Review of “Anthropogenic aerosol influence on a mixed-phase cloud precipitation in early Meiyu season over Yangtze River Delta: simulated microphysical and thermodynamic effects” by Song et al. [Research Article, egusphere-2025-43]

This work explored the influence of aerosols on deep convection clouds and precipitation by simulating a 15-hr mixed-phase cloud precipitation event in South China during the Meiyun season using the WRF-Chem model. A series of sensitivity experiments were conducted by varying anthropogenic emissions. Their results suggest that under low aerosol emission conditions, increasing aerosols tend to enhance convection and precipitation through convective invigoration, whereas under high emission conditions, more aerosols suppress convection precipitation. This suppression is due to increased cloud droplets and reduced droplet sizes, which inhibit warm rain via microphysical effects and weaken cold rain processes by enhancing evaporative cooling. The authors also discussed the aerosol-induced changes in precipitation pattern.

The research explored is interesting and important, and it might help further our understanding of aerosol invigoration mechanisms. However, I find the manuscript lacking in several key aspects. The overall organization and the logical flow of the manuscript are weak, and sentence-level coherence is poor, making the manuscript difficult to follow. I am also concerned with the novelty of this work, as the main results of deep convection responses to aerosols under different pollution levels are already well documented in previous studies, yet the authors did not clearly articulate their unique contribution in the manuscript. Moreover, the manuscript aims to address ACI problems through modeling, but how aerosol and microphysical processes are represented in the model is not discussed in detail or even not introduced. The validation of the baseline simulation and process-level analysis of the results also needs further improvement. Given these issues, I believe this manuscript requires substantial revision before it can be considered for publication in ACP. I hope the following specific comments can help the authors improve the manuscript.

1. The overall writing is disappointing and requires significant improvement, particularly regarding organization, sentence structure, and comma usage. For example, the Introduction lacks a coherent structure and reads more like a pile-up of previous studies than a logically developed narrative. The key points of each paragraph are unclear, and the research gaps are not identified, making the motivation of the study and its unique contribution remain ambiguous. Additionally, an extended discussion of the synoptic background is included in Section 3.1, which would be better placed in Section 2 as part of the observational case description (see comment 2). In the Conclusion, it would also be very helpful if the authors give a general background of this study before listing specific findings.

Another major concern is the wide use of loosely constructed sentences throughout the manuscript. Many sentences throughout the manuscript are overly wordy, logically unclear, and uninformative (e.g., L31-36 and L56-59). Additionally, comma usage is inconsistent. The Oxford comma (e.g., “0.1, 0.2, 0.5, 2, 8, 20, 30, and 50”) and the AP style comma (e.g., “A0.1, A0.2 and A0.5”) are interchangeably used by the authors. Abbreviations should also be used with care. The acronym MEIC seems to be defined twice. Some typos and grammar errors also need to be corrected. I strongly encourage the authors to significantly revise the manuscript regarding these issues to enhance its clarity and presentation.

2. I recommend that the authors provide a more detailed description of the selected rainfall event/case in Section 2, including its synoptic background and diurnal cycle, followed by the model setup of baseline simulations. This background can help understand the rationale behind these setups and representativeness of the simulation. The experiment design should also be moved to Section 2, which can give a full picture of how this study's simulations look like.

Since this study focuses on ACI, a detailed description of aerosol and microphysics schemes used are necessary and important, which, however, are not included in Section 2. Therefore, we are not sure how aerosols are represented and activated in the model and how warm-rain and cold-rain microphysical processes are parameterized. To reduce the impacts of large-scale meteorology on the simulation results, it is encouraging to see the authors nudged the model toward the observations. But the authors need to clarify what meteorological variables (e.g., U, V, T, Q, etc) have been nudged and what nudging time scale is used.

3. Given that the baseline simulation forms the basis for the sensitivity experiments, its validation is critical for ensuring the credibility of the study's findings and the physical interpretation of aerosol impacts on deep convective precipitation. However, the current validation is not sufficient. For example, the spatial pattern of precipitation was not compared against the observation. Only the regional mean precipitation was compared. Furthermore, the authors found the model tends to simulate an earlier onset of precipitation relative to observations, but the potential reasons were not discussed.

4. Several aspects regarding the result discussion require clarification. For example, when presenting area-averaged rainfall, is the averaging performed over the entire inner domain, or limited to the convective core? Similarly, when referring to the *entire layer*, does this indicate the full model depth or just the PBL? In addition, when discussing the dispersion or clustering of precipitation, it would be helpful to quantify the precipitation area. For instance, defining the area where precipitation exceeds a certain intensity threshold could support a quantitative analysis of spatial changes in precipitation coverage.

5. The interpretation of results is closely related to two underlying mechanisms of convective invigoration, yet the manuscript lacks sufficient discussion of these: (1) *warm-phase invigoration*, driven by enhanced condensational heating due to more efficient condensation, and (2) *cold-phase invigoration*, resulting from increased latent heat release associated with ice processes such as droplet freezing, riming, and deposition. The warm-phase invigoration is more plausible and theoretically supported, while the cold-phase invigoration depends on the assumptions of instantaneous freezing and unloading of condensate in growing (Varble et al., 2023). In the current version, The authors generally argued that under low emission conditions increasing aerosols enhance deep convection via latent heat release due to condensation/desublimation of water vapor. However, enhanced latent heat release at high altitudes do not necessarily lead to stronger convection, as it might be cancelled out by the greater condensate loading. More evidence is

therefore needed to help support the invigoration mechanisms. Analysis of changes in supersaturation, buoyancy fluxes, and vertical velocity in the convective core (rather than domain-average) would be helpful to understand both warm-phase and cold-phase invigoration processes. The current area-averaged vertical velocity (Figure 5) does not appear to change significantly with varying emissions, particularly at low altitudes. The invigoration signal from the convective core may be more pronounced.

Under high emission conditions, the author argued that the negative response of deep convection to aerosol is partly related to enhanced evaporative cooling due to more cloud droplets produced. Could the authors examine the vertical profiles of cooling rates due to droplet evaporation? Such diagnostics would help substantiate this proposed mechanism.

6. The authors examined the advection tendencies of cloud water and ice to understand regional variations in precipitation. I was wondering if the authors could examine the full budgets of cloud water and ice, particularly microphysical terms in addition to advection terms. This can help determine the local production of cloud water or ice.

7. The comparison of the results with previous studies is superficial. The authors should clarify the specific synoptic conditions and pollution regimes under which their findings align or differ from earlier work and provide a more in-depth discussion on how to reconcile any inconsistencies. Also, the robustness of the results remains uncertain, as they may be sensitive to meteorological conditions, aerosol loading, model configurations, and physical parameterization schemes. These limitations should be discussed and acknowledged.

Reference:

Varble, A. C., Igel, A. L., Morrison, H., Grabowski, W. W., & Lebo, Z. J. (2023). Opinion: A critical evaluation of the evidence for aerosol invigoration of deep convection. *Atmospheric Chemistry and Physics*, *23*(21), 13791–13808. https://doi.org/10.5194/acp-23-13791-2023