Responses to Reviewer 2

This manuscript presents results of numerical simulations that consider impacts of cloud microphysics parameterization on convective clouds near Houston. Overall, this is an impressive study that includes simulation and validation of aerosols that play key role in cloud dynamics and microphysics, and subsequently investigates the CCN impact on convective dynamics. My main problem is with the context of this study and with the interpretation of model results. Obviously, the authors are strongly for the invigoration and I am one of those who oppose their views as scientifically unjustified. The manuscript should provide less biased view of the invigoration and needs to include additional analysis of model results as suggested in my specific comments.

We thank the reviewer for your time and constructive comments. We have provided detailed responses point-by-point as below.

Major comments:

1. The introduction needs to provide a better context for this work. A brief discussion of invigoration in the second paragraph of the introduction is misleading. It presents the authors view that is not supported by simple arguments and by other studies. For instance, the "cold-phase invigoration" as described in lines 42-45 is simply not possible because the latent heat released by freezing the cloud water carried across the melting level in the polluted case only balances the weight of the water carried upwards. So where does the invigoration come from? The explanation of the "warm-phase invigoration" is simply incorrect and it repeats the incorrect argument used in papers the authors cite. The latent heating does not depend on the droplet concentration and droplet radius as long as the updraft velocity does not change. This is strictly true when the in-cloud supersaturation is equal to the quasi-equilibrium supersaturation. The validity of the quasi-equilibrium supersaturation approximation has been argued in many studies, at least in the absence of ice (e.g., Politovich and Cooper JAS 1988). Such an incorrect interpretation is repeated in lines 334-337. I suggest the authors consult the recently accepted manuscript by Grabowski that provides a thorough discussion of the two invigorations, see section 2 there. The manuscript is available on EOR in JAS (https://journals.ametsoc.org/doi/abs/10.1175/JAS-D-20-0012.1). I also suggest the authors consult and cite a paper by Varble (JAS 2018) for a less biased discussion of the invigoration problem.

First, for the "cold-phase invigoration", the latent heat released by freezing is not compensated by the mass of cloud water. The buoyancy is mainly contributed from the thermal term corresponding with latent heating and hydrometeor loading. The freezing process does not lead to any change in the hydrometeor loading, so drop freezing increases the buoyancy through latent heat release of fusion. Also, the ice particles after freezing may participate in other ice microphysical processes (i.e., deposition and riming) to further release latent heat.

Second, for the "warm-phase invigoration", there is limitation to use quasi-equilibrium supersaturation as an estimation of in-cloud supersaturation. Previous studies (e.g., Korolev and Mazin 2003, Pinsky et al. 2013, Politovich and Cooper 1988) showed that the quasi-steady assumption is invalidated in conditions of (a) low droplet concentrations (pristine condition) and (b) intense condensation and evaporation (e.g., cloud base and strong updrafts) due to long relaxation time (larger than a few seconds). Figure r2 shows modeled supersaturation and quasi-equilibrium supersaturation, updraft velocity, phase relaxation time and droplet number concentration from the simulations SBM_noanth and SBM_anth. The calculation of S_{eq} and phase relaxation time τ follows Eq. 3 and Eq. 4 in Pinsky et al. (2013), respectively. Seq is much higher than the true supersaturation especially above cloud base (Fig. r2a) where low droplet number concentration (Fig. r2c) and strong updrafts (Fig. r2b) are seen. The averaged τ are around 10-15 s. The exact closed supersaturation equation (Eq. 6 in Pinsky et al. 2013) shows that condensation depends on droplet number and size, and more droplets in the polluted clouds increase condensation and decrease supersaturation. Although S_{eq} in SBM_anth is indeed much smaller than SBM_noanth, assuming $S \approx S_{eq}$ would lead to some invigoration effect but the effect would be much reduced because the condensation rate in the clean condition would be significantly overestimated by S_{eq}.

