

Authors' Response to Reviews of

Aging of Droplet Size Distribution in Stratocumulus Clouds: Regimes of Droplet Size Distribution Evolution

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RC: Reviewers' Comment, AR: Authors' Response, □ Manuscript Text

*All line numbers in this response letter refer to the revised manuscript unless stated.

0.1. Line 115: Mixing fraction definition and alternative using θ_l

RC: *The mixing fraction used here is not newly defined; Equation (3) is a variant of the moisture conservation equation (see Yang et al. (2016) and Lu et al. (2018)). In addition, I am curious about how the mixing fraction would behave if it were calculated using the conservation equation of liquid water potential temperature.*

AR: We apologize for any confusion caused by the phrasing. We did not intend to claim Equation (3) as a novel definition, but rather to specify that we adopted this specific formulation among various methods for calculating the mixing fraction. Therefore, we have revised the text to clearly state that this definition follows established methods, and we have added citations to Yang et al. (2016) and Lu et al. (2018) as suggested.

Based on the sharp contrast in q_t and θ_l between the boundary layer and the free troposphere, we ~~define~~ adopt a mixing fraction,

$$\chi = \frac{(q_t - q_{t,bl})}{(q_{t,ft} - q_{t,bl})}, \quad (1)$$

which represents the fraction of free-tropospheric air mixed with boundary-layer air ~~-In this simulation,~~ (Yang et al., 2016; Lu et al., 2018). ~~While calculating χ is estimated using using θ_l yields results highly correlated with those based on q_t , where we select q_t as the conserved variable because it exhibits a more constant profile in the free troposphere (Fig. 3a), making the choice of representative free-troposphere value easier.~~

AR: Regarding the reviewer's question about using liquid water potential temperature (θ_l), we performed a sensitivity analysis and confirmed that calculating the mixing fraction using θ_l yields results that are highly correlated with the q_t -based definition ($r > 0.78$; Fig. AR1c and d). However, we explicitly chose q_t as the conserved variable because it offers a more robust definition. As shown in Fig. 1a, q_t exhibits a more constant vertical profile in the free troposphere compared to θ_l , which can be subject to radiative cooling effects near the cloud top. Therefore, using q_t minimizes ambiguity in defining the reference states and effectively represents the mixing state of the system without loss of generality.

0.2. Section 3.4: Optical properties and Figure 13

RC: *While the authors attempt to analyze the potential effects of DSD aging on optical properties, the presentation of Figure 13 may lead to confusion. Despite the pronounced regional differences in DSD evolution shown earlier, these differences are not evident in Figure 13. The authors may wish to reconsider the necessity of this section. If Figure 13 is kept, additional discussion on the impacts of regional DSD*

evolution on optical properties is required.

- AR: We agree with the reviewer that Figure 13 did not effectively convey the regional differences in DSD evolution and could lead to confusion regarding the link to optical properties. To avoid ambiguity and maintain a sharper focus on the core Lagrangian analysis of DSD aging regimes, we have decided to remove this section and the corresponding figure from the revised manuscript.
- AR: Instead, we have moved the content from the Appendix to the last section of the results. This section presents the combined equation for the trajectory derived from the analytical solution suggested by Liu, Daum, and Yum (2006) and the empirical fit we observed. As noted by the reviewer, this “proposed combined analytical–empirical formulation for relative dispersion is a valuable contribution with potential implications for cloud parameterization.”

1. Reviewer #2

- RC: *This is a clearly-written paper presenting results and interpretation of a novel simulation of cloud micro-physical behavior in stratocumulus clouds. The investigation rests on the Lagrangian representation of microphysics, and it is exciting to see the insights that come from this powerful tool. The L^3 model is especially compelling because it includes sub-grid representation of turbulent fluctuations that are likely important for droplet growth and evaporation, especially in regions where supersaturation variability is large due to entrainment and mixing. I find the simulation results to be rigorous, and most of the presentation of results is clear and convincing. At the culminating stage of the paper, however, I have a fundamental disagreement with the interpretation that is provided, and either I have misunderstood the authors’ argument, or the authors have reached a conclusion that I believe is a misinterpretation of the results. I say this with full respect, wishing to engage the authors as a curious and truth-seeking colleague. My recommendation is that the paper should eventually be published, but I sincerely hope the authors will meaningfully engage and, as needed, reconsider the interpretation of their results in Section 3.3.3, revising the paper as needed. My central concern is described in the next paragraph, followed by several other significant questions or requests. Finally, the review ends with a list of detailed comments that I hope will improve the paper.*
- AR: We thank the reviewer for the valuable comments that have substantially improved the quality of this manuscript. We particularly appreciate the constructive feedback regarding our interpretation of the mixing scenarios. We largely agree with the reviewer’s perspective and acknowledge that our original text did not present these results clearly. Therefore, we have revised our interpretation to clarify these points and ensure they align with the physical insights highlighted by the reviewer. Please refer to our point-by-point author’s response below.
- RC: *The simulation results convincingly show that inhomogeneous mixing is predominant near cloud top (e.g., lines 247–248), and although there is a confusing statement on lines 391–392 that sounds contradictory to this, my understanding is that the earlier data presentation agrees with this: i.e., droplet diameter changes only slightly, and primarily it is droplet number concentration that is reduced due to entrainment and evaporation near cloud top. The authors then lead us through the compelling investigation of the diversity of pathways for droplet evaporation upon descent in the cloud. The confusion, or at least the argument that I do not follow, comes on lines 309–323, where the authors suggest signatures of homogeneous-like mixing. It is stated that the mixing is homogeneous-like because the phase relaxation time is small compared to the evaporation time, and that “droplets undergo gradual changes in r_m and N_c during descent. . .”. First, how does N_c change if there is no complete droplet evaporation? That seems inconsistent with the rest of*

