Responses to the reviewers

Aerosol effects on convective storms under pseudo-global warming conditions: insights from case studies in Germany

by L. Lucas et al. December 4, 2025

We thank both reviewers and the editor for reading the manuscript again and providing detailed comments. We have carefully considered all comments and changed the manuscript accordingly. Please find below our responses in blue.

Comments from the editor

1. There is some jumping around in the abstract between responses to warming and CCN. For example, the sentence "In some cases, ... thermodynamic expectations." could be moved higher with the other warming-related content.
Thanks for this remark, we moved that sentence higher as suggested so that the warming-related content is together.
2. The caption of 8a) says "...using the continental CCN (C3) for case 1." Is this correct? Is C3 only used in case 1, or in all cases?
Thanks for pointing that out, the caption text for Fig. 8a was misleading with respect to the evaluation domains. Figure 8a shows only size distributions for continental CCN for case 1 and illustrates the response to different warming scenarios. Figs. 8b-d, however, show the results of all 4 CCN concentrations for all three cases. We rephrased the caption to make that clear.
3. It would be nice to have uniform colormap limits for panels b, c, and d of Figure 8.
Good point, we now use the same colormap boundaries for these figures.

Comments from Reviewer 1

Thank you to the authors for working through the extensive comments of both reviewers. I believe the manuscript improved greatly. For future review processes, I recommend to the authors to add the line numbers of the changes (from the final, revised manuscript) in the responses to the reviewers. This way, it is easier to see where the changes were made and how they now fit in the adapted manuscript. I refer to the line numbers of the final manuscript here.

We thank the reviewer for reading our manuscript again and providing additional feedback. As we uploaded a version with tracked changes, we did not include line numbers in our responses, but will do so in the future.

Major comments

- Domain averages: I agree with the authors that domain-averages smear out the signal, however, for convective storms it makes sense to first define a threshold, e.g., via total hydrometeor mass and then average, such that grid points with no storm are excluded. This should be considered when conducting domain averages (e.g., Figure 12).
We agree with the reviewer that adding a threshold to analyse cloudy grid points only would lead to a more distinct signal. However, if the number of clouds is different, the signal could lead to wrong conclusions as for example few strong convective clouds could dominate the signal. To be able to really compare, if more or less hail is simulated in a specific evaluation volume, the domain must be identical for all different sensitivity studies. Especially Fig. 12 with domain-averaged hail profiles could look completely different if only cloudy grid points had been used

for averaging. We therefore believe that the technique of using the entire evaluation domain is adequate.

- Heatmaps: The authors argued that additional numbers in the heatmaps (such as Figure 8) are not needed, which I still disagree with, however, at a minimum the same range for should be used such that a direct comparison across the three cases is possible. I would still say numbers should be add as well.
  We decided to follow the reviewer's suggestion and added the numbers in these plots. Moreover, we now use the same colormap range in all subplots.

Minor comments

- Line 2: emissionS regulations → emission regulations
  Done
- Line 100: The model description is missing the model time step as well as the output frequency.
  We added this sentence in L111-113 of the revised manuscript:
  "A time step of 10 s is used, and the data is written out every 30 min. In addition, data for tracking convective cells is written out every 5 min."
- Line 105: What are summertime INP concentration? Please add the number concentration, as it is kept constant.
  The INP concentration (immersion and deposition) depend on the temperature and relative humidity over ice, so there is not one value that we can give here. An equation for calculating the INP concentration together with profiles over Germany is given in Hande et al. (2015). This paper is already cited in the text where we state that summertime conditions are used. As the INP concentration remains constant, we believe that the reference to the Hande paper is sufficient.
- Line 134: The authors responded that CCN concentrations of 1700 cm^{-3} are rare in the response to reviewer 1, but in the main text they chose to still state that these are typical conditions. Please remedy that and just define C3 as the reference, which is also fine.
  In our reply to reviewer 1, we stated that the continental assumption represents typical values for central Europe and especially Germany, although very high CCN concentrations are rare in central Europe, as shown by the Schmale et al. 2018 paper. We refer to continental CCN concentrations as "high" and continental polluted as "very high", this is probably the misunderstanding here. In the manuscript, we clearly state that C3 (continental) is the reference: L138-139: *"The continental CCN concentration (C3) is chosen as the reference concentration, as this aerosol assumption represents typical conditions of central Europe (Hande et al., 2016; Costa-Surós et al., 2020)"*
- Line 215: What density is assumed for the hail particles?
  The density of pure ice used in ICON is 916.7 kg/m^3.
- Line 380: References missing
  Thanks for finding that mistake, we included the correct reference now.

Comments from Reviewer 2

The authors undertook a great effort to implement all comments. The manuscript is much improved, especially in terms of framing the findings and improving readability. A few points remain, they are detailed in the following:

We thank the reviewer for reading our manuscript again and providing additional feedback.

Major comments

1. Figs 3 and 4: part of my confusion stemmed from the fact that the colorbar extends to -300% frequency change, which should not occur as a relative change. Looking at the values in the Figures, it appears they actually never decrease under -100%. The colorbar should be cropped to avoid confusion.

   We believe that the reviewer refers to Figs. 4 and 5 with the 2d-histograms of vertical velocity. We agree with the reviewer that reductions by -300% do not occur, it was just our goal to have the white colors at zero. With this pre-defined colormap, the boundaries have to be between +300 and -300%. We now adapted the colorbar to show only the meaningful range between -100% and +300%.

