Review for egusphere-2025-5149 titled “Aerosol-deep convection interactions based on joint cell-thermal tracking in Large Eddy Simulations during the TRACER campaign”

The authors use thermal tracking within large-eddy simulations of isolated deep convection for the DOE TRACER field campaign near Houston with an IOP in summer 2022. The authors employ a thermal tracking approach to “investigate the response of cold- and warm-phase microphysics and electrification processes within thermals to varying aerosol loading conditions during isolated deep convective events”. They simulate isolated convection for 2 dates in August using observed aerosol measurements from the campaign to address the aerosol impacts on cloud microphysics and convection aggregation.

This is a well-executed study that addresses key shortcomings in existing literature w.r.t. aerosol effects on deep convective cloud microphysics. The results are easy to follow, and the manuscript presents insights into aerosol-cloud microphysics interactions that can be built upon in future work. I recommend this manuscript be published after minor revisions are made to address the comments below.

We thank this reviewer for this positive overall summary of the manuscript. We will now respond to each comment one by one (blue italics font). A track-changes version of the manuscript is also available, with all additions in blue, and supplementary material at the end.

I have a general question related to the applicability of the results to other cases from TRACER. Two dates are selected and there are some distinct patterns in convective activity between these dates already. There is also a lack of discussion of the observed convection on these dates (from NEXRAD or ARM radar scans) and the observed aerosol concentrations. Aspects related to convection aggregation can be placed in contrast with observations.

We agree with the reviewer that investigating the representativity of these two cases and comparing the simulated convection to the observed would be a very interesting point. However, a rigorous comparison to observations would require an extensive analysis, which goes beyond the manuscript limit unfortunately. However, we have significantly extended section 3.2, including more analyses and discussion on the mesoscale feedback that develop towards the end of our simulations, which is also related to the aggregation of convection. Also, since we investigate two extreme aerosol concentrations when *simulating these two dates, we do not necessarily expect them to represent the observed convection, and hence a direct comparison would require more simulations.*

Given that thermals were tracked, the lifecycle evolution of the thermals is missing. See related comment below regarding the possibility of creating an additional figure similar to Figure 6 where multiple lines represent clean, polluted, and/or clean-polluted cases where each line represents the vertical profile of key thermal properties as a function of the normalized thermal lifetime. This is somewhat done with the full-domain time series and the composites for different altitude levels but those seem indirect ways of representing the thermal evolution when a better methodology is available to the authors given the tobac tracking.

See responses below regarding the lifecycle evolution of thermals (comment regarding line 341), and the following one (regarding line 362).

Minor Comments:

Abstract: The abstract is hard to follow and lacks clarity from Line 10 onward. I suggest re-wording it based on the following comments:

- Line 11: “higher” and “faster” in this context must be clarified. Does “higher” refer to a thermal’s base altitude? Can you quantify the aerosol impacts as a relative change from cases with low aerosol concentrations?
- Line 12: This sentence needs to be rephrased.
- Line 13: Please specify the feedback.
- Line 14: “aerosol-reinitialization”?
- Line 15: Do you mean smaller spatial scale? Or shorter time scale?
- The abstract should mention that two case dates chosen for detailed analysis form the basis of these results.

*We have adjusted the abstract to incorporate all these comments, while keeping to the 250 word limit (see **lines 1-15**).*

Line 24: Suggest re-wording this sentence to be more specific.

*We have reworded this sentence to (see **lines 23-24**):*

"The former process (i.e., impact of droplet nucleation and condensation) is called warm-phase invigoration, whereas the latter process (i.e., droplet freezing in the mixed-phase layer) is called cold-phase invigoration (Rosenfeld et al., 2008)."

Line 52: Suggest specifying for what is thermal size the most relevant determining factor

*This sentence refers to entrainment rate. According to Hernandez-Deckers and Sherwood (2018), thermal size is the most determining factor for entrainment rate. We have re-worded this to make it more clear. (see **lines 52-53**)*

Line 100: Are these daily percentiles or for the entire period? Please specify either way. If latter, please provide the aerosol concentration values for each percentile.

*We have rephrased this part to make this more clear (see **lines 100-102**):*

"Two aerosol regimes are defined using percentiles calculated over the entire period from 17 June to 07 August 2022. Periods with aerosol concentrations below the 10th percentile (1181 cm^{-3}) are classified as clean, while periods with concentrations above the 90th percentile (9769 cm^{-3}) are classified as polluted."

Line 104: Does either the reference or the current study have a size range for each of the three aerosol modes? Can the ranges be provided here or in the table?

In this study, following the aerosol size distribution measurements, the aerosol modes are represented using lognormal distributions characterized by the median diameter (Dm) and geometric standard deviation (σ), rather than fixed size ranges. Please refer to Table 1 for the values. However, the three modes broadly correspond to the conventional definitions: nucleation mode (< ~30 nm), Aitken mode (~30–100 nm), and accumulation mode (> ~100 nm). We now also provide Figure S1 in the supplementary material, where the aerosol size distributions and their fit to the data are shown.

