Responses to the reviewers

Pseudo-Global Warming Simulations Reveal Enhanced Supercell Intensity and Hail Growth in a Future Central European Climate

by L. Lucas et al. October 10, 2025

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

Reviewer 1

Summary. This study investigates the response of convective storms to CCN perturbations and a warming climate, with the help of a so-called pseudo-global warming approach. The authors selected three case studies of convective storms passing over Germany and employed the ICON model in 1 km horizontal resolution with a two-moment microphysics scheme. A strong focus was put on changes in supercells, hail growth, and the underlying microphysical processes within these clouds. Overall, global warming has a dominant effect on the evolution of convective storms, with aerosol–cloud interactions only playing a secondary role. The authors highlighted several aspects important to convective storms and their evolution. However, by having this holistic approach, I believe some structured and in-depth discussions were lost. I also think the authors should try to combine more of their findings in a concise explanation and figures, which currently seem more like a checked-off list. I am detailing my major and minor comments below.

Major comments.

1. Case studies: I have several questions regarding the selection of the case studies. First of all, the selected days are motivated by the MOSES campaign in Germany, and often the analysed domain is focused on that part of Germany. However, no comparison to observations were done, so why this underlying motivation? If the MOSES campaign is important, then why was the third case selected? It falls out of line in several aspects throughout the analysis and it makes it difficult to fully grasp the important aspects of the findings. This is, e.g., illustrated by varying y-axes in Figure 6, 10, and 13. Is the third case really helping the analysis? Because the authors often attribute the deviations to the synoptic forcing, which to some extent is not satisfying. Moreover, it is often not clear to me what part of the domain was now analyzed for which part, which ideally should not change (i.e., Germany or MOSES) because it also makes the comparison between the results trickier. If the MOSES domain is not crucial, I would recommend that for all results the whole domain of Germany is analyzed. If the MOSES domain is crucial, then the third case study does not contribute much to the scientific findings. Also, often figures are only for one case, and the next figure shows again all cases. A more consistent approach here would be helpful. Either first doing an overview for all three and then selecting one, or show all three cases in detail. Regarding the clarity of the writing: The authors switch between the naming of the cases, i.e., Case 1-3 or the dates, and the date format changes. I would highly recommend to find a clear naming convection and stick to that. Also Figure 2 could be improved if the border of Germany and the MOSES domain are highlighted in the left columns of subfigures.

   Thank you for these comments. We chose these three case studies as we were looking for convective cases with possible supercells in Germany. The two field campaigns, Swabian MOSES 2021 and Swabian MOSES 2023, were conducted by our institute, and these cases were (are) under investigation by analysing the data from the various instruments. The third case took place during Swabian MOSES 2023 and therefore fits in with the other days. These field campaigns helped to identify candidates for our sensitivity study. We do not compare our model data to these observations because this work is a sensitivity study about aerosol effects in a warmer climate. Other cases could have been selected in principle, but these storms are currently investigated by others on the basis of observations and this work is considered to be a useful complement to that. Thus, an in-depth model evaluation is out of the scope of the present study. However, it was necessary to judge the forecast quality of the reference run. The comparison to Radar-derived precipitation amounts showed that the ICON model can reproduce the general precipitation patterns of the selected cases and therefore serves as a good basis for our sensitivity runs. To make that clearer, we included a statement in section 2.3 about that:

"No systematic comparison with observations and no evaluation in this direction are carried out, as this is not the goal of the present study. The RADOLAN data are only used to ensure that the simulations reproduce the general precipitation pattern of the selected day. The study is designed as a sensitivity study, focusing on how the model responds to changes in the prescribed parameters."

The third case was also part of these field campaigns, but the main convective activity, including supercell storms, occurred in northern Germany. Therefore, we enlarged the evaluation domain to the Germany domain. As the other two cases had storms more in southern Germany, the use of a smaller evaluation domain (the MOSES domain) makes the individual differences much clearer in contrast to averaging over a larger domain, where large parts have no convective activity. In addition, a restriction to two case studies would further weaken the validity of our results. We also tried to add information about the used evaluation domain wherever necessary.

