Responses to Reviewer #2

"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 #2

General comment:

The manuscript background and motivation to study urban aerosol effects on convective precipitation and the underlying microphysics is presented well. The modeling effort is well thought out and highly detailed including many aerosol sources, a chemistry model, urban land surface effects, an ensemble method, and so forth. I do find that 1km grid spacing for the inner domain to be on the borderline with regards to resolving the details of convective cells. While the methodology is reasonably sound, I am largely concerned that the "urban enhancement in aerosol concentration" is not really an enhancement since you only see a 2-3% increase. This is a very small change in aerosol for urban enhancement when you consider the other urban aerosol studies you cited in the introduction. Likewise, the changes you see in several of the figures are incredibly small. A 10-20% change of a very small number is still a very small number. As such, I am left wondering if the results are worth publishing. While the analysis seems sound, the aerosol change and the impacts are small. If the aerosol change was truly "urban-ish" and the changes were still small, then that would be worth sharing to the community. Finally, I think it is difficult to draw significant conclusions regarding warm or cold phase invigoration with such a small change in aerosol and very small change in W over a very small area. I think these conclusions are overstated given these issues. Please see more detail in the comments below.

We thank the reviewer for these thoughtful and critical comments. The reviewer is correct that the urban enhancement is small. However, this is a key finding of our study rather than a known limitation. Our motivation was to investigate urban aerosol effects under realistic Central European emission levels, without prior knowledge of the magnitude of these effects. Previous urban aerosol-convection studies have often examined idealized scenarios with large aerosol perturbations or compared highly polluted megacities to pristine backgrounds. In contrast, our goal is to investigate realistic urban emission perturbations from a European mid-sized city and determine whether such modest changes are sufficient to produce detectable effects on convective precipitation. This is exactly why we developed the moving box analysis and used an ensemble base statistical approach to systematically test for significant changes and rule out internal variability. The results show statistically significant changes. We believe this finding is valuable to the community precisely because it demonstrates that realistic urban aerosol changes can have detectable impacts on convection. Furthermore, we emphasize that our ensemble approach represents methodological advancement over previous studies. Most urban aerosol-convection studies do not employ ensemble methods, making it difficult to distinguish aerosol signals from meteorological variability. Our 5-member ensemble with statistical significance testing provides a more robust framework for detecting subtle effects that might otherwise be masked by internal variability.

We have revised the manuscript to better communicate this motivation and the methodological strength of our approach.

We agree with the reviewer that our interpretation of "invigoration" may be too strong given the small magnitudes and spatial scales involved. We have revised the manuscript to adopt more cautious language.

<u>Specific comments:</u>

1.Model description: Does the microphysics scheme in COSMO used here (Seifert and Beheng 2006b) use a saturation adjustment scheme? If so, this is likely a problem for trying to assess aerosol impacts on cloud microphysics. In low aerosol conditions, supersaturation should not be fully consumed in each timestep and should be carried within the cloud.

The reviewer correctly notes that the Seifert and Beheng (2006) microphysics scheme employs a saturation adjustment, which is a known limitation for studies of aerosol–cloud microphysical interactions, as supersaturation is diagnostically removed after each advection time step.

In our simulations, cloud droplet activation is treated using the Abdul-Razzak and Ghan (2000) scheme, which explicitly predicts supersaturation during the activation step and determines cloud droplet number concentration as a function of updraft velocity, aerosol size distribution, and composition. Our implementation further allows *in-cloud activation* by representing pre-existing cloud droplets as a competing aerosol mode (with $\kappa \approx 0$), thereby enabling secondary nucleation in updrafts throughout the cloud depth, which is a key mechanism for warm-phase convective invigoration (Fan, 2018; Lebo, 2018).

Following activation, condensational growth is treated using saturation adjustment, consistent with Seifert and Beheng (2006). As shown by Lebo et al. (2012), this approach may damp aerosol effects on buoyancy in deep convection by enhancing condensation primarily at lower levels, leading to larger droplets and earlier precipitation formation. Zhang et al. (2021) quantified this effect for *major aerosol perturbations* (factor of 5–10 increases in CCN), finding reductions in aerosol-induced buoyancy responses by factors of approximately 2–3.

Several aspects are relevant for interpreting our results: (1) The in-cloud activation capability partially compensates for saturation adjustment by allowing secondary droplet formation in convective updrafts. (2) The aerosol perturbations considered here are substantially smaller than the factor of 5–10 CCN changes examined by Zhang et al. (2021), suggesting a weaker damping effect in our case. (3) Our study focuses on realistic mid-latitude convection, whereas the results of Lebo et al. (2012) are based on idealized simulations of deep continental convection, for which the quantitative applicability to specific real-case urban scenarios may be limited.

