Responses to Reviewer #1

“Tracking the Impact of Urban Air Masses on Convective Precipitation: A Multi-Member Modeling Study”

by Keil et al.

First of all, we thank the editor and reviewers for their thorough evaluation and constructive feedback on our manuscript. We have carefully addressed all comments and believe the revisions have substantially strengthened the paper. Below, we provide detailed point-by-point responses to each comment. Our responses are shown in blue text, and corresponding changes or additions to the revised manuscript are presented in *gray italic*.

Reviewer #1

This study uses the coupled COSMO-DCEP-MUSCAT modeling system to investigate the impact of urban aerosols on deep convective clouds. Two case studies in the area of the German city of Leipzig are conducted. For each case, a five-member ensemble with different initial conditions is produced. To isolate the impact of urban aerosols, each ensemble is run with either all emissions or no urban emissions (but all other emissions). The authors find that the aerosol effects are not uniform between the two cases and the same pollution source can have different effects depending on the convective system. Overall, I think the design of this study is strong and shows a clear vision by the authors on how to conduct a study trying to investigate aerosol impacts on deep convection. The approach using chemistry cloud coupling, an urban parameterization, an ensemble with multiple initial and boundary conditions, and tracking of the air mass are a strong foundation, which leads me to believe that this study can be a good addition to the literature and suitable for publication in ACP.

We thank the reviewer for the thorough evaluation of our manuscript and the positive assessment of our methodological approach. The reviewer's constructive feedback has helped us strengthen the manuscript, particularly regarding the clarification of our methodology, the interpretation of modest aerosol signals, and the discussion of case-specific responses. We have carefully addressed all comments and believe the revisions have substantially improved the clarity and scientific rigor of our work.

However, in my opinion there is a major flaw. In the introduction the authors correctly state “the influence of urban aerosols is more pronounced in less industrialized regions, where lower background aerosol concentrations amplify their impact on convection and precipitation processes”. Then later they describe that “The [study] region is characterized by a mix of urban and industrial emission sources and an overall relatively high background aerosol concentration.” In effect this leads to minimal differences between the simulations in terms of their aerosol loading. The differences are only around a few percent as shown by Figure 2. Many previous studies have used much more significant aerosol differences often with factors between 2 and 10.

The reviewer correctly identifies that our aerosol perturbations are modest compared to many previous studies. However, assessing whether such urban emission perturbations produce detectable convective responses was our central research question. While our introduction notes that urban effects are more pronounced in cleaner regions, the Leipzig case represents a more challenging and realistic test case for a region with relatively high background aerosol concentrations. Whether realistic urban perturbations remain detectable above model noise in high-background aerosol environments was previously unknown and represents an important finding. Additionally, these realistic emission scenarios are particularly relevant for future cleaner cities, where urban aerosol perturbations are expected to be of comparable magnitude. The key methodological advance of this study is demonstrating that urban aerosol signals are detectable even under such small perturbation conditions when using our trajectory-based ensemble approach. A systematic investigation of emission scaling (including significantly larger perturbations) is the focus of our ongoing follow-up study, which we plan to submit at ACP in the next weeks. Preliminary results of the comprehensive sensitivity study (which is of such extent that its incorporation into this study is impractical) demonstrate a more profound impact of highly elevated emissions.

Given these minimal differences in the aerosol loading, I am not fully convinced that the results the authors show are attributable to aerosol effects. Figures 4-6 and 9 suggest a spatial redistribution that mostly averages out. Only the vertical profiles seemingly show a more systematic aerosol effect.

We are aware that the differences in accumulated precipitation and the time series are small. This is precisely why we conducted the vertical microphysical analysis. The vertical profiles reveal more pronounced changes in cloud structure and microphysics that are not immediately apparent in the time series. This vertical reorganization in cloud microphysics ultimately affects both the location and intensity of maximum precipitation

Furthermore, I do not really see much evidence for the statement "The same air pollution source can either delay, enhance or suppress convection [...]". Figures 6 and 9 show barely any difference between the aerosol setups, that would allow for such a statement. Please make this statement more accurate.

We agree with the reviewer. We have removed this statement from the abstract, as the observed differences do not support such a broad conclusion.

In my opinion, the authors can consider two things to address this concern. First, I suggest showing that there is no systematic difference in other convective parameters along the trajectory that might have influenced the convective evolution, i.e., make a figure similar to Figure 6 that looks at the evolution of CAPE for example. If there is no difference it would increase my confidence that changes are attributable to aerosol indirect effects.

We thank the reviewer for this suggestion. We have examined CAPE evolution along the trajectories and find no systematic differences in the ensemble means between BASE and NOURBAN scenarios (see Figure 1). This supports our interpretation that the observed hydrometeor changes are attributable to aerosol indirect effects rather than differences in the thermodynamic environment.

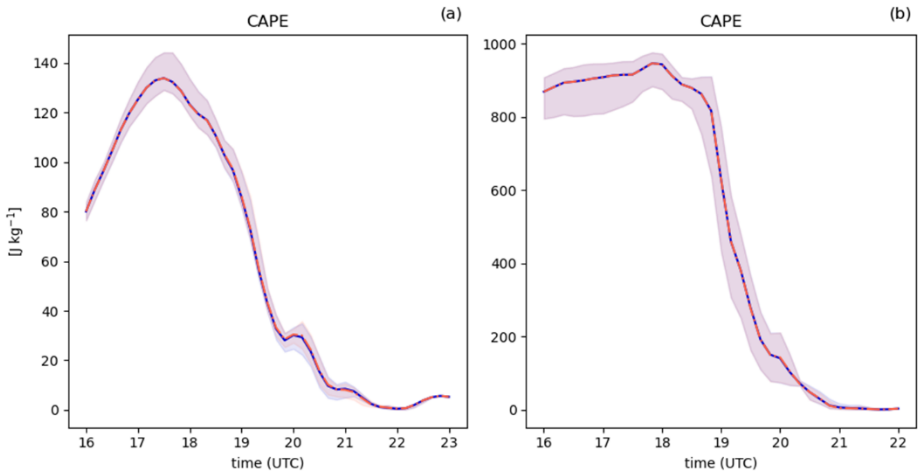

Figure1: Box-averaged (case I: 15 x 15 km2; case II: 35 x 35 km2) and layer-thickness-weighted vertical averages (0 – 13 km) time series of CAPE for case I (a) and case II (b). Lines indicate the ensemble mean, while shading represents the ensemble minimum–maximum range. Blue lines correspond to the BASE experiment, red to the NONURBAN experiment.

However, we note that CAPE itself can be modified by aerosol indirect effects through their influence on vertical wind and latent heat profiles. Therefore, while the similar CAPE evolution increases confidence in our results, we emphasize that our ensemble approach with 5 members and paired t-test analysis was specifically designed to account for internal variability. This statistical framework allows us to distinguish actual aerosol-induced signals ($p < 0.1$) from natural meteorological variability, thereby systematically addressing concerns about confounding factors in convective evolution. Also, we note that the urban parametrization remained unchanged between the base and nonurban experiments. Consequently, dynamic changes due to the urban parametrization can be ruled out.

