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. Our detailed point-by-point responses are provided below. As the reviewer is one of those who oppose the convective invigoration concept, we are standing at the other side as one of those who support the concept based on our theoretical analysis and modeling studies (but we do not mean that it occurs in every case since in reality many other factors are in play). The two sides of arguments have been existing for a while, and it is not the role of this paper to resolve this issue. We have submitted a comment paper on Grabowski and Morrison (2020, 2016) to J. Atmos. Sci. to detail the theoretical analysis and modeling designs between the two arguments, which would allow both sides to further discuss there.

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.

We put our responses to the convective invigoration questions that the reviewer raised separately at the end of this file to avoid a distraction. Here, in the introduction, we have added text to provide different arguments existing in literature.

For the cold-phase invigoration, we have added in Line 51-56 "Grabowski and Morrison (2016; 2020) rejected this invigoration concept by arguing that the increase in the buoyancy by freezing

is completely offset by the buoyancy for carrying the extra cloud water across the freezing level. However, Rosenfeld et al. (2008) showed that the buoyancy restores and increases after the precipitation of the ice hydrometeors that form upon freezing of the high supercooled liquid water content into large graupel and hail (Rosenfeld et al., 2008)".
For the warm-phase invigoration, we have added in Line 63-71 "Grabowski and Morrison (2020) proposed a different interpretation of the warm-phase invigoration from the literature listed above. They argued that condensation rates only depend on updraft velocity with the quasi-steady assumption (i.e., the true supersaturation is approximated with the equilibrium supersaturation), therefore they interpreted that it is the lower equilibrium supersaturation in polluted conditions that lead to a larger buoyancy, thus enhanced updraft speeds, and condensation. Several studies showed that the quasi-steady assumption is invalidated in the conditions of low droplet concentrations (Politovich and Cooper, 1988; Korolev and Mazin, 2003) or acceleration of vertical velocity (Pinsky et al., 2013)".
We also added text of "Meteorological buffering effects were also found for aerosol effects on convective clouds over a large region and sufficiently long-time (over a few days and weeks) simulations (Stevens and Feingold, 2009; van den Heever et al., 2011). Dagan et al. (2019) showed that the lifetimes of cloud systems are mostly much shorter than that and rarely reach this buffering state" in Line 75-79 and "Confidently isolating and quantifying an aerosol deep convective invigoration effect from observations requires very long-term measurements: data of 10 years are still not enough over the South Great Plains due to the large variability of meteorological conditions (Varble, 2018)" in Line 80-83.

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.

What we said is that saturation adjustment is an often-used approach in bulk scheme, so our description should have no problem. But we have added a sentence to describe the bulk schemes used the explicit supersaturation, i.e., "Some bulk schemes take the explicit supersaturation approach to allow supersaturation to evolve (e.g., Li et al., 2008; 2009a; Morrison and Grabowski 2007, 2008)." (Line 92-93).

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, two nested domains were run separately, and the purpose of running outer domain is to get a good estimation of aerosol fields to feed to inner domain for the initial and boundary chemical and aerosol conditions. We have added a sentence in Line 179-181 to clearly state this, "The simulations for Domain 1 and Domain 2 are run separately and the Domain 1 simulations serve to provide the chemical and aerosol lateral boundary and initial conditions of Domain 2."
The 51 vertical grid levels allow 50-100m resolution below 2-km altitude and ~500 m above it (added in Line 178-179), 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?

We presented the ensemble mean results in most of the analysis results for Domain 2 simulations. As the reviewer suggested, in the revised manuscript, we added the shaded areas for the ensemble spread for all the profile figures (Fig. 9a and Fig.11-14). For the spatial distribution figure (Fig. 7), we now show the results for each ensemble member. Yes, SBM has more organized convection than MOR in all three ensemble members. This information has been added to Line 297-298, "All three ensemble members consistently show smaller but more scattered convective cells with the Morrison scheme compared with 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 droplet nucleation rate (i.e., aerosol activation rate) was indeed shown in Figure 13. The droplet nucleation rates simulated by SBM is comparable with the parameterization used in Morrison scheme, as shown in Figure 13. In this response letter, we also further showed the spatial distribution of droplet number concentration at cloud base: droplet number concentration at cloud bases in SBM_anth are similar with the observation and MOR_anth in magnitudes, suggesting the SBM is doing an okay job in cloud base activation.

