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Reply on RC2

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-AC2, 2021

In the following we provide a point by point response to comments by Reviewer #2, where we quote in italics the original comments. 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).

"Overview
This study applies cloudy updraft and tracked thermal frameworks to analyze updraft statistics in LES simulations of relatively isolated deep convection near Houston, TX. Sensitivity of updrafts to aerosol concentration between 500 and 4000 cm-3 is analyzed. Although cloud droplet and raindrop concentrations change significantly in response to aerosol changes, latent heating and vertical wind speed show little sensitivity. Buoyancy and thermal number sensitivities to aerosol concentration are non-monotonic. Both frameworks show similar sensitivities of updraft properties to aerosols. The primary difference is in the upper troposphere were the tracked thermal framework produces stronger updrafts. Magnitudes of effects also vary, but this is understandable given the two different sampling methods.
Overall, this is an interesting study comparing two different techniques that are commonly used for studying convective updrafts. I'm not aware of other such comparisons, which makes the results publishable. The aerosol sensitivities are also publishable, particularly since they disagree with many papers, some of which are case studies, that claim that increasing aerosol concentration increases convective vigor through the ice phase."

R2GC: *"The primary issue with the study is that it stresses how the tracked thermal framework is superior to static cloudy updraft frameworks and that thermals are fundamental building blocks of convection that act as natural cloud chambers, but that is all very subjective without much evidence to support it. There are differences between the two framework results that make sense based on how they are sampling the model output. Despite that, they give results that are more similar than different with respect to aerosol sensitivities. Why one or the other is better connected to convective dynamics and microphysics understanding and parameterization is not clearly presented. Rather, it seems like each could be useful, particularly in providing context to each another and in supporting greater confidence in results when similar microphysical and dynamical sensitivities are similar in each, like seems to mostly be the case in this study. Without* *further results, it seems that this should instead be the message that is stressed most."*

Thank you for this insightful review of the manuscript. We agree that the main message should not be that the thermal framework is “better” than the cloudy-updraft grid point framework, but rather that both methods are consistent in most situations, and that by comparing them we can gain additional insights of updraft microphysics that are expected to be useful for understanding and parameterizing the coupling of aerosol and cloud dynamics. The way we had phrased several parts of the manuscript could be interpreted as the former rather than the latter, so we made the following revisions (see the tracked changes version of the manuscript for comparing with the previous version):

Modified lines 12-13 (abstract) to: “...results suggest that thermals are more selective than cloudy-updraft grid cells in terms of sampling the most active convective air masses.”

Modified lines 57-58 (lines 60-61 in the track changes version) to: “All this suggests the possibility of exploring an alternative, more objective-based definition of the active cloudy regions arising from cumulus thermals.”

Modified text starting at line 69 (lines 73-79 in the track changes version) to: “The more complex cumulus thermal framework enables a direct, three-dimensional, structure-based analysis of how the internal updraft dynamical structure is coupled to the microphysical processes, something that is difficult to obtain from the grid point framework. Both frameworks are expected to provide important information about the impact of aerosol concentrations on the dynamical and microphysical properties of deep convection, and here we compare the approaches in a systematic fashion.”

Modified lines 281-282 (lines 325-326 in the track changes version) to: “Overall, these results highlight how both frameworks are generally consistent, while subtle differences between them can potentially provide additional useful information.”

Added text starting at line 322 (lines 368-370 in the track changes version): “This increases the level of noise in the thermal framework compared to the cloudy-updraft grid point framework, but that may also represent information content regarding the scarcity of what have sometimes been referred to as "lucky updrafts".”

