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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.

Anonymous Referee #1

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In this manuscript, the authors address one of the basic assumptions taken in bulk microphysical models: that supersaturation readjusts instantaneously to equilibrium at every time step. To explore this, different treatments of the condensation/evaporation process, which include an explicit treatment of supersaturation, and, a "full" treatment of bin microphysics with supersaturation are included within the WRF and used to simulate the evolution of deep convection.

The paper is well written, and the studied cases well thought out and insightful. I particularly like the development of the bulk-explicit and bulk-cond schemes to help understand the differences between the bulk-original and bin microphysics approaches.

Overall the topic of study is important, the results are interesting so the study should be published after the following comments are addressed:

1. My main concern is that the manuscript brings up important issues that remain at the discussion level; the simulations presented could be used more completely to support all the points made. For example, in page 10066 (line 17), the condensational timescale can be explicitly computed from the simulation and compared against the integration timestep – ideally you should be able to explicitly show where in the model domain (or lifetime of the storm) the supersaturation adjustment assumption fails and causes the differences seen. Another related example is section 5. There is considerable discussion presented around Figure 14, but you could use the actual simulations to quantitatively support the arguments. There is another reason why Figure 14 should be re-examined quantitatively: the supersaturations presented in Fig. 12 are often so high that most Kohler curves should be effectively indistinguishable from s=0 throughout most of the cloud.

2. Most of the results presented in the study focus presenting differences between simulations at specific times. It is hard to be quantitative when results are presented this way, as e.g., a slight shift in the timeline of storm evolution will show as differences that may not be important. Ideally, expressing more results as PDFs of over the lifetime of the storm (as done for example in Figure 12) is a better way to show differences.

3. In much of the discussion, the adjusted hydrometeor phase was not clearly specified. I would like the authors to be more explicit about this. A discussion on treatments separately for ice, liquid water and mixed-phase clouds would also be appropriate in the introduction.

4. The issue of model resolution (also brought up by A.Seifert) is important, and not addressed in the simulations.

5. I know the authors have tested the supersaturations presented in Figure 12 against predictions with a parcel model, but they still seem to be very high; in fact, some simu-

lations suggest a non-negligible chance of approaching the threshold for homogeneous nucleation. More discussion on this is appropriate.

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