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Comment on acp-2021-586

Daniel Hernandez-Deckers et al.

Author comment on "Updraft dynamics and microphysics: on the added value of the cumulus thermal reference frame in simulations of aerosol–deep convection interactions" by Daniel Hernandez-Deckers et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-586-AC1, 2021

In the following we provide a point by point response to comments by Reviewer #1, where we quote in italics the original comment. We have numbered all comments in boldface, based on the reviewer number and comment number so that the different replies can be easily referred to within the text (e.g. R2C3 refers to comment number 3 from Reviewer #2).

"This paper uses three hours from a borderline-resolution large-eddy simulation of convection over Houston to analyze differences between results from two approaches to sampling cloud elements. The first method, called the cloudy-updraft method, identifies the active portions of clouds as the regions where there is coincident cloud condensate and upward motion. The second method, referred to as the tracked thermals method, is more complicated and uses a tracking algorithm to look for peaks in the vertical velocity field, infer the associated thermal bubbles, and then track them over time. Each method is applied to a series of simulations that differ solely by the aerosol concentration used with the cloud microphysics. The results show that the overall story about aerosol-cloud processes is similar between the two approaches, but an apparent sampling bias leads to subtle differences. The use of a range of aerosol concentrations also highlights the difficulty of separating signal from noise in aerosol-cloud analyses.

Overall, the paper is well presented—it is both organized well and the English is very clean. The science is sound and the comparison between methods is an important investigation that informs how researchers can use and intercompare results from these methods in the future. This is where the primary value lies in this paper, as the findings for the aerosol-cloud processes essentially mirror what has been found in prior studies.

*This paper will be suitable for publication after addressing a small number of questions and
suggestions noted below."*

We thank the reviewer for this summary and reply in the following lines to each specific comment:

"Specific Comments

R1C1: *(1) Intro.*

The introduction builds from the ACPC MIP in terms of indicating that the differences between models are typically larger than the sensitivity to aerosol details within a model. This makes it hard to then infer true process rates and details for use in subsequent parameterization development, etc. The argument is then made that one should disentangle the dynamical details of the deep convection from the microphysical processes. The implication is that the bubble tracking procedure achieves this. However, this misses the points that the model-to-model differences still exist, and many of the microphysical factors within the thermals rely upon the dynamics, such as the supersaturation being dependent on the vertical velocity. More importantly, if the present study were repeated using a different model, e.g., that treats autoconversion and other processes differently, the results could substantially change, such as happened between models in the MIP. There is likely a better way to frame the introduction to be more useful for the study that is presented."

Indeed, one limitation of this study is that results are based on a single model, and it is important to mention this more explicitly. We have added now the following text to the introduction starting at line 81 (lines 87-91 of track changes version): "Here we investigate the dynamics/microphysics coupling using a single model and case study with two analysis approaches; because differences between both models and case studies are expected (e.g., Tao et al., 2012; Marinescu et al., 2021), however, it will not be possible to establish the generality of our results to other models and scenarios without future work, whose potential merit may nonetheless be in part guided by our initial findings here."

R1C2: "*(2) Even though this set of simulations is based on a realistic setup, the presentation of the results essentially assumes this is an idealized setup. There is no reference to an observation anywhere in the paper in relation to the simulation or results. This paper would be strengthened by putting the aerosol and meteorological state in the context of reality. Is the simulation anywhere close to the observed conditions for this day? Which aerosol concentration is closest to reality?"*

We have added the following sentence starting at line 109 (lines 123-126 of the track changes version): "The polluted and clean aerosol size distributions and vertical profiles were based on the data from Deriving Information on Surface conditions from Column and Vertically Resolved Observations Relevant to Air Quality (DISCOVER-AQ) in September 2013, as well as satellite-based estimates (Rosenfeld et al. 2012) near Houston on 19 June 2013. Timing of satellite CCN observations are identical to the simulation dates."

R1C3: "*(3) L96*
Please state the model top, as this is relevant in relation to both the number of levels and the height of the convection."

We have added the following sentence at line 97 (line 109 of the track changes version): "Model top is approximately 22 km (50 hPa)."