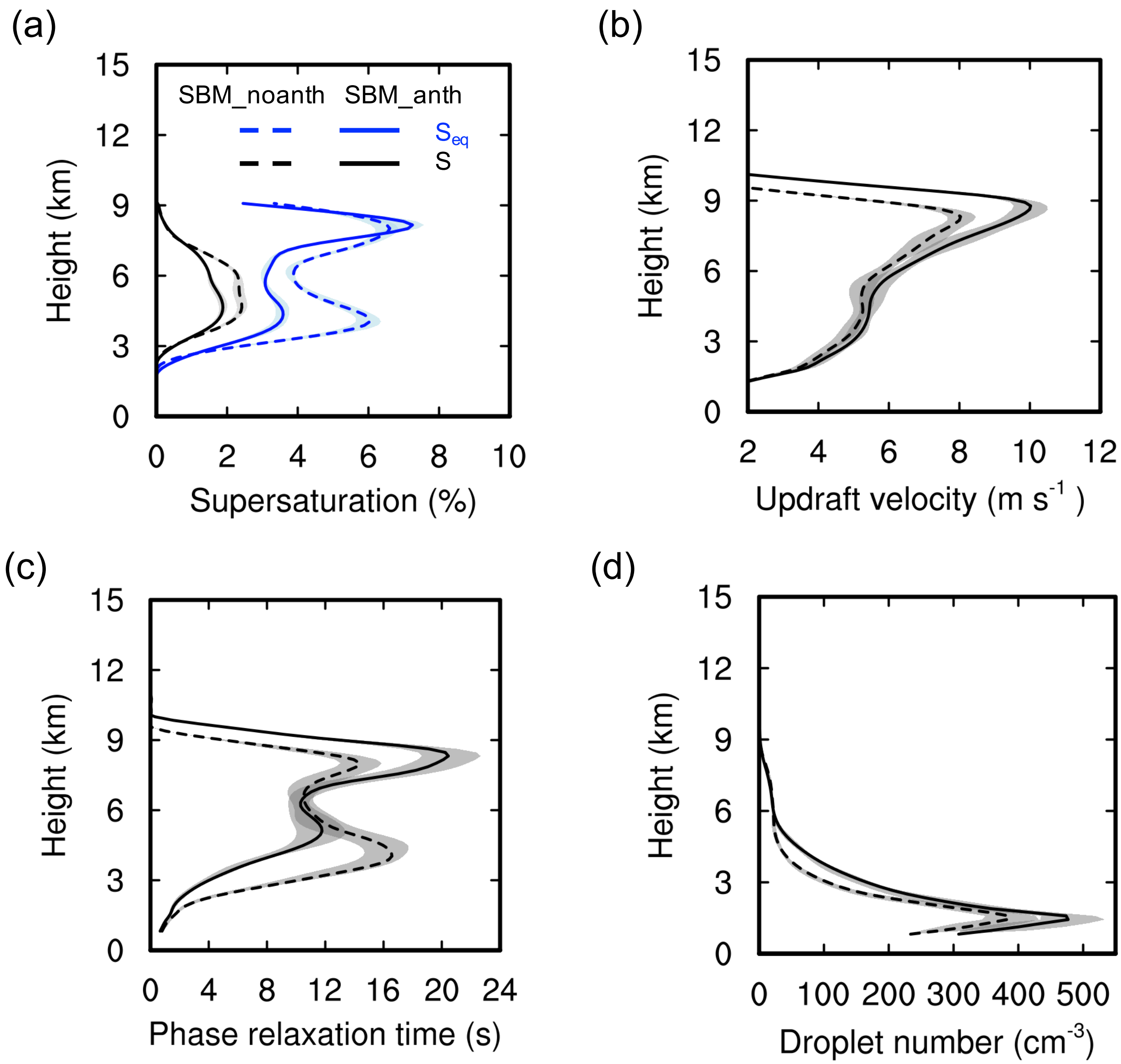

Figure r2 Vertical profiles of (a) supersaturation and quasi-equilibrium supersaturation, (b) updraft velocity, (c) phase relaxation time and (d) Droplet number concentration averaged over the updrafts with value greater than 2 m s−1 from the simulations SBM_anth and SBM_noanth, MOR_SS_anth and MOR_SS_noanth over the analysis domain as shown in the red box in Figure 7 during the strong convection period (2000 – 2300 UTC, 19 Jun 2013). The shaded area marks the spread of ensemble runs.

