Comment response
Referee 2

Masaya Iwashima, Ryo Onishi

Major Comments

1

comment

The claim that the study provides "new insights" appears overstated. The interaction between turbulence and cloud microphysics has been extensively studied, and the manuscript in its current form does not sufficiently distinguish its findings from existing literature.

(1) The explanation provided for condensation growth (e.g., lines 243-245: " This discrepancy can be attributed to the differences in condensation growth histories between the two cases. Since condensation growth depends on the droplet size, changes in the droplet size distribution can alter the growth history") is overly generic and largely tautological. Korolev (1995) and subsequent laboratory and numerical studies (Sardina et al., 2015; Chandrakar et al., 2016, 2021, 2022; Prabhakaran et al., 2022) have shown that supersaturation fluctuations and turbulent transport can contribute to the broadening of cloud-droplet size distributions. However, the manuscript does not present the supersaturation field or discuss how it differs between the two cases.

(2) Work on the eddy-hopping mechanism (Grabowski and Abade, 2017; Abade et al., 2018; Grabowski et al., 2025; Grabowski, 2025; Grabowski et al., 2026) further shows that the extent to which turbulent velocity and scalar mixing produce or modulate Lagrangian supersaturation fluctuations—and whether this leads to sustained local DSD broadening at a fixed height—depends intricately on various factors. These include vertical transport, sampling strategies, phase relaxation times, aerosol conditions, and the treatment of the computational domain. The current manuscript does not adequately address these important physical and methodological factors.

(3) The manuscript presents the vertically elongated domain as a way to overcome the limitations of a periodic box, but periodicity remains central to the setup: particles are periodic in all three directions except at the ground, water vapor is horizontally periodic, and a triply periodic HIT snapshot is repeated vertically. My understanding is therefore that the main extension is the inclusion of cloud-depth thermodynamic structure, rather than the removal of periodicity. This distinction should be stated more clearly.

Consequently, the use of broad framing statements in the Introduction (e.g., lines 68-70: "We aim to investigate how turbulence influences the warm-cloud microphysics using a vertically elongated quasi-one-dimensional domain and implemented turbulence fields") is inadequate for a specialized study. I strongly recommend that the authors substantially revise the whole manuscript. They should incorporate a more comprehensive review of classic literature and recent similar studies, thereby precisely defining the scope and specific gap this research attempts to fill.

response

According to your comment, we substantially revised the whole manuscript. We revised the Introduction section to clarify the scope, focus, and novelty of the present study (L54, 114-118). We expanded the review of previous studies on turbulence effects on cloud microphysics, including turbulent entrainment and the eddy-hopping mechanism (L41-53), and clearly stated that the present study focuses on turbulence-enhanced collision-coalescence induced by micro-scale turbulence (L54).

(1), (2)

The objective of this study is to investigate turbulence-enhanced droplet collision-coalescence induced by micro-scale turbulence. Our computational framework does not represent large scale of convection because of its limited horizontal domain. Consequently, mechanisms associated with large scale of convection, such as turbulent entrainment and the eddy-hopping mechanism are beyond the scope of the present study. In addition, the turbulence-induced supersaturation fluctuations was not considered (The explanation was added to the Kinematic model section: L200-201). Accordingly, the condensation-related discussion from the results in Fig. 12 is not intended to attribute droplet growth to these mechanisms. We revised the corresponding interpretation accordingly (L413-454: Specific Comment 24).

(3)

We agree that the main extension of the present framework is not the removal of periodicity, but the inclusion of cloud-depth thermodynamic structure within a direct tracking framework with explicitly resolved turbulence. Therefore, "*The novelty of this study lies in extending the direct tracking framework with explicitly resolved turbulence, thereby enabling the investigation of turbulence-induced collision-coalescence under cloud-depth thermodynamic conditions.*" (L114-117).

Therefore, the new insight of the present study is as follows (L8-10): *"The experiments provide detailed collision and growth statistics of individual droplets that cannot be obtained from conventional Eulerian schemes or Lagrangian schemes employing stochastic collision models."*

2

comment

The use of HIT is not problematic in itself for idealized studies of dissipation-scale droplet dynamics. My concern is the way it is extrapolated here. A single frozen HIT snapshot is repeated throughout the 2.25 km column and used to interpret height- and time-dependent precipitation development. Particles experience spatial velocity variations as they move through this field, but the turbulence itself has no intrinsic temporal evolution. Thus, Eulerian temporal decorrelation, evolution of the turbulent cascade, and feedback from microphysics to the velocity and temperature fields are absent. Cloud-scale motion is represented only by the prescribed horizontally uniform updraft.

(1) While the authors attempt to introduce vertical structure for thermodynamic scalars and DSD, they completely neglect the vertical structure of the turbulence itself. Turbulence in real cumulus clouds is profoundly inhomogeneous and anisotropic, varying significantly from the cloud base, through the turbulent core, to the cloud top. Using a vertically uniform, repeated HIT field directly contradicts the premise that vertical spatial variability should be accounted for.

(2) Cumulus clouds are inherently buoyancy-driven. By merely stacking HIT boxes, the model artificially truncates the integral scale, thereby suppressing the large energy-containing eddies (coherent updrafts and downdrafts) that span the depth of a cumulus cloud. More critically, this idealized setup entirely precludes the representation of entrainment-mixing processes at the cloud boundaries (particularly the cloud top and lateral edges). As is well known in cloud physics, large-scale kinematics and the subsequent entrainment-mixing are pivotal drivers of macroscopic DSD broadening.

(3) Because the simulation ignores the true turbulent vertical structure and the macro-scale boundary interactions essential to cumulus development, no dynamically evolving kinetic-energy cascade is represented during the cloud calculation. The configuration may be useful as a controlled kinematic sensitivity experiment, but it should not be presented as a dynamically realistic simulation of a marine cumulus cloud.

(4) Matching the dissipation rate alone is insufficient to establish atmospheric representativeness. The reported $Re_\lambda = 5.47$ and the very limited separation between the representative scale and the Kolmogorov scale imply that the prescribed flow lacks an atmospheric-like inertial range. The authors should justify which turbulence-induced collision mechanisms remain meaningfully represented at such a low Reynolds number.

Above all, I do not think this setup should be described as a dynamically realistic simulation of a marine cumulus cloud. It is better viewed as a kinematic sensitivity experiment with prescribed small-scale velocity fluctuations. The manuscript should state this limitation clearly and narrow the atmospheric interpretation accordingly.

response

According to your comment, we clarified the limitations in terms of the present framework (L486-497).

(1)

The vertical structure of turbulence is not considered because the same HIT snapshot is stacked vertically. In the real clouds, turbulence is inhomogeneous and anisotropic. The turbulent statistics (e.g. turbulence intensity) is different between cloud top, inner cloud region, and cloud base. This is a limitation of the present framework.

(2)

The present framework is intended to investigate the effects of turbulence on collision-coalescence growth due to micro-scale eddies. On the other hand, the present framework does not represent the large-scale buoyancy-driven dynamics of cumulus clouds. This is a limitation of the present framework.

