Review of *Tracking the Impact of Urban Air Masses on Convective Precipitation: A Multi-Member Modeling Study* by Keil et al. (2025)

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.

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.

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. 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.

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. 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 the urban aerosol showed changes.

If the authors can address this concern, I can suggest the manuscript for publication.

Below I list additional major and minor comments that the authors should address.

Major comments

1. I find the title to be somewhat misleading. Strictly, the study tracks the impact of urban aerosols on convective precipitation and not of urban air masses since the sensitivity experiments do not remove all urban influences. It might also make sense to add a geographical reference since aerosol impacts may vary substantially depending on location. Therefore, I suggest changing the title to "Tracking the Impact of Urban Aerosols on Convective Precipitation [over eastern Germany]: A Multi-Member Modeling Study". You can also consider other options.

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.

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.
    - Lines 155-158: Please show a figure of the meteorological characteristics of the two cases.
    - 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.
    - Lines 244-249: Figures might be needed to verify these statements about the convective development of the cases.
    - Lines 265-270, 296-301: Again, all these fields should be shown, such that the reader can verify the statements made here.
    - 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.

Minor comments

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

6. Line 20: Please add a reference for "[…] expected to further intensify precipitation events." or make the connection to the following sentence clearer.
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?
8. Lines 29 -31: Please add references for this sentence.
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.
10. Line 71: "[…] their findings […]", please clarify whose findings are talked about.
11. Line 85: "two-moment bulk microphysics scheme"
12. Line 90: I would say Leipzig is a "city" not a "town".
13. Lines 105-106: Please add references to studies using this standard configuration.
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.
15. Lines 134-136: The authors could make it clearer that this capability was specifically developed for this study, if that is the case.
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.
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.
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.
19. Lines 174, 202: DWD is referred to as German Weather Service but later as the German Meteorological Service.
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)?

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.
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).
23. Line 240: Please explain the selection of the significance level. I assume that it is because of the small sample size.
24. Line 272: “merging the cell at higher altitudes.” I am not sure what the authors mean here.
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.
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.
27. Line 306: The classification as a heavy precipitation event needs a reference.
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”.
29. Line 334: “relative to the NONURBAN experiment”, add figure reference.
30. Line 342: I would choose a different word than “significant”, since no statistical testing was done in this specific case.
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.
32. Line 370: “[…] intensifies the cloud dynamic.” This statement seems vague. Please clarify.
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.
34. Figures 7, 8, 9, 11: Please add “NONURBAN – Base” above panels (b) and (d) similar to Figures 4 and 5.
35. Line 406: I suggest removing “dominant” or replacing it with another word such as “strong”.
36. Line 407: I think “experimental” can be removed here.
37. Line 409: You probably mean Fig. 10b.
38. Line 436: “the rest being kept.” This sentence is not finished.
39. Line 441: Please clarify what “intensified the convective core” exactly means.
40. Lines 452-460: This discussion is very valuable, thank you.

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.
42. Lines 485-490: If possible, include links to these datasets.
43. Line 490: It might make sense to mention the specific AI tools.

Typographical

44. Line 175: “are the focus of this study.”
45. Line 178: “simulated from the coarser”
46. Line 203: “covering the entirety of Germany”
47. Line 252: “a precise agreement”
48. Line 304: “through”
49. Line 316: Missing period.
50. Line 322: “downwind of the urban source region”
51. Line 330: “In this case”

References

Fan, J., Zhang, Y., Li, Z., Yan, H., Prabhakaran, T., Rosenfeld, D., & Khain, A. (2025). Unveiling aerosol impacts on deep convective clouds: Scientific concept, modeling, observational analysis, and future direction. Journal of Geophysical Research: Atmospheres, 130, e2024JD041931.

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