*the argument. Unless there is gradual mixing between more- and less-diluted parcels that are descending together, leading to some parcels having increasing N_c and others having decreasing N_c . After studying these results over quite some time, I find them largely consistent with (not contradicting and not providing an alternative interpretation to) the picture that has emerged over the last decade or so to explain the commonly-noted change from inhomogeneous signatures near cloud top to more homogeneous mixing signatures deeper in the cloud. That is, parcels that are mixed and diluted with entrained air at cloud top, to varying extents, i.e., resulting in different levels of χ in this paper's notation, then have reduced liquid water content, and therefore increased z at which all droplets evaporate upon descent. Assuming liquid water content decreases approximately linearly with height, droplets have to evaporate at larger z , and this is perfectly consistent with the observation in the paper that the deactivation zone height z increases steadily with increasing χ (e.g., see Figure 9 e and h, specifically the blue region, and Figure 12 f and i, specifically the cyan region). The implication of this diversity in entrainment mixing near cloud top, leading to diversity of parcel properties when descending through the cloud, is that droplet radius will change at different rates with decreasing height, in order to reach zero at different heights corresponding to the adjusted liquid water contents. Thus, what was unambiguously characterized as inhomogeneous mixing at cloud top, looks increasingly homogeneous with descent due to the different r vs z profiles. This concept was first introduced by Telford and Chai ("A new aspect of condensation theory" *extitPure Appl Geophys* 1980), and the picture has been filled in with greater detail by many others, including Wang et al. ("Observations of marine stratocumulus microphysics and implications for processes controlling droplet spectra: Results from the Marine Stratus/Stratocumulus Experiment", *extitJGR* 2009), Yum et al. (2015), and Yeom et al. (2023). [The latter two are already cited in the paper, the former two are not, so I have provided the journal and title for each.]*

- AR: We thank the reviewer for these insightful comments. We acknowledge that some of our original arguments, particularly regarding the "homogeneous-like" signatures, were phrased in a way that caused confusion. We want to clarify that our physical interpretation is largely the same as the reviewer's.
- AR: We agree with the reviewer's assessment: the "homogeneous-like" signature observed below the cloud top is not a result of active mixing at that level. Instead, it is a result of evaporation during descent. As the reviewer correctly noted, parcels with different entrainment histories (different χ) experience complete droplet evaporation at different depths. When sampled by in-situ measurements without knowledge of the Lagrangian history, this diversity of evaporation heights resembles the *signature* of homogeneous mixing. We did not intend to suggest that our findings contradict established theory. We agree that our results are consistent with the previous studies (Telford & Chai, 1980; Wang et al., 2009; Yum et al., 2015; Yeom et al., 2023).
- AR: In this study, with a model that can track individual Lagrangian particles (and broadly parcels) history, we demonstrate why the cloud-top and below show different mixing *signatures*, regardless of their actual physical changes from mixing (near cloud top) and evaporation (during descent), and how this could be misinterpreted in in-situ measurements.
- AR: Regarding the reviewer's confusion about the phase relaxation time and Damköhler number: We acknowledge that our original explanation was unclear. The point we intended to make was that the timescales simply indicate whether droplets have the potential to evaporate completely faster during descent (where complete droplet evaporation is considered as *signature* of inhomogeneous mixing). We have revised this section to remove the confusing "homogeneous-like" terminology derived from timescales. However, it is important to mention that this is important as the Da is used as a proxy for mixing scenarios directly from different measurements, which should acknowledge this limitation. We clarified this in the manuscript.
- AR: We apologize for the confusion regarding the description of the "gradual" N_c change. Our original intention

was to characterize the different evaporation depths associated with dilution history. Specifically, we wished to elaborate that trajectories with low $\max \chi$ (low dilution) propagate further into the cloud, exhibiting complete evaporation only closer to the cloud base. In contrast, high $\max \chi$ trajectories tend to exhibit complete evaporation abruptly in the upper layers of the cloud. However, we agree that the term "gradual" was misleading in this context. As shown in Fig. AR2, for these less-diluted parcels (low $\max \chi$), the updraft and downdraft actually follow mostly adiabatic pathways for r_m , N_c , and Q_c , in particular, constant N_c . While there is a slight deviation from the adiabatic curve due to minor entrainment effects (resulting in the smaller N_c observed on the descent), the primary behavior is not "gradual" mixing but rather quasi-adiabatic transport. We have revised the manuscript to clarify this physical distinction and remove the confusing terminology.

AR: Finally, again, we confirm that mixing events near the cloud top are genuinely inhomogeneous for both pathway types, and the vertical descent appears homogeneous, although this signature does not result from active mixing, but rather from evaporation during descent (Fig. AR3). In response, we have now clearly stated that our finding is consistent with all previous studies (Telford & Chai, 1980; Wang et al., 2009; Yum et al., 2015; Yeom et al., 2023) and cited them properly with clear arguments. As the results and discussion sections are substantially revised, please refer to our tracked change version of the manuscript. All the specific points we have revised are listed below:

1.1. Lines 90–91: Grid resolution justification

RC: *The resolution of 10 m x 10 m x 5 m is specified as essential for resolving the small-scale turbulence inherent in the entrainment problem. But of course we all know that the cascade extends to ~1 mm scales, so is there a justification for this being small enough, or somehow adequate for the entrainment problem? It's fine if the answer is that this is a limitation due to computational resources, but if that's the case, then perhaps it's better not to imply that this resolution is adequate for a physically-based reason. If there is a reason, then that's great. . . I look forward to learning what it is.*

