Responses to the Reviewer 1 comments.

The authors appreciate thoughtful comments from the Reviewer 1. Below we reply to those comments and outline revisions that will be included in the revised manuscript. The original comment text is in black, our responses are in red. However, the review as published on the ACPD webpage is poorly organized, so we took the entire text without any changes and put it below with all comments sequentially numbered.

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:

1. Title: Since deep convection is the focus of the paper, "deep convective dynamics" would better reflect its content.

We agree. The title has been changed to "… deep convection dynamics".

2. 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.

The abstract has been shortened to about 300 words.

3. 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.

We do not want to discuss anelastic versus compressible solutions as this has been addressed in previous studies, for instance, in Kurowski et al., *J. Atmos. Sci*. 2013, 2014, and 2015. The fully-compressible equations (i.e., with gravity term as "… + g + …) are seldom used in the atmospheric dynamics, and the Boussinesq form of compressible equations (… + g rho'/rho + …) does include a hydrostatically-balanced background state (through rho). For instance, please compare equation sets in sections 2a and 2b in Bryan and Fritsch (*Mon. Wea. Rev*. 2002). The Boussinesq form is sometimes referred to as the reduced gravity method.

No changes to the manuscript in response to this comment.

4. 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?

This comment is only partially true. Both IAB and 2MOM schemes include temperature-dependent latent heat of condensation. I think the reviewer was confused by the "=" sign in the second paragraph of the introduction. We changed it to "≈" there. Parcel analysis indeed assumes a constant latent heat of condensation. Our tests show that this has a small (below 10%) impact on actual values of cCAPE and other quantities derived in the analysis. For instance, total CAPE is smaller when variable latent heat is assumed because latent heating is reduced in the lower troposphere where the latent heating is the smallest. We decided not to bring this subject in the revised text as it is only marginally relevant to the main thrust of the paper.

No changes to the manuscript in response to this comment.

5. 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.

Not necessarily. Higher resolution implies more resolved entrainment and thus may have the opposite effect. Grabowski and Prein (*J. Climate*, 2019) compared simulations as in the paper under review and shorter higher resolution (truly LES-type) simulations applying a modified LBA case. Comparison of Figs. 7 and 14 there shows that the cloud fraction profiles evolve similarly in the lower and higher resolution simulations, at least up to hour 4 of the simulations. The impact of entrainment is mentioned in the paragraph starting in line 86 in the introduction. We feel this sufficient.

No changes to the manuscript in response to this comment.

6. How does supersaturation affect mass flux? You show in Fig. 7 different vertical velocities with/without S, but what happens to the area?

Cloud fraction profiles are only weakly affected before significant anvils develop as shown in our previous papers. For instance, see Fig. 4 in G15, Fig. 1 in Grabowski and Morrison (*J. Atmos. Sci*. 2016), and Fig. 1 in GM20. Arguably, Fig. 2 in the current paper documents that as well. Since the paper focuses on the convective dynamics, buoyancy and updraft strength in particular, we do not want to discuss this aspect.

No changes to the manuscript in response to this comment.

7. 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?

Keeping IAB and 2MON ensembles allows comparing results with not only saturation adjustment and supersaturation prediction, but also significantly different microphysics parameterizations.

We will add a comment on the in the revised text.

8. 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?

The purpose of this analysis is to compare idealized entraining parcel calculations with the dynamic model simulations. The parcel results are included in the Table 2 and compared with the dynamic model results in Fig. 4. We feel this is a useful element of the manuscript.

We will add a comment on the in the revised text.

9. Provide the definition of equivalent potential temperature here.

DO IT…

10. 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?

It is a traditional approach to consider the lowest-500-meter-averaged properties to derive convective indices. In response to this comment, we ran parcel simulations with initial conditions taken as surface values. These values are about 0.1 K warmer and about 0.5 g/kg more humid that 500-meter averages. The outcome – as one might expect – is significant for specific values shown in the Table 1 in the original submission (Table 2 in the revised text), with some parameters (e.g., CAPE) increasing by up to 20%. However, the relative differences between various parcel simulations remain similar. We mention this in the revised text.

11. 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

This is a valid point. The paper the reviewer refers to argues that thermals are more appropriate than plumes as building blocks of deep convection. We agree with such an argument. However, it is not clear to us what the reviewer suggests us to do. We assume that this comment is in regard to the statement on lines 291-294 in the original manuscript that vertical velocities in the simulation are 2-3 times smaller. Besides drag as the reviewer mentions (or, more generally, an adverse vertical perturbation pressure gradient force), the impact of entrainment and condensate loading likely plays a role in explaining the difference between the theoretical and simulated vertic al velocities. All of these factors are mentioned in that sentence. Since drag is a component of the perturbation pressure forcing already mentioned in this sentence, we have not modified the text.

12. 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?

We do not think so. We think this is because of the latent heating differences between pristine and polluted conditions that are more significant at 4 km than at 9 km. Please note that the equivalent potential temperature at 9 km is closer to the environment with less scatter than at 3 km. This can arguably explain differences in the statistics shown in Fig. 12.

We will add a comment on the in the revised text.

13. Is piggybacking only useful to look at tiny effects due to microphysics or is it a more universal method?

We feel piggybacking can be used to study impact of any element of the model physics. Grabowski and Prein (*J. Climate*, 2019) compared the impact of different temperature and moisture profiles on convective development in the context of the climate change. Kurowski et al. (*Geophys. Res. Lett.* 2019) applied piggybacking to study the impact of environmental heterogeneities (e.g., remnants of previous clouds) in shallow convection simulations. One can think of various other processes that can be studied using piggybacking, such as radiative transfer, surface heat fluxes, etc.

14. 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.

We feel this is a misunderstanding. What we mean is that the convention is scattered, that is, there are no squall lines, bow echos, or other organized convection systems. We will change "unorganized" into "scattered" in the revised text.

15. 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.

We will revise the conclusion section following this suggestion.

16. 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

We do not think including convection parameterization aspect is needed in this already quite a long manuscript.

No change to the text is response to this comment.