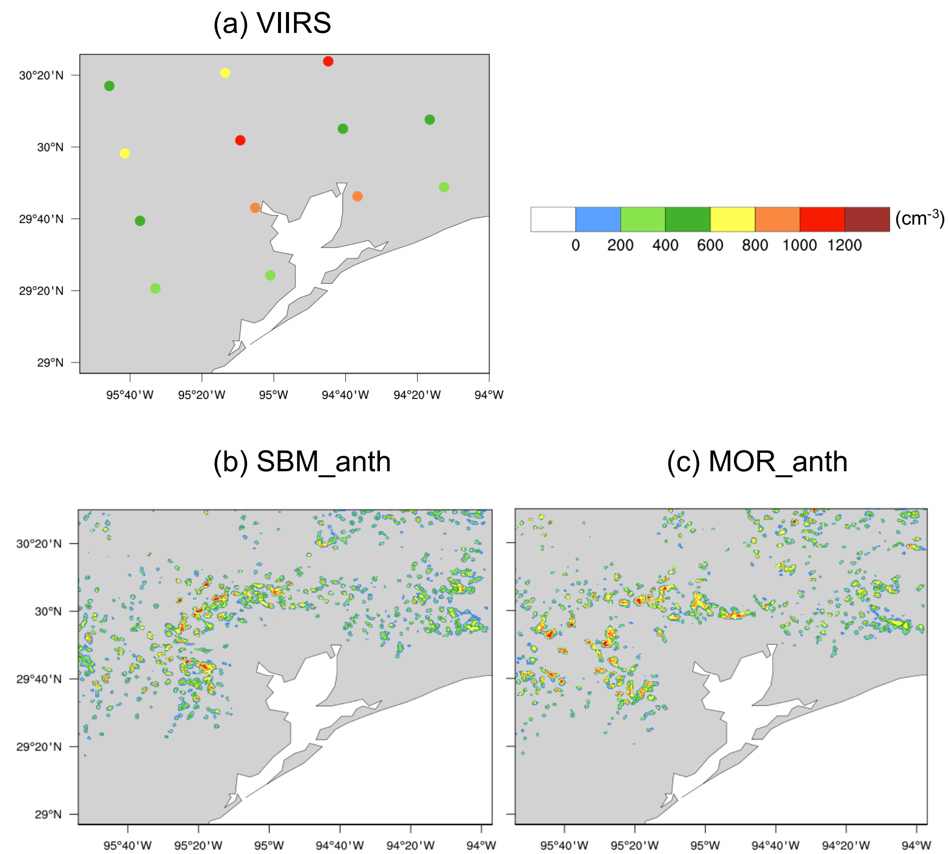


Fig. r1 CCN number concentration at cloud base from (a) VIIRS satellite retrieved at 1943 UTC (Rosenfeld et al. 2016) and model simulation (b) SBM_anth, (c) MOR_anth at 2000 UTC, 19 June 2013.

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.

For the role of saturation adjustment, we have added more analysis as shown in Fig. 16-18 and two paragraphs (Line 416-444) to the revised manuscript, also to address a comment from Reviewer #1.

“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”.

To satisfy the reviewer’s curiosity about the relationship of condensation rate and vertical velocity, we plot their relationships in the simulations with the two schemes and for the two aerosol conditions at two different heights over the period 16-18 UTC where the warm cloud dominated (Fig. r2 and r3). For the same updraft velocity, the Morrison scheme with the saturation adjustment predicted larger condensation rates compared with SBM as expected because reducing supersaturation to zero gives more condensation (Fig. r2-r3, left vs right). The larger condensation rate leads to larger buoyancy and therefore strong updraft velocity as shown in Fig. 17. With the anthropogenic aerosols added, the condensation rate is not changed much with the saturation adjustment at both altitudes (right panels in Fig. r2-r3) because the approach removes the dependence of condensation on droplet properties. However, in the bin scheme, we find that SBM_anth tends to have larger condensation rates for the same updrafts than SBM_noanth (Fig. r3, a vs c) above cloud base where the increase of cloud droplet number is significant (Fig. 15a). This clearly shows that higher droplet number has larger condensation rate for the same vertical velocity, which is different from what the reviewer predicted because the reviewer’s argument is that that the condensation rate is only dependent on updraft, not droplet properties, which is true for saturation adjustment approach, not for the explicit calculation in SBM.

