Referee comment on the manuscript "Direct Lagrangian tracking simulation of droplet growth in vertically-developing turbulent cloud" by Iwashima and Onishi

Overall assessment

This manuscript introduces an explicit cloud microphysics model that combines direct numerical simulation (DNS) with Lagrangian particle tracking to investigate warm-cloud processes. The topic is scientifically relevant, and the approach may provide useful information on turbulence–droplet interactions. However, the manuscript has several major weaknesses that preclude publication in its present form.

My main concerns are fourfold. First, the physical basis of the numerical configuration is not sufficiently established, particularly with regard to whether homogeneous isotropic turbulence (HIT) in a vertically elongated domain can adequately represent a marine cumulus environment. Second, the novelty of the reported findings is not clearly distinguished from prior work. Although the manuscript presents quantitative analyses of turbulence–droplet interactions, the robustness of these results remains uncertain, and several conclusions appear to restate established effects without offering sufficiently new physical insight. A more comprehensive review of the literature is therefore needed to define the study's specific contribution. Third, several methodological elements are underdescribed and insufficiently connected to the relevant theoretical literature. Fourth, some key interpretations are not directly supported by the simulation results.

For these reasons, I do not recommend publication in its present form. My recommendation is based mainly on the amount of additional numerical work and analysis needed to support the main conclusions, rather than on a fundamental objection to the modeling framework itself. The framework has potential, and I encourage the authors to address these issues and consider resubmitting a substantially revised version.

Major Comments

1. 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.

2. 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.

3. 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 (~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.

4. 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 (~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.

5. 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.

Specific Comments:

1. (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".

2. (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.

3. (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)".

4. (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.

5. (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.

6. (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.

7. (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.

8. (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.

9. (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?

10. (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."

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

12. (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.

13. (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.

14. (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.

15. (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 $r_{\overline{m}} = \langle r^3 \rangle^{1/3}$, and distinguish it from the arithmetic mean radius $\langle r \rangle$ or any mass-weighted mean radius.

16. (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.

17. (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.

18. (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.

19. (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 alone. A fine-grid LAM control and perturbation-matched sensitivity experiments are needed to separate these effects.

20. (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.

21. (P9, lines 187-188) "In addition, the average diameter ratio of the colliding droplets with the threshold diameter of 80 μ m 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.

22. (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.

23. (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).

24. (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.

25. (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.

References

Abade, G. C., Grabowski, W. W., and Pawlowska, H.: Broadening of cloud droplet spectra through eddy hopping: Turbulent entraining parcel simulations, Journal of the Atmospheric Sciences, 75, 3365–3379, https://doi.org/10.1175/JAS-D-18-0078.1, 2018.

Chandrakar, K. K., Cantrell, W., Chang, K., Ciochetto, D., Niedermeier, D., Ovchinnikov, M., Shaw, R. A., and Yang, F.: Aerosol indirect effect from turbulence-induced broadening of cloud-droplet size distributions, Proceedings of the National Academy of Sciences, 113, 14243–14248, https://doi.org/10.1073/pnas.1612686113, 2016.

Chandrakar, K. K., Grabowski, W. W., Morrison, H., and Bryan, G. H.: Impact of Entrainment Mixing and Turbulent Fluctuations on Droplet Size Distributions in a Cumulus Cloud: An Investigation Using Lagrangian Microphysics with a Subgrid-Scale Model, Journal of the Atmospheric Sciences, 78, 2983–3005, https://doi.org/10.1175/JAS-D-20-0281.1, 2021.

Chandrakar, K. K., Morrison, H., Grabowski, W. W., Bryan, G. H., and Shaw, R. A.: Supersaturation Variability from Scalar Mixing: Evaluation of a New Subgrid-Scale Model Using Direct Numerical Simulations of Turbulent Rayleigh–Bénard Convection, Journal of the Atmospheric Sciences, 79, 1191–1210, https://doi.org/10.1175/JAS-D-21-0250.1, 2022.

