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Interactive Comment

Interactive comment on "Cloud and aerosol effects on radiation in deep convective clouds: comparison with warm stratiform clouds" *by* S. S. Lee et al.

Anonymous Referee #1

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General comments.

This manuscript investigates indirect aerosol effects on radiation via the modification of clouds and precipitation in deep convective and warm stratiform clouds using a cloud-scale model. The authors compare and contrast results for these two different cloud types, while minimizing differences in conditions between the cases. Overall, they find that radiative forcing of clouds and the indirect aerosol effects differ significantly between deep convective (DEEP) and shallow stratiform (SHALLOW) clouds. This is not an especially novel finding (it has been well known that deep and shallow clouds have different radiative impacts for at least 20 years) and in fact is expected from basic principles of thermodynamics and radiative transfer. That said, I think the key point that the authors are making is that almost all current GCMs only include indirect aerosol effects on the liquid component of stratiform clouds; thus, they may be missing potentially important aerosol indirect effects on ice or convective clouds which are likely to be much different (in magnitude and possibly sign) from the indirect effects on liquid stratiform clouds. This is an interesting and important point. Thus, I think this study in principle could make a good contribution to the literature. However, I also question whether this study, as it has been designed, can really address the fundamental question posed here concerning how aerosol indirect effects differ between the shallow liquid clouds and ice or deep convective clouds in terms of the climate impacts. The reasons for this are given in detail below. The writing is good in general but could be improved in terms of its conciseness and flow.

Specific major comments.

1. The results presented here are based on short-duration simulations (24 hours). My question is, how much does this reflect climate impacts versus more transient, short term behavior? The authors discuss significant differences between shallow and deep clouds due to dynamical-microphysical interactions; however, they do not address potential feedbacks of the clouds back on the environment that may be critical over longer timescales, especially for the convective cases. In other words, the impact of aerosols on a cloud system over short timescales may be entirely different than the impact on a field of clouds over longer timescales that is relevant to climate (e.g., see discussion in Grabowski 2006). This is important because the authors place this study in the context of climate impacts - I don't think climate impacts can be addressed in a robust way in this framework. The fact that this study really examines short-term transient behavior may be one reason for the very large aerosol impacts in the deep convective cases on the cloud liquid and ice contents (as shown in Fig. 5). Because of the different nature of the feedbacks between the large-scale environment and SHALLOW and DEEP and hence the different response times for these interactions, the transience of these sys-

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tems may vary significantly and the differences reported by this study may reflect this point. Thus, I think a more robust approach to examine the climate impacts would be to examine the equilibrium behavior of each case (i.e., run the cases until they reach quasi-equilibrium with the applied large scale forcing and boundary conditions). If the authors do want to focus on the transient behavior then they need to shift the focus of the paper away from the climate impacts.

2. The authors suggest that many of the differences between SHALLOW and DEEP (e.g., much greater change in cloud water content in DEEP than SHALLOW with modification of aerosol) are due to stronger microphysics-dynamical interactions in DEEP than SHALLOW because of the greater detrainment, evaporation, and cloud depth. This is then illustrated in Fig. 6 with a schematic diagram showing these microphysical-dynamics feedbacks. This greatly oversimplifies the problem in my opinion and is not well-supported by the results shown here. It also misses the key point that microphysical-dynamical feedbacks are fundamentally different in their nature between stratocumulus and deep convective clouds. In stratocumulus, boundary layer processes, cloud-top radiative cooling, and entrainment at cloud top are key controlling factors in the stably stratified large-scale environment. Microphysics can impact this, for example, by modifying the entrainment rate via evaporation of drizzle in the sub-cloud layer. However, in deep convection, the controlling factors are quite fundamentally different, which is not addressed by the authors. Microphysics can impact both triggering (for example, changing cold pool strength and low-level convergence via changes in evaporation, as is mentioned by the authors and shown in Fig. 6) as well as buoyancy above the level of free convection (for example, by changing the amount of water lofted above the freezing level and subsequent impact on latent heat release during freezing). The authors imply that the mechanisms of dynamic-microphysics interactions are similar in SHALLOW and DEEP (as shown in Fig. 6), but are much stronger in DEEP which leads to the large changes in cloud properties that are seen in this simulation. For example, on p. 15307, it is stated that "The limited vertical extent of shallow clouds reduced differences in evaporative cooling, convergence and updrafts between

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the high and low aerosol cases, leading to smaller increases in condensation and deposition." I believe this misrepresents the fundamental nature of these interactions and their differences between the different cloud regimes.