Second, I suggest doing a similar analysis of convective systems that do not interact with the urban aerosols (or to a much lesser degree), to test whether these systems also change between the aerosol setups. You could produce Figures 4 and 5 (and potentially 7, 8, 10, 11) for these other systems. This could provide evidence that only the system that interacted with urban aerosol showed changes.

We agree that analyzing a convective system without urban aerosol influence would be valuable. To adress this concern we modified our trajectory filtering approach: instead of following the mean urban trajectory we followed the overall mean trajectory as seen in Figure 2. Hence, we can follow the convective system before it reaches urban influence.

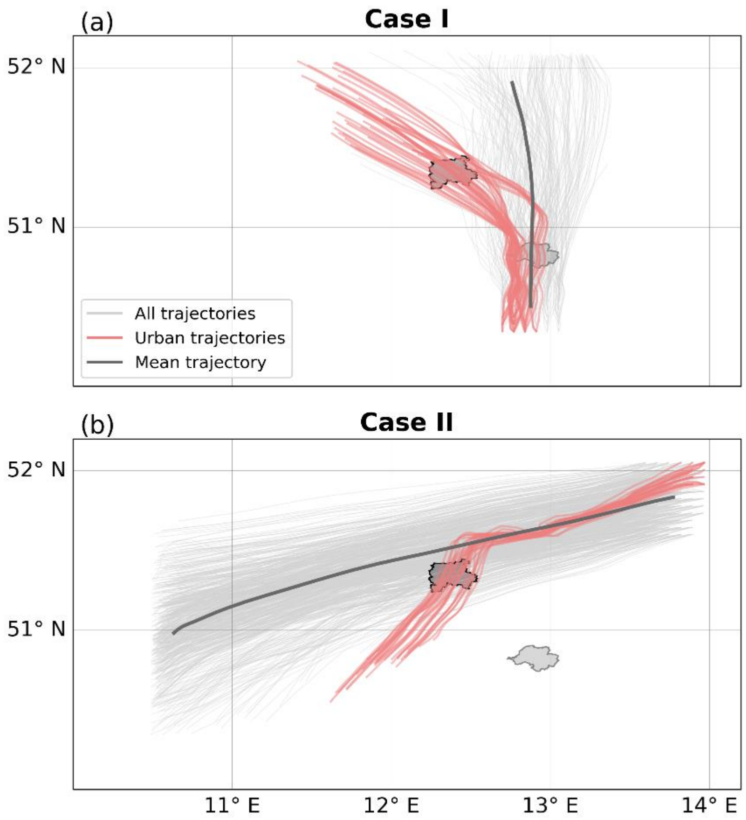

Figure 2: Trajectories for case I (a) and case II (b) showing every 5th trajectory in gray. Trajectories passing the urban area of Leipzig in red and mean trajectory in dark gray.

Both cases show a clear temporal pattern in urban aerosol influence (case I Figure 3; case II Figure 4). In case I, clouds and updrafts developed before 18 UTC show no differences between base and nonurban simulations only after the system encounters the urban plume at approximately 18-20 UTC. In case II, the squall line exhibits cloud development before 19 UTC, with urban effects becoming apparent from 20 UTC onward. In both cases, the onset of urban influence coincides with the time when the convective systems intersect the urban aerosol plume, confirming that direct exposure to elevated urban aerosol concentrations is required for detectable modifications to convective processes.

We added these results to the appendix of the manuscript.

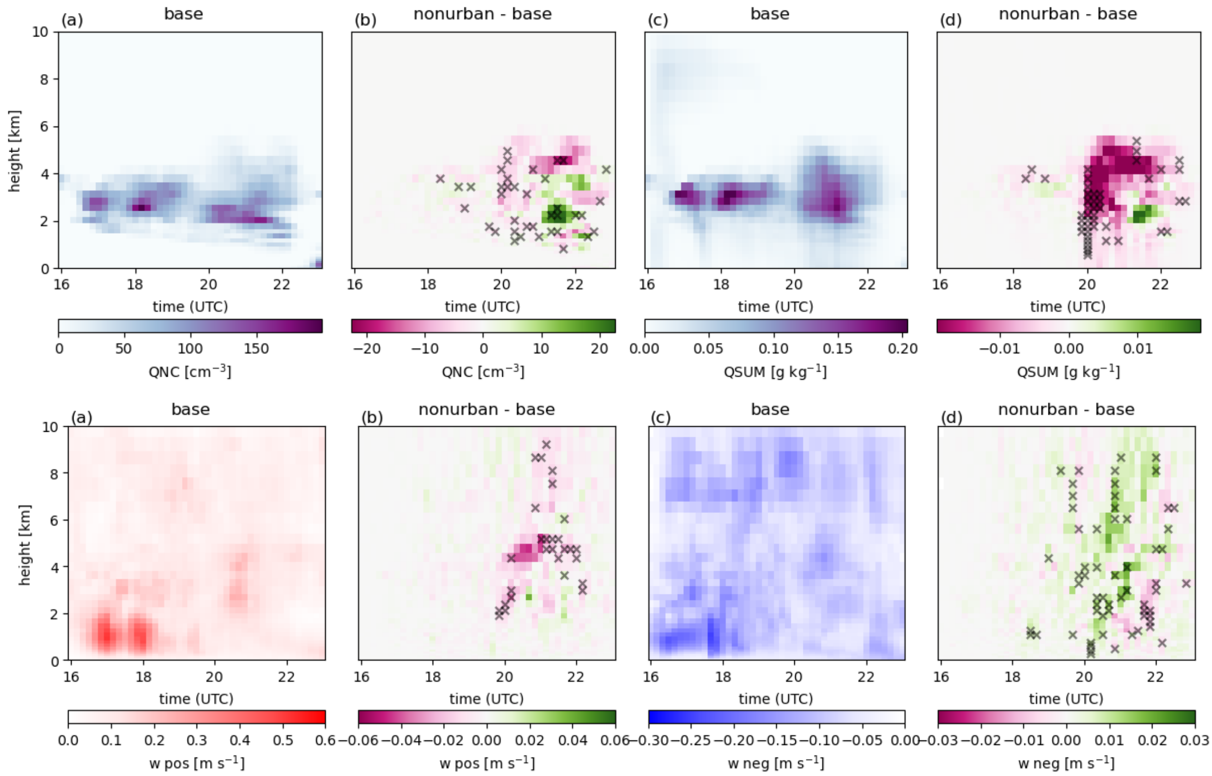

Figure 3: Top row is showing box spatially averaged vertical profiles of number of activated cloud droplets QNC (a) and total cloud condensate QSUM (c) along mean trajectory, showing ensemble means for the BASE experiment for case I. (b) and (d) show the corresponding differences between the NONURBAN and BASE experiments for QNC and QSUM, respectively. Bottom row is showing ensemble mean vertical profiles of positive (w pos) (a) and negative (w neg) (c) vertical velocities from the BASE experiment, spatially averaged over the analysis box along the mean trajectory for case I. Panels (b) and (d) display the deviations from the BASE case in the NONURBAN experiment. Black crosses indicate levels where the differences are statistically significant at the 90 % confidence level.