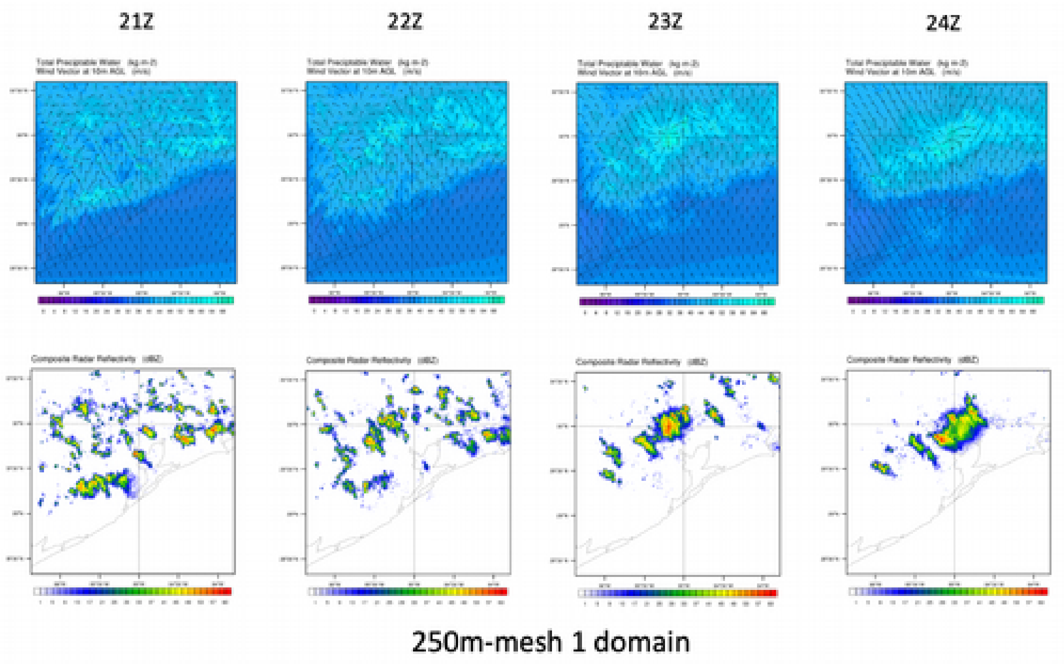
R1C4: "*(4) L109–10*
I have some concern about the use of a single domain with such high resolution driven only by the FNL. Assuming the 0.5° FNL data set was used, that is roughly a factor 200 jump in grid spacings along the lateral boundaries. How much more accurate would the results be if a more traditional nested approach were used to step down to Δx=250 m from the Δx=0.5° of the FNL?"

This is a case-specific semi-idealized simulation. Obviously, the model downscaling in these experiments far exceeds the traditional stretching ratio (3:1~5:1), because our intention is to create thermal bubbles of isolated convection driven by sea-breeze circulation (Fig. 1). In particular, NU-WRF have used quilting and compressed IO options in WRF, which asynchronously passes model output memory to a set of computational nodes for dumping WRF output with compression format (sizes of 1000x1000x90 grids) at one-minute intervals. While this option enables us to generate a number of LESs with limited computational resources, this often crashes with multiple nested domain options.

Fig. R1 shows that the single-domain simulation tends to invigorate isolated convection and is more concentrated over the Houston area later in time than in the triple domain simulation. It also misses the convection in the northeast of the simulation domain in comparison with the radar observation (Fig. R2).

Certainly, forecasting skill can be degraded in the single domain. But our purpose is to generate sea-breeze-driven isolated convection for a given set of land-ocean contrast in surface temperature and pressure with mean flow from boundary conditions. Thus, the single domain set is convenient and enough for our science objective.

We added the following sentence starting at line 95 (lines 105-107 of the track changes version): "This type of domain setting exceeds the traditional downscaling ratio (1:3~1:5), resulting in reduced precipitation forecasting skill compared to multi-nested domains. However, it successfully generates thermal bubbles of isolated convection driven by sea-breeze circulation for a given computational resource."



