Review of "Aerosol loading delays droplet activation and suppresses drizzle mode Ka-band radar signatures in LES–LCM simulations of shallow cumulus clouds" by *Lee* et al. The manuscript presents an analysis of large-eddy simulations of shallow cumulus using Lagrangian super-droplet microphysics to investigate the impact of aerosol loading on cloud-droplet activation and subsequent growth into warm rain. The authors further employ a radar forward simulator to investigate whether aerosol-induced differences precipitation properties produce signatures observable at Ka-band.

This manuscript provides an interesting Lagrangian perspective on aerosol–cloud interactions that is not widely represented in the peer-reviewed literature. In particular, the aerosol-dependent differences in activation timing relative to cloud lifetime are potentially important and warrant consideration of the manuscript for publication. However, this contribution is challenging to access given the overall length of the manuscript, which also includes several other parallel analyses. I estimate the manuscript to be approximately 39 publishing units (~11,500 words, 10 figures, and 6 tables), which substantially affects its readability and makes the central scientific contribution difficult to identify.

I have four principal concerns. First, the manuscript would benefit from substantial refocusing and shortening around the scientific questions for which the Lagrangian particle tracking model provides genuinely new insight. Second, the representation of warm-rain development needs to be more clearly established to support the extensive interpretation of mature precipitation, particularly given continuing uncertainties regarding the processes controlling warm-rain initiation and growth, particularly in turbulent clouds. Third, some trajectory analyses are conditioned on particles present at the end of the simulation, and the sensitivity of the central activation-timing result to this population selection needs to be established. Finally, while the radar forward simulations demonstrate that the modeled aerosol perturbation propagates into observation space, it is not yet clear that the resulting signatures provide a practical observational constraint on aerosol effects since differences in cloud life cycle, dynamics, thermodynamic environment, and other factors could produce similar radar responses in nature.

Major comments

1. Excessive manuscript length and breadth of analysis

The manuscript addresses a broad sequence of processes and diagnostics, extending from aerosol-dependent cloud-droplet activation and thermodynamic histories through mixing and deactivation, warm-rain development, and ultimately radar observables. I appreciate that the authors aim to demonstrate how the aerosol perturbation manifests across multiple diagnostics. However, presenting and interpreting each perspective at the current level of detail results in a very long manuscript. The accumulation of successive analyses contributes to reader fatigue and makes it difficult to maintain focus on the findings that provide the most important new scientific insight.

I find the aerosol-dependent delay in activation, and its implications for the remaining growth time, to be one of the most interesting results of the study. I encourage the authors to make this timescale problem a central focus of the manuscript. The Lagrangian particle-tracking framework is particularly well suited to investigating how delayed activation and subsequent deactivation reduce the effective time available for individual droplets to grow.

More generally, I strongly recommend substantially shortening the manuscript. Detailed recommendations concerning the Introduction, individual analyses, and figures are provided below.

2. Regarding the introduction and simulation of warm-rain formation and growth processes

Given the model description, I have greater confidence in the simulated cloud-droplet activation and early condensational growth than in the subsequent development of mature drizzle and rain. This distinction is important because much of the radar analysis, including the Doppler spectra and Z–R relationships, depends on the realism of the simulated precipitation-size population. Warm-rain initiation and growth in turbulent shallow clouds remain active areas of research, with continuing uncertainty regarding the roles of condensational broadening, turbulence, entrainment and recirculation, collision–coalescence, and other dynamical and microphysical processes. The manuscript should therefore establish more clearly how the transition from activated cloud droplets to precipitation is represented in the model and what limitations this puts on interpretation of the simulated rain.

In particular, the authors should more clearly describe the processes controlling development of the large-drop tail in their model, including the collision–coalescence formulation, collision efficiencies, the role of resolved dynamics, and stochastic rare-drop growth, while identifying relevant processes that are unresolved or parameterized. I am not suggesting that the model must represent every process proposed to influence warm-rain formation, but its capabilities and limitations should be sufficiently clear to support the extensive downstream interpretation of precipitation and radar observables. Along the same lines, the Introduction should better acknowledge the range of existing hypotheses and continuing uncertainty surrounding warm-rain initiation and growth rather than presenting the progression from activation to precipitation as comparatively well established.