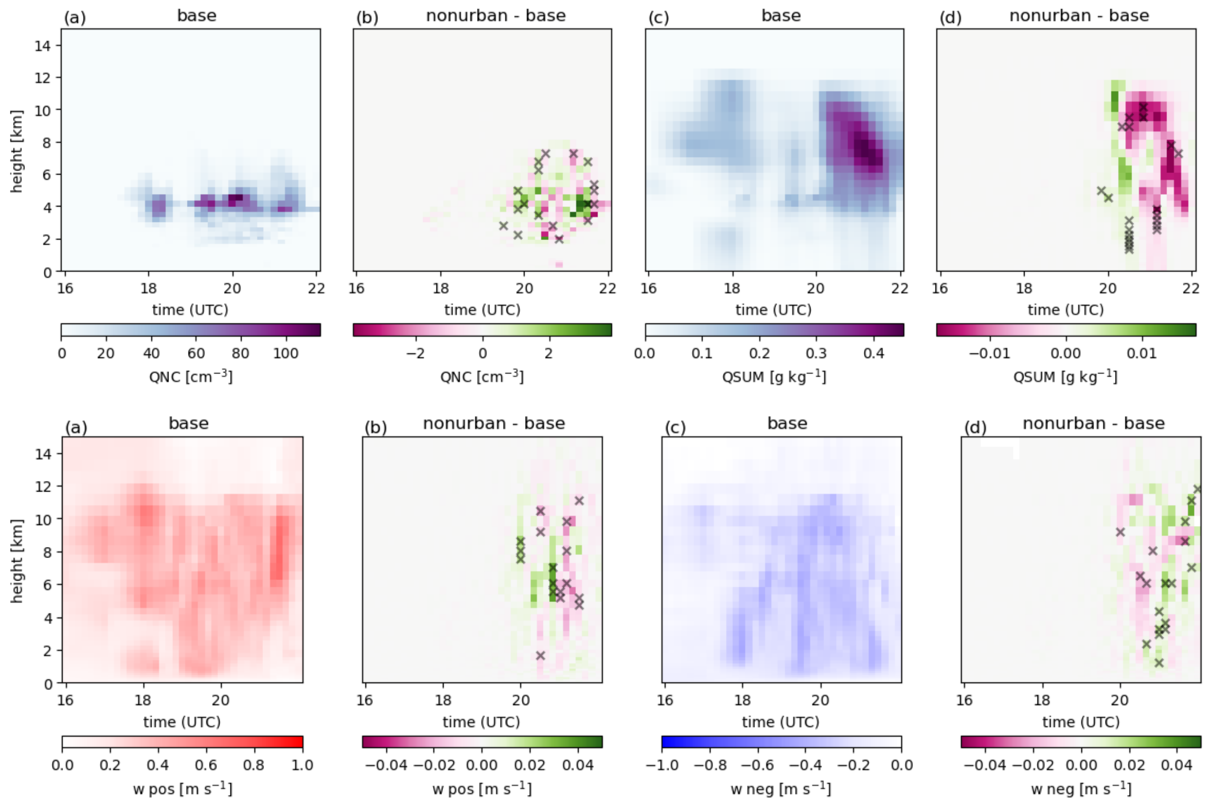

Figure 4: Top row is showing box spatially averaged vertical profiles of number of activated cloud droplets QNC (a) and total cloud condensate QSUM (c) along mean trajectory, showing ensemble means for the BASE experiment for case II. (b) and (d) show the corresponding differences between the NONURBAN and BASE experiments for QNC and QSUM, respectively. Bottom row is showing ensemble mean vertical profiles of positive (w pos) (a) and negative (w neg) (c) vertical velocities from the BASE experiment, spatially averaged over the analysis box along the mean trajectory for case II. Panels (b) and (d) display the deviations from the BASE case in the NONURBAN experiment. Black crosses indicate levels where the differences are statistically significant at the 90 % confidence level.

Major comments

1. I find the title to be somewhat misleading.

We agree with the review that the title was misleading. Therefore, we changed the title of the manuscript to: "Tracking Air Masses for Assessing the Effect of Urban Aerosols on Convective Precipitation: A Multi-Member Modeling Study "

2. Introduction: The description of the invigoration hypotheses (lines 32-53) is somewhat lacking. I suggest reworking these paragraphs by revisiting Varble et al. (2023) and the recent review by Fan et al. (2025) (and reference in both of those papers) for a balanced discussion of the state of the research on aerosol impacts on deep convection. Below, I mention a few points in particular.
    - The authors discuss aerosol cloud interactions in warm-phase clouds, in particular the first and second aerosol indirect effects. However, then they suddenly talk about the 'cold-phase invigoration hypothesis' without any transition, which might leave some readers confused.
    - A definition of invigoration is missing.
    - Cold-phase invigoration is described in such a way that it leaves the impression that it is scientific consensus. However, as the authors mention in the next paragraph the existence of a significant cold-phase invigoration effect is being debated. I would like to see more careful language when describing the potential mechanisms behind cold-phase invigoration.
    - To my knowledge, the hypothesis is that a delay in precipitation development allows more condensate to be lofted to altitudes where it can freeze, not a delay in downdraft development.
    - Warm-phase invigoration is mentioned without any reference or description. I think a more in-depth description of warm-phase invigoration is warranted, since anthropogenic emissions of ultra-fine aerosols may play a role in warm-phase invigoration. Such aerosols are often from urban sources and thus it is possible that invigoration found in this study might be a result of warm-phase invigoration.

We have substantially revised this section in the introduction to improve clarity and accuracy based on these suggestions:
"In deep convective systems, warm-rain suppression is an integral component of aerosol invigoration mechanisms. Condensational invigoration occurs through enhanced condensation and latent heat release (Cotton and Walko, 2021). When large particles are abundant, they can get activated at the cloud base and suppress the warm rain formation, prolonging the condensation phase and releasing additional latent heat. In contrast, when ultrafine aerosol particles (UFPs), a significant component of urban emissions (Kumar, 2014), are abundant, the low concentration of large particles allows a rapid warm-rain formation, resulting in a high supersaturation, which then activates the UFPs, producing massive additional condensation and latent heat release (Fan et al., 2018). Freezing-induced invigoration is a more complex and uncertain mechanism where warm-rain suppression allows more liquid water to ascend to freezing levels, where latent heat release at high altitudes and ice processes (riming, deposition) can invigorate convection (Rosenfeld et al., 2008; Andreae et al., 2004). The net effect on convection depends on three competing processes. First, condensate loading during transport and depositional growth weakens updrafts (Fan and Khain, 2021); second, latent heat release from freezing at high altitudes

potentially strengthens updrafts; third, condensate offloading through hydrometeor sedimentation enhances buoyancy. "

3. The manuscript includes some descriptions of results without showing evidence. Please consider including figures in the supplement such that the reader can verify the statements made by the authors. Since the authors make these descriptions, I suspect that these figures exist and it should not be much additional work to include them in the supplement and reference them in the text. Below, I list some examples, but the author should make sure to provide evidence for everything:
    - Lines 9-11, 446-447: The contrast in instability between the cases is only mentioned in the abstract and the final section but is not shown anywhere in the manuscript. Please show instability at least in the supplement.

