

Final author reply for submitted manuscript egusphere-2025-4495 titled “Driving Mechanisms for Subsiding Shells in Simulations of Deep Moist Convection”

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Reviewer #1 General Comments:

This study uses Large-Eddy Simulation (LES) experiments based on field campaign data in the Amazons to investigate drivers of subsiding shells surrounding deep convective clouds. The study builds on previous research, in particular findings on subsiding shells in non- or weakly-precipitating shallow convection. Two forces have previously been found to dominate the downward acceleration: i) negative buoyancy (B) due to evaporative cooling of cloud droplets, and ii) upward vertical pressure gradients (VPG) counteracting the downward acceleration. Debate still exists on the formation of convective downdrafts, in particular concerning the role of the vortex feature around the cloud and evaporative cooling at its edges. The main goal of this study is to gain more insight, and specifically to assess to what extent these shell-driving mechanisms differ in deep convective clouds. A key hypothesis to be tested is that drier deep cloud environments favor stronger downdrafts due to more efficient evaporative cooling, suggesting a dominant role by B in this regime. Another hypothesis tested is that differences in updraft strength contribute to differences in downdraft strength, through the VPG. These questions are then investigated using LES results and Lagrangian particle analysis.

The study and its findings are relevant not only for process understanding of convective downdrafts, but also for improving their representation in larger-scale models. The paper is well-written, in particular the introduction is informative and functions as a mini-review of past and recent research. The hypotheses to be tested are well-defined and relevant. The (numerical) experimental design and analysis method are in principle clearly described and well thought-through; however, a few potentially important aspects of the model setup are not as fully described or motivated as they could have been. This mainly concerns the spatial resolution

(rather low I think) and the choice of microphysics scheme. I also have a few questions about the results of the simulations and their analysis, in particular i) the shape of the cloud field, the individual clouds, and the time evolution of the convective layer in general (currently not shown), and ii) the potential role of mixing of downdraft air with its environment in accelerating/decelerating the downdraft.

These main points are described in more detail below. In my opinion they can well be addressed by providing additional information and/or clarification.

General recommendation: Accept for publication after minor revisions.

Author Response: We thank the reviewer for taking the time to review this work and for showing a great interest in it and its potential implications on understanding convective processes and the implementation in climate models. We are grateful for all recommendations supplied by the reviewer and have worked to implement such suggestions as shown below.

Reviewer #1 Main Points:

Main Point (i): *The experimental setup of the LES makes sense, in that it contrasts dry versus moist conditions, reflecting seasonality in the Amazons. In that sense the study by Derbyshire et al. (2004, <https://doi.org/10.1256/qj.03.130>) comes to mind, which could be mentioned for reference (even though it did focus on bulk updraft mass flux, not downdrafts). However, I missed a few important details in the description of the model setup and the choices made:*

Why is a spatial resolution of 100 m considered adequate for studying this problem? Most recent LES studies of moist convection typically use a finer gridspacing. Downdrafts can be pretty small and narrow, so a discussion of what resolution is adequate for capturing their dynamics would be beneficial for gaining confidence in the numerical results presented here. Ideally, a sensitivity test on resolution could be included (perhaps as an Appendix).

Author Response: Given the constraints of our computing resources, running simulations at a finer resolution (e.g., 50 m) was not feasible with our domain size and integration length. To evaluate how sensitive our results might be to resolution, we ran an additional set of simulations at coarser resolutions of 160 m and 200 m in the horizontal and vertical. The results from these simulations are included in Appendix A.

As shown in Fig. A2, coarsening the grid spacing did not significantly impact the behavior of the accelerations, but rather reduced the amplitude of them. As such, the subsiding shell vertical velocities in Fig. A1 are reduced. Parcel time series curves of B were shifted upward, while VPG behavior remained the same with reduction of origin and termination acceleration magnitudes. Despite reductions in both the B and VPG , the total acceleration behavior remains unchanged. With this, it is evident that larger grid spacings have a “smoothing” impact on resolved accelerations, which would effectively weaken the shell. These coarser resolution experiments suggest that the general characteristics of accelerations, which are the most important element of our results, are consistent across a range of grid spacing.

How is pressure treated in the model? There are various options out there on how to do this in an LES: prognostic, Poisson solver, etc. Line 160 briefly states that pressure is prognostic, but I am not entirely sure what this means. The treatment of pressure could affect how the VPG acts in downdrafts.

Author Response: The authors thank the referee for raising this point. We have solved for pressure explicitly using compressible equations with Klemp-Wilhelmson time-splitting (psolver = 2 in the CM1 namelist file). It is recommended to use such a pressure solver when horizontal and vertical grid spacing is roughly equal.

To clarify this in the text, we have added the following sentence in Lines 182-184: “Pressure in our simulations is explicitly solved for using compressible equations with Klemp-Wilhelmson time-splitting (psolver = 2 in the CM1 namelist file), which is recommended when horizontal and vertical grid spacings are nearly equal (Bryan and Fritsch, 2002).”

The microphysics scheme is minimally described, but could matter here, in particular through the evaporative cooling that partially drives downdraft acceleration. One question that comes up is if cloud ice plays a role in these simulations. The convective layer is tropical and pretty warm, but also very deep, and q_i is mentioned in line 134.

Author Response: We used the Morrison double moment scheme, which includes prognostic equations for nonprecipitating cloud ice (q_i), snow (q_s), and graupel (q_g). Upon investigation, we

see that cloud water dominates at cloud boundaries below 6.5 km and cloud ice is a greater contributor above this level. With that, in terms of relative importance, cloud water is more important for shell boundaries in the congestus regime whereas cloud ice is more crucial for deep convection with shells originating above 6.5 km.

With respect to microphysics scheme importance, the authors believe that choice of scheme and its behavior will impact results. For one, any negative buoyancy arising from evaporative cooling is directly dependent on the microphysics scheme and how phase changes occur. Both drop size distribution and number concentration of cloud droplets / cloud ice particles will determine both where cloud edge is and the magnitude of negative buoyancy in the shell following mixing.

Given this importance, the authors have included the following paragraph in the manuscript concerning the microphysics in the conclusion section in Lines 539-545: “Though subsiding shell accelerations are driven by negative buoyancy and pressure perturbations, we emphasize that such acceleration behavior and magnitude is strongly tied to subgrid scale choices such as the selected microphysics scheme. Shell identification itself is based on cloud edge, which is determined based on the number concentration of cloud water and cloud ice. Additionally, the magnitude of negative buoyancy from evaporative cooling is directly linked to number concentration, particle size distribution, and the magnitude of mixing from the subgrid turbulence scheme as well. With all this, selection of subgrid parameterization schemes must be taken with care, as acceleration magnitudes are at least partially dependent on these choices.”

