spacing which was another reason why we did not discuss the supersaturation, because we thought that a much higher resolution would be necessary for a robust result on this specific question. From that exercise we found that the assumptions about ice microphysics and the details of the warm-rain autoconversion scheme are decisive to get a good agreement between bin and bulk models. Without an effort to make the bulk scheme consistent with the assumptions of the bin scheme (e.g., particle geometries and fall speeds, collision efficencies etc.) such a comparison will, of course, show large differences in the simulations.

For the discussion of the invigoration of convection clouds and the effects of aerosol assumption on cold pools and secondary deep convection the authors should refer also to the earlier articles and just not start with Rosenfeld et al. (2008) (e.g., Andreae et al. 2004, Seifert and Beheng, 2006; Khain et al., 2005; Koren et al., 2005; van den Heever et al., 2006).

Another reference that should be included is

Kogan, Yefim L., William J. Martin, 1994: Parameterization of Bulk Condensation in Numerical Cloud Models. J. Atmos. Sci., 51, 1728–1739.

who also discuss the errors of bulk condensation schemes.

This could be a very important study, but I think it needs some more work and discussion otherwise the conclusions could be misleading.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 10059, 2012.