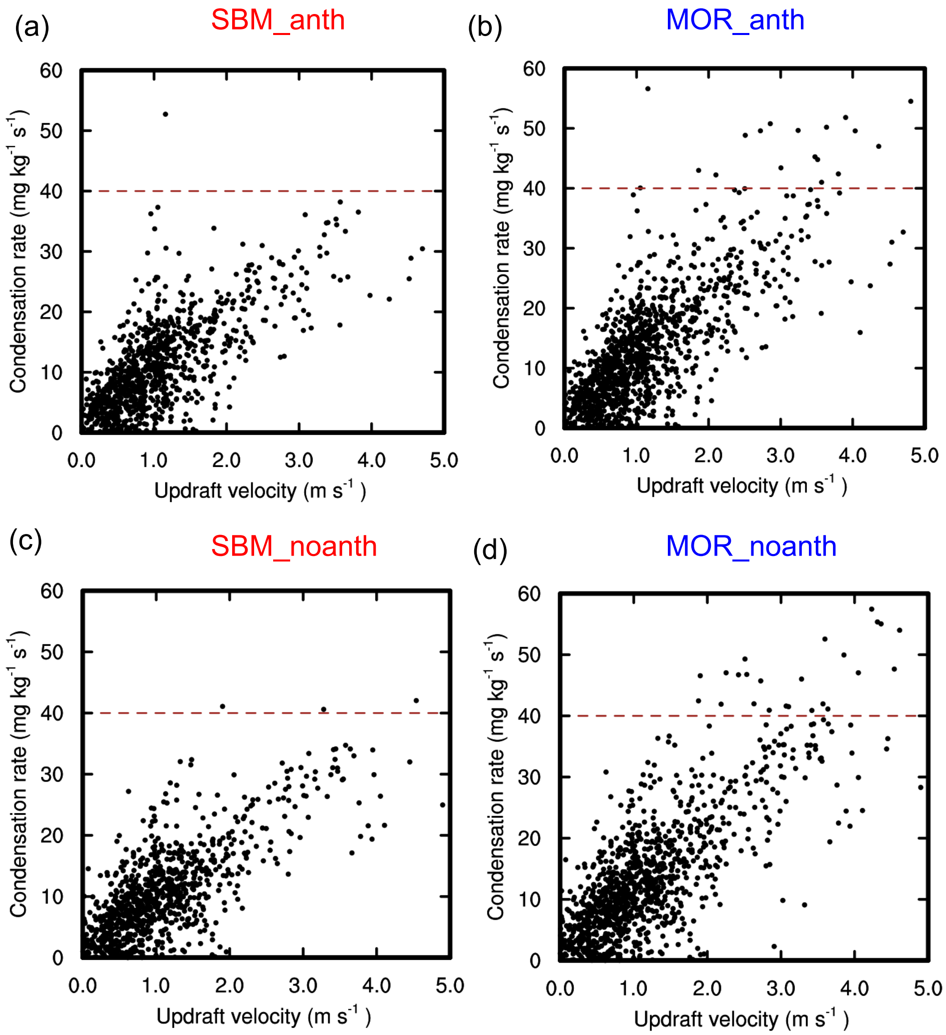


Figure r2 The relationship between condensation rate and updraft velocity at 1.7 km (near cloud base) for SBM_anth, SBM_noanth, MOR_anth and MOR_noanth at warm cloud stage (1600 – 1800 UTC).