2. The discussion of bulk versus bin microphysics starting in l. 61 misses an important point: not all bulk schemes apply saturation adjustment. For instance, the scheme of Morrison and Grabowski (JAS 2007, 2008a,b) allows supersaturation to evolve. The scheme shows a good agreement with bin microphysics in simple tests. This is important for the context of simulations described in the manuscript under review.
Thank you for pointing out this. We added "Instead of using saturation adjustment, some bulk schemes allow supersaturation to evolve (e.g., Morrison and Grabowski 2007, 2008) and show a good agreement with bin microphysics in simple tests." after the discussion of saturation adjustment.

3. The setup of model simulations is not clear to me. I understand the motivation for applying the same boundary conditions for the inner domain in all simulations and hence using the MERRA-2 data on the inner-domain boundaries. However, how this is done with the outer domain present in not clear to me. Is it fair to say that outer domain is ran initially without the inner domain to simulate aerosol evolution and then the inner domain simulations are run without the outer domain using boundary conditions from MERRA-2 for the dynamics and thermodynamics, and applying the outer domain data for the aerosols? In other words, simulations with the two nested domains are actually never run together, correct? If my understanding is correct, then the description on p. 8 and 9 needs to change along my suggestion above. Also, it would be useful to describe in more detail the vertical grid structure. The 51 levels suggest quite a low vertical resolution.
Yes, you are right. Two domain simulations were run separately and an important purpose of running outer domain is to get a good estimation of aerosol fields to feed to inner domain. The 51 vertical grid levels allow 50-100m resolution below 2km and ~500m above, which is not very high resolution but not too bad.

4. The description of the simulation setup mentions 3-member ensembles. However, the ensemble information is never shown in the discussion of results. I think this is important because one may wonder to what extent a specific realization of the convection development affects the comparison. In other words, are the differences systematic or coincidental? All profiles shown in the figures should include the ensemble spread. Also, Fig. 7 should show all ensemble members and not just one realization. Specifically, is the more organized bin microphysics convection present in all ensemble members when compared to a more scattered bulk convection, or is this true only for the example shown in Fig. 7?
Line 222-225 stated that all analysis results for Domain 2 simulations in this study are the ensemble mean. As you suggested, we added shaded area for the ensemble spread for all profiles (Fig. 9a and Fig.11-14). All three ensemble members are added to Fig.7. Overall, MOR has more scattered convections then SBM.

5. Although never mentioned in the manuscript, the vertical resolution near the cloud base is too low to properly resolve CCN activation in the bin scheme. It is well known that the vertical grid

length around 10 m is needed to resolve the cloud base supersaturation maximum. Poor representation of cloud base activation affects droplet concentrations. In fact, droplet concentrations simulated by the two schemes are never compared in the paper. This key parameter should be analyzed and presented. Should the bin scheme use parameterization of the cloud base CCN activation as the Morrison scheme?

The vertical resolution near the cloud base is around 70m, which is not too bad. Increasing vertical levels would also increase a lot of computation cost for so many sensitivity tests and ensemble run. The hydrometeor number concentration is shown in Fig. r3 (will also add to the manuscript). The bin scheme has explicit supersaturation that can be used for CCN activation based on the Köhler theory, which is more realistic. For a baseline simulation of a real case, it is not good to simply change to the droplet activation parameterization as the Morrison scheme. We may try a sensitivity test by changing to the CCN activation parametrization as the Morrison scheme to isolate the responses of droplet activation and diffusional growth as a future work.