AR: We agree with the reviewer that the turbulent cascade extends to the Kolmogorov scale. Our intent, however, is not to imply Kolmogorov-scale resolution. The present simulations are performed in the Large-Eddy Simulation (LES) framework, which resolves the energy-containing motions and represents the inertial and dissipative subranges using a subgrid-scale (SGS) closure. Resolving $\eta \sim 1$ mm over our domain (3.2 km horizontally and 2.56 km vertically) would correspond to a DNS-type requirement of approximately $(3.2 \times 10^6) \times (3.2 \times 10^6) \times (2.56 \times 10^6) \approx 2.6 \times 10^{19}$ grid cells, which is computationally intractable. Within the LES framework, the role of grid spacing is to (i) explicitly represent the dominant turbulent motions responsible for entrainment and (ii) minimize numerical diffusion across the sharp capping inversion, while SGS modeling accounts for the unresolved smaller scales. In this context, our vertical spacing $\Delta z = 5$ m lies within the range commonly used in high-resolution stratocumulus-topped boundary-layer LES ($\Delta z \sim 2.5$ – 5 m; see (Mellado, 2017) and references therein).

AR: To address the reviewer's concern on the wording 'essential', we have revised the manuscript to specify that high resolution is employed for resolving energy-containing eddies, and especially for sharp inversion near the cloud top.

The model domain is 3.2 km \times 3.2 km \times 2.56 km in x , y , and z directions, ~~with a 10 m \times 10 m \times 5 m grid spacing~~, respectively. ~~This high-resolution setup is essential for resolving the small-scale turbulent structures, which are critical to representing entrainment and mixing.~~ We use a grid spacing of 10 m \times 10 m \times 5 m to resolve the energy-containing eddies and the sharp thermodynamic gradients across the entrainment interface layer. Unresolved scalar inhomogeneity and turbulent mixing that

control droplet response are represented by the coupled LEM (Hoffmann et al., 2019), which redistributes the particle-level supersaturation fluctuation S' at an effective vertical resolution of $\Delta z_{\text{LEM}} = \Delta z_{\text{LES}}/n_p \approx 5$ cm (see below). The model time step $\delta t = 0.5$ s, and the total model integration time is 5 h. The results are analyzed only for the last 2 h of the simulation.

1.2. Line 96: Neglect of droplet sedimentation

RC: *Is it reasonable to neglect droplet sedimentation when the vertical resolution is 5 meters? Droplet settling is thought to be particularly important near stratocumulus cloud top. I guess the answer is yes, since this is a non-drizzling case, but perhaps some brief justification of the neglect could be provided.*

AR: We thank the reviewer for pointing this out. It is important to discuss the potential limitations of the study. Our main results show that the maximum droplet radius can grow up to $12 \mu\text{m}$ (up to $15 \mu\text{m}$ in the most pristine case). These droplets typically have a terminal velocity of $\sim 1 \text{ cm s}^{-1}$. Although sedimentation exists, it is negligible in these non-precipitating clouds. We have elaborated on this in the manuscript:

Note that droplet sedimentation and collision--coalescence processes are not considered, ~~since as~~ we focus on the evolution of the DSD shape in non-drizzling ~~Se-~~ stratocumulus. Given the absence of drizzle, the maximum droplet radii are approximately 15, 12, and $9 \mu\text{m}$ for the N50, N100, and N200 cases, respectively. For droplets in the $5\text{--}15 \mu\text{m}$ range, terminal fall speeds are $\mathcal{O}(1 \text{ cm s}^{-1})$ (Yau & Rogers, 1996), which is negligible compared to typical turbulent vertical-velocity fluctuations near cloud top (e.g., σ_w is $\mathcal{O}(10^{-1} \text{ m s}^{-1})$; (Wood, 2012)). However, neglecting sedimentation may slightly alter droplet residence times and hence microphysical exposure near cloud top.

Line 21

RC: *I wouldn't call the DSD a "parameter"... usually I think of a parameter as being a coefficient or constant that describes the state of a system.*

AR: Thank you for this comment, and we agree. Therefore, we changed it to, **L20:** "The DSD fundamentally governs cloud optical properties"

Line 39

RC: *I think I know what you mean by "transient process," but in a mixed-layer model, entrainment rate is sometimes considered to be a constant. Perhaps this can be clarified.*

AR: Thank you for this comment. We agree that the "intermittent process" is a better term for the entrainment. Therefore, we have changed "transient" to "intermittent".

Line 58

RC: *I didn't notice that STBL has been defined. It's hardly used at all in the paper, so maybe it's easier to just spell it out.*

AR: It is defined at L15: “Stratocumulus-topped boundary layers (STBLs) are characterized by a Rayleigh-Bénard-type circulation, driven predominantly by longwave radiative cooling at the cloud top...”. As STBL is used 17 times in the manuscript, we would like to keep this for readability.

Line 62

RC: *Typo with “discusses”.*

AR: Done.

Line 87

RC: *I would have assumed that “dynamics” and “chemistry” would be capitalized since their part of the title of a field project.*

AR: Done.