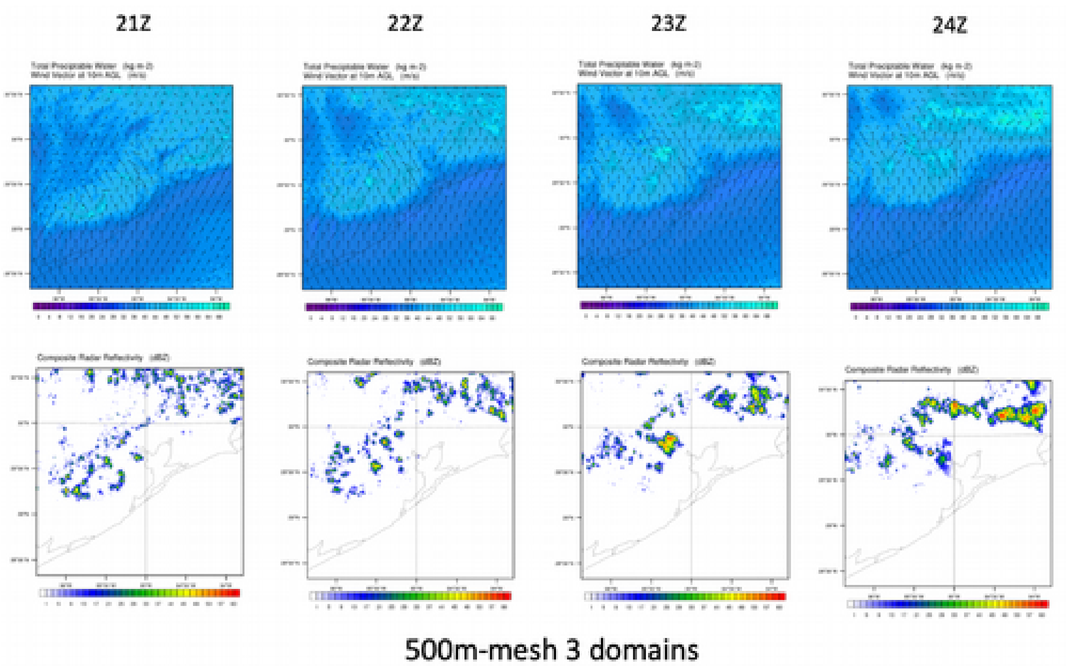


Fig. R1: Time series (21Z, 22Z, 23Z, and 24Z) of total precipitable water (blue shade), near-surface wind vector, and composite radar reflectivity from 250m-mesh single domain simulations used in this study (top panel) versus 500m-mesh triple domains (bottom panel) used in Marinescu et al. (2021).

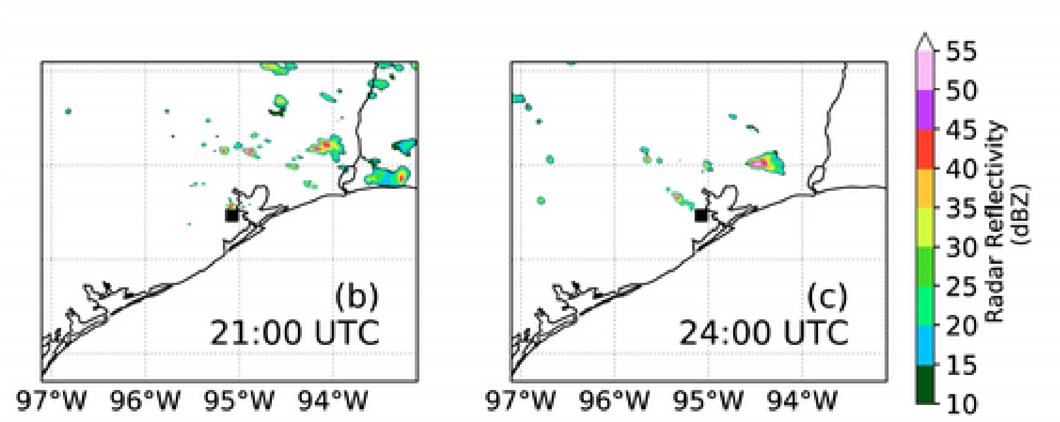


Fig R2. Observed radar composites from NEXRAD from Marinescu et al. (2021).

R1C5: *"(5) L109–10*
Given the non-steady-state nature of the land-sea breeze driving the convection, what is the impact of the infrequent boundary updates on the convection? The lack of nesting would likely exacerbate this issue. Please state the frequency of the boundary condition updates (likely 6h)."