(3)

As you show in the comment, the correct purpose of the current study is ”*to conduct controlled kinematic sensitivity experiments*” rather than “*to simulate realistic cumulous clouds*” . Thus, we modified the aim of this research more specific in the Introduction section (L117-119).

(4)

Onishi and Vassilicos (2014) showed that, although inertial-particle collisions exhibit a significant dependence on Re_λ*, this dependence is secondary to the dependence on* ε*. Since* ε *is the primary parameter controlling dissipation-scale turbulence, we matched it to a value representative of atmospheric clouds* (L236-239). Consequently, dissipation-scale microphysical processes associated with small-scale eddies can still be meaningfully represented even at the present low value of Re_λ. On the other hand, the low Re_λ resulting from the limited horizontal size of the HIT blocks leads to a different limitation, which is the formation of artificial curtain-like particle structures. This issue was discussed in the response to Major Comment 3.

3

comment

To avoid artificial curtain-like structures caused by the periodic turbulent field, the authors state that they added a random horizontal perturbation (uniformly distributed between -1 mm and $+1$ mm) to the falling particles when crossing horizontal boundaries (P4, lines 85-90). The justification provided is that this perturbation is smaller than the mean inter-particle distance (2.71 mm). This is not sufficiently justified and raises concerns about the core premise of using DNS for turbulence-microphysics interactions.

(1) The primary advantage of explicitly resolving turbulence using DNS is to capture preferential concentration (clustering) and local kinematic correlations. By artificially displacing particles by up to 1 mm, the model introduces independent random horizontal displacements that may disrupt physically generated pair-separation and velocity correlations.

(2) Justifying the 1 mm perturbation by comparing it to the bulk mean distance (2.71 mm, which assumes a uniform distribution) might be misleading. The relevant collision radius $R_1 + R_2$ is pair dependent. For cloud-droplet pairs it is typically tens of micrometers, so a 1 mm displacement is one to two orders of magnitude larger. For raindrop pairs, however, the two scales may become comparable. A 1 mm (i.e., 1000 μm) random motion is orders of magnitude larger than the collision distance. It could artificially disrupt closely clustered droplet pairs or artificially force non-interacting droplets into collision, thereby corrupting the collision kernel statistics.

(3) The magnitude of this artificial perturbation ($\sim$ 1 mm) is also comparable to the Kolmogorov dissipation scale typical for cloud turbulence. Consequently, the perturbation may alter dissipation-range clustering and radial relative-velocity statistics and may also change whether particle pairs enter the near-field hydrodynamic-interaction range.

(4) The random displacement is introduced to suppress curtain-like structures generated by repeatedly tiled periodic turbulence, but it creates an additional systematic artifact. Because the displacement is applied whenever a particle crosses a horizontal interface between adjacent 5 mm tiles, the frequency of the perturbation depends on the particle' s vertical boundary-crossing rate. The resulting artificial horizontal random walk is therefore settling-velocity-dependent and potentially size-dependent. It may differentially alter pair dispersion, preferential concentration, radial distribution functions, relative velocities, and collision rates across droplet-size classes. Its influence must be quantified separately from the effect of the prescribed turbulent velocity field.

Consequently, the claim that the model explicitly resolves turbulence-microphysics interactions while manually adding random noise to particle positions at scales far exceeding the collision distance requires further substantiation. I would suggest the authors to perform no-perturbation controls and sensitivity experiments with different perturbation amplitudes and random seeds. The resulting differences should be evaluated using the radial distribution function, radial relative velocity, collision rate, DSD evolution, and precipitation-onset time.

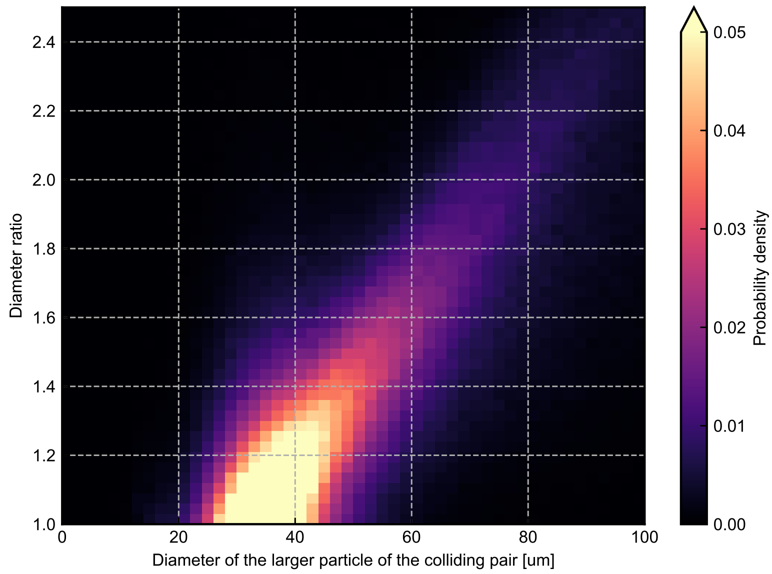


Figure S1: 2D histogram of collision probability as a function of the larger particle diameter in a colliding pair and the particle-diameter ratio of the pair in TURB-case.

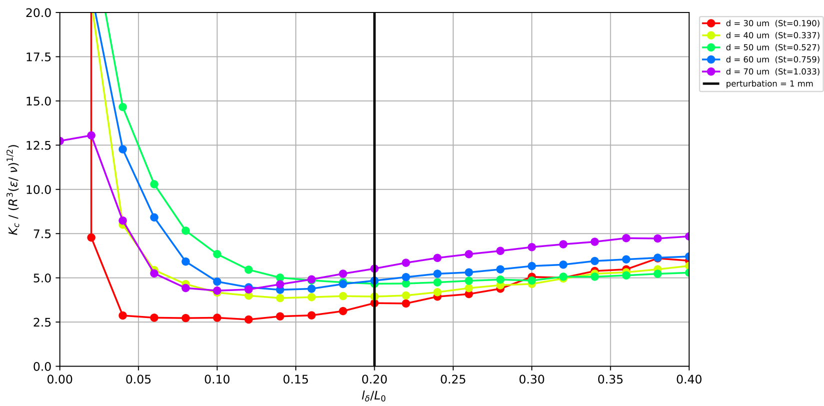


Figure S2: (Figure 3 in the revised manuscript.) The dependency of the collision kernel K_c to the perturbation length.

response

According to your comment, we have expanded the discussion of the vertical stacking of HIT snapshots and the random particle position perturbations. The paragraph describing the perturbations was moved from Overview section to Homogeneous isotropic turbulence section (L247-273). In addition, the limitations of this treatment were added in the Conclusion section (L490-491).

