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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 #2

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Review of a manuscript entitled "Impacts of Cloud Microphysics Parameterizations on Simulated Aerosol-Cloud Interactions for Deep Convective Clouds over Houston" by Zhang, Fan, Li, and Rosenfeld, considered for publication in ACP, manuscript acp-2020-372.

Recommendation: not acceptable in its current form (major revision or rejection)

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 im-

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Discussion paper

pact 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.

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.

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.

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.

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?

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?

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.

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. 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.

Specific comments:

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

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.

3. L. 162: The grid length of the MERRA data should be mentioned here.

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?

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.

6. What is "thermal buoyancy"? I think this is just "buoyancy", correct? Please change.

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.

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.

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.

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?

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.

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