Boundary conditions are compiled at 6 hourly intervals, but actually it is interpolated in time and space to force the model at every model time step. The simulated sea-breeze circulation is driven by land-ocean contrast in temperature and pressure gradients within the domain. This sea-breeze dynamics was explicitly simulated within the domain. We have added the following clarification starting at line 111 (lines 135-137 of the track changes version): "Six-hourly lateral boundary conditions from GFS are spatially and temporally interpolated to update the model lateral boundary conditions at every model time step, while sea-breeze dynamics are explicitly simulated by model physics and dynamics within the domain."

R1C6: *"(6) L124–5*
The phrasing about the thermal radius was not immediately clear without referring back to the Sherwood (2013) paper for clarification. Please reword this to add a little more detail. For example, my impression the first couple times I read this sentence was that there was some sort of scaling relationship being used, which is not the case (a misconception that stuck in my head from reading too fast the first time). It is clearer now that I understand what is being done, but it took me a little digging to make myself confident I understood how the radius is calculated. Adding a little more detail will help readers out."

We have rephrased this explanation (lines 122-125) in order to make it as clear as possible, but without providing unnecessary details that would end up reproducing the methodology description given by Hernandez-Deckers and Sherwood (2016). The improved text corresponds to lines 149-154 in the track changes version: "To identify thermals, an automated algorithm identifies peaks in vertical velocity throughout a particular volume of the simulation at each output timestep, and assumes that these indicate the instantaneous locations of thermals' centers. By comparing these locations in consecutive output timesteps, the algorithm can estimate each thermal's trajectory, which also yields an estimate of their ascent rates at each timestep. Assuming spherical shapes, a thermal's size can be estimated by choosing the radius that makes the average vertical velocity of the enclosed volume match the corresponding ascent rate. Notice that each thermal's ascent rate can vary between timesteps, and hence the estimated size of a thermal may also vary in time."

R1C7*: "(7) L139–41*
This sentence infers that the thermal/bubble structures identified both in this study with 250 m grid spacing and in Hernandez-Deckers and Sherwood (2016) with 65 m grid spacing are a strong function of the model numerics and not a resolved feature. Both studies are just identifying features at WRF's effective resolution—dynamical structures finer than this would tend to not be coherent. Thus, the actual behavior of the thermals in nature could be somewhat different than what is being seen in the simulations. While I realize it is not feasible to use a model domain that has converged results for the identified cloud features, one should at least note this limitation to the reader."

It is true that average thermal size seems to be strongly influenced by the model's spatial resolution, which raises the question whether the resolution of the model is good enough. On the one hand this implies that one should be careful not to take these simulated thermal sizes as the exact "real" ones in nature. On the other hand, the fact that their dynamical properties are similar at both resolutions is reassuring and suggests that we can expect these to also hold in nature, as already stated in lines 143-145: "Owing to the similarity of results to Hernandez-Deckers and Sherwood (2016), we expect that finer resolution results would be more converged but similar in nature."

R1C8: *"(8) Figs. 2 & 3*
Visually the presentation of the composited thermal characteristics in Figs. 2 & 3 is quite effective in conveying how the aerosol concentrations impact the cloud state within the thermals. Having a bit more description about how the composites were constructed would help interpretation. Most importantly, what portions of the thermal lifetimes are averaged together? Would it be more useful to look at certain times within the lifecycle, such as at maximum translation velocity or at a given altitude? Otherwise, conditions with low and high cloud water concentrations are averaged together and could hide important features."

Composites are constructed using only one "instant" of each thermal's lifecycle. The "instant" chosen for each thermal is that in which the thermal has its highest ascent rate, i.e., when it is most vigorous. We now mention this in the main text after line 149 (lines 179-180 in the track changes version): "For these composites, only the timestep of maximum ascent rate of each thermal is considered." However, as the reviewer points out, this could smooth out certain features by averaging over different stages of the thermals' lifecycle. Figure R3 shows the same composites of Fig. 2, but based on the instant 3 minutes before (left panel) and 3 minutes after (right panel) each thermal reaches its maximum ascent rate.