(1), (2), (3), (4)

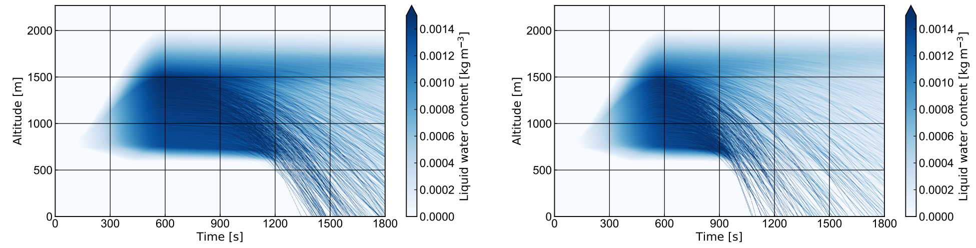
When particles settle under gravity in a periodic turbulent field, curtain-like particle structures form because of the interaction between large-scale turbulent eddies and the particle settling velocity (Woittiez et al., 2009). The formation of the curtain-like structures is related to the ratio of the time required for particles to cross one HIT block by gravitational settling, T_L*, and the large eddy turnover time,* T_e*.*

The structures form when $T_L/T_e < 1$. In order to avoid those artificial structures affecting the particle collisions, we added random perturbations to the horizontal particle positions when settling particles cross the horizontal boundary of the vertically aligned cubic snapshot of HIT. The perturbations were set to follow a uniform distribution between $l_\delta/L_0 = 0.2$ and $l_\delta/L_0 = -0.2$, where l_δ is the maximum perturbation length and L_0 is the size of the HIT snapshot.

To examine the influence of this treatment, we performed test simulations in a periodic box domain. The flow field was given by the same HIT snapshot used for the simulation in the vertically-elongated domain. The number of particles was set to be 10000 to obtain sufficient collision statistics. The particle positions were homogeneously distributed at the initial time. In the present HIT setting, the condition $T_L/T_e < 1$ corresponds to $d_\mathrm{p} > 31.4$ μm. Therefore, monodisperse particles with diameters of 30, 40, 50, 60, 70 μm were considered. The upper diameter limit of 70 μm was selected because collisions between particles of comparable size larger than 70 μm was negligibly rare compared to the different-sized droplet collisions due to the settling velocity differences in the vertically-elongated simulations (figures not shown). This is supported by Figure S1, which shows the 2D histogram of collision probability as a function of the larger particle diameter in a colliding pair and the particle-diameter ratio of the pair in TURB-case. *The time interval was set to be 5.0×10^{-5} s, which was identical to that used in TURB-case (see Table 4 in Section 3.4). Duration time was 50 s. The statistics were averaged from $t = 20$ s to $t = 50$ s, which was enough to reach the statistically steady state since $20 \text{ s} > 100T_e$.*

Figure 3 shows the dependency of the collision kernel, K_c, on the maximum perturbation length normalized by the domain size l_δ/L_0. K_c was larger for small perturbation length, indicating an artificial enhancement associated with the curtain-like structures. It decreased as the perturbation length increased. Around a perturbation length of $l_\delta/L_0 = 0.2$, K_c became much less sensitive to further increases in the perturbation length.

In the size range of $30-70$ μm, $\tau_\eta/T_L = 0.067-0.37$, where τ_η is the Kolmogorov time scale. The ratio τ_η/T_L represents the extent to which particles can experience dissipation-scale turbulent motions while settling across one HIT block, which is related to the RDF. Furthermore, in this size range, $\tau_\mathrm{p}/T_L = 0.015 - 0.46$, where τ_p is the particle relaxation time. The ratio τ_p/T_L represents the degree to which particle velocities can adjust to the local flow during the settling motion, which is related to the RRV. These time-scale ratios suggest that the perturbation treatment is not expected to substantially alter the contact RDF, RRV, or collision kernel in the present parameter range. However, the values of τ_η/T_L and τ_p/T_L are not much smaller than unity, indicating that some influence on the detailed collision statistics may still remain.



(a) LAM-case (b) TURB-case

Figure S3: (Figure 4 in the revised manuscript.) The temporal evolution of vertical liquid water content.

4

comment

> The authors apply periodic boundary conditions in the vertical direction, stating that only "dry CCN" exit the domain top (2250 m) and re-enter the bottom, concluding that this treatment "does not affect the physical results." (P6, section 3.2).
>
> (1) Given the prescribed updraft, whose peak value is 2 m/s, and the relatively shallow 250 m buffer layer between the cloud top ($\sim$ 2000 m) and the domain top, please briefly explain how the model ensures that all liquid droplets completely evaporate before crossing the upper boundary.
>
> (2) More importantly, even if complete evaporation is achieved, claiming "no physical effect" overlooks the phenomenon of aerosol processing. The underlying LCS formulation appears not to prognose dry solute mass and therefore may not retain the aerosol-processing history of droplets that have undergone coalescence. Please clarify how a completely evaporated, previously coalesced droplet is mapped back to the dry-CCN category, whether its solute mass or activation properties are retained, and whether repeated recycling through the vertical periodic boundary affects CCN number concentration or subsequent activation.
>
> Please soften the absolute claim that this boundary treatment "*does not affect the physical results.*" Please also discuss the possible effects of CCN recirculation and any loss of aerosol-processing history on the reported DSD evolution.

response

According to your comment, we modified the explanations for the particle boundary conditions to avoid misleading interpretations (L214-220).

(1)

The explanation "*does not affect the physical results*" was misleading. Our intention was not to imply that all liquid droplets completely evaporate before reaching the upper boundary. Rather, under the present configuration (L_z = 2250 m), no activated liquid droplets reach the upper boundary during the simulation. This is due to the characteristics of KiD warm-1 and the localized micro-scale eddies. To show this, we added a results of temporal evolution of vertical liquid water content (LWC) in the Results and Discussion section (L310-315, Figure 4). Figure S3 demonstrated that no liquid water reached the

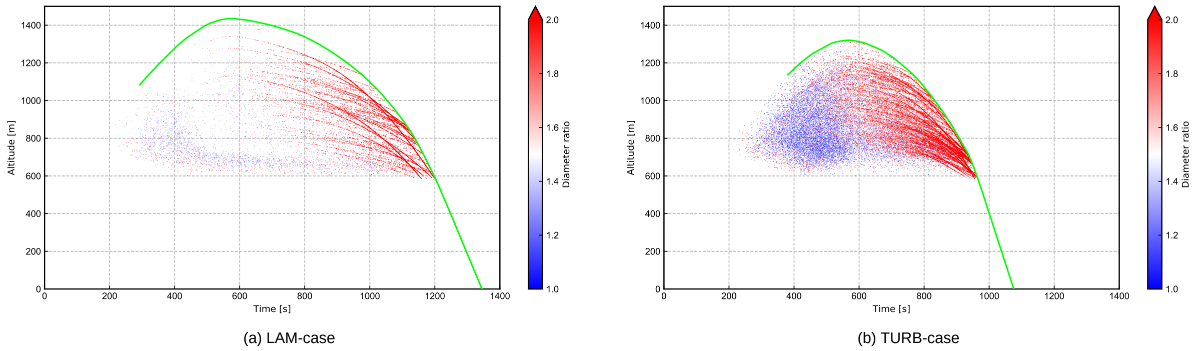


Figure S4: (Figure 8 in the revised manuscript.) Collision histories of the 1st surface-reaching raindrops. The green line represents the trajectory of the particle that reached the highest altitude (mother particle).

top boundary during the simulation.

(2)

Since no activated droplets reach the upper boundary, coalesced droplets are never recycled through the periodic boundary. Therefore, aerosol processing associated with boundary recirculation does not occur in the present simulations.