More generally, Seifert and Beheng (2006) argue that clouds usually relax rapidly toward thermodynamic equilibrium between water vapor and cloud droplets, making saturation adjustment a practical and robust approximation. Although conceptually paradoxical - since droplet activation depends on supersaturation that is subsequently eliminated by saturation adjustment - the operator-splitting method allows supersaturation to control activation prior to condensation, providing an efficient and robust numerical treatment. At present, neither COSMO nor its successor ICON provides a fully explicit prognostic treatment of supersaturation suitable for general cloud microphysics.

Consistent with Grabowski and Morris (2017), differences between saturation-adjustment and explicit supersaturation treatments are expected to be small for shallow and moderately deep convection, where supersaturations typically remain below ~1% due to relatively weak updrafts.

Taken together, we interpret our results as a conservative (lower-bound) estimate of urban aerosol impacts on convective precipitation. We have clarified the activation and condensation treatment in the model description and expanded the discussion to explicitly acknowledge this limitation and its implications.

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 amounts."*

2.Line 196: How exactly do you vary the spinup lengths for the ensemble? Does this mean you vary the time of initialization or the period of analysis? It was a little unclear. I ask, because changing the initialization / model start time can sometime drastically alter how convective systems organize since the reanalysis data can have different degrees of truth at different times due to different amounts and quality of data input (soundings, surface stations, satellite obs, etc).

We vary the time of initialization. A standard run has 24 hours meteorological spin-up (only COSMO) and then 24 hours coupled simulation of COSMO-MUSCAT. To create the ensemble, we varied the 24h meteorological spin-up time und the 24h coupled simulation stayed unchanged. Importantly, all ensemble members use the same coarse domain (D1) simulation as input, ensuring that large-scale forcing remains consistent across the ensemble. We understand the reviewer's concern. However, with our approach all ensemble members simulate the same convective events but with slightly different initial atmospheric states due to the varying spin-up duration. The resulting spread captures internal atmospheric variability rather than uncertainties in reanalysis data quality. We clarified this in the revised manuscript.

"For each experiment, we created an ensemble with five members, respectively, by varying the length of the meteorological spin-up run, while the 24h coupled COSMO-MUSCAT simulations remained unchanged. The spin-up lengths are 24h, 21h, 18h, 15h, 12h. All ensemble members

are initialized with the same D1 simulation, ensuring that the large-scale forcing remains consistent across the entire ensemble. This approach allows to assess the impact of slightly varying initial meteorological conditions on the results."

3.Lines 201-205: A comparison to precipitation is mentioned here but no reference to a figure or an analysis of this. It would be good to mention how this data will be used.

Observational data are used to evaluate the model's general ability to reproduce the convective precipitation events. A more detailed comparison of simulated and observed precipitation can be found in Section 3.1. We added this information to section 2.3.

"To evaluate the general model performance, we compared our simulations with observed precipitation data from the RADKLIM dataset (Winterrath et al., 2018), a radar-based precipitation climatology provided by the German Weather Service (DWD). RADKLIM provides high-resolution data on a 1 - km spatial grid with a 5-minute temporal resolution covering the entirety of Germany. The dataset is derived from 17 C-band Doppler radar systems and is offline-adjusted using daily gauge measurements from over 4,400 rain gauges that record both hourly and daily precipitation. A more detailed comparison of simulated and observed precipitation is presented in chapter 3.1."

4.Figure 2: It is unclear what is being shown here. What is meant by "the mean of all aerosol species"? The mean of aerosol mass or number? Please clarify. And is the difference taken as Base – Nonurban? Finally +/- 3% seems like a rather small difference.

The mean of all aerosol species refers to the aerosol mass concentration averaged across all aerosol species considered in the two-moment microphysics scheme: five dust size classes, ammonium sulfate, ammonium nitrate, sulfate, organic carbon, elemental carbon, and two sea salt size classes. The plot shows the relative difference in aerosol mass between the NONURBAN and BASE scenarios, calculated as (NONURBAN - BASE) / BASE × 100%, temporally averaged over ~2 hours (case I: 17:50–19:00; case II: 18:20–20:00) and vertically averaged below 1000 m. The average specifically captures the period and altitude range where the trajectories pass through the urban aerosol plume before reaching the convective system. We have revised the figure caption to clarify these aspects.

"Mean trajectories for case I (a) and case II (b). Pink -green shading shows the percentage difference in aerosol mass concentration (NONURBAN - BASE) averaged across all aerosol species considered in the two-moment microphysics scheme (5 dust classes, ammonium sulfate, ammonium nitrate, sulfate, organic carbon, elemental carbon, 2 sea salt classes), vertically averaged below 1000 m and temporally averaged over the periods when trajectories pass through the urban aerosol plume (case I: 17:50 -19:00; case II: 18:20 -20:00). Grey shading indicates the average trajectory height and trajectories run in the direction towards higher altitudes. The filled areas mark the urban regions of Leipzig (dark grey) and Chemnitz (light grey). Grey boxes denote the rectangular averaging areas used to extract vertical profiles around each trajectory point."