The only new feature that arises here is that rain and cloud water mixing ratios are higher during the earlier stages of the thermal than during the later stages, consistent with liquid water content increasing during the initial intensifying stages of thermals and decreasing as thermals decay, as expected. However, we find no new features related to the different aerosol concentrations.

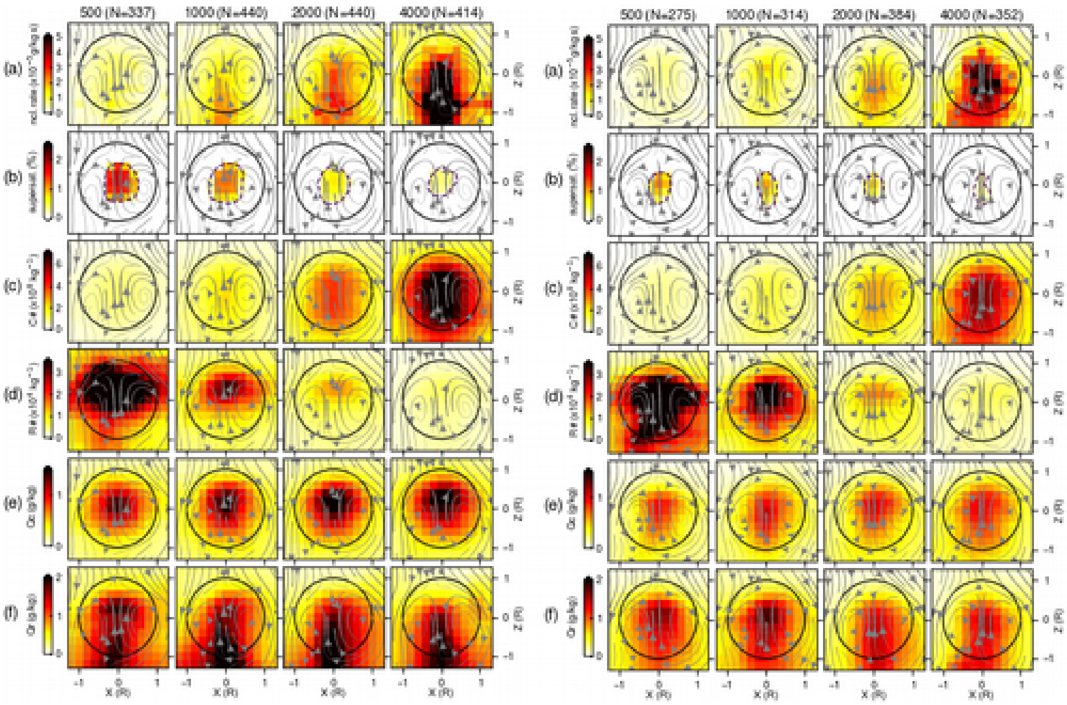


Fig. R3: Composites as in Fig. 2 of the manuscript, but sampling thermals 3 minutes before (left) and 3 minutes after (right) they reach their maximum ascent rate.

Furthermore, Fig. R4 shows the composites corresponding to Fig. 3 in the manuscript, (latent heating rates, buoyancy and vertical velocity), but 3 minutes before and after thermals reach their maximum ascent rate. Once again, the only feature is that these quantities have all higher values during the early stages of thermals, and lower during the later stages. No new feature related to aerosol concentrations is seen here either.