5

comment

In lines 202-206, the authors state that droplets "fell due to gravity" from higher altitudes into the middle and lower layers, contributing to the rapid diameter increase at approximately 800 s. However, the phrase "higher altitudes" is not quantitatively defined, and attributing the downward transport to gravity alone is incomplete because the particle motion also depends on the prescribed mean updraft and the vertical velocity of the HIT field.

For reference, a droplet with a radius of 40 μm and a terminal settling velocity of approximately 0.2 m/s would fall only about 120 m over 600 s in still air. This order-of-magnitude estimate does not by itself rule out the proposed interpretation, because the relevant droplets may have originated only moderately above the diagnosed layer and may have increased in size during their descent. It does, however, demonstrate that their source altitudes and transport histories cannot be inferred from the layer-mean DSD evolution alone.

Because the model explicitly tracks individual particles, the authors should provide trajectories and size histories for the droplets responsible for the rapid diameter increase at approximately 800 s. In particular, time series of particle altitude, particle size, particle vertical velocity, and the local air vertical velocity would establish where these droplets originated and whether gravitational settling, rather than the prescribed mean updraft or the HIT vertical velocity, was the dominant cause of their downward displacement.

response

In response to your comment, we added new analyses tracking the growth and collision histories of surface-reaching raindrops and updated the interpretation of Figure 6 (L365-399, Figure 8-10).

In the Lagrangian tracking simulation, the growth history of each precipitation particle is individually

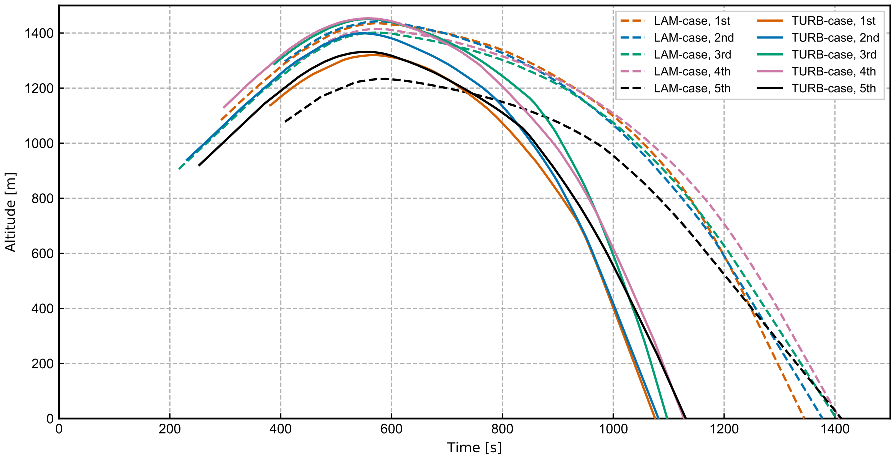
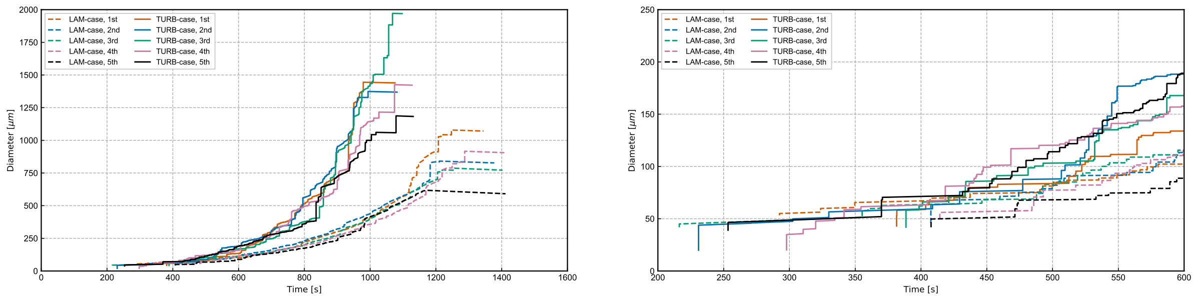


Figure S5: (Figure 9 in the revised manuscript.) Altitude histories of mother particles among 5 surface-reaching raindrops.



(a) 0 - 1800 s

(b) 200 - 600 s

Figure S6: (Figure 10 in the revised manuscript.) Growth histories of mother particles among 5 surface-reaching raindrops.

tracked. Figure 8 shows the collision histories of the 1st surface-reaching raindrops. Each scatter point represents a collision event, plotted against the time and altitude. The color bar indicates the diameter ratio of the colliding droplet pairs. The green line represents the trajectory of the particle that reached the highest altitude. Hereafter, this particle is referred to as the mother particle. The line begins at the time when the mother particle experienced its first collision and ends when the mother particle reaches the ground. When comparing the number of collisions in both cases, it was higher in TURB-case than in LAM-case (LAM-case: 26958, TURB-case: 65314). In the early developing stage, the collisions with small diameter ratios were more frequent in TURB-case than those in LAM-case. This result shows that turbulence promotes collisions between droplets with similar sizes (autoconversions) during the early developing stage. This trend is consistent with the results of the average diameter ratio with the threshold diameter of 80 μm *(See the discussion with Fig. 5). After* 600 s, *the collision events with larger diameter ratios were observed in both cases. However, the number of collisions was higher in TURB-case than in LAM-case. This result directly shows that TURB-case promotes collections of small droplets by larger droplets (accretions) during this period. The autoconversions during the early stage increased the size difference among droplets, thereby promoting accretion during the later stage.*

In addition, when comparing the trajectories of the mother particles, it was observed that the mother particle in TURB-case reached a lower altitude than that in LAM-case. This indicates that raindrop *growth was completed at lower altitudes in the TURB-case because of the larger number of collisions. Furthermore, compared this TURB-case result with the results of the average diameter (Fig. 6), an important observation was made. When focusing on the particles at* $t = 800$ s *in the lower layer in Fig. 8 and their trajectories (although not shown in the figure), it was found that these particles are different from those at* $t = 400$ s *in the middle layer. This indicates that the particles that contributed to the rapid growth in the lower layer at* $t = 800$ s *were different from those that contributed to the growth in the middle layer at* $t = 400$ s *in TURB-case. The particles that contributed to the rapid growth in the lower layer at* $t = 800$ s *were formed between lower and middle layers, and they fell into the lower layer while growing through accretion.*

Figure 9 shows the altitude histories of mother particles in the first five surface-reaching raindrops. LAM-case shows the dashed lines, while TURB-case shows the solid lines. Focusing on the five surface-reaching raindrops, the maximum altitude reached did not differ significantly between the LAM-case and TURB-case, in contrast to the comparison of the first surface-reaching raindrop shown in Fig. 8. However, the falling speed of the mother particles in TURB-case was much faster than that in LAM-case. This indicates that more active accretion occurred in TURB-case than in LAM-case, which was observed in the collision histories of surface-reaching raindrops (Fig. 8).