Regarding lack of evidence that “thermals are fundamental building blocks of convection that act as natural cloud chambers”, we believe there are two main points here. First, the conceptual idea that thermals serve as the building blocks of cumulus clouds (or convection), something that has been put forward by many authors in the past (e.g., Zhao and Austin, 2005; Blyth et al., 2005; Damiani et al., 2006; Hernandez-Deckers and Sherwood, 2016). The main issue here, we believe, concerns the second point, which presents these thermals as “natural cloud chambers”. The reviewer comes back to this point several times in the specific comments below. Indeed, thermals are not closed systems; in fact, as the reviewer points out, entrainment (and detrainment) is significant in typical thermals (e.g., Sherwood et al., 2013; Hernandez-Deckers and Sherwood, 2018; Lecoanet and Jeevanjee, 2019). Our intention is to offer a simple analogy for how a typical thermal’s dynamical structure tends to concentrate most microphysical processes within it, without implying that they do not mix with their environment. This idea results from the composite plots in Figs. 2 and 3, and is certainly one of our key findings (even if it is only qualitative). It further provides the connection for improving subgrid-scale parameterizations (see specific comment below on this topic). This was our intention when using the term “natural cloud chamber”, but we agree that this may not be the best analogy. We now avoid this term, and rather describe this feature more explicitly. This has led to the following changes:

- Removed the last sentence from the abstract (lines 19-20).

- Removed text from lines 69-72 (75-77 in the track changes version).
- Removed sentence from line 162 (193 in the track changes version).
- Removed text from line 246 (286 in the track changes version).
- Removed text from lines 312-313 (358-359 in the track changes version).
- Changed "cloud chambers" for "structures" at line 323 (line 372 in the track changes version).
- We also expanded discussion of potential advantages of the thermal approach for parameterization starting at line 325 (lines 374-379 in the track changes version): "For instance, efforts to extend climate model convection schemes that parameterize updraft velocities and use these to inform microphysical process rates (e.g., Wu et al., 2009) can draw upon the three-dimensionally colocated properties and process statistics directly identified within the structures that they seek to represent. The thermal approach is also likely to naturally avoid inclusion of oscillatory gravity wave motions, which may contribute substantially to mass flux especially in stable regions of the atmosphere such as the upper troposphere (Mrowiec et al., 2015)."

Additional references:

Zhao, M., & Austin, P. H. (2005). Life Cycle of Numerically Simulated Shallow Cumulus Clouds. Part II: Mixing Dynamics, Journal of the Atmospheric Sciences, 62(5), 1291-1310. https://doi.org/10.1175/JAS3415.1

Wu, J., A.D. Del Genio, M.-S. Yao, and A.B. Wolf, 2009: WRF and GISS SCM simulations of convective updraft properties during TWP-ICE. J. Geophys. Res., 114, D04206, doi:10.1029/2008JD010851.

"Comments

R2C1: *The results and conclusions that are stressed most (use thermal framework for analyses; thermal framework yielding an abundance of additional information; thermals are dynamical and microphysical building blocks) are not well supported. If anything, most results are similar between the thermal framework and the cloudy updraft framework. Some are different, notably dynamics at upper levels, which is understandable given the low thresholds in the cloudy updraft framework that will pick up on detrained, buoyant air. How relevant these differences are for understanding or parameterizing convective clouds is not clear. It is simply stated that they are important with the thermal framework being superior, but what analyses support this? However, I don't believe that these are the most important conclusions anyway. I suggest shifting some of the focus to reflect the most important conclusions: (i) for liquid convective clouds, the thermal and cloudy updraft frameworks provide similar results (which is great since we don't have to disregard many past studies), (ii) for mixed phase and ice portions of convective clouds, substantial dynamical differences appear but microphysical sensitivities to aerosols remain similar, (iii) non-monotonic aerosol effects on liquid cloud updraft thermal number and buoyancy are seen, but no clear effects on the mixed phase and ice portions of updrafts are seen despite large sensitivities of cloud droplets to aerosol concentration. This last result is consistent with some recent studies showing warm phase invigoration without cold phase invigoration but goes against much of the aerosol deep convection invigoration studies concluding cold phase invigoration occurs, particularly in warm cloud base, isolated deep convection like this study examines."*

We thank this reviewer for this excellent suggestion, which we believe summarizes most of the other more specific comments in this review. Based on the various additions and modifications that derive from them, our main results and conclusions are now oriented in this direction, except in one respect: the "warm phase invigoration without cold phase invigoration". Although it seems plausible, we consider that our simple representation of cold microphysics is not enough in order to support this discussion (see comments

R2C3.4, R2C3.5 and R2C3.6). However, regarding points (i) and (ii), we have modified several parts of the text that follow this useful suggestion. Most of these additions or modifications follow from the general comment R2GC above, which touches on the main aspects of this idea, or from other specific comments below. For example, modified text at lines 12-13, 69-75 (73-79 in track changes), 281-282 (325-326 in track changes), 312-313 (357-358 in track changes), and added text starting at line 325 (lines 374-379 in track changes).