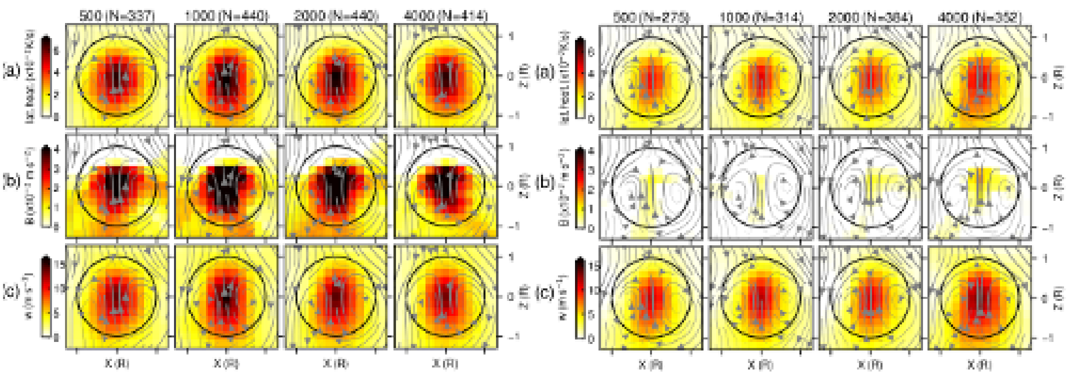


Fig. R4: Composites as in Fig. 3 of the manuscript, but sampling thermals 3 minutes before (left) and 3 minutes after (right) they reach their maximum ascent rate.

Finally, creating composites by altitude could be interesting, but for that to be really informative, a much larger sample of thermals at different altitudes would be required. However, the vertical profiles in Figs. 4-6, as well as the scatter plots in Fig. 7, already provide useful information in this respect.

R1C9: *"(9) Figs. 4–6*
The profiles in Figs. 4–6 are presumably averaged over the three analyzed hours. Is there any evolution of the clouds during this time? For example, how far inland has the land-sea breeze moved? Does this impact the results at all?"

First of all, the sea-breeze penetrates inland, where we have set the sampling box (see Fig. R1), and as it does this, isolated convection becomes stronger and starts to aggregate over the Houston area. This may indeed make certain features of convection different throughout the 3 analyzed hours. However, since we are interested in the possible impacts of different aerosol concentrations upon convection in general, we believe it makes sense to consider the three hours together. Furthermore, by doing so we are able to obtain a larger sample of updrafts/thermals, reducing the noise level. However, we agree that it is also important to assess if these impacts, or in general the response of convection to aerosols is different throughout the different stages of convection.

In order to do this, we have constructed 3 versions of Figs. 4-6, each corresponding to a different stage of the simulation. The early stage starts at 21Z (same starting time of the full 3-hour period), the middle stage starts at 22Z and the final stage starts at 23Z (see Figures R5-R7). The resulting profiles are consistent with the time evolution of convection as it deepens. Mass flux increases at upper levels during the middle and final stages, and vertical velocity also increases aloft with time (Figs. R5A-C). This deepening is also very clear in the profiles of the number of thermals (Figs. R7Aa-R7Ca). An important point here is that due to the fact that these stages have fewer thermals (and updrafts) than when analyzing the full 3-hour period, the profiles are significantly noisier.

In terms of the responses to the different aerosol concentrations (Figs. R6A-C), we still see the same picture in the individual stages as over the entire three hours, but now with substantially more noise. One might argue that there are some differences in terms of how vertical velocity and latent heating rates respond to aerosols in the different stages. For example, during the early and late stages the average response in vertical velocity suggests a stronger invigoration, whereas during the middle stage the average response is rather a weakening. However, the individual responses of each doubling of aerosol concentrations are much more variable and noisy, so we argue that the averaged 3 hour-response is more robust. Furthermore, the responses in both frameworks are generally consistent with each other, but with more noise, particularly in the thermal framework.

We will now include Fig. R5 as supplement to the manuscript (as three separate figures), since we believe it provides some additional insight into the time-evolution of this convective case. However, Figs. R6 and R7 are too dominated by noise due to the short analysis time-interval, and thus do not justify additional supplementary material. Also, we have added the following paragraph after line 189 (lines 225-229 of the track changes version):

"It is important to point out that throughout the 3-hour period analyzed here, convection evolves and may behave differently at different stages. To assess this, Figs. S1-S3 show profiles as in Fig. 4, where the three-hour period has been divided into three stages. These profiles reflect the fact that convection deepens with time, but otherwise show consistency with Fig. 4. Furthermore, considering the entire 3-hour period provides a larger sample of updrafts, which in turn aids in reducing the noise."

