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Interactive comment on "Impacts of Cloud Microphysics Parameterizations on Simulated Aerosol–Cloud-Interactions for Deep Convective Clouds over Houston" *by* Yuwei Zhang et al.

Anonymous Referee #1

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This is a WRF-Chem modeling study using a case of summertime convection in Houston from the ACPC Model Intercomparison Project. The focus is on the indirect aerosol effects on deep convection, using both SBM and Morrison microphysical schemes. The paper is certainly within the scope of ACP. It is well organized and clearly written, with adequate introduction and scientific review. The goals of the study, as elucidated in the first paragraph of section 5, are to (1) evaluate the performance of the WRF-Chem-SBM scheme, (2) explore the differences in aerosol effects on deep convective clouds produced by the SBM and Morrison schemes, and (3) explore the major factors responsible for the differences. The first two goals are descriptive in nature, they are fulfilled and clearly documented. However, I found the deductions made re-

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Discussion paper

garding the third goal to be questionable and poorly-supported by the data presented. The manuscript concludes that, the “warm-phase invigoration” effect is absent with the Morrison scheme, and this is “mainly due to limitations of the saturation adjustment approach for droplet condensation and evaporation calculation”. While the saturation adjustment is probably the root cause, I find it unlikely that it is the DIRECT cause of the simulated sensitivities. Other processes have to be involved, and they need to be identified and properly analyzed. I’ll elaborate on this in my specific comments. This flaw needs to be addressed before the manuscript is published.

Specific Comments:

There are three sets of model sensitivity tests using either realistic anthropogenic aerosol loadings or no anthropogenic aerosol: the explicit SBM scheme, the 2-moment Morrison scheme with saturation adjustment technic, and the Morrison scheme improved with a super saturation formula. The SBM scheme simulated stronger convection and more aerosol sensitivity compared with the original Morrison scheme, whereas the improved Morrison scheme produced results and sensitivities closer to the SBM results. The conclusion followed was that “...the saturation adjustment method for the condensation and evaporation calculation is mainly responsible for the limited aerosol effects with the Morrison scheme.” This should be the correct conclusion, that the limitations in saturation adjustment are the root cause of the simulated differences in sensitivities. However, it cannot be the DIRECT cause. I can think of two pieces of evidence to support my assertion.

1. In the conclusion, the authors stated: “...the saturation adjustment method actually leads to a smaller condensation latent heating than the explicit calculation with supersaturation...” (L407). Fig. 12b was given to support the statement. However, saturation adjustment cannot be the direct reason for the smaller latent heating in Fig. 12b (or in any of the plots in Figs. 11∼14). Figs. 11∼14 only showed mean vertical profiles of various variables for the “top 25 percentiles” of the simulated updrafts. The main reason the Morrison scheme has smaller latent heating in Fig. 12b is not because

of the saturation adjustment, it is because the updrafts are weaker (Fig. 11 a, b). The dynamics already determined the differences in the latent heating, buoyancy, condensation rate, et al. shown in Figs. 11∼14, not the other way around. In other words, the top 25 percentile of the updrafts are already weaker in the Morrison scheme simulation. As a result, latent heating should be weaker. Whether saturation adjustment causes this or not cannot be established by Figs. 11∼ 14.

2. If saturation adjustment were the immediate/main cause of the simulated sensitivities, then the original Morrison scheme should produce stronger convection than SBM, given the same aerosol loading. This is because the saturation adjustment converts ALL supersaturation into cloud water, and thus should release the most latent heating among all schemes used. The fact that the SBM_anth case has much stronger convection than MOR_anth clearly precludes this possibility. If the authors plot Fig. 12 for the same vertical velocity (or super saturation), the Morrison scheme should have more latent heating, not less.

In conclusion, the saturation adjustment cannot be the direct cause of the simulated sensitivities. Something else must interact with it to cause these sensitivities. The authors actually observed the oddity of their conclusion in their conclusion, L401∼L405. They noted that their study differs from Lebo et al. (2012). In this sense, Lebo et al. (2012) gave a feasible explanation, that the the "cold-phase invigoration" is in play together with saturation adjustment. The current case study may or may not have the same mechanism. Nevertheless, the authors need to find the missing link between the saturation adjustment, which produces the maximum possible latent heating by eliminating all super saturation, and the enhanced convection when super saturation is allowed.

Another result that puzzles me comes from Fig. 9a, where the high aerosol loading cases (SBM_anth and MOR_anth) rain earlier than the low aerosol cases. Why? The conventional wisdom is the opposite. High aerosol loading will produce more, smaller cloud droplets, reducing auto conversion and delaying surface rainfall onset. Can this be checked and explained?

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