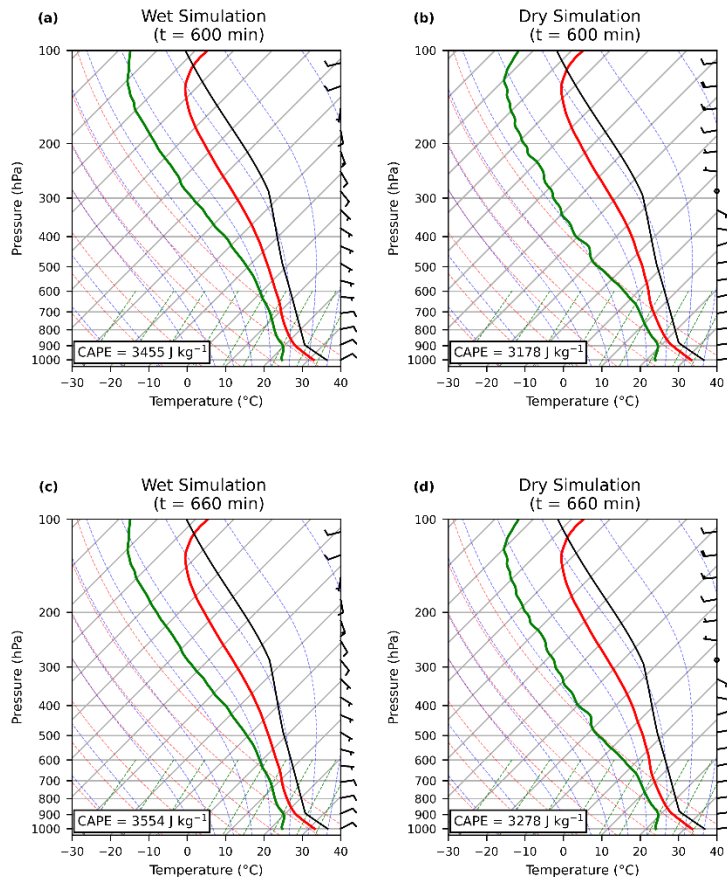
Main Point (ii): *I am missing a more detailed description of the general behavior of the simulation. This in particular concerns the cloud field. Do single clouds at all resemble the idealized picture of rising single cells as shown in Fig. 14? Do they consist of single rising thermals with a bipolar structure, or are they more complex, perhaps featuring more than one simultaneously? After all, one expects that some spatial organization takes place during these simulations. Such cloud shape heterogeneity could affect the downdraft behavior.*

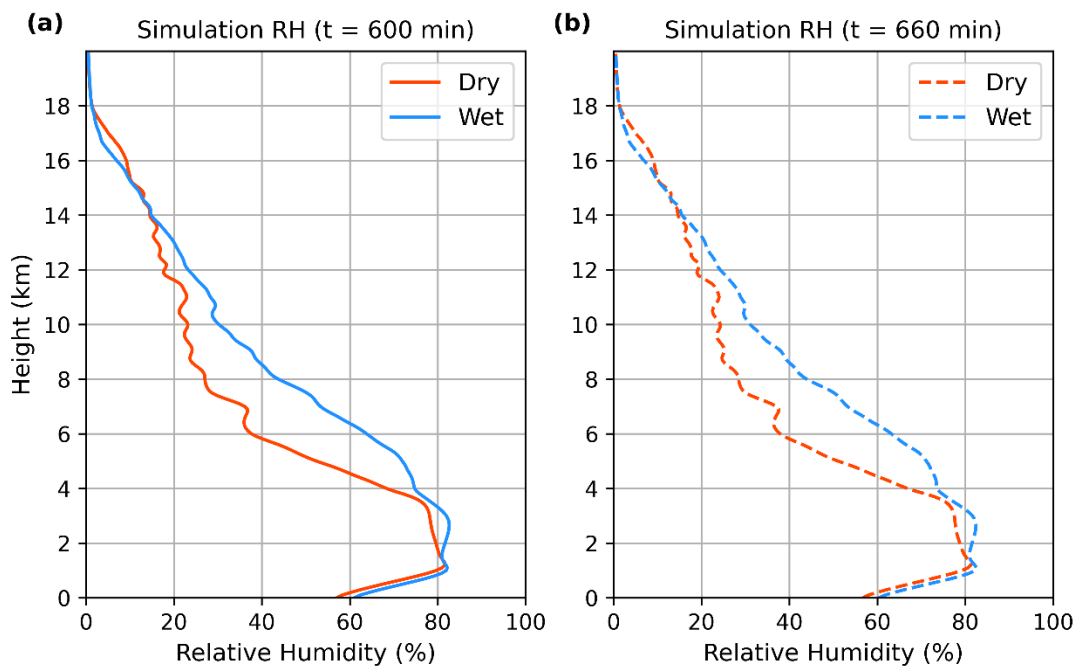
Author Response: The structure of thermals shown in Fig. 14 (now Fig. 18) is smoothed and implicitly simplified by the compositing procedure. The actual clouds were more complicated than this simple picture and often featured more than one thermal rising in succession. With that, the downdraft behavior may vary, especially when it comes to the relative contributions from the different types of accelerations. However, in this work, we aim to assess the general properties of

the shells over a cloud population. Future work may aim to look at the shell characteristics for different cloud types.

The second aspect I was wondering about is the evolution during the time window of analysis. Is the convective layer really in steady state, or not really? Nudging is the only prescribed larger-scale forcing, and only applied above 4 km. This probably means that the atmospheric profile below that height is still evolving. It would be informative to show this, also because it affects the deep convection aloft.

Author Response: The mean model profiles do not change much during the analysis window. However, the atmosphere continues to evolve over longer timescales, so this is not radiative convective equilibrium (RCE). We have clarified this point in the text on Lines 142-143, where we state, “However, the atmosphere continues to evolve over large time scales, so longer analysis periods may experience greater differences from start to end.” Additionally, we have attached versions of Fig. 1-2 for the beginning and end of the analysis window, highlighting the minimal change that the atmosphere undergoes.