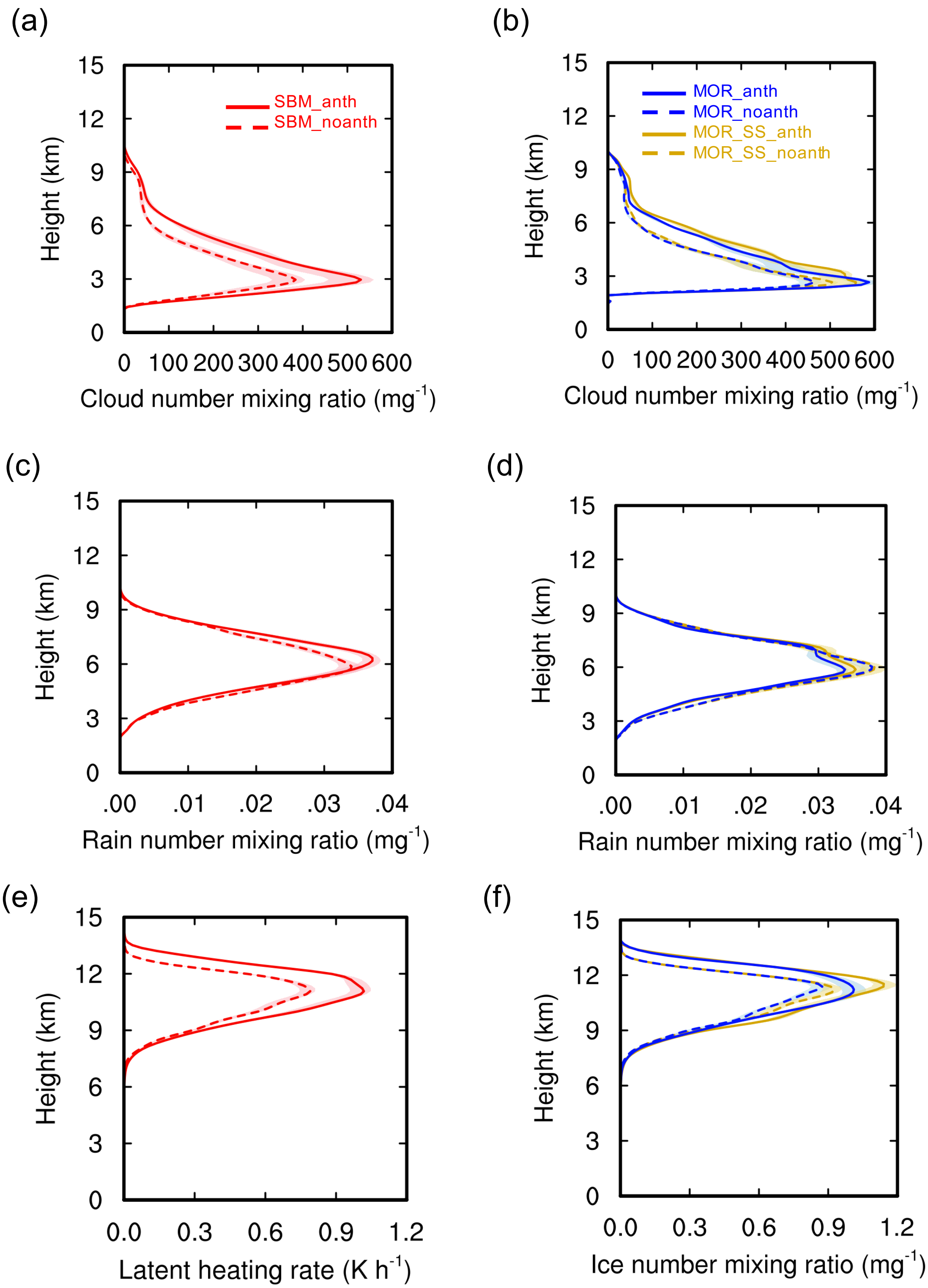

Figure r3 Vertical profiles of (a, b) cloud droplet, (c, d) rain drop and (e, f) ice particle (including ice, snow and graupel) number mixing ratios averaged over the top 25 percentiles (i.e., 75th to 100th) of the updrafts with value greater than 2 m s−1 from the simulations SBM_anth, SBM_noanth, MOR_anth, MOR_noanth, MOR_SS_anth and MOR_SS_noant over the analysis domain as shown in the red box in Figure 7 during the strong convection period (2000 – 2300 UTC, 19 Jun 2013)