The vertical difference in equivalent potential temperature between 850 and 500 hPa is used as a diagnostic measure of convective instability. Positive values indicate a decrease of θ_e with height and thus a potentially unstable stratification (Markowski and Richardson, 2011). As seen in Figure 5, case I exhibits near neutral stability at midday, suggesting that the convection in the afternoon was triggered by local instabilities. In contrast, case II shows extensive instability throughout southern Germany, which favored large-scale convective development.

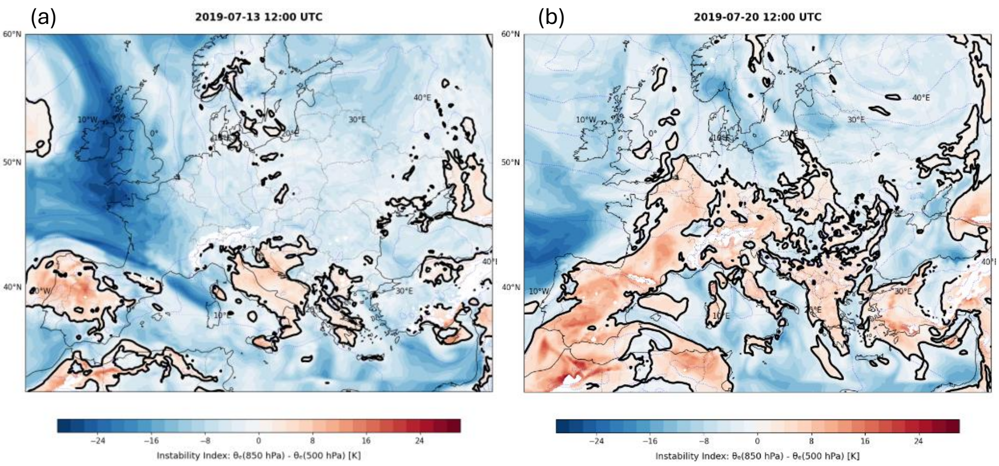

Figure 5: 12:00 UTC analysis of vertical differences in equivalent potential temperature between 850 and 500 hPa for case I (a) and case II (b).

- Lines 155-158: Please show a figure of the meteorological characteristics of the two cases.

We show the meteorological characteristics of both cases in the following figure:

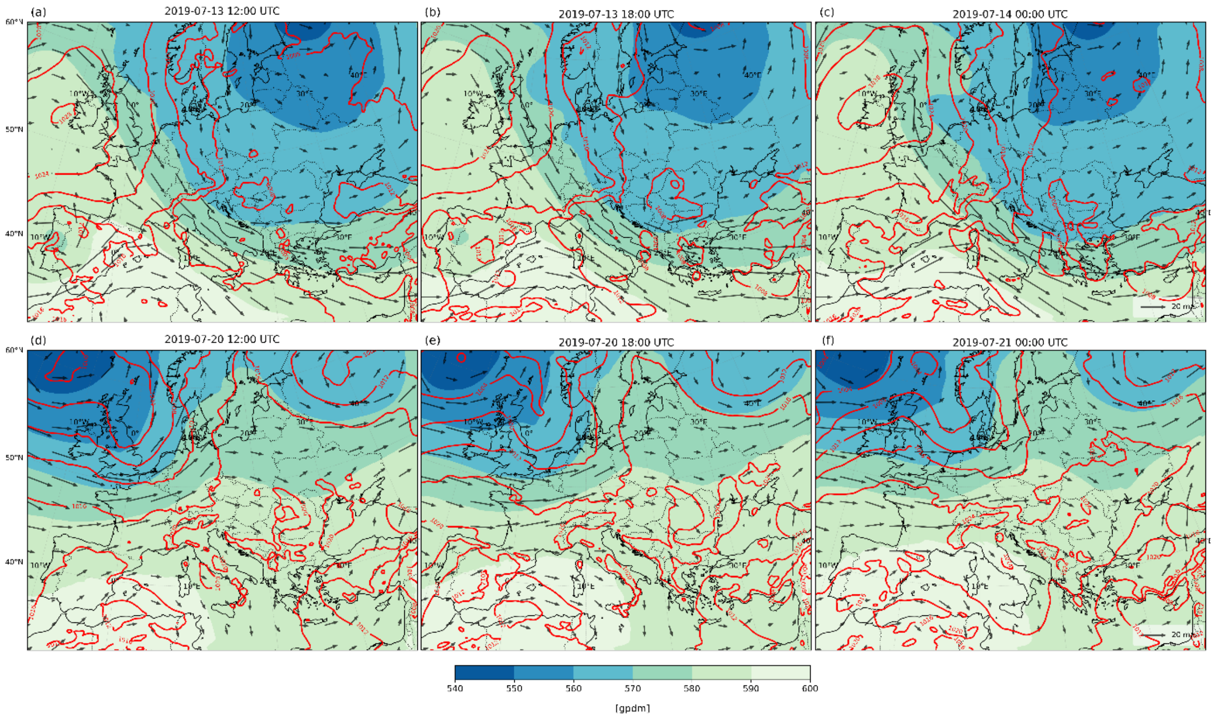

Figure 6: Synoptic scale forcing for case I (a-c) and case II (d-f). Analysis at 12:00, 18:00 and 00:00 UTC showing 500 hPa geopotential height (gpdm; shading), sea level pressure (hPa, red contours), and 500 hPa wind barbs.

- Section 2.4.1: A figure in the supplement could help to illustrate the trajectory analysis. You could use a figure showing some of the trajectories.