Grabowski, W. W.: Broadening of Cloud Droplet Spectra through Eddy Hopping: Why Did We All Have It Wrong?, Journal of the Atmospheric Sciences, 82, 443–453, https://doi.org/10.1175/JAS-D-24-0082.1, 2025.

Grabowski, W. W. and Abade, G. C.: Broadening of cloud droplet spectra through eddy hopping: Turbulent adiabatic parcel simulations, Journal of the Atmospheric Sciences, 74, 1485–1493, https://doi.org/10.1175/JAS-D-17-0043.1, 2017.

Grabowski, W. W., Chandrakar, K. K., and Morrison, H.: Untangling the broadening of adiabatic cloud droplet spectra through eddy hopping in a high-resolution cumulus congestus simulation, Journal of the Atmospheric Sciences, https://doi.org/10.1175/JAS-D-25-0003.1, 2025.

Grabowski, W. W., Chandrakar, K. K., and Morrison, H.: Broadening of Adiabatic Droplet Spectra through Eddy Hopping: Polluted versus Pristine Environments, Journal of the Atmospheric Sciences, 83, 559–576, https://doi.org/10.1175/JAS-D-25-0148.1, 2026.

Khain, A., Ovtchinnikov, M., Pinsky, M., Pokrovsky, A., and Krugliak, H.: Notes on the state-of-the-art numerical modeling of cloud microphysics, Atmospheric Research, 55, 159–224, https://doi.org/10.1016/S0169-8095(00)00064-8., 2000.

Khain, A. P., Beheng, K. D., Heymsfield, A., Korolev, A., Krichak, S. O., Levin, Z., Pinsky, M., Phillips, V., Prabhakaran, T., and Teller, A.: Representation of microphysical processes in cloud-resolving models: Spectral (bin) microphysics versus bulk parameterization, Reviews of Geophysics, 53, 247–322, https://doi.org/10.1002/2014RG000468, 2015.

Korolev, A. V.: The Influence of Supersaturation Fluctuations on Droplet Size Spectra Formation, Journal of the Atmospheric Sciences, 52, 3620–3634, https://doi.org/10.1175/1520-0469(1995)052%3C3620:TIOSFO%3E2.0.CO;2, 1995.

Prabhakaran, P., Thomas, S., Cantrell, W., Shaw, R. A., and Yang, F.: Sources of Stochasticity in the Growth of Cloud Droplets: Supersaturation Fluctuations versus Turbulent Transport, Journal of the Atmospheric Sciences, 79, 3145–3162, https://doi.org/10.1175/JAS-D-22-0051.1, 2022.

Sardina, G., Picano, F., Brandt, L., and Caballero, R.: Continuous Growth of Droplet Size Variance due to Condensation in Turbulent Clouds, Phys. Rev. Lett., 115, 184501, https://doi.org/10.1103/PhysRevLett.115.184501, 2015.

Shima, S., Kusano, K., Kawano, A., Sugiyama, T., and Kawahara, S.: The super-droplet method for the numerical simulation of clouds and precipitation: A particle-based and probabilistic microphysics model coupled with a non-hydrostatic model, Quarterly Journal of the Royal Meteorological Society, 135, 1307–1320, https://doi.org/10.1002/qj.441, 2009.

Shima, S., Sato, Y., Hashimoto, A., and Misumi, R.: Predicting the morphology of ice particles in deep convection using the super-droplet method: development and evaluation of SCALE-SDM 0.2.5-2.2.0, -2.2.1, and -2.2.2, Geosci. Model Dev., 13, 4107–4157, https://doi.org/10.5194/gmd-13-4107-2020, 2020.

Twomey, S.: The nuclei of natural cloud formation part II: The supersaturation in natural clouds and the variation of cloud droplet concentration, Geofisica pura e applicata, 43, 243–249, https://doi.org/10.1007/BF01993560, 1959.