3. **The purpose and observational relevance of the radar forward-modeling analysis need to be more clearly established**

I am not yet convinced that the radar component, in its current form, provides an observational constraint on the aerosol-dependent activation process central to the manuscript. The forward simulations demonstrate that aerosol-induced microphysical differences propagate into radar observation space, but this forward sensitivity should be distinguished from an observational aerosol diagnostic. The simulations provide experimental control by varying aerosol loading under otherwise similar imposed conditions; observations do not. Reflectivity, Doppler spectra, and Z–R relationships can also vary with thermodynamic and dynamical conditions and aerosol properties other than number concentration. Thus, the fundamental observational challenge is attribution, not simply sampling volume or temporal averaging. Testing the proposed aerosol signatures could potentially be achieved via statistical analysis of many tracked clouds stratified by environmental and life-cycle characteristics and accompanied by aerosol information. Thus, I suggest that the authors clarify the observational strategy they envision and more explicitly acknowledge the limits of attribution: demonstrating that an aerosol perturbation produces a radar response in a controlled simulation does not establish that the same response can be uniquely attributed to aerosols in observations.

There is also a disconnect between the activation-focused analysis (the first part of the manuscript) and the radar analysis (the last part of the manuscript). The precipitation signal observed by the radar is a downstream consequence of activation and subsequent droplet growth. Interpreting it therefore depends on confidence in the modeled transition from cloud droplets to precipitation, which, as discussed above, is not sufficiently described and remains uncertain given the range of processes proposed to influence warm-rain development.

If the radar forward modeling is retained as a major component of the manuscript, I encourage the authors to ask a more focused question: *do the aerosol-dependent differences in activation and early condensational growth produce a detectable radar signature before substantial drizzle development?* Fig. 8 appears to contain potentially interesting evidence of such a life-cycle-dependent response that is not adequately discussed. Prior to approximately 1000 s, the polluted T73 simulation appears to produce larger Z_e than T13 over portions of the developing cloud, whereas later as the cleaner simulation develops a stronger large-drop population we see the opposite. This early behavior is particularly interesting given the results shown in Fig. 4, where T73 contains a much larger concentration of large wet particles, many of which the authors classify as subcritical. If this early reflectivity is associated with that population, the authors should also explain why these substantially swollen particles remain below their activation threshold. Establishing the origin of this early signal would provide a much more direct connection between the activation analysis and the radar forward simulations than focusing predominantly on the mature precipitation stage.

4. **Final-time population conditioning in the trajectory analysis requires clarification and sensitivity testing**

   The aerosol-dependent delay in cloud-droplet activation is, in my view, one of the most interesting results of the manuscript. However, I have an important concern regarding the population used for the trajectory-based analyses. The authors state that particle IDs present at the final output time are selected and traced backward through earlier outputs. Thus, at least some trajectory statistics are conditioned on particles being present at the end of the simulation rather than representing the complete particle population at earlier times. The authors themselves demonstrate one consequence of this selection in Fig. 3, noting that preferential loss of smaller particles from the drier clusters before t = 1800 s can shift the reconstructed early-time mean radii toward larger values.

   It is unclear how broadly this conditioning applies to the subsequent trajectory analyses and, in particular, whether the activation-time distributions in Fig. 5 are calculated from the same final-time population. If so, Fig. 5 would represent the activation-time distribution conditional on particles being present at t = 1800 s, rather than necessarily the activation-time distribution of the complete population. I therefore recommend comparing these distributions with activation statistics calculated from the complete population of activation events. Such a sensitivity test would establish whether the magnitude and ordering of the reported aerosol-dependent activation delay are robust to the particle-selection procedure.

   More generally, the manuscript should clearly identify which figures and statistics use the final-time-conditioned trajectory population and which use the complete particle population. Sect. 3.4 appears to use a different population: the authors state that the mixing/deactivation analysis includes "all super-droplets that experience subsaturated exposure" and is performed "across the whole population without reference to the pre-assigned clusters". If this interpretation is correct, the distinction between these two sampling approaches should be made explicit when the results are discussed.

<u>**Other comments**</u>

1. I found the **opening paragraph vague and somewhat awkwardly worded**. In particular, the statement that warm-rain production "occurs at or near the transition between precipitating and non-precipitating conditions" is unclear and does not effectively motivate the scientific problem addressed in the manuscript. I suggest rewriting the opening paragraph to state more directly the unresolved question concerning aerosol effects on cloud-droplet activation and subsequent growth.