Main Point (iii): *Perhaps my most important comment concerns the downdraft kinetic energy budget. It makes total sense to investigate downdraft acceleration due to B and VPG. But are these two forces considered to be the only contributors to the net downdraft acceleration? Judging from eq. (7) that seems to be the case. However, applying the common concept of an updraft model to a downdraft, I was thinking that diabatic mixing at the downdraft edge with its environment could also affect w . For example, a very recent study by Gu et al (2025, <https://doi.org/10.1175/JAS-D-25-0017.1>) also uses particle tracking, and shows that entrainment of air with different vertical velocity does affect the particle velocity budget, albeit for rising particles. I might be wrong, but I can imagine a similar mechanism also applying to sinking particles as part of a downdraft. I would recommend discussing the potential role of mixing on the net downdraft acceleration in Section 3.2, and perhaps also Section 3.4. A few more remarks on this:*

In LES such mixing takes place through resolved advection and subgrid diffusion at downdraft grid points. A quick check to find out if some term is missing could be to simply compare the sum of VPG and B to the net dw/dt (see Fig. 6). I assume the latter is diagnosed from the time series of w (Fig. 5)? If downdraft dilution by mixing is important, does it have more impact in the dry season than the wet season? And a related question: does mixing between shell and updraft air accelerate or slow downdrafts?

Author Response: We thank the referee for this suggestion and agree that greater clarification must be given as to how acceleration contributions are addressed. We also thank the referee for supplying the Gu et al. (2025) citation. Upon investigation of individual parcels, as well as the mean parcel properties, it is evident that the dw/dt term is not simply equal to the sum of B and VPG , and that other subgrid scale processes are contributing non-negligibly to the vertical velocity budget. Ultimately, we see they serve as a “dampening” term for dw/dt , where the general behavior of dw/dt is driven by B and VPG but magnitudes of dw/dt are altered by residual subgrid scale processes, specifically vertical acceleration from turbulence. We have decided to integrate multiple sections into the text discussing this subgrid scale acceleration and have updated all figures accordingly. For the scope of this study, we will continue to focus on B and VPG , as they are the two directly output accelerations in our model and make up the largest contributions to shell subsidence (and termination). We have also added the acceleration from subgrid processes to all figures that depict total acceleration. We thank the referee for bringing this concern to light, as it provides greater insight into the additional processes driving subsiding shells. The following paragraph was added in Lines 309-317:

“One acceleration that must also be addressed is that from subgrid scale processes such as turbulence. The subgrid scale acceleration, which does contribute to total acceleration within shells, is plotted in Fig. 7c. Though the acceleration is weak compared to B and VPG , it ultimately provides a dampening effect in both the dry and wet simulations. This subgrid term is likely physically representative of turbulent mixing. Because we see dampening, it is likely that, while shells are accelerated downward by B and VPG , turbulent mixing of updraft or environmental air into the shell acts to reduce net downward acceleration. This upward acceleration persists through the entirety of the downdraft. As for seasonal differences, dry season subgrid effects are larger than that of the wet season. Because turbulent mixing is tied to the horizontal shear at the interface of the updraft and shell, and since dry simulation updrafts and shells are stronger, the dampening effect from dry simulation turbulent mixing is greater as well.”

Subgrid acceleration mentions are now found in any text associated with figures that show it, all within section 3.2 and 3.3. We have also adjusted the equation (7) and added another KE equation to include subgrid acceleration. We again thank the reviewer for bringing the potential implications of the subgrid scale to light.

Main Point (iv): *My last point is more out of personal interest. Figures 5-9 show an impressive data collapse in downdraft properties as a function of time, both in w and its tendencies (B and*

VPG). But in Section 3.3 a significant variation in downdraft initiation height is shown (Fig. 10). Representing downdrafts in convection schemes requires information on downdraft initialization, so it is good to see it discussed here. Do downdrafts always initiate at the top of a convective cloud, as suggested in schematic Fig. 15? And if so, at what stage in the life cycle of the convective cloud? Or can downdrafts also start somewhere else, say, halfway the cloud? Plotting downdraft initialization height against convective cloud top height would give insight: is it a reasonable 1-to-1 relation? I understand that such analyses are not a direct objective of this study, so I leave it to the authors to include them or not.

Author Response: This is an interesting question that we plan to address with future research. To keep the scope of the paper narrow, we decided to not include plot of downdraft origin height versus convective cloud height. However, we added the following text to the manuscript in Lines 491-495 to highlight the implications of cloud life stage on downdraft origin: “Additionally, shell downdrafts may not necessarily begin at cloud top, as depicted in Fig. 18. Investigation of individual parcels revealed most subsiding shell downdrafts originated above a thermal, whether it be at cloud top or potentially a rising thermal near the base of the cloud. Such thermals may be present at different stages of a cloud's lifetime, which may have implications on the relative contributions of each acceleration to shell subsidence. The link between subsiding shell properties and cloud life stage will be investigated in the future.”

Reviewer #1 Detailed Comments:

Detailed Comment 1: *Line 84: At this point the Derbyshire et al (2004) study could be mentioned. Even though it was not based on Amazonian conditions and used idealized profiles, it did focus on RH variation in the free troposphere, similar to what is observed in the Amazons.*

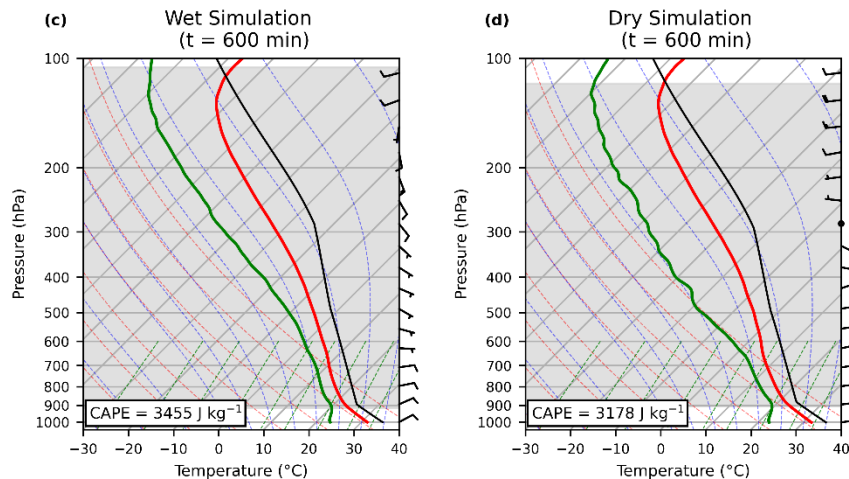
Author Response: We have included the Derbyshire et al. (2004) study by adding a sentence discussing the importance of RH variation for convective behavior. The new sentence is located after the discussion of free tropospheric RH varying based on season in the Amazon and is in Lines 83-84. It reads, “Such RH variation could have significant impacts on convective behavior, especially in relation to mixing of cloudy and environmental air (Derbyshire et al., 2004).”

Detailed Comment 2: *Line 105: Why is 100m sufficient to resolve downdrafts in this convective regime? Would an anisotropic grid spacing, often used in LES, affect the results negatively?*

Author Response: The first question is addressed under main point (i) with newly-added Appendix A. We chose the isotropic grid spacing out of simplicity.