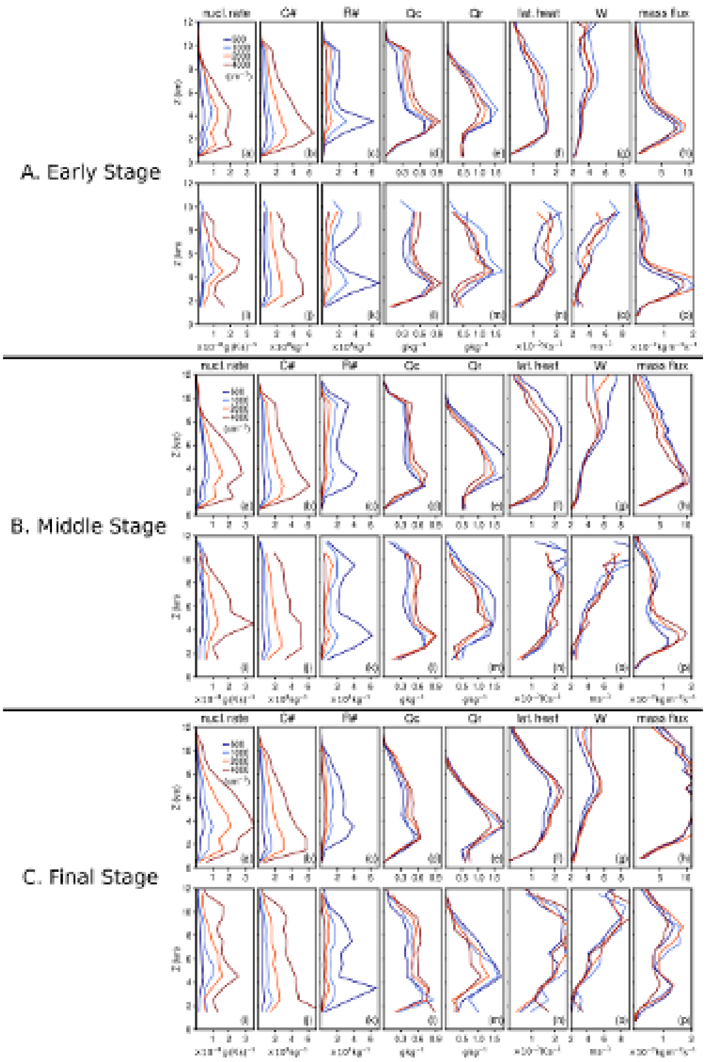


Fig. R5: As in Fig. 4 in the manuscript, but for 3 stages of the simulation, early (A), middle (B) and final (C).