6. Saturation adjustment and its role in the simulations. I think this aspect is poorly represented in the manuscript. First, one needs to clearly explain that saturation adjustment affects cloud buoyancy and thus simulated vertical velocity. The impact on the cloud buoyancy has been shown theoretically in Grabowski and Jarecka (JAS 2015) and discussed in the context of deep convection simulation in Grabowski and Morrison (JAS 2017). There are two aspects: 1) the increase of the vertical velocity because of the increased buoyancy (that does lead to the increased condensation), and 2) the increase of the condensation rate when the updraft is the same (this is because reducing supersaturation to zero gives more condensation). One way to separate the two effects is to show the condensation rate for a given vertical velocity (at a given height) and then repeat it for different vertical velocities. And do it separately for bin and bulk schemes. I expect that in undiluted or weakly diluted cloudy volumes the condensation rate is similar for the same vertical velocity in the two schemes and for the two aerosol conditions. I leave it to the authors to figure out what it means if my prediction turns out correct. Note that such an analysis eliminates the impact of different convection realizations and properly demonstrates the impact of the microphysics scheme on the condensation rate.
Fig. r4 shows the condensation rates for the same vertical velocity (1m s^{-1} and ~5m s^{-1}) at 3-km for SBM_anth and MOR_anth. Clearly, the condensation rates range quite much for the same updraft velocity. Because the condensation rate $C_d \sim N\, r\, S$ (N for droplet number and r for droplet radius, and should be only dependent on updraft w if assuming $S = S_{eq} \sim w\, (N\, r)^{-1}$. So, besides the updraft, the droplet number and size also affect the condensation rate. From Fig. r4, the change of the condensation rate between SBM_anth and MOR_anth when the updraft is the same is not that significant. The first effect that increased updrafts because of the increased latent heating is dominant (shown in Fig. 11).

