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Comment on acp-2021-472

Anonymous Referee #1

Referee comment on "Supersaturation, buoyancy, and deep convection dynamics" by Wojciech W. Grabowski and Hugh Morrison, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-472-RC1, 2021

Supersaturation, buoyancy, and moist convective dynamics

Wojciech W. Grabowski and Hugh Morrison

This paper deals with the impact of supersaturation, condensate and precipitation loading, and entrainment on parcel buoyancy for moist deep convection. The authors analyze those factors using both a 1D parcel model and 3D cloud-resolving simulations to conclude that supersaturation can suppress deep convection, especially in the lower troposphere. However, both water/precip loading and entrainment have a more significant cumulative impact across the entire troposphere. Additional tests for either pristine or polluted air conditions indicate only minor changes due to microphysics.

I find this study an interesting and important contribution to our improved understanding of deep convective dynamics. Small-scale models and convection parameterizations are often agnostic to supersaturation-buoyancy feedbacks, which may potentially deteriorate their simulation of deep convection for certain scenarios. The paper is quite well written and the conclusions are supported by a convincing set of analyses. However, some parts of it would benefit from a better structure and additional explanations, as suggested below.

Comments:

- Title: Since deep convection is the focus of the paper, “deep convective dynamics” would better reflect its content.
- Abstract: it has almost 400 words and includes (too) many details. I suggest to shorten it (~250 words) and make a take home message more succinct.
- What is the impact of your anelastic approximation on the simulation of deep convection? Apart from the lack of baroclinicity in your equations, and a simplified form of continuity equation, the buoyancy term is normalized on the arbitrarily-chosen base state temperature and your results may differ from the most accurate fully compressible solution.
- Your latent heat of condensation is assumed constant while in reality it somewhat depends on temperature (for this range of temperatures it may vary by several percent) – is this effect important for the calculated buoyancy?
- How strongly do your results depend on model resolution? Your dx=400m is quite coarse and one may expect higher vertical velocities (and supersaturations) for finer resolution simulations.
- How does supersaturation affect mass flux? You show in Fig. 7 different vertical velocities with/without S, but what happens to the area?
- I don’t see a clear justification for using IAB in this study and thus removing it may help in keeping the main message clearer.

Instead of comparing G15 and GM20, can you simply use GM20 with and without supersaturation (S=0 for the latter) to directly evaluate its impact?

- Section 3: Can you clarify the purpose of your analysis at the beginning of this section and explain how it relates to your supersaturation considerations? d/dz, psi, and psi_e can all be obtained from the 3D model output, so what exactly do you want to calculate?

Provide the definition of equivalent potential temperature here.

Since rising plumes represent the right tails of moisture and temperature distributions near the surface, using 15-20% of that distribution instead of mean values in the lowest 500m as the initial conditions may be more relevant. That would be similar to what convection parameterizations do, e.g.:

https://journals.ametsoc.org/view/journals/atsc/76/8/jas-d-18-0239.1.xml

Have you looked at the impact of initial conditions on your parcel model results?

- There are LES studies (see below; also for LBA) showing that rising plumes/thermals can reach a quasi-steady velocity due to the balance between buoyancy and drag, which may be an explanation of the differences between the theoretical sqrt(2CAPE) and your simulation results.

https://journals.ametsoc.org/view/journals/atsc/73/10/jas-d-15-0385.1.xml

- In Fig. 12, the spread of your results is larger at 4km than at 9km. Is it because the updrafts are more separated from the environment at higher velocities?
- Is piggybacking only useful to look at tiny effects due to microphysics or is it a more universal method?
- In Conclusions, "unorganized deep convection" – this statement is questionable. When cold pools are not present, buoyancy-driven plumes can only reach up to ~9km for this case. Your updrafts reach to the top of troposphere (14-15km) as for organized deep convection although autocorrelation scale may be limited by the size of your domain. You could actually cite this paper https://journals.ametsoc.org/view/journals/atsc/75/12/jas-d-18-0031.1.xml around the discussion of the LBA setup. Even for the 50km domain, your convection reaches the tropopause, as for larger-domain simulations.

  Your conclusions mostly focus on invigoration (mentioned 7x), whereas they should also describe briefly your analysis results, that is the impact of supersaturation, water loading, and entrainment on the buoyancy.
- Please mention about some 1D convection parameterizations based on an entraining plume approach. Typically, they additionally employ a steady-state velocity equation affected by buoyancy, e.g.:

https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015MS000502

https://journals.ametsoc.org/view/journals/atsc/76/8/jas-d-18-0239.1.xml