2. **The Introduction would benefit from substantial reorganization and shortening.** In the Introduction the scientific motivation moves between precipitation formation, drop activation, thermodynamic heterogeneity, Lagrangian modeling, and radar forward simulation without a sufficiently clear progression toward the central question. I suggest reorganizing these concepts so that they follow the physical sequence more naturally: aerosol properties and supersaturation to activation to condensational growth and spectral evolution to possible transition to collision–coalescence and precipitation.

3. **In the Introduction, the motivation for the two "additional ingredients" required by the Lagrangian framework is unclear.** The authors write that trajectory-resolved thermodynamic histories make the Lagrangian framework naturally suited to the problem, but then state: "Realizing this potential in practice, however, requires two additional ingredients." I do not see why either is intrinsically required to realize the benefits of Lagrangian particle tracking. The trajectory framework could be used directly to investigate activation timing and subsequent growth without RH clustering, and a radar forward simulator. The authors should explain why these two ingredients are necessary for the scientific question posed here rather than presenting them as general requirements of the Lagrangian approach.

4. The statement that forward modeling avoids having to **"force observations into model variables that the instrument does not directly measure"** is awkward and somewhat pedestrian. A more common terminology is "retrieving geophysical quantities from observables can carry appreciable uncertainty".

5. **The Introduction currently spends considerable space summarizing the Methods before the central scientific hypothesis is sufficiently clear.** In my view, the strongest motivation is only presented later in the manuscript: "in a short-lived shallow cumulus, aerosol-dependent differences in activation timing may consume a substantial fraction of the finite time available for subsequent droplet growth". Centering the Introduction around this problem would provide a much clearer foundation for the rest of the manuscript.

6. **Please clarify why exact Mie scattering is emphasized and distinguish the cloud-droplet and precipitation regimes.** Using Mie calculations for the full particle population is reasonable because the simulation ultimately produces drizzle/rain particles. However, the small particles relevant to cloud-droplet activation are deep in the Rayleigh regime at 35 GHz. The manuscript should make this distinction explicit, particularly because much of the scientific discussion concerns droplet activation.

7. **The radar sampling geometry requires a clearer description.** The native sampling volume is described as a 20-m grid cell, while spectra are calculated after aggregating 3 × 3 horizontal cells into 60 × 60 m columns and collecting particles over a ±10 m vertical interval. Is this intended to approximate a particular radar sampling volume or is it simply a convenient model averaging volume?

8. **Figure 2 does not allow the frequency of occurrence of trajectories to be assessed.** Panels (a) and (b) are heavily overplotted scatter diagrams. The caption states that each point represents a trajectory and that the scatter representation was retained specifically to expose rare rapid-growth trajectories and the upper envelope. The upper envelope may indeed be visible, but the number or density of trajectories occupying different portions of the distribution is not. Consequently, statements such as "fewer rapid-growth trajectories are available" in T73 cannot be quantitatively determined from the present visualization. I suggest replacing these plots with density distributions or quantile bins. The authors should also clarify whether every analyzed trajectory is displayed and whether/how super-droplet multiplicity enters these panels.

9. **"Rapid-growth" or "fast" trajectories should be quantitatively defined.** At present this appears to be inferred from the upper envelope rather than from a stated criterion.

10. **In Fig. 3, I find the statement that "the separation between the clusters is striking" substantially stronger than the figure demonstrates.** Some RH separation is apparent at the beginning of the simulation, but the curves and their envelopes become much closer in the later stage of the simulation and the ordering is not consistent across variables. In RH, Clusters 1 and 3 appear relatively similar during portions of the evolution, as do Clusters 2 and 4. In radius, by contrast, Clusters 1 and 2 are much more similar to each other, while Clusters 3 and 4 form the smaller-radius group. The ±1σ ranges

are also appreciable. This behavior should be noted and discussed.

11. **The description of Cluster 2 as simply "falling in between" the other clusters does not appear consistent with Fig. 3b.** Clusters 1 and 2 are nearly overlapping for much of the later simulation, and Cluster 2 appears comparable to, and perhaps slightly larger than, Cluster 1 near the end. Given the spread, I would not infer a statistically meaningful ordering between them without quantitative support.