We added the following figure to the supplements showing the trajectory pathway and filtering for both cases:

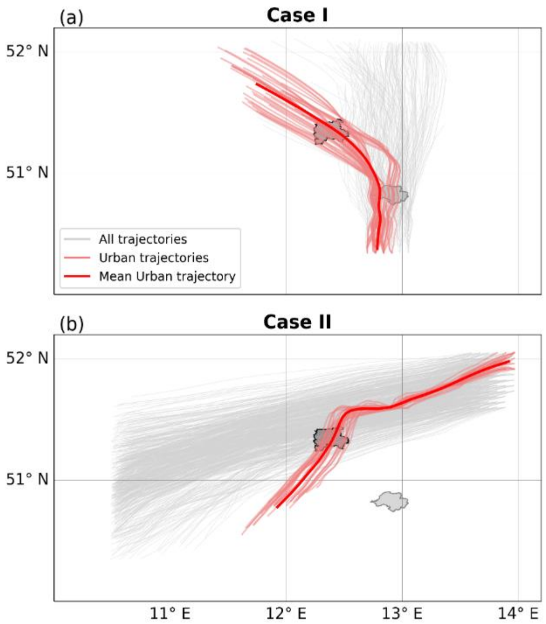

Figure 7: Trajectories for case I (a) and case II (b) showing every 5th trajectory in gray. Trajectories passing the urban area of Leipzig in light red and mean

- Lines 244-249: Figures might be needed to verify these statements about the convective development of the cases.

As seen in Figure 8a case I is characterized by a more localized convective development, whereas the second case (Fig. 8b) involves a broader, more organized convective system.

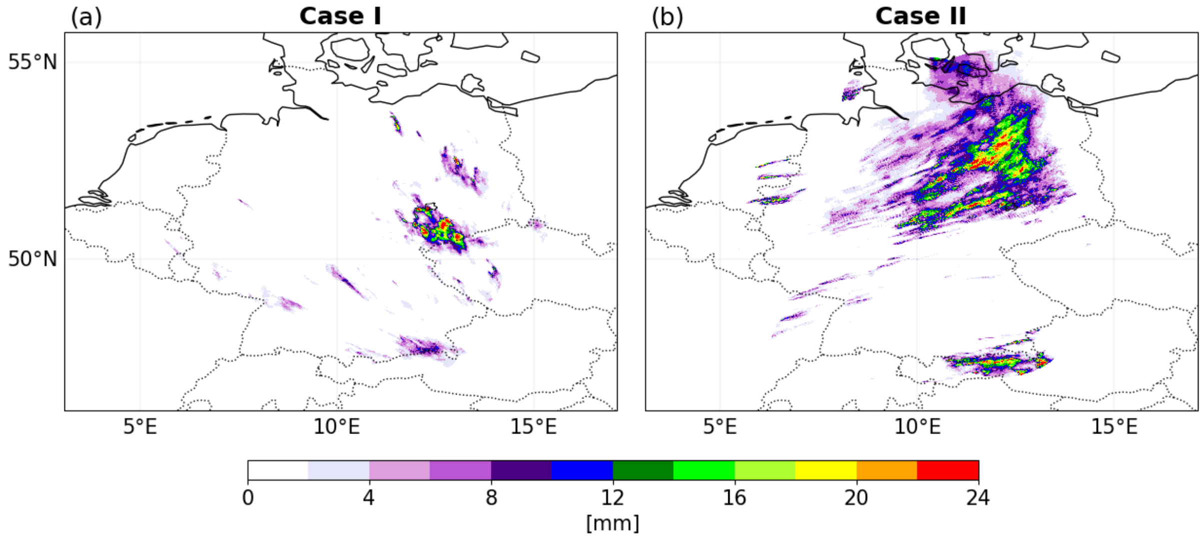

Figure 8: RADKLIM accumulated precipitation over a 4-hour period for case I (a) and case II (b).

- Lines 265-270, 296-301: Again, all these fields should be shown, such that the reader can verify the statements made here.

The descriptions of the synoptic setups can be verified by Figure 6 and 9. Figure 6 shows 500 hPa geopotential height, sea level pressure, and 500 hPa wind barbs for both cases, illustrating the high-pressure system over the Norwegian Sea/British Isles and Eastern European low with northerly flow for case I, as well as the shortwave trough and southwesterly upper-level flow for case II. Figure 9 (850 hPa equivalent potential temperature, wind barbs, and sea level pressure) clearly shows the contrasting air mass advection patterns: cold air advection from the north in case I versus warm, moist subtropical air advection from the south in case II.

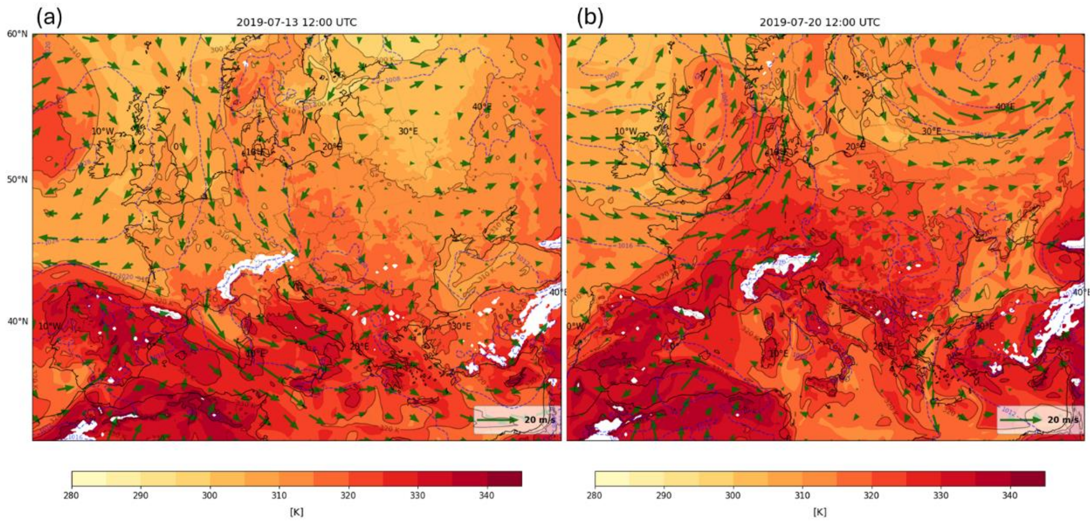

Figure 9: 12:00 UTC analysis of equivalent potential temperature at 850hPa, 850hPa wind barbs and sea level pressure (hPa, blue contours) for case I (a) and case II (b).

Information on aerosol background concentrations can be found in Figure 10, showing temporal (24h) and domain-averaged vertical profiles of the relevant chemical species.

