Final review of the revised manuscript "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: accept after minor revisions

I have to stress that my recommendation is NOT based on the satisfactory revisions (the revisions are relatively minor), but because of my expectation that the peer review process has to stop at some point. I strongly feel that the authors' interpretation of the invigoration problem is biased and this affects the discussion of past studies and current model results. For instance, on lines 52/53 the authors say that Grabowski and Morrison (2016, 2020) "reject […] invigoration concept". This is not true: Grabowski and Morrison argue that the simplistic view of the invigoration is difficult to understand based on the buoyancy below and above the freezing level in situation without and with pollution. On lines 81/84, the authors refer to Varble (2018) arguing that Varble's study suggests that 10 years of SGP data is not sufficient to separate meteorological factors from the impact of aerosol. This is not true: Varble shows that meteorological conditions over SGP explain observed changes in convection without referring to aerosols, and that the 10-year study the authors implicitly refer to is severely flawed. It is impossible for me to point all places where the authors are biased. I thus suggest that the paper is published, perhaps with some final changes following my comments below. That said, I have to stress – as I did before – that the manuscript does not need to refer to the invigoration. The difference between saturation-adjustment in the Morrison bulk scheme and saturation-prediction in the Khain bin scheme is sufficient to explain the differences. The strength of the manuscript is in a direct prediction of CCN formation and removal, and how these can be linked to cloud processes.

The only more important and still unanswered point made in a couple of my previous comments is that the differences in the representation of ice processes in the two schemes, and how they are linked to warm-rain process, need to be clearly explained. For instance, is the ice initiation the same in both schemes? A short paragraph for each scheme should be sufficient.

Another general comment is that English needs some improvements throughout the text. Some places are listed below.

Specific comments:

1. The last sentence of the abstract is unclear. First, saturation adjustment provides the maximum buoyancy, so I do not understand why "saturation adjustment […] limits the enhancement in condensation…". I would think the opposite should be true. The (2) seemingly refers to ice processes, but it is unclear why saturation adjustment affects conversion of droplets into raindrops, and how these are related to ice processes.

2. L. 195. "The meteorological fields were reinitialized…"

3. L. 225/227. The sentence: "…the supersaturation is solved by the source and sink in terms of dynamic forcing and condensation/evaporation within a one-timestep" needs to be rewritten. I commented on the subsequent sentence in the previous round ("…the supersaturation for condensation and evaporation is calculated after the advection."), see my point 2 in the previous review. Please revise.

4. L. 252 should be "Results".

5. Discussion of Figs. 7 an 8. I am not sure if I see differences between left and right panels in Fig.7. These are just different convection realizations to me. Can one apply an objective analysis to prove that? In Fig. 8, it is difficult for me to see which model simulation is closer to the observation with the exception of the echo top, higher in bin microphysics and in agreement with the radar plot.

6. Fig. 9. The authors ignored my suggestion to plot rain accumulation as more meaningful and less dependent on the specific realization of the flow.

7. Section 4.2, lines 405-456, the interpretation of results related to the impact of pollution on the supersaturation and particle growth between the two microphysics schemes. As I stated previously (and above), a brief description of ice microphysics between the two schemes, and the key differences, would provide a proper context for this discussion.

8. L. 481: “condenses all supersaturation”. Perhaps “removes”?