Reply to second review of Reviewer #1

We thank Reviewer #1 for going once more through our paper and bringing up these comments, which certainly improve the clarity of our paper. A revised track-changes version of the manuscript is available, with the latest new additions in blue text. In the following, we reproduce the comments by Reviewer #1, with our point-by-point reply in blue-italics font:

Review of Revisions to egusphere-2025-5149

Summary: In this manuscript, the authors assess aerosol impacts on convective clouds in the Houston, Texas area using simulations of two case studies during the TRACER field campaign. They present a thermal-centric assessment of aerosol microphysical effects, which is a nice and novel way to look at understanding what is happening within the clouds. They also look at dynamical, mesoscale feedbacks, with a focus on low-level convergence (0-5 km). The thermal-centric view contributes to our understanding of aerosol-cloud interactions, and their assessment of dynamical feedbacks adds to the extensive literature that dynamical feedbacks are both important and very complex with respect to understanding of aerosol impacts on deep convective clouds. I appreciate the work the authors have done with additional analyses to glean additional insights on the dynamical feedbacks, though some of these analyses seem somewhat rushed in terms of sufficient explanations of how the analysis is conducted and how it relates back to the bigger picture. As such, I have a few additional questions that should be addressed to provide the reader with enough information to understand what is being presented.

Thank you for this overall assessment. Indeed, we think that dealing with the comments below make the section on mesoscale feedbacks more thorough and clear.

Minor comments:

L229: Typo: missing the word Table?
Yes, thank you for pointing this out.

L177: Typo: 25-dBZ should be 35-dBZ?
Yes, thank you for pointing this out.

Figure 14b,c,e,f: Consider changing the y-axis limits, since it is very difficult to see differences and most of the panels have no data on them.

What this figure shows is actually that the thermal and convective energetics counts are broadly consistent, with no large systematic differences. Therefore, if the differences are difficult to see visually, that is itself an important result: the energetics are not substantially different between the cases. If we zoom into these plots, we would mislead the reader making these small differences appear larger.

L482: The authors state that aerosols were re-initialized at 22:00 UTC on August 7th but do not mention when they are initialized for the other case on August 4th? Was it at the same time of day or a different time? This information should be included as it impacts what times they choose for their analyses for the different case studies.

August 4th case was reinitialized at 23:30Z. We have added this to the revised manuscript (line 482)

L491: Why do the authors only assess 1-hour of simulation data from the August 4th case but 2 hours from the August 7th case?

Precipitation difference only occurs during the 1hour time window (from 23:30Z to 00:30Z) on the August 4th case (see Figure 10). We have added text to clarify this (line 492-493)

L525: The authors choose to focus on a subdomain to focus on mesoscale feedbacks. However, the specific area that they choose seems to overlap well with their low-level convergence for their polluted case (Fig. 12f), but not their clean case (Fig. 12e). Could they add a box on Figures 12 and 13 to show where their focused subdomain analyses area is? This will make it more clear to the reader what features are being represented in the subdomain analysis of Figures 17 and 18.

The subdomain box has been added to Figures 12 and 13 (please see revised manuscript). In the August 4 clean case, the LLC group partly overlaps the boundary of this subdomain. However, the main purpose of this diagnostic is not to define a strict spatial separation, but to examine why convection becomes more clustered in the polluted case during the subsequent period.

L449: Do the authors normalize (e.g., remove the mean) of their LLC data before conducting the Fourier analysis?

This code does not explicitly remove the spatial mean before the Fourier analysis. In fact, the spatial mean was intentionally retained in the Fourier analysis because the objective was to quantify the total scale-dependent energetic signal, including both domain-wide forcing and spatial variability. Technically, the zero-wavenumber component represents the domain-mean convergence contribution, which is physically meaningful for comparing clean and polluted simulations. Removing the mean would isolate only perturbation-scale variability, but it would also discard any bulk shift in convergence strength between the two experiments.

L525: Typo: Should the reference for Figures 15 and 16 be for Figures 17 and 18?

Actually, it should be Figures 12 and 13 (LLC maps shows convective cluster). We have fixed this in the revised manuscript (line 526)

L552: How is the local background temperature specifically calculated? The authors state it is estimated over a broad area, but it is unclear what this means?

The local background temperature is estimated from a broad local neighborhood around each grid point. In this code, the 0–500 m potential temperature field is smoothed using an approximately 20-km running window, based on the 250-m grid spacing. Pixels that are more than 1 K colder than the initial smoothed field are excluded so that cold-pool anomalies do not contaminate the background estimate.

L556: Is the cold pool analysis on the subdomain region or on the entire domain?

The cold-pool intensity was calculated over the entire model domain, but this specific diagnostic is restricted to the focused subdomain, defined as 29.8–30.2°N and 96.0–95.25°W. This subdomain was selected because intense convective clusters are simulated within this region during the analysis period. We now specify this in the revised manuscript (captions on Figs. 17, 18, 19).