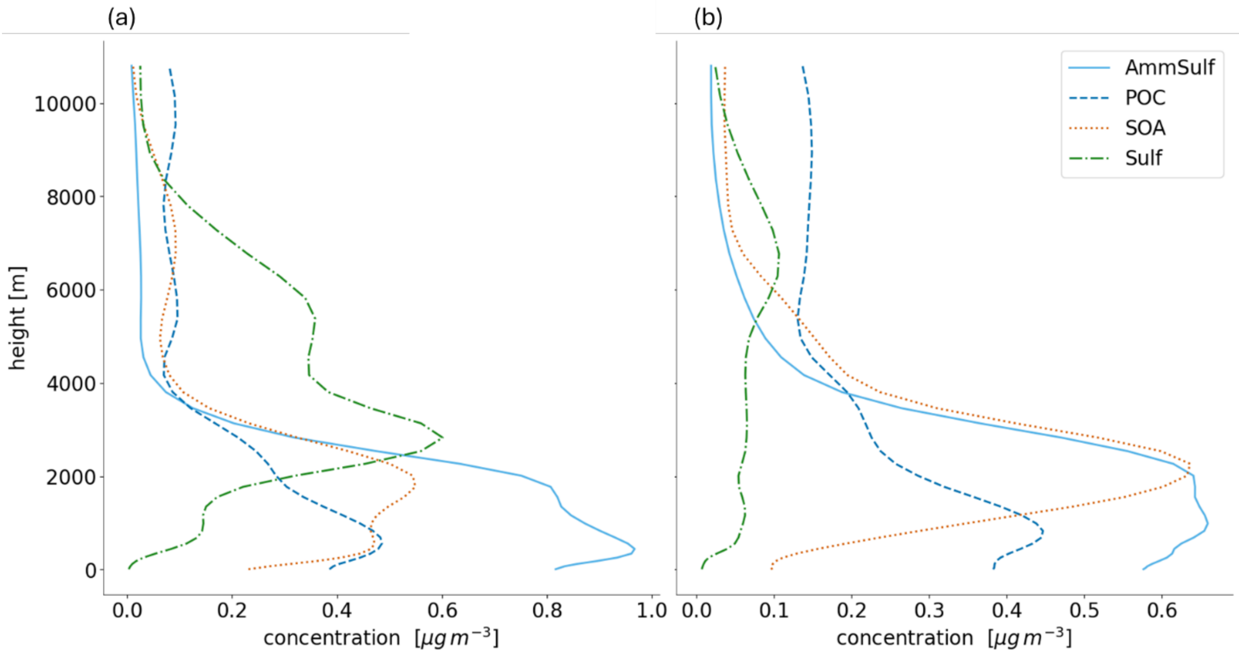

Figure 10: Spatially and temporally averaged vertical profiles of ammonium sulfate (light blue), sulfate (green), primary organic carbon (dark blue), and secondary organic aerosols (SOA; brown) for case I (a) and case II (b).

- Line 421-422: I don't really see any evidence for an earlier decay of the system in the BASE experiment. Figure 9 basically shows no difference in the timing of the precipitation between the experiments. Please add a figure or better reference the figure that shows this result.

We thank the reviewer for this critical comment. Upon re-examination, we agree that our data does not clearly support the claim of "premature decay" or earlier system collapse. While Figure 11d in the manuscripts shows stronger downdrafts in the BASE experiment, Figure 9 in the manuscript demonstrates that precipitation timing is similar between experiments. We have revised the text to remove the claim of premature decay. The text was revised to:

Abstract: *"Under stronger initial instability, the urban emissions intensify the precipitation, leading to stronger downdrafts and weaker updrafts, altering the convective system's evolution compared to the zero urban emission scenario."*

Results: *"The resulting increased precipitation leads to a stronger downdraft and suppresses the updraft essential for sustaining the convective system. Analysis of vertical velocity fields confirms significantly stronger downdraft regions by 5 - 10 % in the BASE experiment (Fig. 11d), supporting this mechanism. In contrast, the NONURBAN experiment is characterized by continued graupel and snow production in the later stage of the system (20:00 – 21:00 UTC). Weaker downdrafts allow sustained updraft activity, supporting ongoing aggregation and riming, leading to fewer cloud droplets and ice in the NONURBAN experiment. This is supported by increased updraft regions above 10 km and reduced downdraft regions after 20:30 UTC (Fig.11). The comparison reveals different storm evolution patterns with significant implications for precipitation efficiency. When including the Leipzig emissions, the system produces enhanced* *early precipitation accompanied by stronger downdrafts that suppress further updraft development. Without urban emissions, the system maintains more balanced updraft-downdraft circulation in the later stages, enabling sustained aggregation and riming processes. This sustained microphysical activity in the NONURBAN experiment could indicate a longer convective lifetime."*

Discussion: *"In case II, urban emissions accelerated droplet activation and ice formation, which initially intensified precipitation by 10-20%. However, this led to stronger downdrafts of 5-10%, which appeared to contribute to earlier decay of the system."*

Minor comments

4. Lines 4, 59: It is probably better to say, "passing over" instead of only "passing".

Corrected.

5. Line 5: The sentence "[...], with five-member ensemble [...]" needs to be clarified, i.e. the ensemble needs to be described better. In its current form it reads more like five different emissions scenarios are compared and not five different IC/BC.

We revised the sentence:

"Using the coupled COSMO-DCEP-MUSCAT modeling system, we simulate two convective events passing over the city of Leipzig, Germany, with experiments comparing total emissions to zero urban emissions, with five ensemble members for each setting."

6. Line 20: Please add a reference for "[...] expected to further intensify precipitation events." or make the connection to the following sentence clearer.

Reference (Yan et al., 2024) added.

7. Lines 27-28: The relevance of this sentence needs to be clearer. In which way do these types of aerosols relate to urban aerosols?

We have revised the sentence.

"Moreover, urban areas emit aerosols from traffic, heating, and industry that, depending on their size and chemical composition, serve as cloud condensation nuclei (CCN, particularly sulfates; Pruppacher and Klett, 1997) and ice-nucleating particles (INP, e.g. black carbon and organics; Burrows et al., 2022)."

8. Lines 29 -31: Please add references for this sentence.

Added Rosenfeld (2008) as a reference.

9. Line 33: "narrower" needs to be clarified. In which way is the distribution narrower? I assume the authors mean more numerous and smaller cloud droplets.

We corrected this statement.

"However, the resulting cloud droplet size spectrum tends to be more numerous and include smaller cloud droplets, which reduces coalescence and collision efficiency, thereby decreasing raindrop growth."

10. Line 71: "[...] their findings [...]", please clarify whose findings are talked about.

Done.

"Although focusing on a single event limits the generalizability of results to other meteorological contexts or regions, such process-level studies are essential for improving our understanding of aerosol-cloud interactions."

11. Line 85: "two-moment bulk microphysics scheme"

Corrected.

12. Line 90: I would say Leipzig is a "city" not a "town".

Corrected.

13. Lines 105-106: Please add references to studies using this standard configuration.

Reference Wolke et al. 2012 added.

14. Lines 125-129: The description of the single-moment scheme seems somewhat unnecessary and could be omitted. I think it would suffice to mention this in line 132: "[...] than the simpler single-moment scheme employed in the standard COSMO setup [...]" or something similar.

We revised the description paragraph according to the reviewer's suggestions:

"Here we use the two-moment bulk microphysics scheme developed by Seifert and Beheng (2006a, b). It distinguishes between the five hydrometeor classes: cloud droplets, rain, ice crystals, snow, and graupel, and employs prognostic equations to estimate the mass densities and number concentrations of these hydrometeor particles. Therefore, it can provide more accurate predictions of cloud formation and precipitation than the simpler single-moment scheme employed in the standard COSMO setup (Doms et al., 2018)."