Line 129: Do the authors have any comment on why no PBL scheme was used and whether that might have an impact on the subsequent analyses?

Following Dudhia (2021), a PBL scheme was used for simulations with grid spacings of 3 and 1 km, while simulations with grid spacings of 200 m did not use a PBL scheme.

Typical (non-scale-aware) PBL schemes assume that PBL eddies are not resolved in the model grid structure and the PBL schemes handle subgrid-scale vertical fluxes (including dry thermals). However, this assumption breaks down as the horizontal grid spacing becomes finer than 1 km. At such fine grid spacings, a large eddy simulation (LES) configuration is recommended. In this configuration, the PBL schemes are turned off, and a three-dimensional turbulent kinetic energy (TKE) subgrid mixing scheme is turned on to resolve the remaining subgrid mixing. Dudhia (2021) provided an excellent review of PBL and turbulence/diffusion schemes in the WRF model for simulations at the "PBL gray zone" scale. Research or reviews about the PBL gray zone can be found in Shin et al. (2019), Juliano et al. (2022), and Hope et al. (2024), for example.

*Regarding the impact on subsequent analyses, we think that the discussion on model performance in representing eddies and turbulence in the PBL is beyond the scope of this study, although it may affect convection initiation in PBL. This topic will be addressed in future studies. Nevertheless, we have added the following lines to support why we do not use a PBL scheme (see **lines 132-135**):*

"The level-2.5 Mellor-Yamada-Nakanishi-Niino turbulence scheme (Nakanishi and Niino, 2006, 2009) was used as the planetary boundary layer (PBL) parameterization scheme for simulations with grid spacings of 3 and 1 km. Following the guidance in Dudhia (2021), simulations with grid spacings of 200 m did not use a PBL scheme and a three-dimensional turbulent kinetic energy (TKE) subgrid mixing scheme was activated to resolve the remaining subgrid mixing."

Line 170: Can you provide the rationale for removing "anomalous" cells as defined here? The exclusion of aggregated cells based on "number of neighboring cells" warrants further clarity – was a distance threshold used? What was it? Did a cell have to have a certain number of cells within a threshold distance to be termed "aggregated" ?

The maximum number of features within a 5-km radius is set to one in order to identify isolated cells. Under this criterion, if multiple cells exceeding the 35-dBZ threshold are located within 5 km of one another, they are treated as a single isolated cell.

Line 206: Can you expand on what is meant by "careful investigation of these results"? Based on the current text, it seems the implication is that these dates saw a high (highest?) number of isolated cases compared to other days and the forecasting skill score was thus high?

*We have added the following sentence (**lines 212-214**).*

"Performance-check LESs were conducted for several promising candidate cases, and a visual comparison with NEXRAD observations (not shown) led to the selection of two representative cases."

Figure 2: The red contour lines don't add much to the panels but make them busy and cluttered, I suggest removing the contour lines or keeping them only in one panel.

Please see the new version of Fig. 2. We have adjusted the line widths and transparency of the red lines so that they would not obstruct the other lines and color contours. Because the data for the red lines is very high resolution, the line width appearance and color contrast in the vector format might be largely affected by the PDF generation system in the Copernicus publication compared to the original PDF plot file. We may adjust the properties of the lines further at the production stage.

Figure 3: Panel (a) shows tracked cells, their size, and lifecycle in a very inefficient manner. I see little utility in panel (a) in its current form for the following reasons:

- Why are squares used to represent cell size when tobac can provide polygons based on the cell mask representing cell area from the segmentation step?
- Plotting all cells overlapped on top of each other presents no use. Perhaps it shows only cell 3 from the left cluster and a single cell of interest from the right cluster? Or even showing multiple panels would be better?
- Cell locations can be better represented by lines showing the cell tracks or just translucent, partly overlapping polygons colored by lifecycle stage.
- There is virtually no way to distinguish which cell is Cell 3 within that cluster despite knowing that it is meant to be the largest one.

Figure 3: Panel (b) is a very nice and useful figure. I suggest making this a standalone panel given the issues with panel (a). A minor suggestion – is there a way to distinguish between thermals tracked from near cloud base versus thermals only identified at some height above cloud base (since this is also mentioned explicitly in the text and seems an important distinction).

- There are many instances of overlapping lines in this panel. Does that mean that there were multiple thermals at the same altitude at the same time, but at a different horizontal location?

- If the authors can think of a good way to represent these thermals visually in a 3D field, that would be very useful. Alternatively, a statistical time series of the number of thermals and their relative distance could be added.
- It seems there is some correlation between the mass flux values and the base of the corresponding thermals. Darker red lines seem to start at higher altitudes. Do the authors have any comment on this? The high mass flux values likely represent high upward vertical velocity values or are they related to the thermal sizes? Could it be a tracking issue/artefact?