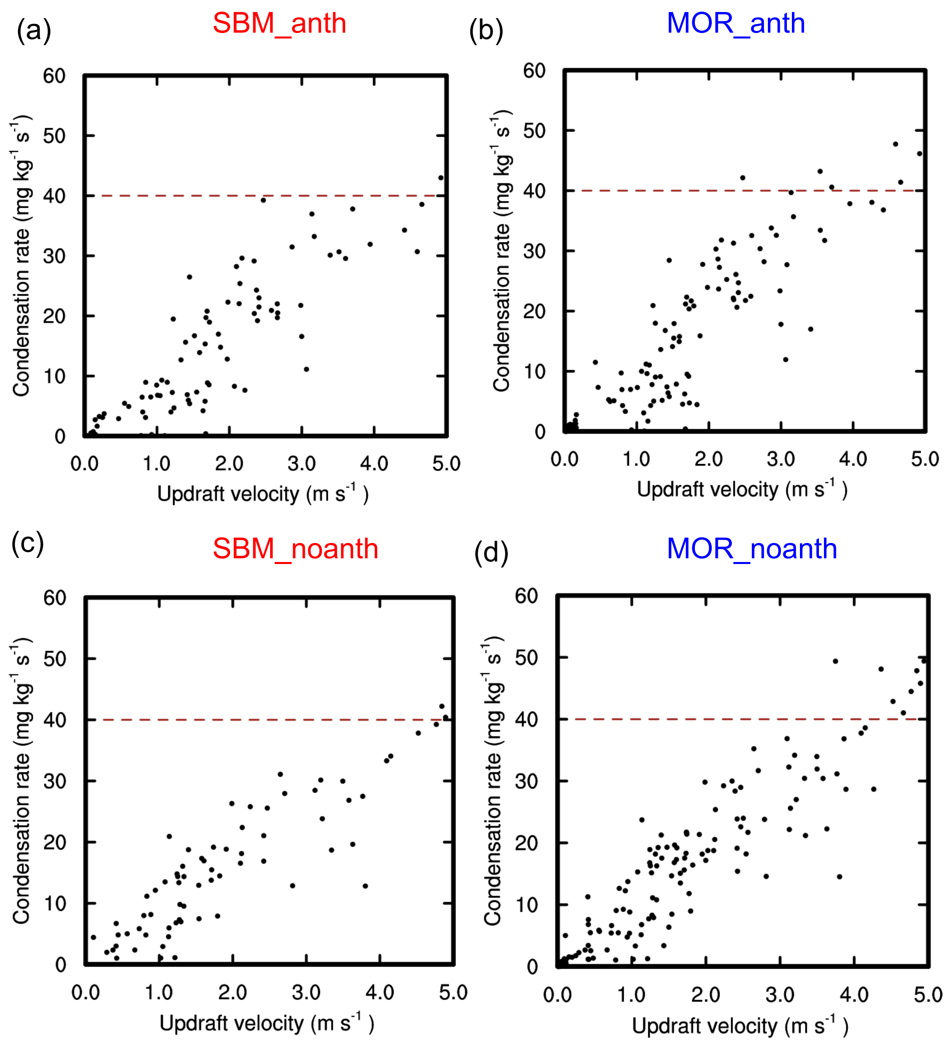


Figure r3 Same as Figure r2, but for 3 km altitude.

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.

See our response to comment #6. The saturation adjustment weakens the ice processes due to less droplets remaining for being lifted above freezing level as a result of efficient conversion from cloud droplet to rain because of larger condensational growth. We also add the findings of Grabowski and Morrison (JAS 2017) in the discussion session: “Grabowski and Morrison (2017) also showed that the saturation adjustment affected ice processes by producing larger ice particles with larger falling velocities compared with the explicit supersaturation approach, leading to the reduction of anvil clouds.” (Line 477-479).

We have added a figure for the hydrometeor number concentrations (Figure 15) corresponding to the mass mixing ratios shown in Figure 14.

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 with more than 10 s relaxation time, which is mainly due to low droplet number. Please see Fig. r5 for more details.

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

A new key point has been added to the abstract. That is "Whereas such an effect is absent with the Morrison two-moment bulk microphysics, mainly because the saturation adjustment approach for droplet condensation and evaporation calculation removes the dependence of condensation on droplet properties and limits the ice processes by a more efficient conversion of droplets into raindrops, which leads to less cloud droplets being transported to the altitudes above the freezing level" (Line 25-29).

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 a sentence to the discussion part: "The increased condensation is significant for the enhanced warm clouds when saturation adjustment is used. This is different from the points of Grabowski and Jarecka (2015) that the cloud edge evaporation effect is more important for the nonprecipitating shallow clouds" (Line 479-482).

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°. This information was indeed included, and now it is at Line 183-184, "meteorological lateral boundary and initial conditions were created from MERRA-2 at the resolution of 0.5° × 0.625°(Gelaro et al., 2017)."

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?

We have added a sentence to describe it after that sentence since we do not think it is needed to copy the equation from Lebo et al. 2012 and put there, i.e., "That is the supersaturation is solved by the source and sink terms of dynamic forcing and condensation/evaporation within an one-timestep" (Line 226-228).