Figure 2

RC: *It looks like there are data points shown for $\chi < 0$, which does not make sense. Please explain... or correct if it is an error.*

AR: We appreciate the reviewer’s careful observation. The data points with $\chi < 0$ represent valid physical variability rather than errors. In our analysis, the mixing fraction χ is defined based on the domain-averaged thermodynamic properties of the two end-members: the boundary layer ($q_{t,bl}$) and the free troposphere ($q_{t,ft}$). However, due to turbulent fluctuations within the boundary layer, individual fluid parcels can locally exhibit q_t values slightly higher than the domain-averaged mean. These fluctuations result in calculated mixing fractions slightly outside the range of $[0, 1]$. We confirmed that these points constitute a negligible fraction (approximately 0.02%) of the total data. We have clarified in the revised text (and figure caption) that these values reflect the natural turbulent variability of the end-member reservoirs, rather than numerical errors.

In this simulation, the reference values $q_{t,ft}$ and $q_{t,bl}$ are set to their initial values of 1.5 and 9.5 g kg^{-1} g kg^{-1} , respectively, which remain nearly constant throughout the simulation. Note that turbulent fluctuations can cause local parcels to exhibit q_t values exceeding the domain-averaged $q_{t,bl}$, resulting in slightly negative χ values (accounting for $\sim 0.02\%$ of the total data; Fig. 2). These minor deviations reflect the natural physical variability within the boundary layer reservoir.

AR: To verify the robustness of our choice, we performed a sensitivity analysis on the boundary layer reference value ($q_{t,bl}$). As shown in Figure AR1 (below), setting $q_{t,bl}$ to the strict maximum (e.g., 10.0 g kg^{-1}) eliminates negative values but artificially shifts the mixing fraction distribution, misrepresenting the bulk properties. On the other hand, using the boundary layer domain mean (9.5 g kg^{-1}) results in a minimal fraction of negative values while preserving the representative statistics of the boundary layer. Therefore, we concluded that sticking to the domain mean (9.5 g kg^{-1}) is physically more representative.

Line 145

RC: *I'm curious to know how the transit time for the chosen trajectory compares to the simulation time of 13320 s. Also, is there a reason that particular simulation time was chosen? Is the stratocumulus layer approximately in steady state, or continuously evolving? Presumably, at least, it is in approximate steady state on the time scale of the trajectories that were considered.*

AR: We clarify that Figs. 1 and 3 present a snapshot of the instantaneous field at $t = 13320$ s. The trajectory represents the Lagrangian history of a droplet ending at the white dot at that instant, with a lifetime (from cloud base to current position) of approximately 20 minutes, which is much shorter than the total simulation time. The simulation time of 13320 s was selected as a representative time step after the cloud field had reached a quasi-steady state. We have revised the caption to explicitly state that the field is a snapshot and the trajectory represents the droplet's history leading up to that moment.

a) Vertical profiles of q_t (red solid line) and θ_1 (black solid line). Vertical cross-sections of b) buoyancy B , c) vertical velocity W , and d) mixing fraction χ at $y = 2400$ m and, presented as a snapshot at 13320 s a simulation time of $t = 13320$ s. In panels b), c), and d), the Lagrangian trajectory of a selected particle is shown overlaid as a thick black line, starting tracing its path from the entry point (red dot at $t = 12298.5$ s and ending at the) to its current position (white dot at $t = 13320$ s). The thin black solid lines indicate where the cloud water mixing ratio $q_c = 0.01 \text{ g kg}^{-1}$ $q_c = 0.01 \text{ g kg}^{-1}$.

Equation 5, lines 179–180, and Appendix A

RC: *Apparently Equation 5 is an analytic result, but it is not really explained, either in the text or the appendix. Equation 6 appears to be purely empirical, which is fine, but then in the text it is stated that a “detailed derivation is provided in Appendix A.” The appendix provides no derivation of either equation. Please provide the derivation(s) or revise the wording if not provided.*

AR: We agree with the reviewer that the term "derivation" was misleading, particularly for the evaporation pathway. Equation 5 (Growth): As the reviewer noted, this is indeed an analytic result derived by Liu et al. (2006) based on the physics of condensational growth (where relative dispersion decreases as r_m increases). Equation 6 (Evaporation): This is an empirical formulation constructed to capture the inverse trend observed in our Lagrangian statistics. It is not derived from first principles but is designed to mirror the functional form of Eq. 5 while satisfying the boundary conditions of the evaporation regime ($d_r \rightarrow 0$ as $r_m \rightarrow r_{m,max}$). We have revised the text and Appendix A to clarify that the formulation is a "combined analytical-empirical parameterization" rather than a purely derived result. We removed the phrase "detailed derivation" and replaced it with "detailed discussion on the formulation."

A detailed ~~derivation~~ discussion on this empirical formulation is provided in ~~Appendix A~~ Sec. 4.

Figure 4

RC: *What is the gray color in panel a that is not shown in the color bar? Also, is this really a “contour plot” as stated in the caption? I would have called it a 2D histogram or frequency plot or similar. I think “contour plot” is used elsewhere as well, such as in Figure 5.*

AR: We thank the reviewer for pointing this out. We have clarified in the caption that the gray regions represent areas with low sampling density ($N < 10,000$) masked to ensure statistical robustness. We also agree that "2D histogram" (or "2D frequency plot") is a more accurate description than "contour plot" for this figure, and we have updated the terminology in both the text and the caption for Figs. 4, 5, 6, and 7 accordingly.

a) ~~Contour plot of the Two-dimensional frequency distributions histogram~~ in r_m and d_r phase space. [...] In panel a, the gray-shaded regions indicate areas where the sample count is below 10,000, which are masked for statistical robustness.