R2C2: "*There is a lot of subjective language and confusing terms used:*
R2C2.1*: Lines 42-43: "in which the dynamics of convection are resolved" is ambiguous. What dynamics? The primary updraft or downdraft size, average intensity, peak intensity? Many would not consider 250-m grid spacing sufficient to resolve primary updrafts of many types of moist convection. Studies like Bryan et al. (2003) and Lebo and Morrison (2015) show that 250 m is barely enough to resolve the peak in the kinetic energy spectra, but those studies are also for continental squall lines that may have larger, more intense updrafts than in other regimes such as those in oceanic regions."*

To avoid this ambiguity, we changed lines 42-43 to "...in which cumulus convection does not require being parameterized...".

R2C2.2: *"Lines 47-48: Not all moist convection is necessarily constituted of short-lived thermals. Supercells, for example, can have 10-km wide plume updrafts with slab inflow layers. Morrison et al. (2020) and Peters et al. (2020) describe a thermal to plume spectrum dependent on updraft width and environmental conditions such as humidity, instability, and wind shear."*

We have rephrased lines 47-48 (49-50 in track changes version) to: "However, with notable exceptions as in supercells, moist convection commonly constitutes a series of many short-lived thermals…"

We now note a recent observational publication that finds a predominance of small updrafts in tropical convection (Yeung et al., 2021) starting at line 57 (lines 59-60 in track changes version): "For instance, recent observations by Yeung et al. (2021) indicate that most updrafts are less than 2 km deep, suggesting that a large fraction of mass flux may be left out by such selection criteria."

Yeung, N. K. H., Sherwood, S. C., Protat, A., Lane, T. P., & Williams, C. (2021). A Doppler Radar Study of Convective Draft Lengths over Darwin, Australia, Monthly Weather Review, 149(9), 2965-2974, https://doi.org/10.1175/MWR-D-20-0390.1

R2C2.3: *"Lines 70-72: I don't understand what it means for microphysical processes to be contained within thermals and driven by their internal circulations. This seems obvious that a microphysical process rate will depend on its local environment, whether advection, condensation, phase changes, or hydrometeor interactions."*

Indeed, this sentence describes something obvious, and what we should rather stress here is that the thermal framework produces additional information. We have removed the sentence referred to above, and modified this fragment as pointed out in comment R2GC (lines 73-81 in the track changes version).

R2C2.4: *"Line 73-74: What does it mean to be the basic dynamical entity of a cumulus cloud?"*

Since this sentence may be too vague here, we have removed it, as part of the changes from the previous comment (R2C2.3) and the general comment R2GC.

R2C2.5: *"Line 89: Is this implying that the urban region heating is key for the sea breeze forcing initiating convection? A review of NEXRAD from this event shows convective precipitation initiating all along a sea breeze between Galveston, Houston, and Beaumont regardless of land cover with the most intense observed cells over rural locations in between Houston and Beaumont."*

We have removed "especially urban regions" from line 89 (99 in track changes).

R2C2.6: *"Line 115: Thermals are possible once resolution is sufficiently high, but that doesn't mean that these have been observed as is stated."*

We have changed "observed" to "expected" at line 115 (142 in track changes).

R2C2.7: *"Lines 142-143: Clarify what is meant here. Bryan et al. (2003) say that 250 m is sufficient for obtaining an inertial subrange, but this is also for a squall line and all results still do not converge at 125 m."*

We have removed this sentence (lines 172-173 in track changes).

R2C2.8: *"Line 162: I understand the thermal as an entity, but it seems overboard to call it a natural cloud chamber when it clearly has significant exchanges across its boundaries."*

We have removed this sentence (line 193 in track changes).