The ±3% enhancement represents the realistic urban aerosol signal for this region and case study. Our aim is to quantify the detectable urban effect under real-world conditions, rather than exploring idealized or artificially amplified perturbations.

5.Lines 227-228: Does this imply that Fig. 2a is supposed to be showing a localized precipitation structure? The figure caption indicates we are seeing differences in aerosols. Please clarify this text and the associated figure 2.

The references to Figure 2 refer to the trajectory paths, not the aerosol differences shown in the color shading. The figure provides geographical context for the trajectory analysis. We have revised the text to clarify this.

"This selection ensured that the dominant precipitation features were included in the spatial averaging for each case. The associated backward trajectories are shown in Fig. 2a (case I) and Fig. 2b (case II)."

6.Line 251: "differences in spatial and temporal resolution" between what things?

We revised the unclear formulation in the manuscript to:

"Due to model simplifications, uncertainties in input and observational data, and limitations in parameterizing sub-grid-scale processes, a precise agreement between simulation and observation is not expected."

7.Lines 255-259: Not sure I agree that the precipitation systems are well simulated; particularly for Case I in which not much precipitation was simulated compared to that observed. However, I understand model limitations and the difficulty in simulating case studies.

We acknowledge that case I shows quantitative deviations from observed precipitation, likely due to its smaller spatial scale which is more challenging to capture in convection-permitting simulations. However, both systems are qualitatively reproduced in terms of spatial extent, intensity range, and propagation (as stated in the text). Crucially, our analysis focuses on relative differences between emission scenarios rather than absolute agreement with observations.

8.Figure 3: What is happening regarding the wave-like structure to the precipitation from RADKLIM in Case II? Is this physical?

We appreciate this observation. Since this is not our area of expertise, we consulted radar experts at the Meteorological Institute at the University of Hamburg, who confirmed that the wave-like structure visible in the RADKLIM observations for case II is a radar artifact rather than a physical precipitation feature. This artifact occurs when convective systems move rapidly relative to the radar scan. Between consecutive radar scans, the fast-moving system shifts position significantly, creating an apparent wave-like pattern in the composite product. This is a known limitation of radar-derived precipitation products when observing rapidly propagating convective systems.

9.Line 334: A 3% enhancement in aerosol concentration hardly seems like an urban influence. At least some of the urban aerosol studies cited in the introduction showed that urban aerosol enhancements can increase aerosol number concentrations by an order of magnitude. So 3% seems quite small. Your comments on this would be helpful.

The 3% enhancement reflects the realistic urban aerosol perturbation from a mid-sized Central European city (Leipzig, ~600,000 population) embedded in high regional background aerosol levels. Unlike previous studies of megacity impacts with order-of-magnitude increases, our focus is on detecting whether such modest but realistic urban signals are distinguishable from meteorological variability and model noise in a region not previously examined for urban aerosol-precipitation effects. This required developing a refined trajectory-based analysis methodology to isolate the urban signal. The modest aerosol enhancement, which was not known prior to this analysis and represents a novel finding for Central European settings, motivated a follow-up study (currently in preparation) systematically varying emission strengths to quantify precipitation sensitivity across a wider range of aerosol perturbations. We revised the manuscript to more clearly communicate our motivation.

10.Figure 6: Both simulations seem very clean with very low cloud droplet concentrations, and there's almost no difference in the droplet number. I would be hard pressed to call this an urban enhancement compared to the studies you cited earlier. Given on a 3% change in aerosol concentration enhancement, this is perhaps expected.

We thank the reviewer for this comment, which highlights an important aspect of our analysis methodology. The clean appearance results from three layers of averaging: (1) ensemble averaging over 5 members, (2) spatial averaging within the trajectory boxes, and (3) layer-thickness-weighted vertical averaging (0 – 13 km), also the inherent spread in cloud positions across ensemble members, where clouds do not occur at exactly the same locations despite similar synoptic conditions. This averaging dilutes the local peak values.

To better illustrate the actual range of QNC values, we have created Figure 1 showing the frequency distributions of QNC within the trajectory boxes. These reveal that the most frequent values reach ~300 cm^{-3} with maxima up to 700 cm^{-3} for case I and up to 1400 cm^{-3} for case II. These values are substantial and consistent with polluted continental conditions.