2. Hail size discussion and observational references: While I understand that an absolute reproduction of observations is not the goal here, at least a literature-based discussion of absolute values is warranted. Figs R1 and R2 show that the max. hailsize does indeed appear realistic and I think it is important for the reader to get this impression from the manuscript. These figures could e.g. be added in a supplement. The same goes for observational comparisons from radar data or the field campaign. If anything, it is an opportunity to increase the credibility of the results, without claiming a verification of the simulation.

   We disagree with the reviewer at this point. Our main objective was to see how the hail size distribution changes with different CCN concentrations and higher temperatures. Comparisons to observed maximum hail sizes (which, by the way, are not available for all investigated cases here) is difficult as the upper end of the size distribution is limited for numerical stability reasons. We therefore decided just to investigate how the simulated most dominant hail sizes react to CCN and temperature modifications. Figs. R1 and R2 also did not show the maximum hail size, but the dominant one (the diameter where the size distribution has its peak). Including these figures in the paper would not really help the reader; therefore, we decided to add an additional statement in the text (L354-357) to make clear that the small values are an effect of averaging:

   "Because domain averages of the dominant hailstone sizes are computed from individual size distributions at each grid point, the mean values in Fig. 8b–d are comparatively small and always below 1 cm in diameter. It should be pointed out that larger dominant values up to the maximum extent of the hail size distribution do occur when their spatial distribution in the respective evaluation area is analysed (not shown)."

   As this work is not intended as a model evaluation, we believe that an intercomparison with Radar data or specific campaign data is not necessary. This would be the topic of an entirely new paper and does not fit into this manuscript anymore. However, we have compared the reference setup with radar data to ensure that the model reproduces the general precipitation patterns of the selected day, which we believe is an important basis for our sensitivity runs. This has been mentioned in section 2.3 about the analyzed cases. We do hope that our point of view is convincing.

3. Supercells vs all storms and updraft helicity: I don't quite follow, why the changes in UH cannot

be split into changes in storm number vs changes in UH intensity. A cell tracking has already been performed, so the number of cells meeting supercell criteria should be easily identifiable, as well as their respective mean UH values / UH areas. Sure, the statement on UH changes can be attributed to both cell number and intensity changes is valid, but it would be nice to explicitly state this here, given that the data is available. I am aware that the revised version focuses more on convection overall and has a less pronounced supercell focus.

Thank you for the comment and the opportunity to clarify this point. A cell-tracking algorithm was indeed applied in our analysis; however, it identifies all convective cell types, not only supercells. While this allows us to quantify the total number of detected convective cells, it does not directly separate supercells from other convective cells in a way that would allow us to robustly track changes in their UH characteristics alone.

Even if we were to extract the subset of tracked cells that meet the supercell criteria, an additional level of analysis would be required to disentangle whether changes in UH arise from (1) an increase in the number of supercells, or (2) changes in their structure, such as larger UH areas or more intense UH maxima.

Because UH depends not only on storm intensity but also on the spatial extent of rotating updrafts, answering this question rigorously would require computing and analysing cell sizes and UH-area metrics, which are not part of the existing tracking output. This goes beyond the scope of our current analysis, which in the revised version focuses more broadly on convection rather than on supercells specifically.

We therefore maintain that UH changes can arise from both storm-number and storm-intensity effects, but explicitly separating these contributions would require a dedicated analysis not included here.

4. Constant RH: Thurnherr et al. 2025 does show a decrease in RH of 3% for central Europe in the summer months, when the case studies take place. But this discussion can hinge on any number of climate models producing RH trends for central Europe. WCD - A pan-European analysis of large-scale drivers of severe convective outbreaks also shows a decrease of 1-3% per decade based on ERA5 trends. I don't want to convince you to cite this paper, but just point out that this decision warrants a justification or short discussion.

   Thank you for pointing this out. We have added a clarification in the Methods section explaining our choice to assume constant relative humidity in the PGW perturbations. Although several studies show decreasing summer RH trends over Central Europe, our PGW setup follows the standard approach in which specific humidity is recomputed under constant RH. This choice is intentional, as it isolates the thermodynamic effect of uniform warming. Since the aim of our study is to assess the sensitivity of convective storms to temperature perturbations and CCN concentrations, allowing RH to vary would introduce additional changes in moisture distribution and confound the interpretation. The new text has been added to Section 2.2. (L122-127)

Minor comments

1. Both The Effect of 3° C Global Warming on Hail Over Europe - Thurnherr - 2025 - Geophysical Research Letters - Wiley Online Library and NHESS - Insights from hailstorm track analysis in European climate change simulations were published at the time when the revised manuscript was submitted.
   Thanks for pointing that out, we included both references in the introduction.

2. Line 519: Feldmann et al. 2025 actually show no significant changes in updraft velocity. The way it is phrased currently, could be misleading what exactly this is referring to.

This was meant with respect to the likelihood of supercells. We rephrased the text (L530-533), it now reads: *"The increased likelihood of supercells found by our single case studies agrees well with recent climate simulations of Feldmann et al. (2025). By comparing a current-climate simulation with a pseudo–global warming scenario (+3 K), they found that the future climate simulation shows an average increase of supercell occurrence by 11%"*

3. There are still some instances of convection-resolving instead of convection permitting.
   Done