Figure 10 shows the growth histories of mother particles in the first five surface-reaching raindrops over $t = 0 - 1600$ s *in panel (a) and* $t = 200 - 600$ s *in panel (b). Fig. 10(b) shows that the diameter difference between the two cases appeared at around* $t = 450$ s*. This difference can be explained by the promoted accretion in the early developing stage in TURB-case (Fig. 8). Fig. 10(a) shows that the diameter difference increased after approximately* $t = 600$ s*. This can be explained by the active accretion in TURB-case after* $t = 600$ s*. The more rapid growth in TURB-case can explain the faster falling of mother particles in TURB-case than in LAM-case after* $t = 600$ s*.*

Specific Comments

1

comment

(P1, lines 16-17) "Cloud modeling has generally adopted the Euler method, which treats grid-average quantities (e.g. mass or/and number of liquid water) as continuous fields."
Eulerian bulk microphysics schemes prognose one or more grid-cell-mean moments—such as mass mixing ratio and, in multi-moment schemes, number concentration—for prescribed hydrometeor categories. Additionally, "number of liquid water" is grammatically incorrect; please revise to "number concentrations of hydrometeors" or "droplet number concentration" .

response

According to your comment, we modified the expression (L19-26). The modified explanation is "*Cloud modeling has generally adopted the Euler method, which prognose one or more grid-cell-mean moments, such as the mass mixing ratio and, in multi-moment schemes, the number concentration of prescribed hydrometeor categories.*" .

2

comment

(P1, lines 20-22) When discussing the development of bin microphysical schemes, it is highly recommended to cite classic and authoritative reviews (Khain et al., 2000, 2015) to provide a more comprehensive background.

response

According to your comment, we added Khain et al. (2000, 2015) as references for bin microphysical schemes (L26).

3

comment

(P1, lines 23-24) Please revise "proposed a super-droplet method (SDM) combined with large-eddy simulation (LES)" to "proposed the super-droplet method (SDM) coupled with large-eddy simulation (LES)" .

response

According to your comment, we modified the article of super-droplet method and revised from "combined" to "coupled" (L28).

4

comment

(P1-2, lines 24-27) "SDM is a Lagrangian particle tracking-based cloud modeling, in which each computational particle called super-droplet represents multiple real precipitation particles with the same properties. In this method, the advection of hydrometeors is calculated as the motion of super-droplets. The collision-coalescence growth is calculated by a stochastic scheme (Shima et al., 2009, 2020)."

This definition of SDM is too narrow and potentially misleading. A "super-droplet" does not solely represent precipitation particles; it represents a multiplicity of real particles, including aerosols, cloud droplets, and ice particles. Furthermore, please briefly clarify what "the same properties" refers to (e.g., radius, solute mass, etc.) for readers unfamiliar with SDM, and direct them to the foundational work (Shima et al., 2009, 2020) for a detailed mathematical definition.

response

According to your comment, we revised the description of the super-droplet method (SDM) (L28 – 33). The phrase "*precipitation particles*" was replaced with "*aerosols, cloud droplets, raindrops, and ice particles.*" We also clarified the meaning of "*the same properties*" and added a reference to Shima et al. (2009, 2020) for the detailed mathematical formulation.

5

comment

> (P2, lines 29-30) “In this approach, the motion and growth of individual precipitation particles are explicitly calculated.” Similar to the previous comment, the term “precipitation particles” is too restrictive.
> DNS and Lagrangian tracking approaches track aerosols and cloud droplets as well. Please revise for precision.

response

According to your comment, we revised the word “*precipitation particles*” to “*particles including aerosols, cloud droplets, and raindrops*” (L35-36).

6

comment

> (P2, lines 30-31) “Compared to SDM, direct tracking methods reduce stochastic treatments and can more accurately represent collision-coalescence growth.”
> This is a strong claim that lacks justification. Why is the collision-coalescence representation in DNS inherently “more accurate” than the well-established stochastic SDM? The authors need to elaborate in detail on the physical or mathematical reasoning behind this statement. Otherwise, please cite some references to support this statement.

response

According to your comment, we modified the expression (L37). “*more accurately*” was misleading, so the expression was revised to “*more explicitly*” .

7

comment

> (P2, lines 38-43) In the literature review, the authors merely list what previous studies have done. It would be much more informative to briefly synthesize the key findings and physical insights obtained from these preceding studies, rather than just stating their methodologies.

response

According to your comment, we added the key findings and physical insights from the references (L59-74).

Squires and Eaton (1990) showed that inertial particles preferentially concentrate in regions of low vorticity and high strain rate. Wang and Maxey (1993) demonstrated that particles are tend to accumulate in elongated sheets on the peripheries of local vortical structures and are preferentially swept by downward-moving fluid, resulting in a substantial enhancement of their average settling velocity. Sun-

daram and Collins (1997) performed DNS with finite-size inertial particles. They demonstrated that the collision frequency depends strongly on the particle Stokes number and can be quantitatively explained by the combined effects of preferential concentration and particle relative velocity. Reade and Collins (2000) extended Sundaram and Collins (1997) to coagulating particles in turbulence. They showed that the preferential concentration and particle relative velocity remain the key factors controlling collision-coalescence, while coagulation itself modifies these statistics and consequently the evolution of the particle size distribution. Following this development, similar approaches were introduced in cloud microphysics, where DNS coupled with particle tracking has been used to examine the effects of turbulence on droplet collisions in clouds. Franklin et al. (2005) demonstrated that increasing turbulence intensity enhances the preferential concentration and collisions, indicating that turbulence may accelerate the evolution of the droplet size distribution and the initiation of precipitation. Woittiez et al. (2009) investigated droplet collision statistics under the combined effects of turbulence and gravity. They showed that the interplay between turbulence and gravity is an essential element in determining collision statistics and leads to behaviors that cannot be explained by considering the two effects independently.

8

comment

> (P3, lines 64-66) "From these extensions, the all warm-cloud microphysical processes (CCN activation, condensation/evaporation growth, collision-coalescence growth, and sedimentation) could be treated by the direct tracking method."
> The phrase "the all warm-cloud microphysical processes" is too absolute. Does this tracking method account for aerosol deactivation? Furthermore, the wording "be treated by" is vague. Please clarify exactly how the phase-change process is physically resolved within the Lagrangian framework.

response

According to your comment, modified the description about Kunishima and Onishi (2018) with adding the detailed explanation of the framework (L102-110).

The expression "*the all warm-cloud microphysical processes*" was revised to "*fundamental warm-cloud microphysical processes.*" Aerosol deactivation process was considered in this framework. Activation/deactivation process was stochastically calculated by the bulk supersaturation ratio and the number of CCN. Condensation/evaporation growth is calculated using the diffusional growth equation.

9

comment

> (P3, lines 69-70) "Our approach is positioned as an intermediate between the conventional DNS studies and other cloud microphysics schemes."
> This statement is unclear. Please elaborate on what "intermediate" means in this context. Are the authors referring to the computational cost, the spatial scales resolved, or the level of physical approximation?

response

According to your comment, we remove these sentences as these sentences are confusing. The clear purpose and novelty of this study was revised in the previous sentences, which is enough to explain the position of this study (L112-118). This has already been explained in the response to Major Comment 1.