Figure 5

RC: *There are no axis labels. Also, I puzzled over this figure for a long time before I finally noticed the tiny dots over r_m and d_r . Maybe it would be more reader-friendly to write them out as full time derivatives, or if not, to explicitly state "rates of change" in the caption and in the text.*

AR: We apologize for the lack of clarity in the figure labels and notation. We agree that the "dot" notation (\dot{r}_m) was difficult to distinguish. To improve readability, we have replaced the dot notation with the full time derivative notation (dr_m/dt and dd_r/dt) in both the figure axes and the main text. Additionally, we have explicitly added the phrase "time rates of change" in the figure caption to clearly define the plotted variables.

Bottom of page 12

RC: *Again, it's tricky that you move back and forth between correlations between r_m and d_r , and then between rates of change of r_m and d_r . Perhaps add some wording to alert the reader since the symbols are so similar.*

AR: We agree that switching between state variables (r_m, d_r) and their rates of change was potentially enhancing confusion, particularly due to the subtle "dot" notation. As mentioned in the response above, we have adopted the explicit derivative notation ($dr_m/dt, dd_r/dt$) throughout the manuscript. This distinct visual difference now clearly separates the static properties from their temporal evolution, preventing ambiguity.

Line 235

RC: *For the values in parentheses, specify $\chi =$ so it is clear that they are χ values.*

AR: Done.

Line 247

RC: *Here and in a few other places, the word "mixing" is used, and it's not clear to me whether the authors are using this loosely as entrainment plus mixing, or if they are being precise and focusing on just the turbulent mixing process, which can progress throughout the descending region of cloud. Please clarify.*

AR: We appreciate the reviewer for seeking clarification on this terminology. We acknowledge that the term "mixing" was used somewhat broadly in the original manuscript to describe the combined effects of entrainment

(the engulfment of environmental air) and the subsequent turbulent mixing (homogenization). To improve precision, we have reviewed the manuscript and refined the terminology as follows: When referring to the capture of free-tropospheric air, we now explicitly use "entrainment"; When referring to the subsequent homogenization process, we use "turbulent mixing". When discussing the resulting state (e.g., in the context of χ), we use "entrainment and mixing" or "dilution" to reflect the combined outcome. Specifically, regarding the sentence mentioned (Line 247) and the definition of maximum χ , we have revised the text to state that it quantifies the extent of "entrainment-driven dilution" or "entrainment history" rather than just "mixing influence."

Line 249

RC: *It's not clear to me why this indicates evaporation.*

AR: We have revised the text to clearly state that the observed increase in d_r is "consistent with spectral broadening driven by evaporation" rather than implying it is a direct measure.

Meanwhile, d_r is larger during descent (Fig. 9i), ~~indicating evaporation~~ reflecting the broadening of the droplet size distribution due to evaporation in the absence of the collision-coalescence process.

Line 257

RC: *When you say "directly affected by entrainment", do you mean that there has been complete droplet evaporation, or do you just refer to the dilution effect?*

AR: We appreciate the reviewer's request for precision. By "directly affected by entrainment," we refer to parcels that have experienced significant entrainment events, characterized by high mixing fractions (χ). Physically, this entails both: (1) dilution (reduction of N_c due to mixing with droplet-free air), and (2) the complete evaporation of a subset of droplets due to the inhomogeneous nature of mixing near the cloud top. In the revised manuscript, we have replaced this vague phrase with more precise terminology, such as "parcels subject to strong entrainment-driven dilution," to explicitly convey that these parcels undergo significant microphysical modification, including complete evaporation.

~~...droplets directly impacted by entrainment~~ parcels subject to strong entrainment-driven dilution exhibit a rapid decrease in N_{ad} ...

Line 261

RC: *Perhaps reword "LEM represented SGS supersaturation" to be clearer.*

AR: We revised this expression to: **L273:** "The localized increase in σ_S further supports the conclusion that SGS supersaturation variability, as resolved by the LEM, drives this selective evaporation."

Lines 267–268

RC: *It's not clear to me what is meant by “not directly impacted by entrainment events.” Maybe I have missed something important, so perhaps you can refer back to the relevant place.*

AR: We refer to parcels that circulate within the boundary layer without undergoing significant mixing with free-tropospheric air. Quantitatively, these are characterized by very low mixing fractions ($\chi \approx 0$) throughout their trajectories. These parcels primarily follow the large-scale STBL circulation, experiencing changes in liquid water content due to adiabatic warming and cooling rather than mixing-induced evaporation (Fig. AR2). To avoid confusion, we have revised the text to describe these parcels as parcels experiencing minimal dilution in the revised manuscript.

...the majority of ~~droplets-parcels~~ descending with the STBL vertical circulation ~~are not directly impacted by entrainment events~~ experience minimal dilution through entrainment.

Line 344

RC: *Why do you need to assume a gamma distribution, given that the simulation produces a full droplet size distribution vs height? Is the observed DSD reasonably close to a gamma distribution, at least?*

AR: We agree with the reviewer that the simulation explicitly resolves the full droplet size distribution (DSD), making an a priori assumption of a Gamma distribution unnecessary for analyzing model outputs. Originally, this assumption was employed solely to analytically map isolines of effective radius (r_{eff}) onto the r_m-d_r phase space for visualization purposes, rather than to approximate the simulated DSD itself. However, to streamline the manuscript and focus on the core Lagrangian analysis, we have removed the corresponding section and figure in the revised manuscript. Consequently, the Gamma distribution assumption is no longer used.

Line 361

RC: *How do you see the “high mixing fraction χ ” from Figure 13?*

AR: We thank the reviewer for this comment. This was supposed to be clearer when directed to another figure that shows χ (Fig. 6), to show actual values. However, since the entire section is now removed from the manuscript, we decided not to revise this.

...including activation, condensational growth, entrainment-driven evaporation, and ~~gravitational descent~~ descent within downdrafts, each constituting a stage of an “aging” pathway.