15. Lines 134-136: The authors could make it clearer that this capability was specifically developed for this study, if that is the case.

We have revised the sentence to formulate it more clearly:

"We enhance the model configuration for this study by replacing these prescribed concentrations with calculated activation of cloud droplets and ice particles directly from aerosol mass concentrations simulated by MUSCAT."

16. Line 137: Please rephrase to the following: "[...] a more realistic representation of CCN and INP with the goal of improving the simulation [...]" or something similar.

Done:

"This dynamic approach allows the model to account for spatial and temporal variability in aerosol properties, which is expected to enable a more realistic representation of CCN and INP with the goal of improving the simulation of aerosol-cloud interactions."

17. Line 151: Does supersaturation here refer to vapor supersaturation? Along those lines, the authors should describe how supersaturation is treated by the microphysics scheme. This is especially important because supersaturation plays a critical role in the warm-phase invigoration hypothesis and without predicting supersaturation in the microphysics scheme one cannot expect to accurately simulate warm-phase invigoration according to Fan et al. (2025). To my knowledge, the Seifert and Beheng scheme does use saturation adjustment, i.e., does not predict supersaturation.

Yes, supersaturation at line 151 refers to vapor supersaturation with respect to liquid water.

The reviewer correctly identifies saturation adjustment as a limitation (Lebo, 2012). However, we implement a modified Abdul-Razzak & Ghan (2000) activation scheme. The scheme explicitly calculates supersaturation during activation based on updraft velocity, aerosol size distribution, and composition, and enables in-cloud activation. Nevertheless, saturation adjustment is applied after each activation in the Seifert & Beheng (2006), which may dampen the feedback between qnc and condensation rates (Zhang, 2021). While our setup likely cannot fully capture condensational invigoration by ultrafine particles as described by Fan et al. (2018), other aerosol-cloud interaction mechanisms remain active, including effects on droplet size distributions, autoconversion, and ice processes through the explicit activation scheme. We have added clarification to the model description and expanded the discussion section to address this methodological limitation.

Section 2.1.1: *"In the standard setup of the two-moment scheme, the number of activated cloud droplets and ice particles is calculated using prescribed CCN and INP values, respectively, and saturation adjustment is applied. .... To enable in-cloud conditions, already activated cloud droplets are treated as an additional aerosol mode with κ ≈ 0 and a diameter equal to the mean droplet size. This allows the activation scheme to distinguish between activated droplets and activatable aerosol at each model time step, enabling secondary nucleation in updrafts throughout the cloud depth."*

Discussion: *"Finally, we note that our microphysical setup, while including explicit aerosol activation and in-cloud nucleation, applies saturation adjustment. This approach may underestimate aerosol effects on convective intensity compared to fully explicit supersaturation schemes (Lebo et al., 2012; Zhang et al., 2021), though this dampening is likely modest given our realistic perturbations and focus on real mid-latitude cases rather than idealized deep convection. Future work could employ explicit supersaturation methods to provide upper-bound estimates and better constrain the range of urban aerosol effects on precipitation. Nevertheless, the results of this study underpin that modest urban emission variations can modulate microphysical processes in convective systems and affect precipitation amount."*

18. Lines 155-158: In my opinion, more description about how these two cases were selected is needed. Currently, it reads like two random days with differing characteristics were selected without much explanation. Some questions I have: Why in particular were those two days selected? Were they chosen from a larger number of cases? Were they simulated better than other cases? Please add a few sentences on how these cases were selected.

The two convective cases were systematically selected through a multi-step process: First, local newspaper archives were consulted to identify days with publicly reported heavy precipitation in the Leipzig region during summer 2019. These candidate events were then verified against the RADKLIM CatRaRE (Radar Climatology Catalog of Radar-based heavy rainfall Events) (Lengfeld et al., 2021) event catalog to confirm their meteorological significance. Finally, selected dates were tested for availability and quality in the COSMO-D2 reanalysis dataset, which served as initial and boundary conditions for the simulations. The final selection of July13 and July 20, 2019 was made because these cases were documented both in public reports and CatRaRe database, were captured in the COSMO-D2 reanalysis and exhibited different synoptic patterns, allowing for testing of aerosol-cloud interactions under different meteorological regimes.

We added the following sentence on the selection method to the manuscript:
"The cases were systematically selected through evaluation of public reports and the RADKLIM CatRaRe event catalog (Lengfeld et al., 2021) and whether the cases were captured by COSMO-D2 reanalysis."

19. Lines 174, 202: DWD is referred to as German Weather Service but later as the German Meteorological Service.

Corrected.

20. Section 2.4.1: You are using many more vertical levels for the trajectories than the model has. Does this actually provide any valuable additional information? Furthermore, do most trajectories pass over the city (see major comment 2, a figure would be helpful here)?

The reviewer is correct that our initial trajectory vertical resolution exceeded the model's information content. We have revised our approach to use approximately 2× oversampling relative to model levels (20 trajectory levels for ~10 model levels in case 1; 50 for ~20 in case 2).

We have revised the manuscript accordingly:

"The horizontal start box covers approximately 40 x 40 km^2 with about 5 km spacing between points. Vertically, the range extends from 1 to 4.5 km above ground level and is divided into 20 vertical levels. For case II, starting at 20 July 2019, 22:00 UTC, the start points were similarly traced back for six hours. The horizontal box spans roughly 40 x 60 km^2 with 5 km spacing, and vertically, it extends from 1 to 7.5 km subdivided into 50 levels."

As seen in Figure 7, most backward trajectories from the precipitation regions bypass the city. That's why we filtered the trajectories to retain only those passing over Leipzig and ensure urban influence reaches the convection.

21. Figure 2: The moving boxes in the figure look quite rectangular and not square. Are they plotted correctly? Although it might be related to the map projection.

That's correct, the moving boxes appear rectangular rather than square in the figure, and this is indeed related to the map projection as suspected. The boxes are defined as squares in metric space. However, when plotted on a map with latitude-longitude axes (in degrees), they appear rectangular due to the convergence of meridians at mid-latitudes (~50°N). We added a kilometer scale to the figure to improve clarity.

22. Line 232: Probably you mean "output time" or "every 10 minutes" instead of "every time step" which I would associate with the model time step (10 seconds).

Corrected.

"This produced one vertical profile per ensemble member every 10 minutes."

23. Line 240: Please explain the selection of the significance level. I assume that it is because of the small sample size.

The choice of 90% confidence level is indeed motivated by our limited ensemble size. With only 5 ensemble members, statistical power is inherently limited. The 90% threshold (α=0.10) represents a pragmatic balance between detecting genuine aerosol effects and avoiding false positives.