3. The current manuscript does not adequately address the numerous uncertainties inherent in a study such as this one. For example, what is the impact of various key model assumptions (e.g., ice crystal shape and fallspeed, thresholds for conversion of rimed snow to graupel, rain/snow/graupel size distribution parameters, 2D vs. 3D, vertical resolution, treatment of ice nucleation, etc.) as well as important environmental parameters (e.g., wind shear, surface fluxes, etc.). I realize the authors did investigate in a limited way the sensitivity to CAPE, but certainly there are many other factors involved here that lead to uncertainty. Furthermore, in my opinion the authors did not adequately put their results into the context of prior studies. This would also help to frame the issue of uncertainty (i.e., what are the differences/similarities in the results here compared with previous studies?).

Additional comments.

1. Abstract, "The dependence is also simulated among different types of convective clouds, indicating the assessment of effects of varying cloud types on radiation due to climate changes can be critical to better predictions of climate.". As stated above in major comment #1, I don't believe this study as it has been set up can adequately address these interactions and impacts in terms of climate since this work is focused on short-term transient behavior.

2. p. 15293. Stratiform clouds are also sub-grid in GCMs - that is why these models generally employ cloud fraction/macrophysics schemes.

3. p. 15294. Stratiform clouds are not "considered to be resolved"; by GCMs (see comment #2 above). I think it has also not been established that simulation of changes in the properties of stratiform clouds caused by greenhouse gasses and aerosols is done in a more realistic way as compared to that in sub-grid deep convective clouds.

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4. p. 15298. 500 m for the deep convective case is fairly coarse vertical resolution, especially within the boundary layer. Did the authors explore any sensitivity to vertical resolution? How appropriate is the Hong and Pan PBL scheme at a horizontal resolution of 2 km?

5. p. 15298. Please include a statement here that a more detailed model description is included in Appendix A.

6. p. 15301. The fact that DEEP produces radiative fluxes within 10% of observed fluxes doesn't really demonstrate that "clouds in DEEP are simulated reasonably well." Especially in the case of deep convective clouds with a large optical depth, large biases in cloud (microphysical) properties may not necessarily result in large biases in radiative properties.

7. p. 15303, Suggest replacing subsection title "Radiative properties of clouds" with "Microphysical properties of clouds"

8. p. 15305 and numerous other places. The authors do not describe in adequate detail how various quantities were calculated. For example, how is cloud fraction defined (i.e., was it based on a minimum cloud water content threshold)? How are average in-cloud water contents calculated (i.e., how did you define the locations/times that were in-cloud for the averaging)? What is the "domain-average cumulative condensation"? This has units of mm, which only makes sense if it is a vertically-integrated, temporally-integrated quantity.

9. p. 15307. "...downdrafts with increased intensity from increased evaporation could be accelerated as they descended..". Can't the authors check in their simulations whether or not this occurs? Since this is an important point it should be quantified. Furthermore, the authors describe these results mainly in terms of evaporation of cloud water - what about the relative importance of evaporation of rain? Many previous studies (e.g., Rutledge et al. 1988) have described the importance of rain evaporation in driving the formation of downdrafts and cold pools in MCCs.

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10. p. 15310. "It is expected that environmental conditions do not contribute to these different responses of radiation in this study." The authors need to clarify what they mean by "environmental conditions", since environmental conditions (e.g.„ CAPE, wind shear) do determine cloud-top height and depth, which the authors find are key to explaining the results here.

11. p. 15311. Figure 8 and the accompanying test is not very clear. Specifically, what do you mean by "negative forcing is removed due to the surface moisture flux"? Suggest rewording.

12. p. 15314 (and more general comment). It is my understanding that changing aerosol concentration led to modification of both CCN and IN concentrations. If this is not the case, it needs to be explicitly stated. If this is the case it would be helpful to run additional tests to separate the effects of CCN and IN. This would help to better establish the mechanisms involved in producing the results that are seen here.

13. p. 15315 (and elsewhere). "An additional idealized simulation of warm stratiform clouds with the nearly same environmental conditions as in those in DEEP..." As mentioned above (see comment #11), the authors need to clarify what they mean by "environmental conditions". This term is similarly used several times in the manuscript in other locations, and needs to be clarified.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 15291, 2008.