Detailed Comment 3: Fig 1.: It would be informative to indicate the presence of the convective cloud layer in these diagrams, perhaps using grey background shading.

Author Response: We thank the reviewer for this suggestion. We have attached a version of Fig. 1c-d with the inclusion of grey shading representing the convective cloud layer, which we have defined as the layer from average cloud base to the average cloud top height during the analysis period of our simulations. Such a layer extends from the top of the boundary layer to the equilibrium level. With this, we have decided not to implement the grey shading in Fig. 1, as the entire free troposphere is considered to be the convective cloud layer. We also noticed a small error in the CAPE calculation that has since been fixed. The updated CAPE values are shown on the figure.



Detailed Comment 4: Line 122: The reader might not be aware what the default CMI settings are. That includes the microphysics scheme, the choice of which probably affects the results. See also my 2nd main comment above. This would be a good point in the text to describe it.

Author Response: We added the following description of the Morrison double-moment scheme following its introduction in Lines 124-128: “The Morrison double-moment scheme was used because it predicts both mixing ratio and number concentration for species crucial to our shell identification, including cloud water and cloud ice which are used to identify cloud edge. In our simulations, the scheme allows for resolved supersaturation and droplet activation is determined via Kohler theory rather than a reliance on the subgrid w (Morrison et al., 2005).”

Detailed Comment 5: *Line 123: Was Newtonian nudging applied? I know such nudging is frequently applied in LES and CRM studies, perhaps referring to a few of those as a precedent would work well here. In particular to studies that focused on deep convection.*

Author Response: We thank the referee for asking this question. The domain-averaged nudging applied follows the methodology of Alland et al. (2021). Newtonian nudging of only specific humidity was applied, specifically above 4 km. This height threshold was chosen in order to maintain the middle and upper tropospheric relative humidity differences between the two simulations, while maintaining each of the simulations’ unique lower tropospheric evolution. Nudging back to the initial specific humidity profile was applied throughout the entirety of the simulation, with a nudging time scale of 21600 s.

The authors have made the following addition to the text in Lines 136-137 to include previous studies using nudging: “This method of relaxation nudging is described in Alland et al. (2021), where the domain-averaged profile is relaxed towards its initial state over a specified time scale.”

Detailed Comment 6: *Line 130: An output time step of 30s is used. Do previous particle tracking studies suggest this time frequency is sufficient for resolving up/downdraft life cycles? I am thinking for example about the Hernandez-Deckers & Sherwood study (2016, DOI:10.1175/JAS-D-15-0385.1), what did they use to track thermals?*

Author Response: The Hernandez-Deckers and Sherwood (2016) study tracks thermals with an output time of 1 min. They found that their average thermal lifetime was approximately 4-5 min, so an average of 4-5 output time steps were yielded for a given thermal. Though our convective properties are different from theirs (i.e., greater updraft velocities, wider updrafts, deeper

thermals, longer lifetimes), we see that parcels within subsiding shells may only remain in the shell on the order of a few minutes. The authors therefore chose 30 s output to have a sufficient temporal resolution for accelerations while considering computational expense.

The authors believe that greater emphasis on why this time was chosen should be in the text. Therefore, when introducing the time step initially, we have added the following text to Lines 160-163: “Previous Lagrangian parcel studies have output parcel properties every few seconds to minutes (e.g., Hernandez-Deckers and Sherwood, 2016), but given that parcels are often within shells on the order of minutes, the 30 s parcel output is sufficient in assessing shell parcel properties while considering computational expense.”

Detailed Comment 7: *Line 160: Here prognostic pressure is mentioned. See my 1st main comment.*

Author Response: The authors have addressed this comment with the text supplied under main point (i).

Detailed Comment 8: *Line 196: The distance from the cloud edge of 800 m seems to be an important parameter in the analysis. Why was this value chosen?*

Author Response: We thank the referee for this question, as it has prompted us to include more detail in the manuscript as to why this value was chosen. Based on the Savre (2021) study, subsiding shells at the top of deep convective clouds are, on average, 800 m wide. However, Savre (2021) also note that shells associated with shallower clouds may be 400 m on average, with a maximum width of 2000 m observed with the deepest clouds. In their study, they defined deep clouds as those being greater than 1500 m in depth. The convection of interest in our study is much deeper than 1500 m (including our congestus convection), but the authors determined that using the mean distance of 800 m from the Savre (2021) study was a sound way to identify deep convective shells. Also, the authors would like to further emphasize what this 800 m threshold is. Specifically, we want to gather shells that have their core, or point of strongest subsidence, within 800 m of the shell point closest to the updraft (can be thought of as cloud edge, but does not exclude in-cloud shell points). Because Savre (2021) found shells to be 800 m

wide on average, this threshold ensures that we can gather shells of this width while not sampling other shells or features not associated with the updraft of interest. This does not guarantee shells will be this wide – in fact, only a few shells reach or exceed this width.

We also would like to note that this referee comment prompted us to discover an error in the code constructing the composites, where the 800 m threshold was accidentally applied as the updraft core to shell minimum distance, and not shell edge to shell minimum distance. The new composites have been placed in the manuscript, and the overall result does not appear to change significantly. One result that did change is that dry and wet simulation composited deep convective shells now have nearly identical vertical velocities. We believe that this is potentially an artifact of the compositing process due to shell asymmetry and variance. Such a result does not arise in the congestus composite. We have addressed this in the text in Lines 436-439: “Though composites of w show shells of similar strength, the parcel trajectory analysis supports stronger shells in the dry simulation. One potential reason for not seeing a stronger shell in the dry composite is the asymmetrical nature of shells and potential smearing from that variability. Therefore, it is crucial that the composites are interpreted as a visual for general updraft-shell structure.”

To address this main comment in the manuscript, the authors have added the following text after the initial mention of the 800 m threshold in Lines 219-225: “The 800 m horizontal threshold was chosen in conjunction with findings from Savre (2021), where it was determined that subsiding shells associated with deep convection were 800 m wide, on average. This threshold was also chosen to avoid sampling shells from surrounding clouds or other features away from the updraft of interest. Deep convective shells in the Savre (2021) were found to be upwards of 2000 m wide, despite a conservative deep convection depth threshold of 1500 m. As will be shown, the convection in the present study’s simulations exceeds this depth and, with wider shells being associated with deeper convection, the maximum shell edge to shell core distance of 800 m is appropriate.”