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 have added text to point out the simulation drawbacks. At Line 258-260, "Though not exactly the same, the values from D1_MOR_anth show a similar distribution with the observations in terms of the surface PM2.5 averaged over 24 hours (the day before the convection near Houston)." Also at Line 281-285: "Compared with the coarse resolution NLDAS data, both SBM_anth and MOR_anth capture the general temperature pattern with a little overestimation at the northeast part of the domain (mainly rural area) . The modeled southerly winds do not reach further north as the NLDAS data, possibly because of the feedback of the small-scale features which are simulated with the high resolution to mesoscale circulations."

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. We have added a note about this. Buoyancy can be attributed to temperature and moisture perturbation and condensate loading. The net buoyancy is the sum of thermal buoyancy and condensate 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. This information is now added to the figure captions. The major differences between the two simulations are at the low (<12 dBZ) and high large reflectivity (> 48 dBZ).

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 have also added the information of accumulated precipitation: "The observed accumulated rain over the time period shown in Fig. 9a is about 3.8 mm, both SBM_anth (~4.5 mm) and MOR_anth (~4.2 mm) overestimate the accumulated precipitation due to the longer rain period compared with the observations" (Line 313-316).

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 named as the CCN activation rate. The activate rates from SBM are shown ok. See our reply to the major comment #5

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 not accounted for the average. We made this clear in the figure capture. The ensemble spread is marked as shaded areas for all profiles.
In our case the updraft speeds are indeed stronger, not because of more updrafts, as seen from the PDF figure (Figure 10).

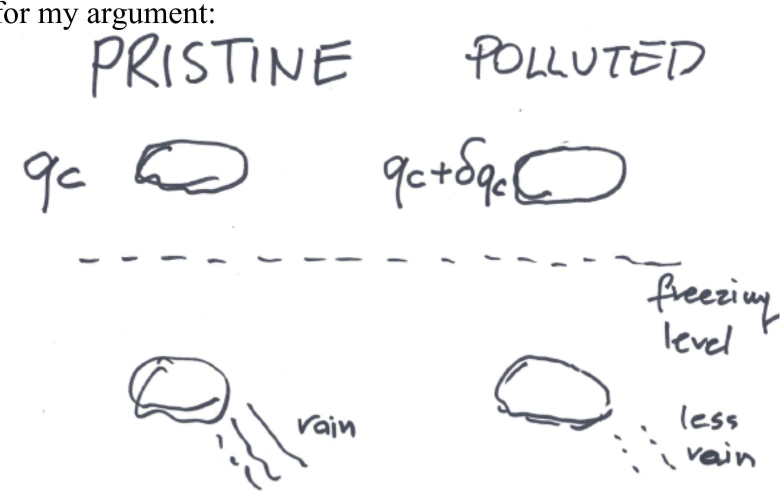
Responses to the reviewer' questions about the cold-phase and warm-phase invigoration by aerosols.

The reviewer's comments:
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.

Here are the follow-on comments from the reviewer on the cold-phase and warm-phase invigoration.

I appreciate the authors' responses to my initial comments. However, the response has fundamental flaws and thus I cannot consider the issues settled. I think the authors need to reconsider my comments about the invigoration and correct obvious flaws in their arguments.

About the cold-phase invigoration:
The authors clearly misunderstood my argument. The figure below illustrates the starting point for my argument:



The left part of the panel shows a cloudy parcel that rises through the melting (freezing) level in pristine conditions. The total liquid condensate above the freezing level is q_c. The right part of the figure shows situation when a similar parcel rises in polluted conditions. Because of the less efficient warm rain processes, the parcel above the freezing level carries more liquid condensate, $q_c + \delta q_c$. Freezing of δq_c in the authors' opinion (and in many other papers) is the reason for the invigoration. However, to carry δq_c across the freezing level requires extra buoyancy when compared to the left panel. As shown in section 2a of Grabowski and Morrison (2020), the two effects approximately balance each other. It follows the original sentences in the manuscript under review that say: "… a well-known theory is that increasing aerosol concentrations can suppress warm rain as a result of increased droplet number but reduced droplet size. This allows more cloud water to be lifted to a higher altitude wherein the freezing of this larger amount of cloud water induces larger latent heating associated with stronger ice microphysical processes, thereby invigorating convective updrafts…" are simply incorrect and require additional explanations. For instance, the invigoration would be possible if the frozen condensate was removed through precipitation processes.

That said, I really do not think referring to invigoration is needed for this manuscript. If the authors insist, then the introduction and references to the invigoration in the text should provide a less biased discussion, for instance, as in Grabowski and Morrison JAS papers and as in Varble (JAS 2018).