24. Line 272: "merging the cell at higher altitudes." I am not sure what the authors mean here.

Changed to:

"Low-level winds transported urban emissions from Leipzig toward the southeast, mixing into the convective cell at higher altitudes."

25. Figures 4 and 5: The authors could mention in the caption that purple means more precipitation with urban aerosols. I might also suggest choosing a more colorblind- friendly color palette for the right column. Furthermore, the mean trajectories could be added to one of the panels.

We added the mean trajectories to the Figures and adapted the captions:

"Purple shading indicates increased precipitation due to urban aerosols."

We have verified the chosen color palette with a colorblind colleague who had no difficulties distinguishing the colors. However, we would appreciate the reviewer's suggestion for an alternative palette that might further improve accessibility.

26. Line 290: "The influence of urban emissions on precipitation ranges between 10 – 20 % [...]", this statement is vague, please clarify whether a decrease or increase is meant and whether total amounts or rates are referred to.

We have clarified the text:

"The influence of urban emissions on local precipitation rates ranges between 10 - 20 %, with both increases and decreases depending on location, although not all differences are statistically significant."

27. Line 306: The classification as a heavy precipitation event needs a reference.

We added DWD criteria as a reference:
https://www.dwd.de/DE/wetter/warnungen_aktuell/kriterien/warnkriterien.html

"Rainfall rates within the core reached 15 -20 mm per 30 minutes, classifying the event as heavy precipitation by German Weather Service (DWD) standards. "

28. Line 313: Please clarify around which precipitation core. Furthermore, "intensity core" seems like unusual language. I suggest changing to "precipitation core" and removing "of the precipitation field".

Changed and clarification added:

"These differences concentrate mainly around the central precipitation core, while other areas of the system are only marginally affected."

29. Line 334: “relative to the NONURBAN experiment”, add figure reference.

Reference Fig. 2a added.

30. Line 342: I would choose a different word than “significant”, since no statistical testing was done in this specific case.

Changed to:

“No substantial differences exist between experiments for total condensate or individual hydrometeor categories.”

31. Line 358: I don’t think I can agree with the statement that negative QNC differences dominate in the first phase. To me it does not really look like there is much dominance by either positive or negative differences. I would consider re-wording this statement.

We agree that this statement was imprecise. Following the reviewer's comment on trajectory analysis (comment 11), we recalculated the trajectories, and the revised analysis shows a clearer and statistically significant negative QNC signal in the early phase. The updated figure now better supports this statement. We revised the sentence to:

“During the first phase between 18:30 - 20:30 UTC, the negative QNC difference values of 5-10 cm^{-3} are more prevalent (Fig.7b), indicating that the urban aerosols act as additional condensation nuclei and the formation of cloud droplets is enhanced by 2-10 % compared to the pristine conditions (see Fig. S3).”

32. Line 370: “[…] intensifies the cloud dynamic.” This statement seems vague. Please clarify.

We clarified the statement to:

“This phase transition releases additional latent heat, which warms the surrounding air and strengthens the updrafts, creating a positive feedback loop between microphysics and dynamics.”

33. Lines 383-385: You used past and present tense in the first sentence. I am not able to understand what the authors are trying to say with the sentences afterwards.

We revised the two sentences to:

“For case II, the mean trajectory originates 40 - 60 km southwest of Leipzig and progresses northeastward, passing over the city between 18:00 - 19:00 UTC (Fig. 2b). North of Leipzig, trajectories follow the urban emissions plume eastward for about 30 minutes before continuing northeastward.”

34. Figures 7, 8, 9, 11: Please add “NONURBAN – Base” above panels (b) and (d) similar to Figures 4 and 5.

We added these above panels to Figures 7, 8, 9, and 11 in the revised manuscript.

35. Line 406: I suggest removing “dominant” or replacing it with another word such as “strong”.

Changed to:

“During this peak intensity period, the system is characterized by strong graupel production.”

36. Line 407: I think "experimental" can be removed here.

We changed the sentence to:

"The comparison between BASE and NONURBAN experiment reveals significant temporal variations in system response, with contrasting effects emerging across different phases of convective development."

37. Line 409: You probably mean Fig. 10b.

Thank you for checking. We indeed refer to Fig. 10d, where the differences in hydrometeor mass mixing ratio are shown, rather than Fig. 10b which displays the differences in number of activated cloud droplets. However, the text before was misleading as it stated hydrometeor number concentration. We corrected this in the manuscript.

38. Line 436: "the rest being kept." This sentence is not finished.

We changed the sentence to:

"The BASE experiment serves as a control and is compared to the NONURBAN experiment, where the urban area of Leipzig has been assigned zero emissions while all other emissions are kept unchanged."

39. Line 441: Please clarify what "intensified the convective core" exactly means.

Clarified in the manuscript.

"In case I, initial changes of precipitation were detected at the system edges, while in the later stages, an increase of ice-phase processes intensified the updraft, leading to enhanced rainfall."

40. Lines 452-460: This discussion is very valuable, thank you.

We thank the reviewer for this positive feedback.

41. Lines 461-464: For the Marinescu et al. study, it is important to note that differences between aerosol number concentrations between the clean and polluted scenarios are 7-8 times. In this study the differences are only a few percent.

We revised this paragraph to:

"The findings of this study generally align with those reported in the multi-model study by Marinescu et al. (2021), who also examined CCN effects on convection, without specifically considering urban influences. Their study reproduced comparable updraft enhancement trends (5 – 15 %), an indication for the latent heating mechanism, although they applied substantially larger CCN perturbations than the moderate urban emission changes examined here. The COSMO version used in Marinescu et al. (2021) exhibited one of the weaker responses compared to the other participating models, likely reflecting limitations in its standard CCN treatment. In contrast, the COSMO-MUSCAT system used here includes a coupled chemistry model that directly calculates cloud droplet activation from prognostic aerosol fields. This explicit aerosol-to-droplet activation process, combined with realistic spatial and temporal aerosol variability, enables a more detailed consideration of aerosol–cloud interactions."

42. Lines 485-490: If possible, include links to these datasets.

We added the following information on data availability:

"Recent German-wide emission data were provided on request by the German Environment Agency (Umweltbundesamt, UBA). Building geometries and orography (DGM1) are available from the State Enterprise for Geographic Information and Surveying Saxony (GeoSN; https://www.geodaten.sachsen.de/downloadbereich-digitale-3d-stadtmodelle-4875.html, last access 14 July 2020)."

43. Line 490: It might make sense to mention the specific AI tools.

We added the used AI tools.

Typographical

44. Line 175: "are the focus of this study." Corrected.

45. Line 178: "simulated from the coarser" Corrected.

46. Line 203: "covering the entirety of Germany" Corrected.

47. Line 252: "a precise agreement" Corrected.

48. Line 304: "through" Corrected.

49. Line 316: Missing period. Corrected.

50. Line 322: "downwind of the urban source region" Corrected.

51. Line 330: "In this case" Corrected.

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