Review of “Thunderstorm and stratocumulus: how does their contrasting morphology affect their interactions with aerosols?” by Lee et al.

This paper shows that the different morphologies of these two cloud types lead to different aerosol-cloud interactions. Increasing aerosols increases droplets and then leads to intensify downdrafts because of stronger evaporative cooling (as the authors claimed). The acceleration of the intensity of downdrafts is larger in convective clouds due to their larger cloud depths (providing longer paths for downdrafts to follow to the surface) than in stratiform clouds, leading to significantly increased updrafts and an enhancement of precipitation with increased aerosols in convective clouds. The motivation and the goal of the study are good but some of the methods and analyses are questionable. The precipitation from the GCE bulk scheme is not reliable since many thresholds or parameters for auto-conversion are fixed. Li et al 2009a, b (JAS, v66) have indicated the overestimation of evaporative cooling in the squall lines for the GCE bulk scheme compared with the size-resolved cloud microphysical scheme, through overpredicting the intensity of rain and underpredicting the stratiform ice formation. Therefore, the results of the paper is questionable since the authors built their points upon the evaporative cooling for deep convective clouds, which was proven to be much overpredicted by the bulk schemes in the studies of Li 2009a,b and Khain et al 2009a, b (JGR, D22203 and D19209).

Another problem is that the authors never tried to establish robust base runs for the cases from the recent field campaigns and compare with many observational data available in their series publications on aerosol-cloud interactions. This is probably their fourth or fifth paper that the same cases observed in 1997 are used. I noticed that this work was supported by the DOE ARM program and the PIs are very aware of the many aerosol, cloud and precipitation data available from many recent field campaigns (after 2003). Those data are so crucial to look into aerosol-cloud interactions. I am very surprised that authors kept using the old case without detailed aerosol and cloud measurements all these years and did not bother to use the latest available observational data to obtain more solid simulations of clouds and precipitation. Many instruments for in-situ measuring aerosol and cloud data have been developed or updated/corrected and many retrieved algorithms have been developed and updated for radar and lidar measurements in the recent years. Those recent data have been very useful for many modelers to constrain/validate simulations. It is known that great progress has been made on measuring cloud microphysical properties and precipitation, and probably nobody is still using a cloud case in nineties without much constraint/validation of cloud properties and precipitation to look at the aerosol-cloud interactions. The essential thing for a good publication is to use the latest available data that accommodate the recent scientific progress to do research, especially for publications in ACP. Back to 7-8 year ago, the case could be fine since rare in-situ cloud measurements were available and the retrieved algorithms were very limited. For this study, validation of precipitation from the simulation with the measured CCN size distribution is especially necessary. At the SGP site where the cases that used in the paper are located, more instruments measuring precipitation in the recent field campaigns provide reliable precipitation data for modelers to use.

I generally agree that stronger downdrafts will occur in the deep convective clouds than the stratiform clouds, but it could be mainly due to the enhanced updraft from the increased latent heat release (condensational growth) in the polluted case. Evaporative cooling would contribute to the increased downdraft, but obviously the model with the bulk scheme overestimates it's contribution (see 1st paragraph) and thus the results built on that would not be valid. Based on many past studies on the cases with bulk schemes and the size-resolved scheme (e.g., Seifert et al. 2006, Atmos. Res.,v80, Khain et al 2009a, b, and Li et al 2009a, b), bulk microphysical schemes have big problems in simulating precipitation, therefore, the results of the paper built on that could be wrong. Even if it is not, the results can not be generalized because evaporative cooling highly depends on dynamic and thermodynamic conditions (e.g., wind shear and RH). A recent work has indicated the increase of the condensational heating could easily exceed the increase of evaporative cooling by CCN under the weak wind shear conditions (Fan et al 2009, JGR).

Based on the problems that I concerned above, I cannot recommend the paper for publication in ACP. The authors either need to carry out this study based on a well constrained recent case (especially in aerosol and cloud properties and precipitation, or need to constrain their cases with the results from a size-resolved cloud microphysics under the same CCN inputs, with the focus on precipitation and evaporative cooling.

There are some other problems regarding the results and the reasoning throughout the paper. For example, the authors examined the convergence instead of the droplet evaporation rate directly in terms of the role of evaporative cooling, which I do not understand because convergence can be affected by many factors and evaporative cooling is only one of them.