Thank you for pointing this out. We have significantly improved this figure (Fig. 4 in the new manuscript). First, we now show only one isolated cell on the left panel, which is the one where we plot the time-height trajectories of thermals from in the right panel. Also, we now show the exact contour of the cell, as opposed to the rectangle we had before. This makes it much clearer. Furthermore, we corrected the way we computed mass flux, which should be in kg $m^{-2}s^{-1}$ *instead of kg m* s^{-1} *(see comment from other reviewer).*

Regarding the suggestion to investigate deeper into the relation between mass flux, starting altitude, vertical velocity, etc., these relations can be approximately inferred from Figure 7, which shows profiles of various quantities. However, our main aim in this study is to investigate the impact of aerosols, and the features suggested here would be more fundamental, irrespective of different aerosol concentrations. Although this would be interesting on its own, it would lengthen the manuscript too much (we already have 20 figures!). We may consider these useful suggestions for a future study more centered on fundamental thermal behavior.

Line 261: Mention section where the discussion resumes for the vertical velocity differences.

We actually do not come back to this, since it would require shifting the focus significantly, and the length of the manuscript would not allow that. We have removed this sentence.

Line 274: Do you mean “polluted” case instead of “clean”?

We have rephrased this sentence to make it clearer (see ***lines 296-298****):*

“Electric potential shows the integrated effect of charge, with a subtle maxima in the clean case above and between the thermal’s internal circulation, and a much greater enhancement in the polluted case extending to ice crystals laterally outside and above the thermal.”

Figure 6: I admit there are already a lot of panels here but adding a panel on temperature or markers for the freezing level in one of the mixing ratio panels will provide further context on the hydrometeor phase.

*We have added the 0°C level to these plots (see **Fig. 7**).*

Line 341: Given that the thermals were tracked in both space and time using tobac, and their lifecycles can be estimated based on the tracking times, is it not possible to create vertical composites such as those in Figure 6 where the difference between the clean and polluted cases is plotted for all thermal properties as a function of the normalized thermal lifetimes? The shading or thickness of lines could change as the lifetime increased, and that would demonstrate the lifecycle evolution of the thermal properties.

We have done this type of composites in the past. However, due to the fact that the vast majority of thermals have short lifetimes, their properties change very little, so these composites show very weak and noisy signals (e.g., Fig. 11d,e,f in Hernandez-Deckers & Sherwood, 2016). That is why it turns out to be much more informative to do these composites as a function of height, for instance (as in Figures 7 & 8). Trying something like this with cross-section composites would require a large effort which we believe would not add much new information to the current manuscript.

Line 362: Again, since thermals and the convective cores are tracked and their spatial coverage easily identified, I am wondering why full-domain-means are used in Figure 9, would it not be more useful to set some distance threshold beyond the cloud boundaries to consider the environmental conditions? This is especially important for the hypothesis regarding the impact of droplet/rain evaporation rates on dQv and dT.

Thanks, we initially intended to do your suggested analysis, but we realized that evaporation-driven sensitivities occur at earlier stages and even smaller cells that are not captured by the cell-tracking analysis here. Therefore, examining the full domain is necessary to properly characterize aerosol–cloud feedback. In fact, the full-domain analysis shows clear sensitivities of aerosol to microphysics and environmental conditions.

Line 370: I'm surprised to see little discussion of the positive trends in dQr, dQis, and dQgh in the last few hours of each date's simulations. The reinitialization experiments later address how changing CCN/aerosol concentration impacts this trend but does not comment on why its observed in the first place.

Thank you for this comment. These are definitely important. We have incorporated more discussion related to the last-few-hour changes in dQr, dQis, and dQgh (see ***lines (397-401)****):*

"This period also exhibits clear increases in graupel and hail (dQgh) as well as ice and snow (dQis), suggesting enhanced cold-precipitation processes and detrained anvils in the polluted case (Fig. 10d,j). Typically, a greater amount of Qc, associated with an increasing number of cloud droplets, tends to suppress warmrain production (Fig. 7 and as discussed in DH22). However, a clear sign reversal of dQr is associated with stronger feedback in the system, as discussed and analyzed later in this section."

Technical corrections/suggestions:

Line 26: "enhanced aerosol concentrations"?

Fixed

Line 283: maintain consistency in terminology? Initiation height in text versus starting height in figure caption.

Fixed

Line 376: "fate" -> "rate"?

Fixed

References

Dudhia, J. (2021). Overview of WRF Physics: Boundary Layer and Turbulence [PDF]. NCAR/MMM2, National Center for Atmospheric Research. https://www2.mmm.ucar.edu/wrf/users/tutorial/presentation_pdfs/202101/dudhia_physics_pbl_turbulence.pdf

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Juliano, T. W., B. Kosović, P. A. Jiménez, M. Eghdami, S. E. Haupt, and A. Martilli, 2022: "Gray Zone" Simulations Using a Three-Dimensional Planetary Boundary Layer

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