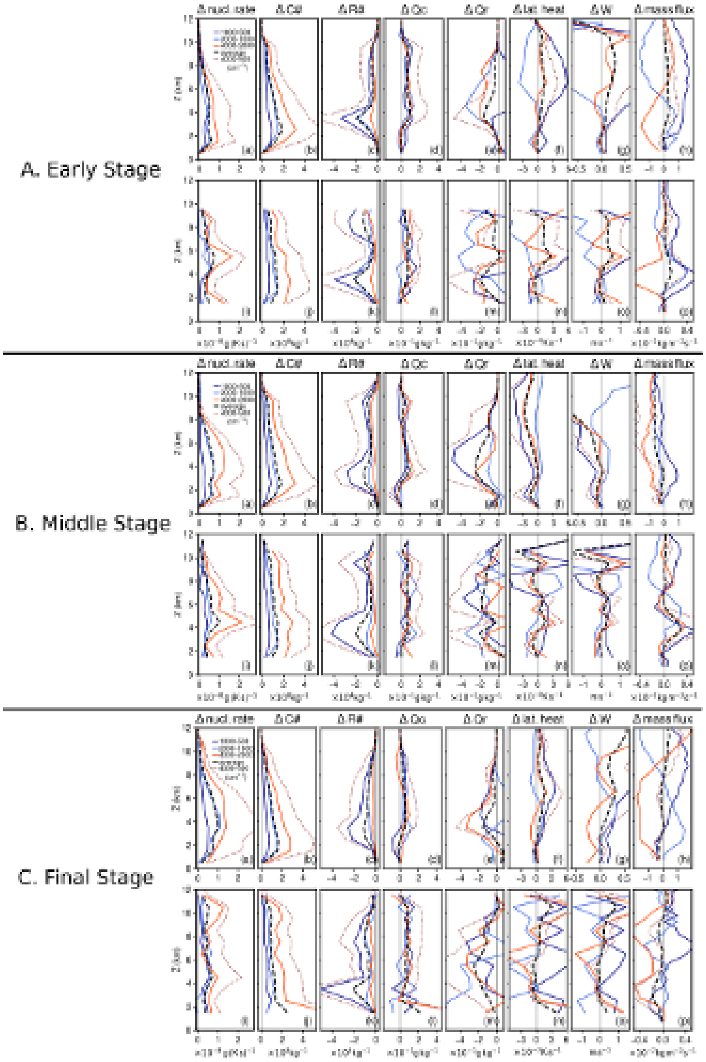


Fig. R6: As in Fig. 5 in the manuscript, but for 3 stages of the simulation, early (A), middle (B) and final (C).

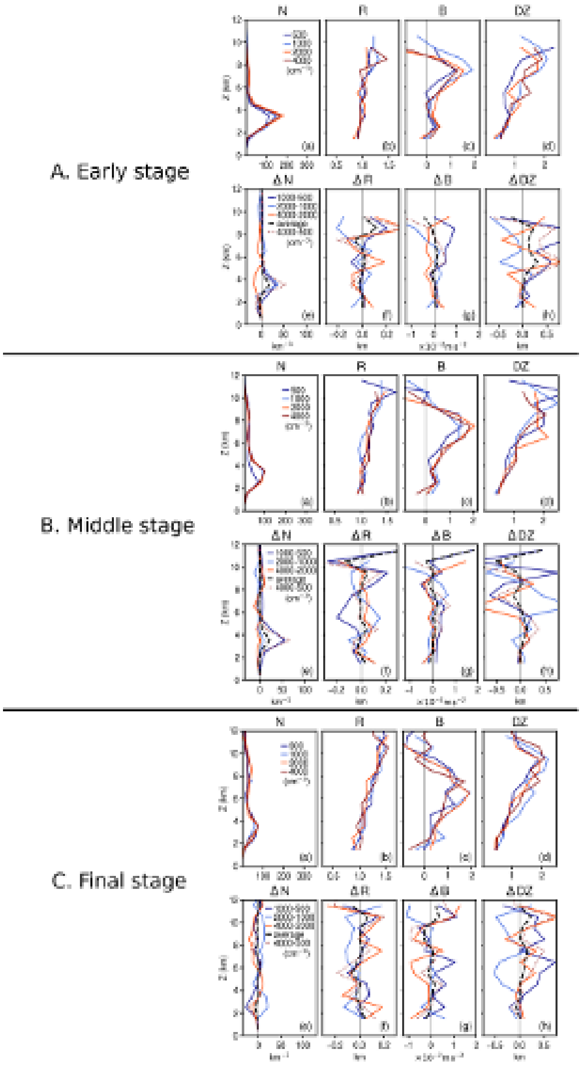


Fig. R7: As in Fig. 6 in the manuscript, but for 3 stages of the simulation, early (A), middle (B) and final (C).

R1C10: *"(10)*
The authors speculate that the differences identified between the cloudy-updraft and tracking methods is due to sampling bias. However, this is not confirmed beyond pointing out a physically consistent argument that sounds plausible. Is there a way to alter the cloudy-updraft method to reduce the selection bias and confirm the speculation? For example, can one add an additional criterion such that the vertical velocity must be within the values that are consistent with the velocities seen in the thermals identified via tracking? Finding a way to make the comparison fairer and seeing if the differences between methods go away would greatly strengthen the paper."

Actually, both frameworks have been implemented in the most consistent possible way, by using the same thresholds of vertical velocity and cloud condensate (see lines 132-133, or 162-163 in the track changes version). Changing these thresholds for the cloudy-updraft gridpoint framework would make them less consistent with each other. The difference that remains is really fundamental in terms of the dynamical coherence of the volume of air that is required for a thermal, something that is not possible to introduce in the cloudy-updraft gridpoint framework. However, following this suggestion, and a similar one by reviewer #2, we have tested two higher thresholds for vertical velocity in the cloudy updraft definition, and produced two figures as supplementary material (Figures S4 and S5). Please see reply to comment R2C3.6 for the complete response and the corresponding additions after line 276 (lines 316-320 in the track changes version).

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2021-586/acp-2021-586-AC1-supplement.pdf