About the warm-phase invigoration:

The phase relaxation time scale and the quasi equilibrium supersaturation estimates in the authors' response above are simply wrong. Below I include a table from Politovich and Cooper (JAS 1988) that shows phase relaxation time scale for different combinations of droplet concentrations and radii. These values are much smaller than those shown in the authors' response.

TABLE 1. Time constant characterizing supersaturation. (Values of $\tau = 1/(a_2 I)$ s for $p = 771$ mb, $T = 4.3°C$)

Radius (μm)	Droplet concentration (cm^{-3}) 100	300	500	1000
2	14.1	4.7	2.8	1.4
3	8.7	2.9	1.7	0.87
5	4.9	1.6	0.98	0.49
10	2.3	0.77	0.46	0.23

The key question is why?

The explanation is relatively simple. The authors say that they use the formulas from Pinsky et al., eq. (4) therein. However, Pinsky et al. apply a simplified (and in my view incorrect) droplet growth equation that is different from the comprehensive formula used in Politovich and Cooper (JAS 1988). The key point is that one has to apply exactly the same droplet growth equation in the phase relaxation time scale calculation (and thus in the quasi-equilibrium supersaturation) as used in the numerical model. I expect Khain's bin microphysics applies a correct droplet growth formulation that is close to the one used in Politovich and Cooper (JAS 1988), and not the simplified droplet growth equation applied in Pinsky et al. The supersaturation simulated by the model can be compared to the diagnosed quasi-equilibrium supersaturation only if exactly the same droplet growth equations are used in both. This was the case for the relatively good agreement shown in Grabowski and Morrison (JAS 2017), at least below the freezing level, see Fig. 15 therein. In summary, the values shown in the authors' response above have to be

Corrected.
The above discussion requires the authors to modify their responses and revise their paper accordingly. Note that the second part of my rebuttal impacts some of my other original comments. I strongly object publication of the manuscript unless those comments are appropriately addressed

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As we noted earlier, the two sides of arguments have been existing for a while, and it should not be the role of this paper to debate and resolve this issue. Here we only provided our key points, the detailed review and comment paper was submitted to J. Atmos. Sci., which would allow both sides to discuss and debate there. The bulk of the above comments are chiefly the expression of the reviewer's view on the aerosol invigoration effect, rather any substantial objection to the scientific importance and the findings of this study.

For the "cold-phase invigoration", the reviewer's argument "the increase in the buoyancy by freezing is completely offset by the buoyancy for carrying the extra cloud water across the freezing level" has several issues:
(1) Droplet ascending and then freezing are subsequent at different locations; also, the two processes can take at very different time scales (freezing is instant but ascending could take much longer time). How do they compensate each other at different time scale and locations? Responses of a complex non-linear dynamical system in deep convective clouds strongly depend on duration and location of the forcing.
(2) In the process of ascending in updrafts, droplets will grow through condensation, and the changes in latent heating and condensate loading from this are not considered in this argument.
(3) The argument neglected the subsequent enhanced riming and deposition resulting from more ice particles formed from enhanced droplet freezing. This leads to (a) a further increase in latent heating and (b) a reduction in condensate loading because more graupel and hail form due to increased supercooled liquid content and precipitate. Rosenfeld et al. (2008) indeed considered the possible compensation between the extra condensate loading and the extra latent heat of freezing. They showed (in line d of their Fig. 3) that the total buoyancy excesses and invigoration occurs after the ice hydrometeors are unloaded (i.e., precipitated) from the cloud parcel. The unloading is quite efficient in the case of rich supercooled liquid water content where large particles like graupel and hail can form. Many modeling studies have showed that the latent heat released from deposition and riming is much larger (at least an order of magnitude) than freezing. The increase in latent heating by aerosols is mainly from the increase in deposition and riming at the high-levels. Overall, the buoyancy increase via latent heat release exceeds the buoyancy decrease resulting from the increase in condensate loading, leading to a positive net buoyancy (e.g., Fan et al. 2012a, 2018; Tao and Li, 2016; Lebo et al. 2012; Chen et al. 2020).