Line 374

RC: *What do you mean by “gravitational descent”?*

AR: We thank the reviewer for this comment. We agree that the term “gravitational descent” was potentially confusing in this context. We have replaced it with “descent within downdrafts,” which is a more precise and physically accurate description.

Line 381

RC: *These “divergent pathways determined by their specific entrainment history” are consistent with the vertical circulation hypothesis, and with laboratory experiments suggesting that homogeneity of mixing is achieved when regions with different entrainment histories are averaged over (e.g., Wang et al. 2009 and Yeom et al. 2023). In other words, I agree with this finding, but I think it should be made clear that it is generally consistent with that conceptual picture. This is related to the main point I raise in the next comment.*

AR: As noted in our previous response, we confirm that our results are consistent with the suggested studies (Wang et al., 2009; Yeom et al., 2023). Accordingly, we have revised the manuscript to explicitly acknowledge this consistency and clarify our argument. Please refer to the tracked changes and the responses provided above.

Line 387

RC: *It’s not clear to me what is meant by “sorting droplets by their entrainment history.” I did not understand that droplets were sorted.*

AR: We agree that the phrasing “sorting droplets” could be misleading, as the sorting mechanism operates on the scale of air parcels rather than individual droplets. To remove this ambiguity and ensure precision, we have replaced “droplets” with “parcels” in the revised manuscript (e.g., Lines 49 and 389). This clarifies that the observed vertical structure results from the redistribution of air parcels with different entrainment histories.

...our trajectory-resolved analysis shows that much of this vertical structure ~~is primarily a consequence of sorting droplets by their entrainment history~~ arises from sorting of parcels with different entrainment histories within the STBL circulation (Telford & Chai, 1980; Wang et al., 2009).

Lines 391–395

RC: *Obviously, given the discussion above, I disagree with these conclusions. I look forward to the authors’ response on whether these findings are significantly different than those attributed to the “vertical circulation” hypothesis that has received strong support in recent years.*

AR: As mentioned in our previous response, we agree with the reviewer’s point and have revised the text to remove any ambiguity. We confirm that our results are consistent with the “vertical circulation” hypothesis. Please refer to the tracked changes and the responses provided above.

Lines 403–404

RC: *I would suggest that Wang et al. (2009; reference above) could be added here, unless I’m misunderstanding the intended meaning.*

AR: We appreciate the reviewer’s suggestion. However, the sentence in Lines 403–404 specifically refers to observational studies that analyzed phase-space patterns (i.e., r_m vs. d_T) to identify microphysical regimes. Since Wang et al. (2009) primarily focus on vertical profiles of mixing signatures rather than phase-space trajectories, we decided that it was not the most precise fit for this specific sentence. Instead, recognizing the importance of Wang et al. (2009) in establishing the vertical structure of mixing signatures, we have

added this citation to the Introduction and Discussion sections, where we describe the vertical distribution of inhomogeneous and homogeneous mixing characteristics.

~~this vertical structure is primarily a consequence of sorting droplets by their entrainment history. By analyzing the Damköhler numbers (Da_{evap}) and mixing fraction (χ), we show that arises from sorting of parcels with different entrainment histories within the STBL circulation (Telford & Chai, 1980; Wang et al., 2009)~~

In situ measurements often reveal inhomogeneous mixing signatures near the cloud top and homogeneous characteristics below (Wang et al., 2009; Yum et al., 2015; Yeom et al., 2021)...

Line 407

RC: *I don't think the proposed formulation is "analytical-empirical"... it is empirical, right?*

AR: We acknowledge the reviewer's point that the decay component of the formulation is empirical in nature. However, characterizing the entire formulation as purely "empirical" would disregard the theoretical basis of the growth equation part, which is derived analytically from parcel theory (Liu et al., 2006). To accurately reflect this combination of an analytical solution for the growth regime and an empirical fit for the decay regime, we have revised the terminology to emphasize that this is a *combined* analytical-empirical formulation. In the abstract, we have retained the *combined* analytical-empirical terminology for conciseness, as we argue that this formulation truly combines both analytical and empirical elements.

~~To better represent the curved structure in the decay regime, especially for intermediate d_r values (0.1–0.3), we generalize the formulation into a piecewise function satisfy these conditions while capturing the observed curvature, we adopt a quadratic formulation. Combining the analytical growth term with this empirical decay formulation, we propose the following piecewise function for the complete evolution pathway:~~

$$d_r = \begin{cases} d_{r,0} \frac{r_{m,0}^2}{r_m^2} & \text{if } S \geq 0, \\ d_{r,\text{max}} \left(1 - \frac{r_m^2}{r_{m,\text{max}}^2}\right) & \text{if } S < 0. \end{cases} \quad (2)$$