10

comment

> (P3, lines 70-72) "It inherits the advantages of direct tracking methods, which are reduced stochastic treatment and explicit representation of turbulence-microphysics interactions. It also enables simulations to span the all warm-cloud microphysical processes through the use of a vertically-elongated computational domain."
> The model explicitly represents interactions with a prescribed frozen HIT field. However, it does not represent dynamically evolving cloud turbulence or feedback from microphysics to the turbulent flow. Again, please avoid using absolute terms like "span the all warm-cloud microphysical processes."

response

According to your comment, we remove these sentences to avoid unclarity as we did for the specific comment 9.

11

comment

> The authors may consider adding a brief outline of the manuscript structure.

response

According to your comment, we added a brief outline of the manuscript structure at the end of the Introduction section (L119-122).

12

comment

> (P3-4, section 2.1) Please provide explicit values for the dimensions of the computational domain in the text. Furthermore, the concept of a "quasi-1D" domain and the specific size of the embedded HIT sub-domains need to be clearly defined and physically justified here.

response

According to your comment, we added explicit values for the dimensions of the elongated computational domain (5 mm $\times$ 5 mm $\times$ 2.25 km) (L131) and those of the HIT sub-domains (5 mm $\times$ 5 mm $\times$ 5 mm) (L138).

We also added the concept of a "quasi-1D" (L128-130). *The domain is referred to as quasi-1D because the particle motion is in three dimensions, whereas the thermodynamic fields (temperature and water vapor) vary only in the vertical direction.*

In addition, we added the justification of the use of frozen turbulence snapshot and its vertical repetition (L137-143). *This methodology is justified as the present framework is intended to investigate the effects of turbulence on collision-coalescence growth. The turbulence-enhanced collision-coalescence growth is primarily governed by the micro-scale eddies, which are much smaller than the cloud depth and at scales close to the Kolmogorov dissipation scale (Grabowski and Wang, 2013). At these scales, the turbulence can be assumed to be homogeneous and isotropic, while the large-scale eddies are inhomogeneous and anisotropic. The use of frozen turbulence snapshot and its vertical repetition were adopted for the computational simplicity.*

13

comment

> (P4, figure2 caption, line 79) For the benefit of readers who may not be experts in fluid dynamics diagnostics, please add a brief explanation or definition of the "Q-criterion isosurfaces" in the figure caption.

response

According to your comment, we added the definition and explanation of Q-criterion (Figure 2 caption). *Q-criterion is defined as* $Q = \frac{1}{2}\left(\Omega_{ij}\Omega_{ij} - S_{ij}S_{ij}\right)$*, where* Ω_{ij} *and* S_{ij} *are the rotation rate tensor and strain rate tensor, respectively. This criterion is used to represent vortical structures in the flow.*

14

comment

> (P5, lines 106-107) "CCN activation is treated based on a stochastic model (Twomey, 1959). This model represents the relationship between supersaturation ratio and the number of activated droplets."
> The classic Twomey (1959) parameterization is an empirical bulk relationship and is not inherently stochastic. Please provide specific mathematical details or cite the exact literature detailing how this stochastic activation scheme is formulated and implemented at the individual particle level.

response

According to your comment, we added the detailed explanations and the reference (Kunishima and Onishi, 2018) of the CCN activation model (L160-177).

As you mentioned, the Twomey activation model (Twomey, 1959) is an empirical bulk relationship. The stochastic activation model proposed in Kunishima and Onishi (2018) is based on this relationship.

The detailed explanation of the CCN activation model is as follows.

CCN activation is treated by the stochastic model proposed in Kunishima and Onishi (2018). In the model, first, the number density of newly activated liquid droplets during a time step is calculated in each

Euler grid cell using an empirical bulk relationships in Twomey (1959). Then, a stochastic judgment is made on whether or not to activate each CCN particle. If the decision is affirmative, another stochastic judgment is made on the size of the activated droplets.

In Twomey (1959), the relationship between supersaturation ratio S_w and the number of activated CCN N_act is expressed as

$$N_\mathrm{act} = C S_\mathrm{w}^k,$$

where C and k depend on CCN type. If S_max is defined as the maximum supersaturation ratio required for activating total particle count $N_\mathrm{CCN} + N_\mathrm{w}$, where N_CCN is the number of CCN and N_w is the number of liquid droplets, then the constant C can be expressed as $C = (N_\mathrm{CCN} + N_\mathrm{w}) S_\mathrm{max}^{-k}$. Therefore, Eq. (4) can be rewritten as

$$N_\mathrm{act} = (N_\mathrm{CCN} + N_\mathrm{w}) \left(\frac{S_\mathrm{w}}{S_\mathrm{max}} \right)^k.$$

Thus, the number of newly activated droplets during a time step Δt is calculated as

$$N_\mathrm{act} = \max\left\{ 0, (N_\mathrm{CCN} + N_\mathrm{w}) \min\left[1, \left(\frac{S_\mathrm{w}}{S_\mathrm{max}} \right)^k \right] \right\}.$$

Parameter values for maritime conditions are assumed, with $S_\mathrm{max} = 0.008$ and $k = 0.6$ (Onishi and Takahashi, 2012). The size distribution of newly activated droplets follows an exponential distribution (Soong, 1974) as

$$f(r_\mathrm{act}) = \frac{3 r_\mathrm{act}^2}{\bar{r}_m^3} \exp\left[- \left(\frac{r_\mathrm{act}}{\bar{r}_m} \right)^3 \right],$$

where $\bar{r}_m = \left\langle r^3 \right\rangle^{1/3}$ is the average-mass droplet radius. $\bar{r}_m$ was set to 11 μm (Onishi and Takahashi, 2012).

15

comment

> (P5, lines 108-110) "Maritime conditions are assumed, with parameter values following Onishi and Takahashi (2012). The sizes of newly activated droplets follow an exponential distribution, with the average-mass droplet radius set to 11 μm (Onishi and Takahashi, 2012)."
>
> To ensure the manuscript is self-contained, please explicitly list the specific maritime parameter values adopted in this study. Furthermore, the term "average-mass droplet radius" is ambiguous. Please define the average-mass droplet radius explicitly, for example as $\bar{r}_m = \langle r^3 \rangle^{1/3}$, and distinguish it from the arithmetic mean radius $\langle r \rangle$ or any mass-weighted mean radius.

response

According to your comment, we explicitly listed the maritime parameter values adopted in this study (L174). In addition, we added the definition of the average-mass droplet radius $\bar{r}_m = \langle r^3 \rangle^{1/3}$ (L177).

16

comment

(P6, table 1) Table 1 provides values at only three vertical nodes. Please state explicitly how the temperature and water-vapor profiles are interpolated between these nodes and consider adding a continuous profile plot.

response

According to your comment, we added the explanation of how the profiles were interpolate between the nodes, which is linear interpolation (L203-204, Table 1 caption). Since the profiles are uniquely determined by these values and the linear interpolation, we believe that an additional figure is unnecessary.

17

comment

(P7, line 150) The term "reduced communication forcing (FDM-RCF)" is introduced without adequate definition. Please expand FDM as "finite-difference model" and briefly explain the numerical role of the reduced-communication forcing scheme and the scales at which energy is injected.

response

According to your comment, we expanded FDM as "*finite-difference model*" (L226).