For the "warm-phase invigoration", our interpretation of the mechanism is the enhanced condensation by larger droplet nucleation in the polluted conditions releases more latent heat, enhance buoyancy this updraft intensity. This is consistent with many literature studies (e.g., Khain et al. 2012; Igel et al. 2015; Sheffield et al. 2015; Chen et al. 2017; Fan et al. 2018; Lebo 2018). The reviewer argued that condensation rates only depend on updraft velocity with the quasi-steady assumption (i.e., true supersaturation is approximated with equilibrium supersaturation), therefore they interpreted that it is the lower equilibrium supersaturation in polluted conditions that lead to a larger buoyancy, thus enhanced updraft speeds and condensation.

This quasi-steady assumption is neither physically justified for the strong updrafts of deep convective clouds nor is it suitable for studying aerosol effects on deep convective clouds which requires the exact solution of supersaturation. Previous studies (e.g., Politovich and Cooper 1988; Korolev and Mazin 2003, Pinsky et al. 2013) 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). Note that both Politovich and Cooper (1988) and Korolev and Mazin (2003) evaluated the phase relaxation time under the assumption of the constant drop radius, which is not as accurate as Pinsky et al. (2013) that used the accurate equation for supersaturation. However, the reviewer mistakenly thought that Politovich and Cooper (1988) used an accurate droplet growth equation but Pinsky et al. (2013) used a simplified one.
So we followed Eq. 3 and Eq. 4 in Pinsky et al. (2013) to calculate Seq and phase relaxation time τ, respectively. Fig. r4 shows the calculated phase relaxation time as a function of droplet number and radius from SBM_anth. The values we got are quite consistent with the Table 1 of Politovich and Cooper (1988) for droplet number concentrations (Nc) of 100, 300, and 500 cm-3, which proves that our calculation has no problem. The reviewer said our values "are simply wrong" in his follow-on comment which were from the same calculation except we showed the mean value for the updrafts with a velocity greater than 2 m s−1 (Fig. r5). In these relatively strong updrafts, the phase relaxation time is long (Fig. r5c) because of low Nc (Fig. r5d) due to fast conversion of droplets to rain. Fig. r4 showed that most of the updrafts have Nc of 5-20 cm-3. The averaged τ for the updrafts greater than 2 m s−1 are around 10-15 s (with large values exceed 60 s). Above the cloud base, the quasi-equilibrium supersaturation is much higher than the true supersaturation (Fig. r5a) where low droplet number concentration (Fig. r5c) and strong updrafts (Fig. r5b) are seen. Therefore, it is clear that the short phase relaxation time (a few seconds) is only true near cloud base with large droplet number concentrations (~ hundreds cm-3) and weak updrafts. However, in relatively strong updrafts, the droplet number above the cloud base is much reduced (~ tens cm-3 in this case) due to fast conversion of droplets to rain, thus the phase relaxation time is much longer (> 10 s and even over 60 s) and the assumption of S=Seq is not valid any more. This is particularly true for the pristine case (SBM_noanth), we can see the Seq can be much higher than the true supersaturation (Fig. r2a), so assuming S=Seq would lead to a large bias in condensation and evaporation in the pristine case.

Therefore, appropriately simulating aerosol effects on deep convective clouds requires an exact supersaturation calculation (Eq. 6 in Pinsky et al. 2013), in which the condensation depends on droplet number and size, and more droplets in the polluted clouds increase condensation and decrease supersaturation, which clearly showed our interpretation is physically solid. In addition, as shown in our responses to the comment #6, we see the polluted case (SBM_anth) has larger condensation rates for the same updrafts than the pristine case (SBM_noanth) (Fig. r3, a vs c) above cloud base where the increase of cloud droplet number is significant (Fig. 15a). This clearly shows that higher droplet number leads to larger condensation rate for the same vertical velocity, which rebuts the reviewer' argument that condensate rates are similar under the same vertical velocity.