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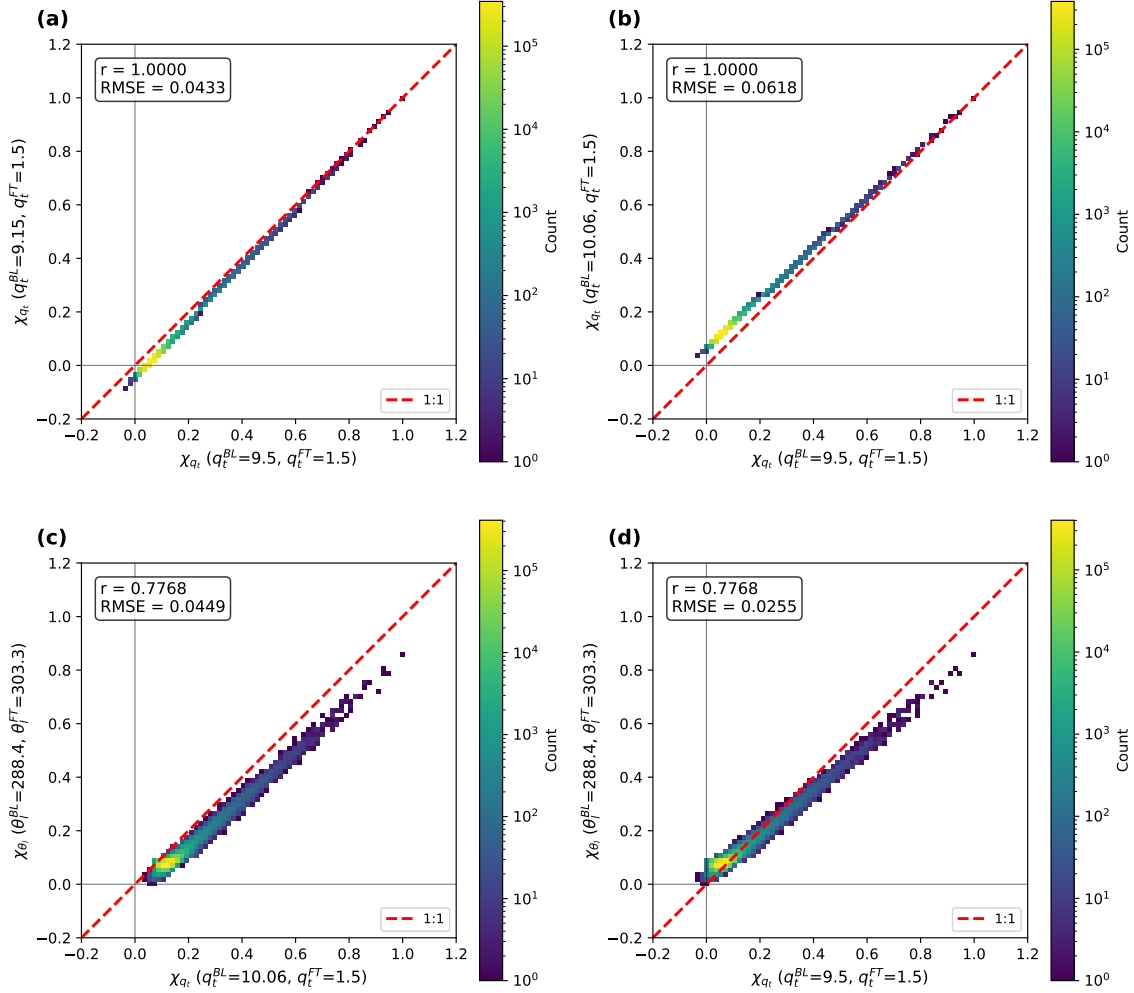


Figure AR1 Comparison of mixing fractions (χ) derived from different thermodynamic variables and reference boundary layer value (q_t^{BL}). (a, b) Sensitivity of the mixing fraction calculated from total water mixing ratio (χ_{q_t}) to variations in different boundary layer (q_t^{BL}) values with fixed free troposphere (q_t^{FT}) value. (c, d) Scatter density plots comparing mixing fractions derived from liquid water potential temperature (χ_{θ_t}) versus those derived from total water mixing ratio (χ_{q_t}). The color shading represents the sample count on a logarithmic scale. The red dashed line indicates the 1:1 relationship. The Pearson correlation coefficient (r) and root-mean-square error (RMSE) are provided for each comparison.

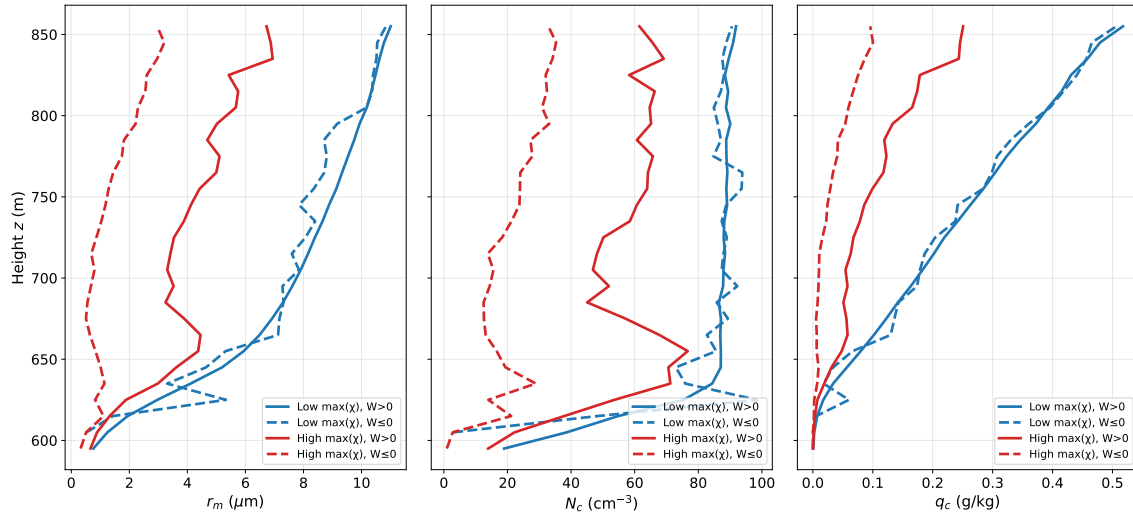


Figure AR2. Vertical profiles of (a) mean droplet radius r_m , (b) droplet number concentration N_c , and (c) cloud water mixing ratio q_c , stratified by maximum χ history and vertical velocity. Trajectories are grouped by the maximum mixing fraction χ_{max} experienced along each trajectory: blue lines indicate minimally diluted parcels (low $\chi_{\text{max}} < 0.08$; 25th percentile), and red lines indicate strongly diluted parcels (high $\chi_{\text{max}} > 0.14$; 75th percentile). Solid lines denote updrafts ($w > 0$) and dashed lines denote downdrafts ($w \leq 0$). Profiles are computed as height-binned means (10 m bins).