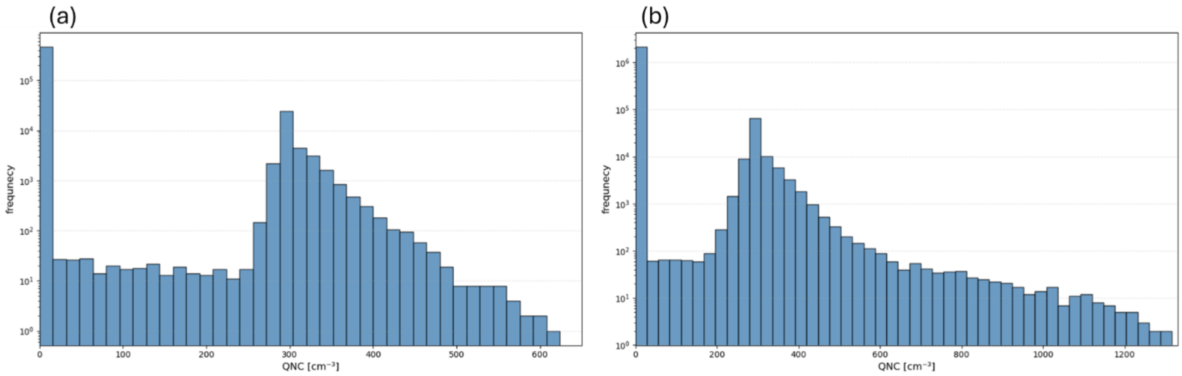

Figure1: Frequency distribution of QNC along trajectory boxes for case I (a) and case II (b).

While ensemble-mean QNC changes are small in Fig. 6, Fig. 7 in the manuscript shows that urban aerosols cause a vertical redistribution of hydrometeors within the cloud rather than uniform changes. This reorganization affects precipitation formation, as seen in the statistically significant changes in rain and graupel. The ~3% aerosol enhancement is sufficient to trigger these microphysical redistributions, which we believe is worth reporting.

11.Line 388: Could you please include a plot or two of the representative aerosol concentrations. Given your maximum droplet number of 45/cm3 (which is quite low and quite clean for a continental case) it would be good to know what fraction of aerosols are activating.

As discussed in our response to comment #10, the maximum droplet number of 45 cm^{-3} shown in Fig. 6 results from triple averaging (ensemble, spatial, and vertical) by which many zeros enter the averaging procedure. The actual QNC values are substantially higher, with most frequent values of ~300 cm^{-3} and maxima up to 600 cm^{-3} (case I) and 1300 cm^{-3} (case II) as shown in the frequency distribution figure. These values are representative of polluted continental conditions.

12.Line 403: It is unclear how Figure 10a indicates a convective system with a vertical extent reaching up to 13km. The feature in this figure panel tops out at 8-9km.

Figure 10a in the manuscript shows the number of activated cloud droplets (liquid phase), which indeed reach up to ~8-9 km. The full vertical extent of the convective system (up to 13 km) is evident when including the ice phase, as shown in Figure 10c in the manuscript. We have corrected the figure reference in the revised manuscript.

13.Line 405: It would be better to refer to the value of 1500 mg/m3 as a mass mixing ratio instead of a "total hydrometeor concentration". Further, mass mixing ratios are typically reported in units of g/kg.

We have revised the terminology to 'mass mixing ratio' and converted all hydrometeor values to mass mixing ratios with unit [g/kg]. All figures and text have been updated in the revised manuscript.

14.Line 412: What figure shows the higher number of rain drops? Also please use "rain drops" instead of "rain droplets".

We have corrected the terminology to 'rain drops' and added a reference to the supplementary material showing the rain drop number concentrations.

"At the time of peak precipitation (20:00 - 20:30 UTC), the BASE experiment shows significantly higher numbers of rain drops, reflecting enhanced rain processes during the most intense phase of the system (see Fig. S4)."

15.Line 414: A 10% increase in droplet number seems very small and is well below the change seen in most urban aerosol and convection studies. I still think it's overstated to call this an urban enhancement, when very substantial urban enhancements are noted in the literature.

We agree with this assessment. The observed changes are modest compared to megacity studies, which reflects our focus on realistic perturbations from a mid-sized city. We have revised the text to use more conservative terminology (e.g., 'increase' rather than 'enhancement') to avoid overstating the magnitude of the urban signal relative to the established literature.

16.Lines 460-462: Another key difference from the multi-model simulations in Marinescu et al. (2021) is that the aerosol loading from Clean to Polluted in that study was close to an order of magnitude difference. Here your differences are only 2-3%. I find it difficult to say these are comparable.

We have revised the text to appropriately reflect the substantial differences in aerosol perturbation magnitudes between the studies.

"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."

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