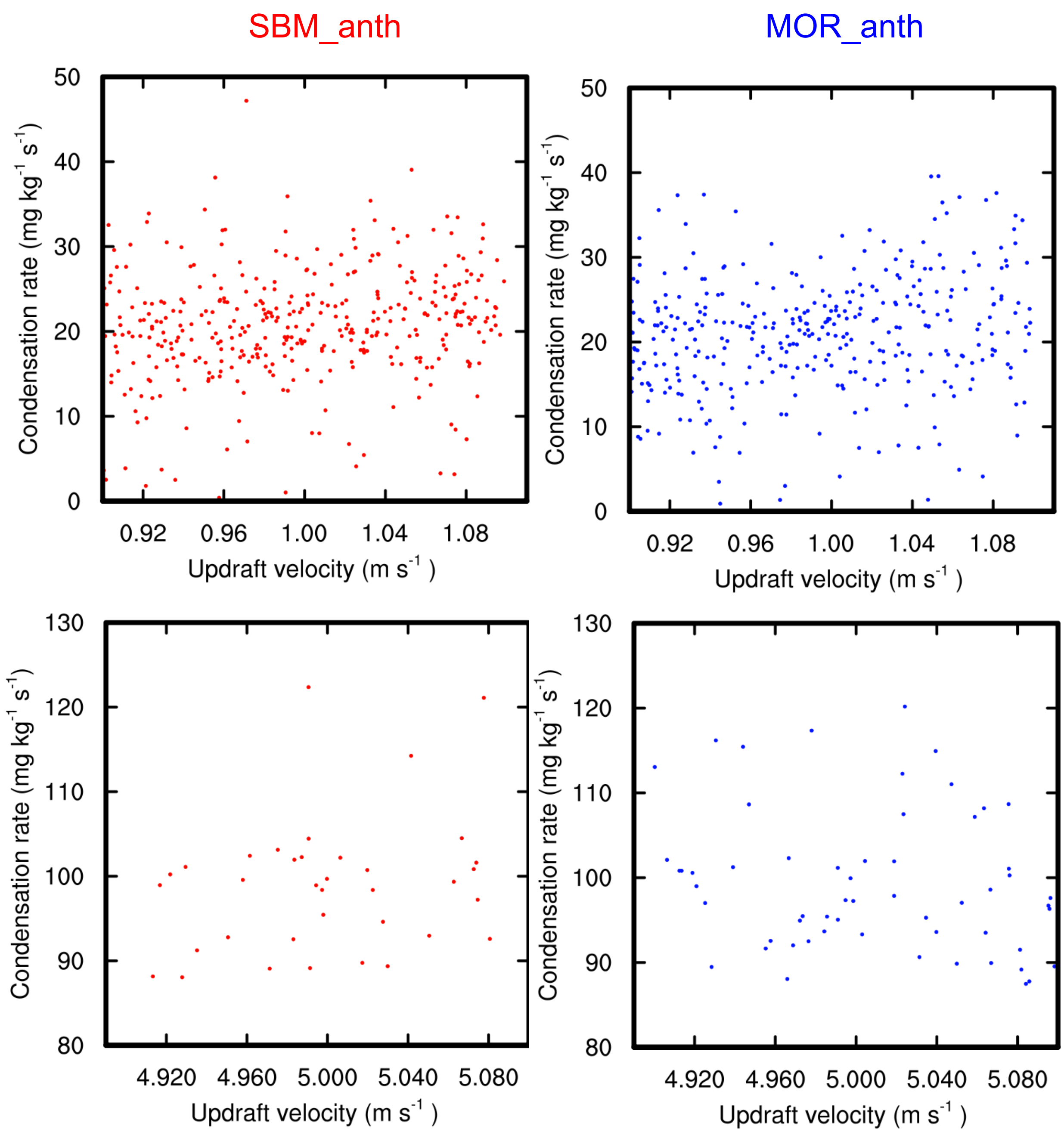

Figure r4 Condensation rate at 3km when w=1±0.1 m s^{-1} (first row) and w=5±0.1 m s^{-1} for SBM_anth and MOR_anth.

7. Saturation adjustment may also affect the way ice processes are simulated. Grabowski and Morrison (JAS 2017) document some possible impacts. This aspect begs the question about the representation of ice processes in the two schemes. I expect there are differences that are never discussed in the paper. Specifically, are ice concentrations similar between the two schemes? If there are significant differences, these have significant implications for the simulated cloud processes. As with the cloud droplet concentrations, this is never shown and discussed in the paper.

There is no significant difference in ice number concentration between SBM and MOR (Fig. r3).

8. Related to some of the points above: How different is the supersaturation simulated by the bin scheme from the quasi-equilibrium supersaturation below the freezing level? The quasi-equilibrium supersaturation can be derived from the local updraft velocity and droplet spectral characteristics. I expect the two are quite close in undiluted or weakly diluted cloudy volumes as suggested by other studies. If so, then please see comment 1 above.
The quasi-equilibrium supersaturation is much larger than simulated supersaturation between 3-5 km (below the freezing level) with more than 10 s relaxation time, which is mainly due to low droplet number. Please see the reply to comment 1 for more details.

Specific comments:
1. The abstract requires revisions after major comments above are addressed.
The abstract has been polished.

2. Grabowski and Jarecka (2015) show that the key impact in shallow convection simulations is the way saturation adjustment affects cloud edge evaporation (either resolved or because of the numerical diffusion). This aspect is never mentioned in the manuscript under review, but perhaps the cloud water evaporation plays some role, for instance, by driving stronger cloud-edge downdrafts when saturation adjustment is used.
We added this aspect into the discussion part.

3. L. 162: The grid length of the MERRA data should be mentioned here.
The MERRA data is at the resolution of 0.5° × 0.625°.