We have also added a new figure to highlight shell widths associated with all cases included in the composites. The new figure is in section 3.3 and is labeled as Fig. 15. Its associated text on Lines 425-435 is as follows: “As shown in the previous section, differences may arise in the properties of shells associated with deep versus congestus convection. Shell width is one of those properties, and the widths of the shells that were used in the compositing process are shown in Fig. 15. Despite a mean deep convective shell width of 800 m identified in Savre (2021), the shells in the present study’s simulations are much narrower. Dry simulation deep and congestus shells have mean widths of 480 m and 420 m, respectively. Mean values are the same in the wet

simulation, meaning shell width primarily depends on the depth of the convection. These low width values are surprising given that the updrafts in this study are deeper and stronger than those in Savre (2021). The congestus shell widths align with the findings of that study, but the deep shell widths are nearly half of their noted deep shell average. However, this reported average was specifically taken from cloud top, and Savre (2021) noted that widths of shallow and deep shells were fairly similar away from the very top of the cloud. With this, shells in deeper convection are slightly wider, but the magnitude more closely matches the mean shell width found away from the very top of the cloud (see Fig. 4 in Savre (2021)).”

Detailed Comment 9: *Line 203: See my 2nd main comment, this would be a good point in the text to discuss the cloud field and the time evolution in general in a bit more detail*

Author Response: During the analysis period, widespread convection was present with precipitating deep and congestus convection present. Deep convective downdrafts produced cold pools which assisted in igniting new convective updrafts. Congestus convection was also present, hence the analysis on congestus shells later in the manuscript. Plenty of cloud heterogeneity existed in the simulation.

To highlight this, we have added a new figure, labeled as Fig. 4. This figure shows an example of one deep convective updraft, which clearly shows the turbulent nature present in and around clouds. A congestus updraft is also shown. In both cases, clear subsiding shell features are present. We have decided to briefly discuss the cloud heterogeneities among the regimes in Lines 240-246: “In addition to deep convection, congestus convection developed as well. Cloud properties of convection in the deep and congestus regime differ which, as shown in Section 3.3, has large implications on the behavior of accelerations driving subsiding shells. Deep convection is wider than that of congestus and is often characterized by multiple updraft thermals. The strongest surface-reaching downdrafts and heaviest precipitation are also associated with simulated deep convection. Though significant heterogeneity exists among cloud structures within each regime, examples of cloud structures in the deep and congestus regimes are shown in Fig. 4. Subsiding shells are present at cloud edge near cloud top in both the deep and congestus clouds in Fig. 4, as well as down the entire right side of the congestus cloud.”

We also added the following sentence in Lines 232-233, briefly characterizing the convection that was initiated: “Deepening of the boundary layer allowed for the widespread development of slow-moving convection by the start of the analysis period.”

Detailed Comment 10: *Fig 3: I always thought Hovmueller diagrams are plots of a horizontal cross section versus time. I am fully ok with calling these time-height plots Hovmuellers, but perhaps one could insert “Vertical” before.*

Author Response: We thank the referee for this suggestion and have decided to implement it. We have inserted “Vertical” within the caption for Fig. 3.

Detailed Comment 11: *Equation (6) and line 17: How dependent are the results on the choice of mixing length L ? Where does this value come from, is it regime dependent (shallow vs deep)?*

Author Response: L should scale with the model grid spacing. This value is slightly larger than the 120 m used in Morrison et al. (2023), who used a 50 m grid spacing. Reducing L decreases the fractional entrainment rate and makes the profiles of parcel buoyancy look more like that of an undiluted parcel.

Detailed Comment 12: *Fig. 5: One could plot w against normalized height as well. Does it show the same data collapse for both seasons? Is it worthwhile to add it as a 2nd panel here?*

Author Response: We thank the reviewer for this suggestion, as it allows us to investigate shell parcels from a new perspective. We created a plot of w versus normalized height, and it appears to be similar to the normalized time plot, but shell vertical velocities are stronger. This result is interesting, but likely indicates that time normalization is smoothed out by maximum parcel downdrafts occurring at different times within the downdraft, whereas the height normalization shows peak downdraft strength at one specific part of the descent. While we maintain normalized time as the x-coordinate for all parcel plots, we have added a second panel to Fig. 6 (previous Fig. 5) to show the difference that arises with normalization coordinate and briefly mention this in the Fig. 6 description within the text in Lines 280-286: “The normalized downdraft time coordinate in Fig. 6a allows parcel properties to be compared based on downdraft start time and downdraft end time, whereas the normalized height coordinate in Fig. 6b is used to compare parcel properties based on downdraft origin height and termination height. Immediately, it is evident that use of the normalized height coordinate yields stronger shells. This is because the strongest shell subsidence may occur at different times in the downdraft but always occurs about

halfway down the downdraft. For the remainder of this analysis, we choose to use the normalized time coordinate (Fig. 6a) because understanding time evolution of parcel properties such as acceleration is prioritized in this study.”

Detailed Comment 13: *Fig 6c: How is net acceleration calculated, simply as the sum of B and VPG , or independently from the time change in w (Fig.5)?*

Author Response: Total acceleration was initially the sum of B and VPG , but is now the sum of those accelerations and acceleration from subgrid processes. The discussion and additional text concerning this change is under main point (iii).

Detailed Comment 14: *Fig 10: Panel c is interesting, in that it seems to suggest that shells don't travel that far on average. Is there an analogy with the limited displacement distances found for rising thermals, in various recent studies (again the Hernandez-Deckers & Sherwood 2016 paper, and others)?*

Author Response: We think that different processes are responsible for the limited downward displacements of downdrafts, compared to that of rising cloud thermals. For instance, downdraft parcels terminate their descent in our simulations due to upward pressure gradient accelerations and upward buoyancy accelerations as they pass below thermal toroids and their levels of neutral buoyancy. Hence, the limited vertical extent of thermal toroidal circulations and lack of latent cooling in shells (which would be needed to offset their adiabatic warming) limits downdraft depth. In contrast, the balance between buoyancy and pressure drag limits how far buoyant thermals ascend.

To further this discussion, we added the following text to the manuscript in Lines 391-394: “Such displacement magnitudes are similar to those found in Hernandez-Deckers and Sherwood (2016), who looked at ascending thermals with a limited displacement due to drag supplied by nonhydrostatic pressure perturbations. In the case of our descending motion within the shell, we believe displacement is limited partially by the overshooting of the level of neutral buoyancy by parcels, and primarily by the upward-directed VPG_{DN} .”

Reviewer #2 General Comments:

The manuscript studies the mechanisms driving downdrafts at the edges of deep convective clouds. It specifically looks into the role of the environment in which these clouds are developing, mainly the effect of relative humidity in the free troposphere. Though such studies have been done before for shallow cumulus clouds, studies for deep convective clouds have been less common. The authors use LES along with a lagrangian parcel trajectory analysis to study the effect of buoyancy and vertical pressure gradients in driving the subsiding shell. The manuscript is well written, and the analysis seems sound for the most part, though I do have some concerns with some more fundamental choices made for the study. My comments and concerns are listed below:

Author Response: We thank the reviewer for dedicating time to both read and review the manuscript. All reviewer comments were insightful and we believe suggested changes by the reviewer improve the manuscript.