We also added the brief explanation of the forcing scheme and the scales at which energy is injected (227-229). *This forcing scheme injects kinetic energy into the largest scales of the flow while reducing communication overhead in massively parallel computations. In the present study, the kinetic energy was injected into the wavenumber of* $0.5 < |\mathbf{k}| \leq 2.5$.

18

comment

(P7, table 2) As I understand Table 2 and Figure 2, the HIT snapshot is a 5 mm cubic domain initially discretized using 32^3 grid points and subsequently coarse-grained to 16^3. The same snapshot is then repeated vertically to fill the 2250 m column. Please state these dimensions and the number of repeated tiles explicitly and justify the use of an identical frozen snapshot throughout the column.

response

According to your comment, we added the statement about the dimension (L230) and the number of repeated tiles (L231-234). The use of frozen turbulence snapshot and its vertical repetition were adopted for the computational simplicity, so we also added this description to the Overview section (L144).

19

comment

(P8, table 4) The grid sizes differ significantly between LAM-case ($1.67^2 9.77$ mm^3) and TURB-case (0.31^3 mm^3). Because the two cases differ simultaneously in turbulent forcing, grid spacing, timestep, and the use of random particle displacements, the present comparison does not isolate the effect of turbulence matched alone. A fine-grid LAM control and perturbation-sensitivity experiments are needed to separate these effects.

response

According to your comment, we showed the justification of the difference in the grid size and time interval (L288-307).

We understand your concern that differences in the grid spacing and time interval may affect the comparison between the LAM-case and TURB-case. However, an additional fine-grid LAM simulation is unnecessary for the reasons as follows.

For LAM-case, the time interval is primarily determined by the tracer-particle threshold. Resolving the equation of motion for very small droplets requires an very small time interval because their particle relaxation times are also very small. To avoid the excessive computational cost associated with such a small time interval, particles with radii smaller than 14 μm are treated as tracer particles following Kunishima and Onishi (2018). The time interval ($\Delta t = 1.25 \times 10^{-3}$ s) is then chosen so that the relaxation times of particles larger than this threshold are adequately resolved. The grid spacing is subsequently selected to be consistent with the adopted time interval, preventing particles from traversing more than one grid cell during a single time step.

For TURB-case, the procedure is reversed. The grid spacing is fixed by the spatial resolution of the HIT snapshot. The time interval is then determined according to this grid spacing, and the corresponding tracer-particle threshold is chosen consistently with the adopted time interval. The threshold in TURB-case is 3 μm.

Consequently, adopting the same fine grid spacing and time interval as TURB-case in LAM-case would simply reduce the tracer-particle threshold. According to Kunishima and Onishi (2018), this modification is not expected to affect the simulation results because, in LAM-case, droplets smaller than the tracer threshold grow predominantly by condensation rather than by collision-coalescence. Therefore, the finer grid spacing and smaller time interval would not significantly alter the results in LAM-case.

In addition, the random particle displacements was not applied in LAM-case (L278). The perturbation-sensitivity has already been discussed in the Major comment 3.

20

comment

(P8, lines 177-180) "The time interval in TURB-case was 25 times smaller than that in LAM-case. This was determined by the ratio of the vertical grid size between LAM-case and TURB-case (9.77 mm/0.31 mm = 31.5), so that droplets would not move more than one grid within a single time interval."
Rather than referring only to the grid-spacing ratio, please report the maximum particle Courant number in each case and demonstrate that the trajectory and collision-detection criteria are satisfied for the fastest particles.

response

According to your comment, we added the discussion of the time interval settings based on the observed maximum particle size in the simulation (L288-302).

We evaluated the maximum particle displacement during a single time step based on the observed diameter of the largest raindrops in each simulation. The detailed analysis is provided in the revised manuscript (L289-307). We found that the largest raindrop in LAM-case (1st surface reaching raindrop) satisfied the condition that it did not move more than one grid within a single time step, whereas that in TURB-case (3rd surface-reaching raindrop) may travel slightly more than one grid spacing. However, large raindrops with high inertia ($St \gg O(1)$ when $d_\mathrm{p} > 1$ mm) are only weakly affected by the prescribed dissipation-scale turbulence. Consequently, the slight excess over one grid spacing is expected to have a negligible influence on the particle trajectories.

21

comment

(P9, lines 187-188) "In addition, the average diameter ratio of the colliding droplets with the threshold diameter of 80 μm was calculated from 300 s to 600 s. The average diameter ratio was 1.88 in LAM-case, while it was 1.66 in TURB-case."
Please explicitly define "diameter ratio" (e.g., the ratio of the larger droplet to the smaller droplet in a colliding pair) and briefly explain the reason why this 300-600 s period was selected.

response

According to your comment, we added an explicit definition of the diameter ratio (L321-323). We also revised the description of the averaging period to avoid confusion.

The diameter ratio was defined as the ratio of the larger droplet to the smaller droplet in a colliding pair.

The average diameter ratio was calculated during the early developing stage of the cloud, when the prescribed updraft was present. Although the manuscript previously referred to the period from 300 s to 600 s, no collisions satisfying this criterion occurred before $t = 300$ s. Therefore, averaging over the period up to 600 s gives the same result while avoiding the misleading impression that the analysis was intentionally restricted to 300–600 s.

22

comment

> (P11, lines 195-196) "In the upper layer, condensation occurred later than in the lower layers." This statement currently reads as an unsupported assertion. The manuscript does not present the supersaturation fields or any explicit condensation rate profiles to substantiate this claim. Please provide the necessary supporting figures (e.g., the temporal evolution of the supersaturation profile) or revise the text to avoid drawing speculative conclusions without direct, data-driven evidence.

response

According to your comment, we added a result of liquid water content (Figure 4: Figure S3 in this document) as a supporting figure and modified the explanation from "*In the upper layer, condensation occurred later than in the lower layers*" to "*In the upper layer, the appearance of the liquid droplets is later than in the lowers*" (L330-334).

In the upper layer, the appearance of the liquid water occured later than in the lower layers, which was observed in Fig. 4. This is due to the thermodynamic characteristics of KiD warm-1. The water vapor-rich air parcel was advected from the lower altitude to the higher. Furthermore, the generated liquid droplets around the cloud base were also advected. This caused the delay of the liquid water appearance in the upper layer.

23

comment

> (P12, figure 7) The authors should state whether the regression is constrained through the origin and assess the robustness of the slope difference using independent simulation realizations or an appropriate resampling procedure that accounts for within-run dependence. The slope units should also be stated explicitly (line 233).

response

According to your comment, we added an explicit statement that the regression was constrained through the origin (L409). The units of the slope were also stated explicitly (L411 and Fig. 12 legend). Regarding the robustness of the fitted slope, we added a discussion of the interpretation (L413-454).

Because this conclusion is based on a single simulation realization under the prescribed kinematic framework, the results should not be interpreted as universally applicable. Further studies using multiple realizations and different environmental conditions are required to evaluate the robustness and generality of the reported relationships. However, this simulation result demonstrated a possibility that the difference in growth scenario between the two case can cause the present result.

The interpretation of this result has also been revised (see our response to Specific Comment 24).