12. I agree that Cluster 4 is generally drier, but I do not see clear evidence that it **"relaxes more slowly toward saturation."** Can the authors clarify that they mean by this statement.

13. **Please clarify exactly what the "activated subset" in Fig. 4 represents.** The Methods define activation time as the *first* crossing of the particle-specific critical radius. Elsewhere, however, the manuscript discusses deactivation and reactivation. It is therefore essential to know whether the dashed curves represent: particles currently above r_{crit}; particles that have crossed r_{crit} at least once; or particles that have remained continuously above r_{crit}. These definitions are physically different.

14. **I have difficulty understanding how particles with wet diameters above approximately 20 µm remain classified as "subcritical."** Please show the corresponding range of particle-specific critical radii and clarify how particles reaching these wet sizes remain outside the activated population.

15. **The statement that the three cases "remain nearly indistinguishable" in the collector-drop range at $t = 1000$s is ambiguous.** Does this refer to the full spectra or the activated spectra? The full spectra clearly exhibit aerosol-dependent differences over parts of the size distribution, while the activated collector tails are difficult to distinguish visually. Please specify which curves support this conclusion.

16. **The terminology "bimodal" appears stronger than what Fig. 4 shows.** I see a dominant mode tapering toward larger sizes, with a shoulder or secondary enhancement/peak in some cases, rather than two clearly separated modes.

17. **I do not understand the interpretation of the "early mode" of the T13 activation-time PDF in terms of reactivation.** The Methods define τ_{act} as the time of the first crossing of r_{crit}. Yet the discussion states that the early mode includes both first-time activation and later reactivations. If Fig. 5 is a PDF of first-activation times, subsequent reactivation events cannot contribute additional events to that PDF. If repeated r_{crit} crossings are included, then Fig. 5 is not showing the quantity defined in Sect. 2.3. This needs clarification.

18. **The description of the activation-time distribution with increasing dilution appears internally inconsistent.** The authors state that "the tails of the activation-time distribution broaden with increasing dilution." Based on the cluster definitions, increasing dilution should correspond to progression toward Cluster 4, the entrained-shell-like and most diluted regime. I would therefore interpret this statement to mean that Cluster 4 should exhibit the broadest activation-time distribution. However, the very next sentence states that the shell cluster "activates over a narrow, concentrated window," while the core and transition clusters retain broader early- and late-time tails. These descriptions imply opposite relationships between dilution and the width of the activation-time distribution. Figure 5 also does not show an obvious monotonic broadening from the core-like to the shell-like clusters. Please clarify what is meant by "broaden with increasing dilution" and revise the interpretation accordingly.

19. **I do not understand the statement that lower activation supersaturation "implies smaller activated droplet sizes."** Lower critical supersaturation does not, by itself, imply a smaller critical wet radius; for a given composition, larger dry particles generally have lower critical supersaturations and

larger critical radii. If the authors instead mean that weaker supersaturation limits subsequent condensational growth and therefore produces smaller droplets after activation, this should be stated explicitly.

20. **The description of T73 as having "little contribution from the precipitation mode" seems too strong.** T73 clearly contains falling hydrometeors and a measurable precipitation signature. Table 5 gives strongly negative V_D, including $-2.37\ m\ s^{-1}$ at 600 m and $-1.97\ m\ s^{-1}$ at 1000 m. The distinction therefore appears to be a reduced precipitation-mode contribution relative to T13, rather than little or no precipitation contribution. The wording should be revised.

21. **I find Fig. 9 difficult to interpret in its current form.** Each panel contains many individual-column spectra, a selected central-column spectrum, and a domain-average spectrum. These represent different sampling concepts and the large number of overlapping curves makes the inter-case comparison difficult. Please clarify which curve is intended to approximate an actual vertically pointing radar observation and which quantities are model/domain diagnostics.

22. **Sect. 3.5.2 should be reorganized and retitled.** "Decomposition into cloud and drizzle components" does not accurately describe the entire subsection. The text moves from decomposition to Fig. 9 spectra, then to Z_e, V_D, and W in Table 5, then back to spectral shape and skewness. I also recommend organizing the discussion so that each analysis is introduced and interpreted together with the figure/table that supports it.