R2C2.9: *"Line 246: I don't understand why this suggests that thermals act as cloud chambers."*

We have removed this sentence too (line 286 in track changes).

R2C2.10: *"Line 318: What are "thermal microphysics quantities"?"*

Indeed, this was a typo. We have removed "thermal" from this phrase at line 318 (364 in track changes).

R2C2.11: *"Second to last sentence of abstract: Cumulus thermals can serve as a stronger foundation for improving sub-grid parameterizations than what? Which parameterizations? Why?"*

Traditional mass flux parameterizations use steady state assumptions (e.g., time integrated fluxes), whereas cumulus thermals are more directly connected to the supersaturation and cloud nucleation processes (i.e., updraft microphysics), which would be crucial information for new parameterization development. As mentioned toward the end of the reply to comment **R2GC**, we have added lines starting at line 325 (374-379 in track changes) to provide support to this sentence of the abstract.

R2C2.12: *"Last line of abstract: How do the result suggest that cumulus thermals are more realistic dynamical building blocks of cumulus convection and what are they more realistic than? What suggests that they are natural cloud chambers?"*

As mentioned in comment R2GC, we have removed this last sentence from lines 19-20.

R2C3: *"There are several results left unexplained or with interpretations not well supported by analyses."*
R2C3.1: *"Line 170: If supersaturation lowers, then condensation (and latent heating) increases, so what is compensating this extra latent heating to produce no net latent*

heating change? This should be explained."

First of all, we have changed in line 169 (200 in track changes) “diabatic heating” for “latent heating”. This is important because “latent heating” should not be considered as diabatic heating since it does not come from an external source. Thus, this phrasing is more appropriate.

Second, the latent heating term in our model simulations include all contributions due to phase changes of water; extracting these individual contributions would be required in order to investigate this. Since we do not have these separated terms available, we defer this to future work.

Clarification has been added starting at line 167 (199 in track changes): “... (summed over all source terms)”, and at line 170 (lines 202-204 in track changes): "A possible explanation is that supersaturation differences are sustained within the context of negligibly different total condensate production rates within the thermal core, but that hypothesis cannot be definitively supported without additional diagnostics that separate the sources of latent heat in future work."

R2C3.2: *"Lines 183-185: How does the vertical wind speed profile highlight the importance of microphysical processes when its impact on microphysical processes aren’t quantified?"*

Our point here is perhaps simpler (and maybe more obvious) than what the reviewer may have understood: when comparing both frameworks, higher average vertical velocity values aloft come together with higher values of microphysics quantities, which reflects the important coupling between dynamics and microphysics. To avoid this confusion, we have rephrased this sentence at lines 184-185 (218-219 in track changes): “This also results in a slightly more top-heaviness of profiles of other quantities, which reflects how strongly coupled are microphysical processes with updraft dynamics.”

R2C3.3: *"It’s not clear how robust (i.e., significant, which is a word that is used in the text) any inter-simulation differences are relative to variability expected from an ensemble with perturbed initial conditions. This is admitted by the authors – that it is difficult to discern a signal from the noise, but then the differences are described anyway as though they are robust."*

It is not clear to which descriptions exactly the reviewer refers to. Perhaps the reviewer refers to lines 200-207 (240-247 in track changes), where we describe responses in terms of vertical velocity? We believe such a description is warranted, since many previous studies investigate such sensitivities in terms of “invigoration”, and by doing so, we can actually illustrate more clearly the issue of signal-to-noise ratio, and the fact that individual pairs of experiments should be interpreted with care. Therefore, after describing these differences, we finish this paragraph with a clear warning (lines 206-207): “however, not all individual pairs of cases show such an increase, and the amplitude of the individual responses is usually larger than the average one.”

Perhaps the reviewer refers to the description given in lines 215-230 (255-270 in track changes), where we mention that the response in buoyancy, w, DZ, mass flux, and number of tracked thermals appears to be “aerosol-limited”. In fact, this may provide at least a partial explanation for why individual pairs of experiments have different responses, so we consider that the fact that responses are different should not stop us from doing so. In fact, in the next comment the reviewer suggests us to investigate this further.