24

comment

(P13, lines 244-246) "Since condensation growth depends on the droplet size, changes in the droplet size distribution can alter the growth history. These results confirmed that turbulence affected the condensation growth through the promotion of the collision-coalescence process."
The logical connection between these two sentences is unclear. Under diffusion-limited growth at fixed supersaturation, the radius growth rate scales approximately as $dr/dt \propto S/r$, whereas the mass growth rate of an individual droplet scale as $dm/dt \propto rS$. Collision-induced changes in the DSD may alter the total condensational conductance, the phase-relaxation time, and consequently the supersaturation field. The authors should diagnose these quantities directly rather than infer the feedback solely from mean droplet size.

response

According to your comment, we revised the interpretation of this result by taking into account the broadening of the droplet size distribution (DSD) (L413-449). In contrast, the present interpretation does not explicitly consider the phase-relaxation time or the spatiotemporal evolution of the supersaturation field. Therefore, we also added a discussion of these limitations (L450-454). The revised interpretation is intended as a physically plausible explanation for the present results rather than a definitive conclusion.

The numbers of activated droplets during the first 600 s *were nearly the same in the two cases:* $N_{\mathrm{act,LAM}} = 1.49 \times 10^6$ *and* $N_{\mathrm{act,TURB}} = 1.51 \times 10^6$. *This is because the effect of turbulence on CCN activation was not considered in the present study. Furthermore, the total liquid water volume condensed in 30 min simulation was* $3.82 \times 10^{-8}\ \mathrm{m}^3$ *in both cases. The total volume was also nearly identical in the two cases because it was determined by the bulk thermodynamic conditions of the KiD warm-1. From these perspectives, the average volumes of constituent particles in the entire domain were almost the same in both cases, which is* $v_{\mathrm{domain,LAM}} = 2.56\times10^{-14}\ \mathrm{m}^3$ *in LAM-case and* $v_{\mathrm{domain,TURB}} = 2.53\times10^{-14}\ \mathrm{m}^3$ *in TURB-case. However, the average volume of constituent particles in the surface-reaching raindrops showed a difference between the two cases. In TURB-case, the average volume of constituent particles in the surface-reaching raindrops* $v_{\mathrm{c,TURB}} = 2.52 \times 10^{-14}\ \mathrm{m}^3$ *is close to* $v_{\mathrm{domain,TURB}}$. *On the other hand, in LAM-case, the average volume of constituent particles in the surface-reaching raindrops* $v_{\mathrm{c,LAM}} = 2.36 \times 10^{-14}\ \mathrm{m}^3$ *is smaller than* $v_{\mathrm{domain,LAM}}$.

The result in TURB-case can be interpreted as follows. Several factors can affect the V/N_{memb} *values among droplets. One is the size distribution of activated droplets, which was considered in Eq. (7). When assuming that all droplets experience the same bulk supersaturation field, and that no coalescence occurs, the larger droplets gain more liquid water through condensation growth due to* $\mathrm{d}m_{\mathrm{p}}/\mathrm{d}t \propto r^1$. *Another factor is the collision-coalescence growth in the early developing stage, where condensation growth is still dominant. Consider the assumption that all constituent particles have equal volumes, some of them coalesce into larger droplets and that the droplets have a size distribution. In this condition, although larger droplets gain more liquid water through condensation growth than smaller ones, the condensation growth per constituent particle is proportional to the inverse square of the droplet radius (*$\mathrm{d}m_{\mathrm{p}}/\mathrm{d}t/N_{\mathrm{memb}} \propto r^{-2}$*). Thus, the larger droplets gain less liquid water per constituent particle. An important point is that although these factors may occur in TURB-case, the* $v_{\mathrm{c,TURB}}$ *was close to* $v_{\mathrm{domain,TURB}}$. *This can be explained by* *the fact that collision-coalescence growth in the TURB-case was highly active throughout the simulation. When the droplets with different* $V/N_{\rm memb}$ *values coalesce, the* $V/N_{\rm memb}$ *value becomes an intermediate value. Consequently, repeated collision-coalescence tended to homogenize the average constituent-particle volume among droplets. Because such collisions occured repeatedly throughout the simulation, the average constituent-particle volume* $v_{\rm c,TURB}$ *was close to the domain-average value* $v_{\rm domain,TURB}$.

In contrast, the smaller value of $v_{\rm c,LAM}$ *than* $v_{\rm domain,LAM}$ *in LAM-case can be interpreted as follows. In this case, the collision-coalescence growth was not active during the early developing stage. Thus, the first factor mentioned in the previous paragraph was dominant in this case, while the second factor was less significant during the early stage. After the early stage (*$t > 600$ s*), the collision-coalescence growth (accretion) was dominant. During the accretion process, the larger droplets with larger constituent particles collected numerous smaller droplets with smaller constituent particles through accretion. This decreased the average volume of constituent particles in the larger droplets. Consequently, although condensation initially increased the average volume of constituent particles in large droplets, the subsequent accretion of numerous smaller droplets reduced this average, leading to* $v_{\rm c,LAM} < v_{\rm domain,LAM}$.

The key difference between the two cases is the growth scenario of droplets. The collision-coalescence began much earlier in the TURB-case, allowing repeated coalescence among droplets with different values of $V/N_{\rm memb}$ *throughout the simulation. In contrast, in the LAM-case, collision-coalescence mainly occured after* $t = 600$ s*, when the droplets have already grown through condensation. This difference may explain the observed difference in the average volume of constituent particles in surface-reaching raindrops between the two cases.*

This interpretation is not definitive because it does not fully resolve the combined effects of the initial size distribution of activated droplets and the early-stage coalescence. Furthermore, in the present simulations, the supersaturation field varies in time and altitude. Therefore, the present results may depend on the environmental conditions and the initial conditions. Nevertheless, this interpretation provides a physically consistent explanation for why $v_{\rm c,TURB}$ *was close to* $v_{\rm domain,TURB}$*, whereas* $v_{\rm c,LAM}$ *was smaller than* $v_{\rm domain,LAM}$ *in the present simulations.*

25

comment

(P13, lines 263-264) "The relationship between the diameter of surface-reaching raindrops and the time when they reach the ground was examined. The onset of surface precipitation in TURB-case occurred earlier than that in LAM-case."
Multiple independent realizations are required to estimate the distribution and confidence interval of precipitation-onset time. This is particularly important because the TURB-case differs from the LAM-case not only through the HIT velocity field but also through the imposed random horizontal displacements.

response

According to your comment, we added a discussion of the limitations associated with conducting a single simulation for each case (L517-523).

In the present study, only one simulation was performed for each case. The results may vary depending on the initial conditions (e.g., the initial droplet positions), the prescribed HIT snapshot, or the treatment of the curtain-like structure. For example, the onset time of surface precipitation may become earlier or later in different realizations. Therefore, the quantitative magnitude of the turbulence effects reported in this study, such as the 270 s difference in the onset of surface precipitation, should not be directly generalized to real clouds.

Nevertheless, the present results demonstrate the capability of the present framework to conduct direct analysis of turbulence-induced collision-coalescence through explicit tracking of the individual droplets while accounting for the cloud-depth thermodynamic structures. (L495-497).

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