Responses to Reviewer 1

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 regarding 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.

We thank the reviewer for recognizing the importance of our study and the nice summary. The reviewer's comment about through what interactions the saturation adjustment does not lead to the convective invigoration as the explicit supersaturation approach is very constructive. We have done more analysis with three figures added (Fig. 16-18). Our detailed response is provided as below.

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.

Thanks for the constructive comments. We addressed (1) and (2) together here since both of them are for the same issue: why the saturation adjustment approach leads to smaller condensational heating than the explicit supersaturation approach in Morrison Scheme and through what interactions it did not lead to the convective invigoration as the explicit supersaturation approach did.

As added in Line 416-444, "Now we explain why the saturation adjustment approach leads to smaller condensational heating than the explicit supersaturation approach in Morrison Scheme and why it leads to a smaller sensitivity to aerosols compared with the explicit supersaturation approach. We examine the time evolution of latent heating, updraft, and hydrometeor properties. At the warm cloud stage at 1700 UTC, the saturation adjustment indeed produces more condensational latent heating which leads to larger buoyancy and stronger updraft intensity compared to the explicit supersaturation because of removing supersaturation (Fig. 16, left, blue vs. orange). By the time of 1900 UTC when the clouds have developed into mixed-phase clouds, the saturation adjustment produces less condensational heating and weaker convection than the explicit supersaturation approach (Fig. 16, middle). The results remain similarly later at the deep cloud stage 2100 UTC (Fig. 16, right).

How does this change happen from 1700 to 1900 UTC? At the warm cloud stage (17:00 UTC), the saturation adjustment produces droplets with larger sizes (up to 100% larger for the mean radius) than the explicit supersaturation because of more cloud water produced as a result of zeroing-out supersaturation at each time step (droplet formation is similar between the two cases as shown in Fig. 13). This results in much faster and larger warm rain, while with the explicit supersaturation rain number and mass are absent at 1700 UTC as shown in Fig. 17d and 18d). As a result, when evolving into the mixed-phase stage (19:00 UTC), much fewer cloud droplets are transported to the levels above the freezing level (Fig. 17b and 18b). Whereas with the explicit supersaturation, because of the delayed/suppressed warm rain and smaller droplets (the mean radius is decreased from 8 to 6 µm at 3 km), much more cloud droplets are lifted to the higher levels. Correspondingly, a few times higher total ice particle number and mass are seen compared with the saturation adjustment (Fig. 17g and 18g) because more droplets above the freezing level induce stronger ice processes (droplet freezing, riming, and deposition). This leads to more latent heat release (Fig. 16e), which increases the buoyancy and convective intensity. With the explicit supersaturation, increasing aerosols leads to a larger reduction in droplet size (up to 1 µm more in the mean radius) than the saturation adjustment, therefore more enhanced ice microphysical processes and the larger latent heat. Besides, the condensational heating is more enhanced by aerosols with the explicit supersaturation (Fig. 16). Together, a much larger sensitivity to aerosols is seen with the explicit supersaturation."

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?

The warm rain is very weak (analysis box averaged rain rate at ~0.02 mm hr-1) and the time period is short (~10min), so the delay of warm rain is too hard to be shown from Fig. 9a. We do see the delay of warm rain by aerosols but only about 5 min (probably due to the humid condition of the case study). We have added the following clarifications to the revised manuscript in Line 335-339, "Note Fig. 9a shows that anthropogenic aerosols lead to an earlier start of the precipitation with both SBM and Morrison, which reflects the faster transition of warm rain to mixed-phase precipitation. We do see the delay of warm rain by aerosols but only about 5 min (probably due to the humid condition of the case study), which is difficult to be shown in Fig. 9a since averaged rain rate for the analysis box is ~0.02 mm hr-1 and the time period is very short (~10 min)".