R2C3.4: *"There are different sensitivities to changes in aerosols depending on the* *magnitude of aerosol concentrations, but it isn't explained why this is and why changes are only visible for low-mid levels where presumably the thermals are dominated by liquid. I suggest reviewing previous studies on these topics."*

In these simulations, we have parameterized aerosol activation on cloud particles, and not yet parameterized ice nuclei impact. We now point this out starting at line 109 (lines 122-123 in track changes): "...while aerosol impact on ice nuclei is not considered."

In addition, this is single-ice P3 parameterization, and probably it is not sufficient to discuss much in overall aerosol impact yet without a 2nd class of ice category, if it is mixed with freshly nucleated ice mass and graupel-size ice mass. That is why this paper focuses on the dynamical and microphysical characteristics in the liquid phase. Of course, we would like to further develop the P3 scheme for a more robust ice microphysics process to steer our gear toward more robust aerosol-cloud interaction processes focusing on ice microphysics in a future study. But this study intends to characterize thermal microphysics states and their sensitivity as the first step. Therefore, we consider that investigating further these differences between low-mid levels and upper levels should be left for a future study.

R2C3.5: *"Line 245, 270-276: This also may be a result of larger regions of detrained, rising cloudy air at upper levels than at low levels, which could easily be examined. Since this is the largest difference between the two frameworks, an attempt at explaining it with a bit of investigation is warranted."*

Indeed, this is a possibility. However, following up on the previous comment (**R2C3.4**), in this study we are interested in the liquid phase, and our model setup is not ideal for a detailed investigation of such responses at upper levels. However, we have tested changing the vertical velocity threshold in the cloudy updraft definition to answer this and the following comment, which is closely related. Please refer to the reply to comment **R2C3.6** below for the complete response.

R2C3.6: *"Lines 281-282: These results show that a thermal framework produces some differences to the cloudy updraft framework, but it isn't clear why this implies their important role in cloud microphysics and dynamics. Clearly the most active portions of updrafts matter, but is the thermal definition needed for analyses of updraft processes? The cloudy updraft definition is admittedly arbitrary and is a low bar for inclusion. If thresholds were increased, would results approach those of the thermal framework? The thermal framework rejects many updrafts. Does that influence interpretation of aerosol sensitivities?"*

As mentioned above in comment R2GC, we have modified this last sentence of the results section (now lines 325-326 in track changes), highlighting the added value of both frameworks.

Regarding the threshold for vertical velocity in the cloudy updraft definition, as pointed out in comment R1C10, the thresholds have been chosen so that both frameworks are as consistent as possible. However, the reviewer's suggestion to test higher thresholds in the cloudy updraft framework is a useful one. Below, Figure R8 is identical to Figure 7 in the manuscript, with cloudy updrafts defined with w>1m/s (reproduced here to facilitate comparison), while Figures R9 and R10 are obtained using higher thresholds of vertical velocity (w>2m/s and w>4m/s), and will be now included as supplementary material (Figures S4 and S5).