Reviewer #2 Main Points:

Main Point (i): *Choice of spatial resolution.*

The resolution of 100m is surprisingly low and I believe you are not close to resolving the shell. How do you justify such a low resolution, and have you done any kind of sensitivity studies to check the effect of spatial resolution? Could you possibly do some kind of grid stretching in one direction to increase your resolution and reduce costs? Was there a reason to choose an isotropic grid? I also do not see any mention of the actual physical width of the shell?

Author Response: Justification of spatial resolution and associated changes to the text are addressed in the response to Reviewer #1 main point (i).

Main Point (ii): *Novelty of the study and new findings*

Though you mention that studies on the driving mechanisms of deep convective shells are less common, there have been a few studies. Could you make very clear what exactly the new findings are? Maybe in the conclusions? Savre 2021 seems to make some similar findings (though using an idealised CRM?), and maybe it is worth comparing the results to existing studies to confirm

which ambiguities you address and solve? You do make a couple of hypothesis in the introduction motivated by observations. Can you sharpen the conclusion to make it more explicit which hypothesis you are confirming?

Author Response: Many of the findings in this work are similar to that of Savre (2021), specifically that cloud top pressure perturbations are a primary driver of subsiding shells. However, our study is different from that and other previous shell studies in that:

- We utilize findings from GOAMAZON and the previous finding that hydrometeor-loaded downdrafts are stronger during convective events with reduced relative humidity aloft to investigate shell differences resulting from differences in relative humidity
- We investigate deep convective shells from both a Lagrangian and Eulerian framework, which was not done in the Savre (2021) study. However, Hernández Pardo et al. (2026) was published during this review, and their results assess shells from both perspectives as well, though they focus on single cloud experiments rather than an ensemble of simultaneously-occurring clouds
- We note that shell strength significantly depends on updraft strength and that the dry season shells are not necessarily stronger because of greater evaporative cooling rates at cloud edge, but rather because the deep convective updrafts in this environment are stronger and have a greater dynamic pressure acceleration
- We show that the relative magnitude (and behavior) of accelerations driving shells is dependent on cloud type or stage, with shallower clouds having a greater contribution from evaporative cooling than dynamic effects. To the authors' knowledge, no shell study has investigated congestus convection with our definition previously, even though it serves an important role in the shallow-to-deep convective transition

The authors believe that the above findings are crucial in advancing our understanding of subsiding shell dynamics, especially those associated with deep convection. However, we agree that our text must be more clear in addressing our hypotheses and emphasizing the new findings. Therefore, we have reworked the conclusions section to include these main findings and address the hypotheses from the introduction:

Addition of numbered conclusions: “After completion of this assessment, the following conclusions were made regarding shell characteristics:

1. Both negative buoyancy from evaporative cooling and flow deformation, as well as dynamic perturbation pressure accelerations associated with thermally-driven toroidal circulations drive shell subsidence.

2. Subsiding shells are stronger in the drier environment not because of greater evaporative cooling rates, but because updrafts and their resultant dynamic perturbation pressure accelerations are stronger. This highlights the large dependency of shell strength on updraft strength.
3. Despite a greater contribution from negative buoyancy, congestus convection has weaker subsiding shells than deep convection due to a significantly reduced contribution from dynamic pressure accelerations.

Text added to Lines 525-529 to support hypothesis we confirmed: “With this, the hypothesis motivated by Giangrande et al. (2023) concerning the link between updraft strength and downdraft response is confirmed. Though greater rates of evaporative cooling may have been found at cloud edge, utilizing an effective buoyancy framework exhibited limited difference in buoyancy between the two seasons, implying that any increased evaporative cooling in the dry season is canceled out by a counteracting buoyancy pressure acceleration.”

We added an additional paragraph in Lines 530-538 to support conclusion #3: “Dividing convection into deep and congestus regimes showed that magnitude and behavior of the accelerations driving subsiding shells is also dependent on cloud properties. Deep convective subsiding shells were much stronger than their congestus counterparts due to an increased contribution from the dynamic pressure accelerations. These larger pressure contributions arose because the deep convective updrafts were stronger, leading to an increase in toroidal circulation strength and its associated pressure gradient acceleration. Interestingly, despite having weaker updrafts and pressure accelerations, a greater impact from negative effective buoyancy arose in the congestus regime. This was likely the result of a weaker buoyant pressure acceleration coupled with more persistent evaporative cooling through the depth of the shell. Such a result emphasizes the importance of treating subsiding shells differently in shallow versus deep convection rather than assuming shells are driven by one primary mechanism for all clouds.”

Reviewer #2 Detailed Comments:

Detailed Comment 1: *L34: Maybe remove ‘recently’? Your first reference itself is from 1990!*

Author Response: We have removed ‘recently’ as suggested.

Detailed Comment 2: *L37: at 'the' cloud edge*

Author Response: We have added 'the' as suggested.

Detailed Comment 3: *L37: protect cumulus clouds from 'dilution'?*

Author Response: We have changed 'from environment' to 'dilution' as suggested.

Detailed Comment 4: *L52: Rodts et al 2003 showed evaporative cooling as the dominant mechanism before Heus and Jonker 2008? Maybe re-order this or rephrase/ remove 'corroborate' here?*

Author Response: We have rephrased, '...corroborated the predominantly evaporation-driven nature of shells...' to '... had previously supplied evidence for this result with aircraft observations...' as suggested.

Detailed Comment 5: *L105: The resolution of 100m is surprisingly low and you are getting nowhere close to resolving the shell I believe. How do you justify such a low resolution?*

Author Response: We have addressed this under main comment #1 and have included an appendix in the manuscript to show the impact of changing the grid spacing.

Detailed Comment 6: *L110: Have you tried this for a different background temperature profile? And also, do you expect changes to your results if you started with different temperature profiles? Additionally, have you tried a case with a different magnitude of RH difference between wet and dry?*

Author Response: Changes to the thermodynamic profile will likely have significant implications on our results. We only used the dry and wet season specific humidity profiles, while using a consistent temperature profile (that of the dry season) for both simulations. The raw temperature profiles for both seasons are not significantly different from each other, so adjusting the wet season temperature profile to match that of the dry season to isolate the RH effects was appropriate. We did not perform any additional simulations with different temperature or RH profiles, or cases with a different magnitude of differences between the two seasons, but we believe that our results would change significantly in that the properties of the convection will change. For example, any changes to the temperature or moisture profiles would change the CAPE. Given the direct correlation between updraft strength and CAPE, and because the shell strength is strongly-dependent on the updraft-dependent magnitude of the pressure gradient associated with the toroidal circulation of thermals, any CAPE change could yield different shell strengths.

