Responses to the Reviewer 1 additional comments.

Reviewer's comments in black, our original responses in red, responses and revisions as a result of the additional comments in green.

The authors' response is minimalist. I understand their arguments but my intention was to ask important questions that the reader may ask as well to get a better understanding of the presented work. Ultimately, you want to convince the reader that the methodology you applied represents the reality with high fidelity.

I suggest that the authors add 1-2 sentences to the manuscript with at least minimal explanations regarding my points 3, 4, 5, 6, 13, 14.

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to strengthen your conclusions.

We agree that our responses and revisions were minimalist, but we felt this was the best approach for the paper. Because the Reviewer seems unsatisfied with our approach, we added text as requested, mostly through several footnotes. Below, our original responses to the points 3, 4, 5, 6, 13, 14 are in red, and responses and revisions are explained in green. References to the recent paper suggested by the reviewer is added to the conclusion section.

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.

We added a footnote in the introduction, just above the Eq. 1. The footnote reads:

"For a discussion of the anelastic versus compressible equations and simulation results obtained from the two in the context of small-scale and planetary-scale dynamics, the reader is referred to Kurowski et al. (2013, 2014, 2015) and references therein."

We added Kurowski et al. information to the list of references.

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.

We added a footnote to the paragraph below Eq. 2. The footnote reads:

"The code for parcel calculations applies a constant latent heat of condensation in contrast to the microphysical schemes applied in the dynamic model. 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 of condensation is assumed because latent heating is reduced in the lower troposphere where the latent heat of condensation is the smallest."

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.

We added the following text to the second paragraph in section 2.1:

"Overall, the horizontal resolution is relatively low making the simulations only marginally LES, especially early in the simulations when the boundary layer is relatively shallow. However, as mentioned in G15 (section 2a therein) applying such a grid provides results broadly consistent with the high-resolution benchmark simulations reported in Grabowski et al. (2006). Results reported here seem also consistent with truly-LES simulations reported in Kurowski et al. (2018) and in Grabowski and Prein (2019)."

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.

The following short paragraph was added at the end of section 4.2:

"Although not directly relevant to the main thrust of this paper, it is worthwhile to mention that the convective mass flux is also insignificantly affected by small differences in the convective dynamics documented in Fig. 7. This is because cloud fraction profiles are only weakly affected by microphysical processes (at least before significant anvils develop in 2MOM simulations) as shown in G15 (Fig. 4 therein), Grabowski and Morrison (2016; Fig. 1 therein), and GM20 (Fig. 1 therein). Arguably, Fig. 2 herein documents that as well.

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.

The following text was added as a footnote in section 4.3:

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

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 echoes, or other organized convection systems. We will change “unorganized” into “scattered” in the revised text.

We replaced “unorganized” with “scattered” in the revised text. We added reference to the Kurowski et al. (2018) paper.