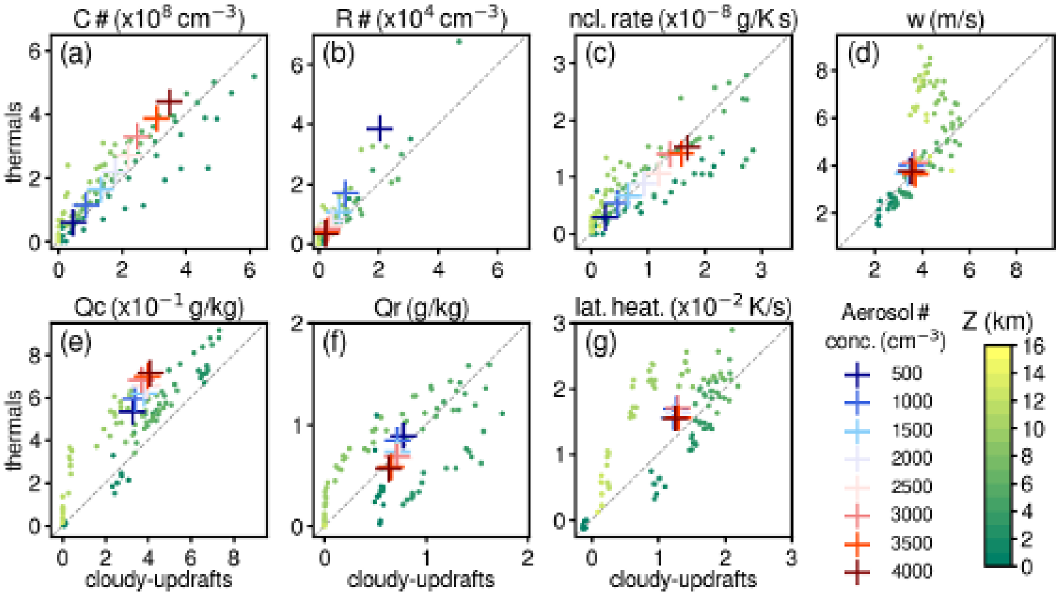

Fig. R8: Same as Figure 7 in the manuscript (with w>1m/s for cloudy updraft grid point definition), shown here for comparison to figures R9 and R10.

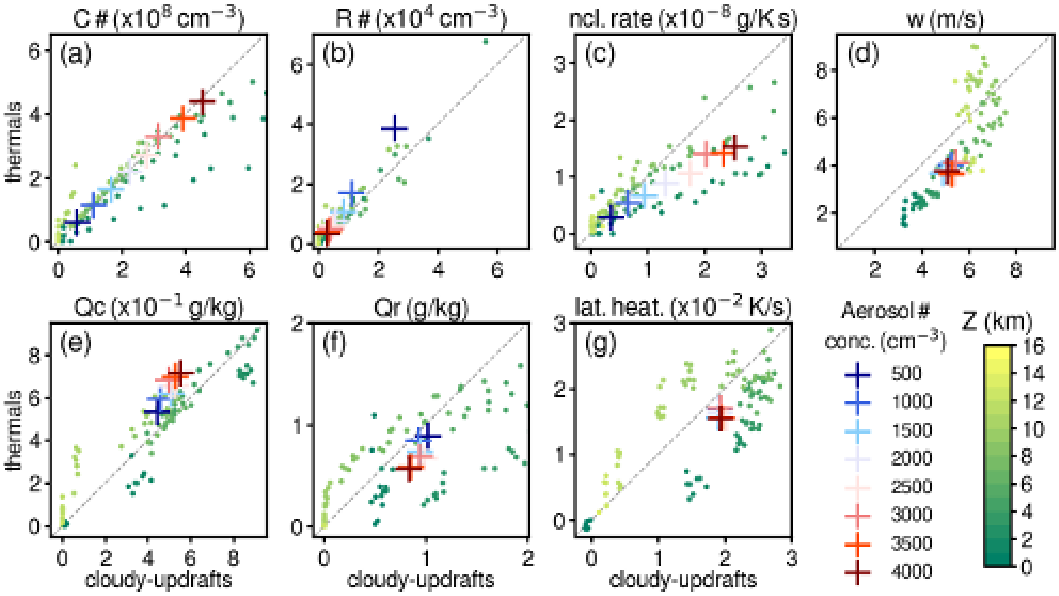

Fig. R9: Same as Figure 7 in the manuscript, but using w>2m/s for cloudy updraft grid point definition.