4. L 198: Rather than sending the reader to Lebo et al. (2012), please explain what is meant by "explicit representation of supersaturation over a time step". Is this close to the quasi-equilibrium supersaturation?
The rate of change in the water vapor surplus (η, the difference between the water vapor mixing ratio and the saturation water vapor mixing ratio) as a function of time (t) as:

$$\frac{d\eta(t)}{dt} = D - G\eta(t)$$

where G is a function of temperature (T), pressure (P), droplet mass, and number concentration, i.e., the loss (gain) of water vapor due to condensation (evaporation), and D represents the dynamical forcing (including advected tendency in the water vapor surplus, changes in the water vapor surplus due to adiabatic compression/expansion, and the effect of changes in pressure on η. The above equation is integrated analytically over the time step, and the integrated water vapor surplus is, in turn, used to explicitly calculate condensation/evaporation.

5. L. 229. I would not call the agreement shown in Fig. 3 "very good". This would imply that a color inside each circle is as in the background. This is not the case in several circles. Similar comment applies to Figs. 4 – 6. I understand the difficult task the model faces, but being honest about the simulation drawbacks would be appropriate. For instance, in Fig. 6, the temperature and wind simulations are closer to each other than to the observations.
We try to avoid using "very good". It is very difficult to get model simulation having very good agreement with the observation. The observation (i.e., satellite retrieval of CCN) also has its own uncertainty.

6. What is “thermal buoyancy”? I think this is just “buoyancy”, correct? Please change.
Thermal buoyancy is the buoyancy contributed from temperature and moisture perturbation. Buoyancy is mostly attributed to thermal buoyancy and hydrometeor loading.

7. Fig. 8. To me, the simulations look close to each other and different than the NEXRAD picture. Is the plot for all three ensemble members? This needs to be clearly stated.
Yes, this is for all three ensemble members. Line 222-225 stated that all analysis results for Domain 2 simulations in this study are the ensemble mean. This information is now added to the figure captions. The major differences show at the range of very large reflectivity.

8. Fig. 9. Again, are the plots for all ensemble members? How large is the variability among the ensemble members? I suggest to show the total accumulation in addition to the rate. Total accumulation tends to eliminate the impact of statistical fluctuations due to different flow realizations.
Yes, this is for all three ensemble members. The shaded area shows the ensemble spread. We also add the information of precipitation accumulation in the text.

9. Fig. 10. Again, all ensemble members? How different are the figures for individual ensemble members? If they are much different, then more ensemble members are needed.
Yes, this is for all three ensemble members. The differences between the individual ensemble members is not very much. And also considering the expensive computation cost, we decide to keep at the current three members.

10. Fig. 13. Again, all ensemble members? What is the “drop nucleation rate”? Is this “CCN activation rate”? To what extent it is affected by the low vertical resolution?
Yes, this is for all three ensemble members. Droplet nucleation rate is also the CCN activation rate. The vertical resolution below 2km is around 50-100m, which is not too bad. The comparison of cloud base CCN with observation in Fig. 5 also shows that the model could get reasonable CCN activated at cloud base with the current vertical resolution.

11. For figures 1 -14, it is not clear how the averaging is done. For instance, if there are differences in the number of updrafts but their strength does not change, some of those profiles would change as well, correct? I think one has to clearly explain how the averaging is done to get a clear picture of the processes involved. And document the ensemble spread. As an example, see section 6 in Grabowski’s manuscript (JAS 2020, Early Online Release) that discusses the incorrect interpretation of the enhanced lighting over south-east Asia shipping lines. More latent heating may simply come from a larger number of convective updrafts, not necessarily stronger updrafts.
The average is done only over the grid points satisfying the thresholds described in each figure caption, meaning other grid points failed to meet the thresholds are treated as missing value and not accounted for the average. The ensemble spread is marked as shaded areas for all profiles.

Reference

Korolev A., and I. Mazin, 2003: Supersaturation of water vapor in clouds. J. Atmos. Sci., 60, 2957-2974.

Pinsky M., I.P. Mazin, A. Korolev, and A.P. Khain, 2013: Supersaturation and diffusional droplet growth in liquid clouds. J. Atmos. Sci. 70, 2778-2793.

Politovich, M. K., and W. A. Cooper, 1988: Variability of the supersaturation in cumulus clouds. J. Atmos. Sci., 45, 1651–1664, doi:10.1175/1520-0469(1988)045,1651:VOTSIC.2.0.CO;2.