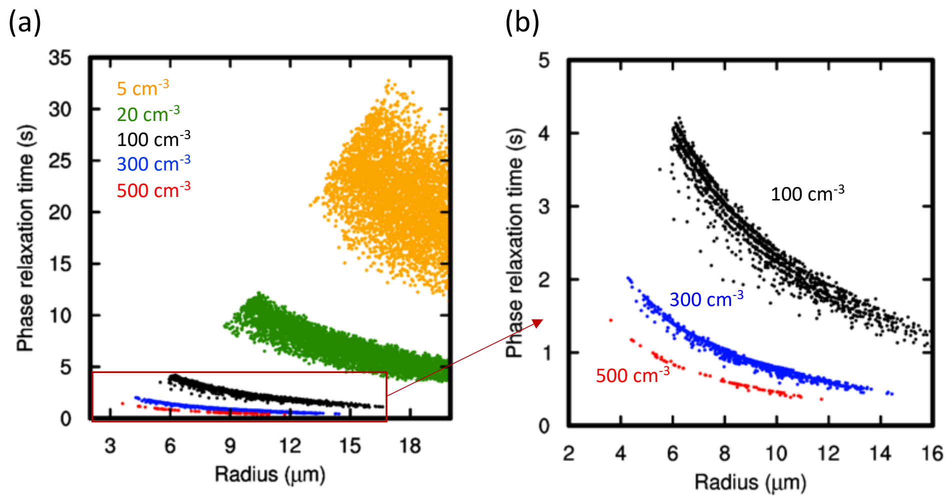

Figure r4 (a) Relationship between phase relaxation time and droplet radius for different droplet number concentrations from the simulations SBM_anth. (b) is the same as (a), except zooming in for droplet number concentrations of 100 cm^{-3}(black), 300 cm^{-3}(blue), 500 cm^{-3}(red).

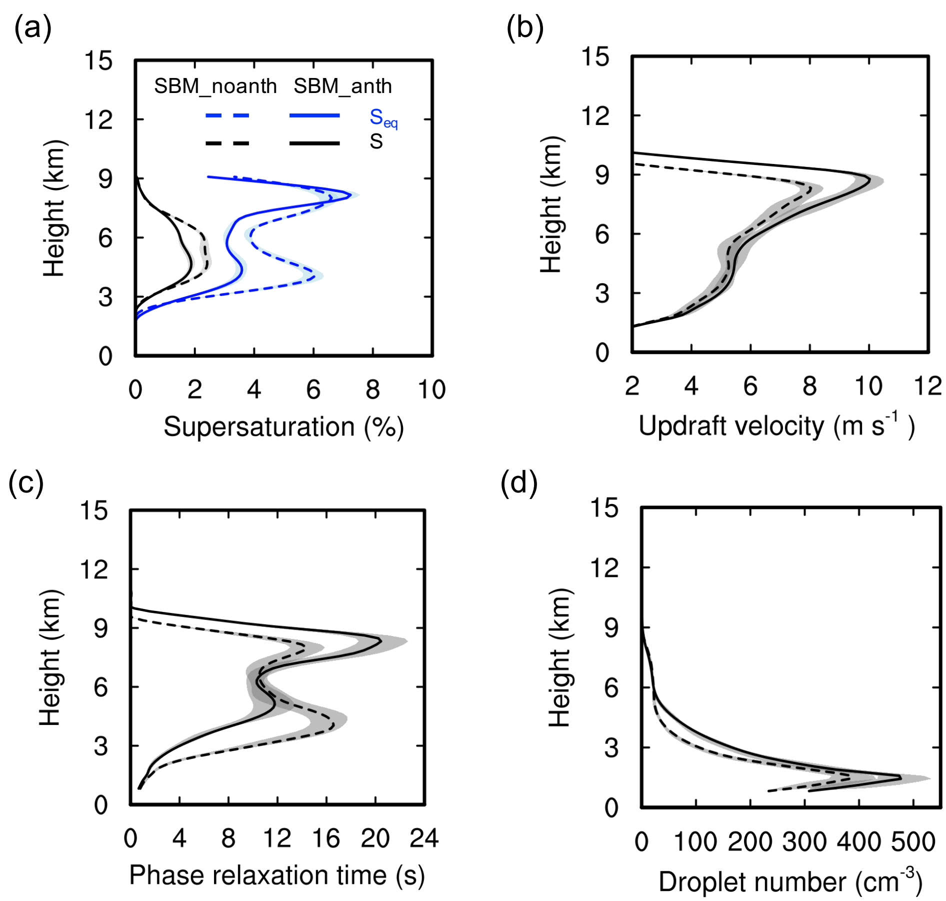

Figure r5 Vertical profiles of (a) supersaturation and quasi-equilibrium supersaturation, (b) updraft velocity, (c) phase relaxation time and (d) droplet number concentration averaged over the updrafts with value greater than 2 m s^{-1} from the simulations SBM_anth and SBM_noanth, over the analysis domain as shown in the red box in Figure 7 during the strong convection period (2000 – 2300 UTC, 19 Jun 2013). The shaded areas mark the spread of ensemble runs.