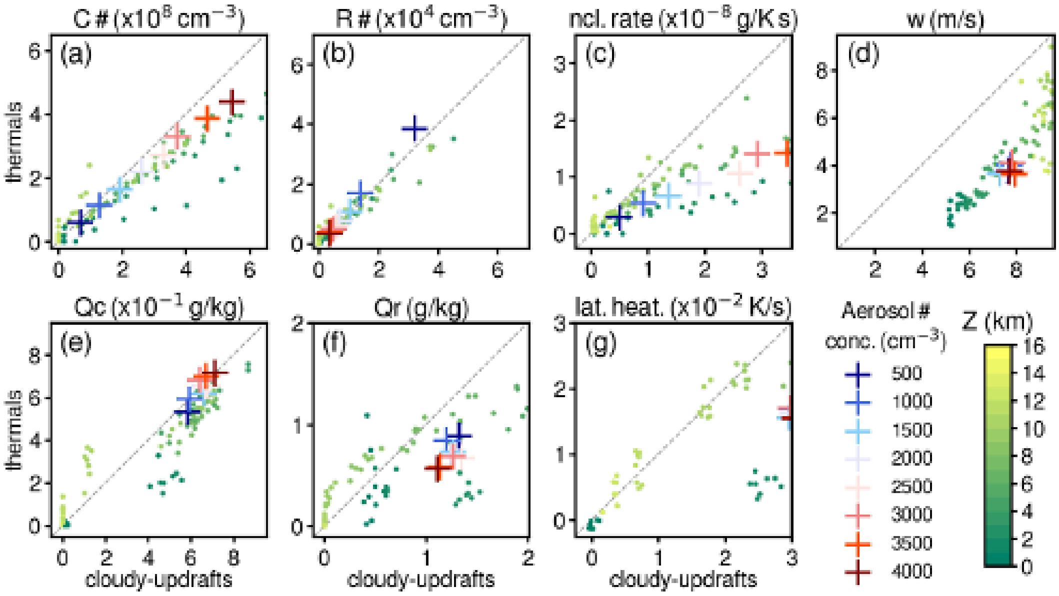

Fig. R10: Same as Figure 7 in the manuscript, but using w>4m/s for cloudy updraft grid point definition.

By increasing this vertical velocity threshold, some values, particularly at upper levels, do approach each other between frameworks. However, at the same time they depart from each other at middle and lower levels (e.g., latent heating rates, vertical velocity, nucleation rates), and by doing so they often make column-integrated values also less consistent between frameworks. There is clearly no unique w threshold for which all quantities agree in both frameworks at different levels. The effect of this threshold increase is most obvious in terms of vertical velocity (Figs. R8d-R10d), where it becomes clear that the differences between the two frameworks are not linear in terms of this threshold. So changing this parameter will not bring results from both frameworks together overall. In fact, based on Figures R8d-R10d, we argue that the most consistent comparison between the two frameworks is when the original threshold of w>1m/s for cloudy updraft grid points is used. Furthermore, differences between frameworks at upper levels should be further investigated with a better ice-microphysics representation. Finally, notice that aerosol sensitivities, which in these figures are evident from the relative changes between column integrated values (large crosses), are not affected by the higher w thresholds used in figures R9 and R10.

To summarize all this, we have added the following text starting at line 276 (lines 316-320 in track changes), as well as Figures R9 and R10 as supplementary material: "Increasing the vertical velocity threshold for the cloudy updraft grid point definition, while it does not modify the aerosol sensitivities found here, yields closer values between frameworks for several quantities at upper levels, but at the expense of larger differences at middle and lower levels that result in less overall consistency (Figs. S4 and S5). Further investigation of detailed differences between the two frameworks at upper levels is left for a future study, with a focus extended to ice microphysical processes."

R2C4: *"How are aerosols initialized in the free troposphere? If they are removed through deposition, how are they replenished?"*

The aerosol vertical profiles are set consistently to ACPC MIP Mode 1 (Fig R11, Marinescu et al. 2021). We have added the following sentences after line 109 (lines 123-126 in track changes, see also R1C2): "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.”

Followed by another addition (lines 126-133 in track changes): “The profiles feature constant values in the boundary layer up to 2.5km and in the free troposphere over 5km with a linear transition between these heights. Aerosol removal/replenishment processes are based on semi-diagnostic methods in Fridlind et al. (2017). This method activates cloud droplets for a given supersaturation rate and aerosol characteristics, and tracks the sum of activated and unactivated aerosol through advection and mixing. Additional cloud droplets can be activated when newly activated cloud droplets number exceeds the present number of cloud droplets. Aerosol number concentrations will be reduced only when cloud droplets are reduced by coalescence process (i.e., autoconversion to precipitation class). The advantage of this approach is to account for activation/regeneration of aerosols without explicitly accounting for aerosols within cloud droplets (see details in Fridlind et al. 2017).”