From the perspective of negative buoyancy, a drier atmosphere may supply a more detrimental effect from entrainment, leading to increased evaporative cooling, more negative buoyancy, and stronger shells as a result. However, the same effect would potentially lead to narrower updrafts, which would have a weaker toroidal circulation and associated dynamic nonlinear pressure acceleration. Though it is likely that the behavior of the accelerations driving shells would not change with altered temperature and humidity profiles, it is more likely that magnitude of accelerations would change, leading to differing shell strengths. Already, we see that difference appearing between the dry and wet seasons, so any additional changes to the thermodynamics would likely manifest in a similar manner.

Detailed Comment 7: *L114: This slightly stronger vertical wind shear should affect your results? Can you not have the same shear?*

Author Response: The authors appreciate this comment, as supplying each simulation with the same shear profile would remove a potential culprit for simulation differences. However, even though we adapted the temperature profiles to isolate RH differences, we wanted to preserve the integrity of each season as effectively as possible. Additionally, given how weak the shear is, we believe that shear has a negligible impact on the characteristics of the convection and results.

We have added the following text in Lines 121-122 to address this: “Though simulation wind profiles differ, wind shear is weak and differences in convective characteristics attributed to wind shear are likely small.”

Detailed Comment 8: *L124: Please define q_v . Maybe in L120 where you talk about water vapor mixing ratios?*

Author Response: We defined q_v in the introduction on Line 53.

Detailed Comment 9: *L135-137: If I understand your description correctly, the shell is defined as regions with $q_l < 0.01 \text{ g/kg}$ and $w < 0$? How about the regions with $q_l > 0.01 \text{ g/kg}$ and $w < 0$? Have you checked how significant these regions are? I guess I am referring to the validity of the assumption that all cloudy points have a positive vertical velocity. Nair et al 2021 talk about different interfaces (though thresholds are set on buoyancy and vertical velocity) and it might be worth checking if there are cloudy parcels with negative velocities that should be assigned as the ‘downdraft’ and if that affects your results.*

Author Response: We thank the reviewer for raising this point, as other shell studies have focused on the “inner” shell that can occur within cloud. The Nair et al. (2021) shell study highlights the interfaces, referring to a shell (buoyancy-based) and a subsiding shell (w -based) tied to shallow cumulus. Both Nair et al. (2021) and Savre (2021), among others, discuss how there is negative buoyancy within the inner shell that is found at the immediate cloud edge. Savre (2021) assume this inner shell is on the outside of the cloud edge, as we have done with our shells. However, the authors have decided to look at negative vertical velocity points near the cloud boundary within the cloud.

When including points near but within the cloud boundary where $w < 0$, $q_l > 0.01 \text{ g/kg}$, and where hydrometeor concentration is less than 0.01 g/kg (to remove any hydrometeor-loaded downdrafts) as shell points, there is a significant increase in the number of parcels that pass through shells. The number of dry simulation shell parcels increase from 1148 to 5024, and from 749 to 4515 in the wet simulation. Such an increase likely resulted in concurrence with the large increase in shell area with the inclusion of the new points. Despite the inclusion of new points

and new parcels, we find that the overall behavior of the accelerations does not change. When focusing on B and VPG , we see a slight increase in the subsidence contribution from B , with a greater decrease in the VPG contribution. With this, inclusion of the in-cloud shell points leads to weaker shells overall, though the driving acceleration behaviors are generally the same as if these new points were not included.

With this, the authors thank the reviewer for this comment again, as it has prompted us to include a new figure in the manuscript and text associated with it. This figure is now labeled as Fig. 9 and the following text was included in Lines 336-353:

“Previous subsiding shell studies have noted the presence of downward acceleration just within the cloud edge as well, where in-cloud negative B may be present (e.g., Katzwinkel et al., 2014; Nair et al., 2020; Savre, 2021). To ensure that the accelerations shown in Fig. 7 - 8 are representative regardless of shell definition, we applied the same analysis to a new set of identified shell points, where $w < 0$ m/s and total cloud water mixing ratio was allowed to exceed 0.01 g/kg. Total non-cloud hydrometeor mixing ratio was also restricted to be less than 0.01 g/kg in order to filter out any in-cloud hydrometeor-loaded downdrafts. However, as was done previously, points were only classified as shell points if they were in the vicinity of the cloud boundary. After identifying the parcels that passed through these new shell points, parcel w , VPG , B , and total acceleration were gathered (Fig. 9).

With in-cloud points included as shell points, mean shell parcel w magnitude decreases, especially in the dry season (Fig. 9a). Such weakening of subsidence is due to a substantial decrease of the VPG contribution, specifically in the VPG_{DN} term. Such decrease could arise due to the increased parcel sample size, but it is also possible that pressure gradients are slightly stronger on the outside of the cloud versus inside. Despite the VPG magnitudes de-amplifying, B becomes more negative throughout the downdraft. Aligned with previous literature mentioning negatively buoyant cloudy air, it appears as though many of the new points and parcels identified are cloudy and negatively buoyant, leading to the greater contribution to subsidence from B . This negative buoyancy arises from injection of environmental air into the cloud and the resulting evaporation/sublimation of a high volume of cloud water or ice. Despite the changes in subsiding shell w from the acceleration magnitude variation, the general behaviors of the accelerations are the same. Therefore, whether in-cloud downdraft points are included or not, the accelerations driving subsiding shells are the same, regardless of shell definition.”

Detailed Comment 10: *L134: Is q_c calculated with a saturation adjustment scheme? The same with ice nucleation. With a resolution of a 100m this could be important? Is there any way in which you can show/plot q_c and q_i means across the width of the cloud and shell?*

Author Response: q_c and q_i are directly output from CM1, specifically from the Morrison double-moment microphysics scheme. This scheme only uses a saturation adjustment for large-scale (grid spacing > 1 km) and large time-step (> 10 s) simulations (Morrison et al., 2005). Large-scale is defined as the scale in which local cooling rates from vertical motion cannot be resolved. In our simulations, which have a model time step of 1 s and grid spacing of 100 m, we see that relative humidity exceeds 100%, meaning that the model is explicitly resolving supersaturation with the microphysics scheme. Therefore, cloud water and ice mixing ratios are calculated using the condensation/evaporation and deposition/sublimation rates in Eqns. (8), (9), and (18) of Morrison et al. (2005), and not with a saturation adjustment scheme.

To address this question, we added a description of the double-moment scheme and its use in our setup in Lines 124-128: “The Morrison double-moment scheme was used because it predicts both mixing ratio and number concentration for species crucial to our shell identification, including cloud water and cloud ice which are used to identify cloud edge. In our simulations, the scheme allows for resolved supersaturation and droplet activation is determined via Kohler theory rather than a reliance on the subgrid w (Morrison et al., 2005).”