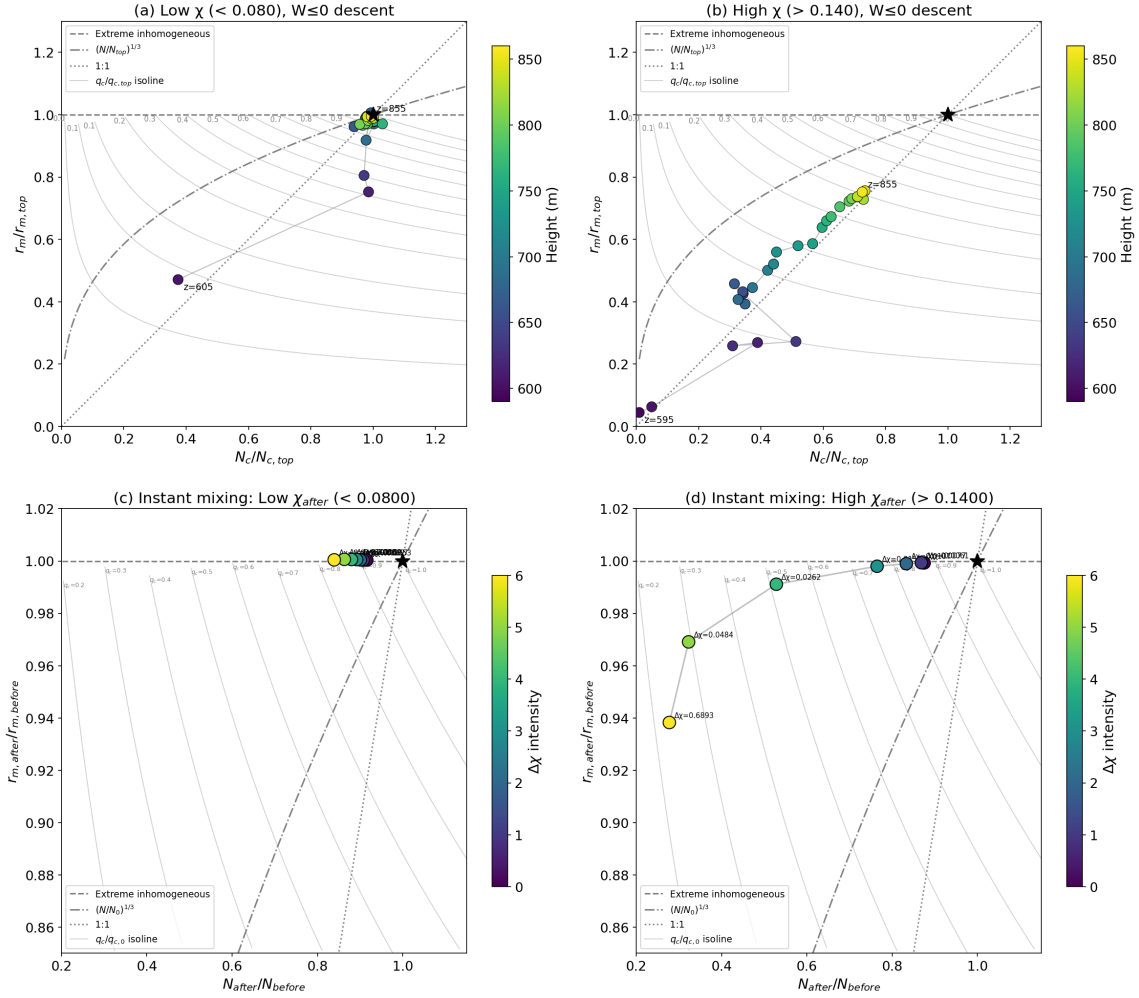


Figure AR3. Mixing diagrams contrasting the descent-phase response (top row) with the instantaneous entrainment response (bottom row). (a, b) Normalized mean radius ($r_m/r_{m,top}$) versus number concentration ($N_c/N_{c,top}$) during the descent phase ($w \leq 0$), normalized by properties at each particle’s cloud-top height. Panels show (a) low- χ_{max} (< 0.08) and (b) high- χ_{max} (> 0.14) trajectories, with points representing height-binned means colored by altitude. (c, d) Instantaneous mixing events detected at cloud top ($z > 800$ m), defined as consecutive-timestep χ increases exceeding 0.005, for (c) low post-mixing χ (< 0.08) and (d) high post-mixing χ (> 0.14). The black star marks the normalization origin (1, 1). Reference lines indicate theoretical limits for extreme inhomogeneous mixing (dashed; horizontal), homogeneous mixing (dash-dot; $(N/N_{top})^{1/3}$), and a 1:1 reference (dotted; diagonal). Gray solid curves show iso- q_c contours labeled by $q_c/q_{c,top}$ ratio. In panels (c, d), points show the median $r_{m,after}/r_{m,before}$ versus N_{after}/N_{before} , binned by mixing intensity $\Delta\chi$.