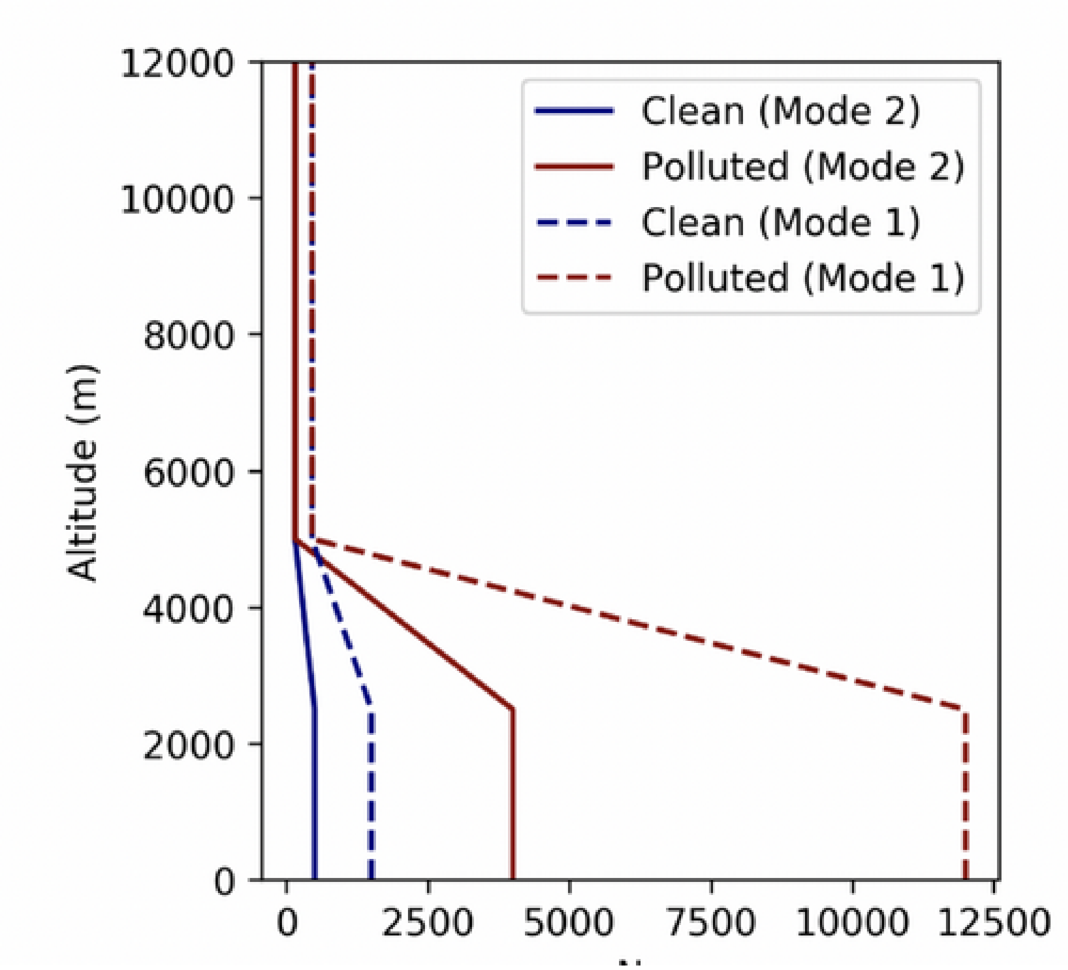

Fig R11. Vertical profiles of initial aerosol concentrations. Unit of x-axis is #/cm3.

"References

Bryan, G. H., Wyngaard, J. C., and Fritsch, J. M., 2003: Resolution Requirements for the Simulation of Deep Moist Convection, Mon. Wea. Rev., 131, 2394–2416, https://doi.org/10.1175/1520-0493(2003)131%3C2394:RRFTSO%3E2.0.CO;2.

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