Based on our definition of the shell, both q_c and q_i would be less than 0.01 g/kg within the shell in all cases. However, to exhibit the location of cloud edge relative to the shell, the authors have added the median cloud edge ($q_c + q_i = 0.01$ g/kg) to all vertical composite plots. Median cloud edge was chosen because mean cloud condensate is inflated by infrequently occurring large values that are away from the updraft (cirrus, for example). Therefore, plotting the median cloud condensate better shows the edge of where 50% of the clouds are, and it aligns nearly perfectly with the shell core.

Detailed Comment 11: *Is q_c or q_i more important in setting the shell boundaries? How big a role do you expect the microphysics scheme and the relatively simple droplet activation/ice nucleation parameterization to play in the identification of the cloud and shell boundaries and ultimately your results?*

Author Response: We thank the reviewer for raising this point. Upon investigation, we see that cloud water dominates at cloud boundaries below 6.5 km and cloud ice is a greater contributor above this level. With that, in terms of relative importance, cloud water is more important for shell boundaries in the congestus regime whereas cloud ice is more crucial for deep convection with shells originating above 6.5 km.

With respect to microphysics scheme importance, the authors believe that choice of scheme and its behavior will impact results. For one, any negative buoyancy arising from evaporative cooling is directly dependent on the microphysics scheme and how phase changes occur. Both drop size distribution and number concentration of cloud droplets / cloud ice particles will determine both where cloud edge is and the magnitude of negative buoyancy in the shell following mixing.

Given this importance, the authors have introduced the following paragraph in the manuscript concerning the microphysics in the conclusion section: “Though subsiding shell accelerations are driven by negative buoyancy and pressure perturbations, we emphasize that such acceleration behavior and magnitude is strongly tied to subgrid scale choices such as the selected microphysics scheme. Shell identification itself is based on cloud edge, which is determined based on the number concentration of cloud water and cloud ice. Additionally, the magnitude of negative buoyancy from evaporative cooling is directly linked to number concentration, particle size distribution, and the magnitude of mixing from the subgrid turbulence scheme as well. With all this, selection of subgrid parameterization schemes must be taken with care, as acceleration magnitudes are at least partially dependent on these choices.”

Detailed Comment 12: *L140: Are the parcels uniformly distributed across x and y?*

Author Response: Yes, parcels are distributed uniformly every 500m across x and y. We have rewritten the sentence in Line 158 to be, “At the initial time step, 5,494,536 parcels were distributed uniformly every 100 m from 1.5 km to 15 km in the vertical and every 500 m horizontally across the entire domain.” Thank you for bringing this to our attention.

Detailed Comment 13: *L150: Is this a commonly applied adjustment in LES or is this the first time this is being done?*

Author Response: We thank the reviewer for asking this question. In LES studies, it is common to compute buoyancy relative to a horizontally-averaged state at a given model time and not relative to the initial model state (which is done in CM1). Though previous LES studies do not explicitly say how buoyancy is computed, as it is often output by the model itself, we performed this adjustment following investigation of the buoyancy field which was positive at all grid points. Such an adjustment not only computes the true buoyancy relative to the horizontally averaged state at a given time, but also maintains the integrity of the horizontally averaged state itself by accounting for what the buoyancy at the initial model time step was. Additionally, applying the adjustment to the pressure field as well allows for dw/dt to be correct no matter what the initial buoyancy was.

Detailed Comment 14: *L226: How are you calculating the diluted and undiluted profiles? Are these from the parcels in the LES?*

Author Response: Diluted and undiluted parcel profiles are calculated using a surface-lifted parcel with the domain-averaged properties of the surface. Individual parcels used for the trajectory analysis are not used for this calculation. Calculations of the diluted and undiluted profiles can be found within the references on Lines 257-259.

Detailed Comment 15: *L284: How much would adding wind shear affect the results?*

Author Response: We thank the reviewer for asking this question. Heus and Jonker (2008) investigated the relative role of wind shear on subsiding shell properties, though their study focused on shallow cumulus. However, they found that the addition of wind shear led to asymmetries in both the humidity and shell distribution around the cloud. On the upshear side, the side in which the cloud core is found to be closer to cloud edge, there was practically no existence of subsidence at cloud edge. On the downshear side, high-humidity air extended well away from the cloud edge, along with a broad region of subsidence. With this, it is clear that the addition of wind shear essentially forces the symmetrical shell to the downshear side only. There may also be impacts from shear on the updraft properties that feed back into shell behavior.

We have added the following paragraph to the manuscript's conclusion to address the implication of wind shear:

“Increasing vertical wind shear would also change the characteristics of the shells. As discussed in Heus and Jonker (2008), the presence of strong wind shear led to shallow cumulus shell asymmetry, with subsidence only existing on the downshear side of the cloud. This shell region was wider and collocated with a region of high relative humidity downshear of the cloud (Heymsfield et al., 1978; Laird, 2005; Heus and Jonker, 2008). In this study's simulations of deep convection, a similar mechanism could arise if shear were to increase. The tilting of clouds, which would predominately be the case for the congestus regime, would likely lead to the shell asymmetries discussed in Heus and Jonker (2008). Shells associated with wider and deeper updrafts may undergo similar changes if the magnitude of the shear was large enough to significantly tilt the updrafts. Additionally, how the increase of shear affects updraft strength should be considered. As has been shown in studies such as Peters et al. (2020), an increase in shear is directly correlated with wider, less diluted updrafts that can have a larger maximum vertical velocity. With this, if vertical velocity of updrafts increases in response to the shear increase, then dynamic nonlinear pressure perturbation accelerations may prove to be larger. A stronger updraft would also lead to more horizontal shearing from turbulence at cloud edge, which could increase mixing, evaporative cooling, and negative buoyancy. Therefore, it is fair to hypothesize that increasing the vertical wind shear could have both a direct impact on shell asymmetry, as well as an indirect impact on shell strength via an alteration of the updraft properties.”

Detailed Comment 16: *L328: Maybe remove the word 'both' and include '(a) and (b) respectively' at the end of the sentence. 'parcels passing through both deep and congestus shells' is a bit confusing/misleading.*

Author Response: We have changed the sentence in Line 399 to read, “Mean w of parcels passing through (a) deep and (b) congestus shells is shown in Fig. 13.” as suggested.

Detailed Comment 17: *L330 and throughout the manuscript: Could you remove the space between m and s^{-1} to make it ms^{-1} .*

Author Response: The authors have reduced the space between each set of units throughout the manuscript, but maintained partial spacing in accordance with SI and ACP guidelines on multiplication of units.