We also restrict some of the result figures to single cases only. We do that because the remaining days show similar results. Therefore, the results can be presented more concisely without too much repetition, which improves the readability of our manuscript. Whenever results are shown for one case only, we state in the text that the other cases behave in a similar way. We do not withhold any results, but only shorten their presentation.

As suggested by the reviewer, we refer to the days now as "Case 1" etc. consistently throughout the manuscript which hopefully also improves the clarity of writing. In addition, we improved Figure 2 by using a thicker line for the German border and by including the MOSES domain rectangle in the synoptic charts in the left column as suggested.

2. Significance: Given the set of ensembles for each case study, I am missing a quantification of the uncertainty of all the investigated quantities / processes. In the text, the authors often say these changes are significant or robust without offering much evidence. While there may be systematic changes, they do not necessarily have to be significant. I would recommend to conduct a proper statistical analysis for each investigated variable. This is notable, e.g., in Figure 5, but also others, often showing only the mean over a differing areas (Germany vs. MOSES). Also in Line 354 the authors state that the hail production significantly increases with CCN, but how was this assessed? With that I disagree with the concluding remark, that these findings are robust (which they very well may be) without a proper assessment. Another question I had is the impact of the windows for the moving averages for the cold pools. Given that the storms do not live longer than 10 hours (according to Figure 3), how is a temporal window of 8 justifiable, and also where do the 166 grid points come from?
We agree with the reviewer that our wording may have been misleading. What we mostly meant was a noticeable or large increase in a quantity or a systematic behaviour. We didn't mean that in a statistical sense. When presenting our results, we removed the term "significant" completely

to avoid confusion that we might be referring to statistical significance. The text on Line 354 refers to the strong reduction of the riming process if CCNs are increased. Here, too, we simply meant a significant reduction in this process, not in a statistical sense. The text now should make this clearer.

We also adapted the text regarding the term "robust". We only use it once in the analysis of the vertical winds to state that the same behaviour was found for all three cases. The conclusion section was rewritten, and "robustness of the findings" has been deleted.

For the investigation of cold pools, we used the same method as outlined in Hirt et al. (2020). In their study, the filter size of 166 pixels corresponds to approximately 200 km horizontally. This combination of spatial and temporal (8 hr) filtering enables the identification of large cold pools while background θ_ρ gradients at the coast are still sufficiently resolved to detect cold pools there. They also conducted sensitivity analyses with different spatial filtering scales and with/without temporal filtering, which did not show a strong influence on the qualitative behaviour of the results. We therefore used the same filtering criteria, although our marginally finer grid resolution results in a spatial filter of 166 km. The moving average of 8 hr is applied for every model output time step, so the entire life cycle of storms is investigated here. We have added more explanation in the method section concerning this point.

3. Storyline: The authors often talk about results and indicate "not shown". I do understand that not all results can be shown, but here I expect that a concrete storyline is built, which may also combine different figures into one, to not overwhelm the reader. However, the result sections actually starts with results that are not shown, which is not satisfying at all. Regarding the CAPE and CIN values, the authors could think of a table summarizing these numbers, or actually using the Appendix, but I would argue, that results that are not shown are definitely not the first result to be discussed. Furthermore, I believe the hail sections and their figures can be combined into one, as especially for hail the size is more important than the mass mixing ratio. Here, I actually do not understand Figure 7, as ICON with two-moment microphysics can well simulate larger hailstones than 1 cm in diameter. 1 What limits are discussed here and what curves have been extended? I do not see that in Figure 7. Restructuring the manuscript and the figures requires time, but this could be helpful to clearer convey the scientific findings. Regarding the introduction: here the sections on PGW (what exactly is perturbed in the IC and BCs mentioned in Line 29?) and convective invigoration should be extended, as in both important aspects and references are only mentioned later in the manuscript. Especially the convective invigoration is a highly debated topic, as the authors correctly state later in the manuscript, but they do not provide the proper context already in the introduction. Within the convective invigoration discussion, I would have loved to actually see changes in number concentrations of the hydrometeors, which to my surprise is not discussed at all, even though a two-moment microphysics scheme is employed. Moreover, the latent release heat could be quantified at least in terms of temperature changes within the cloud if the respective diagnostic is not available in the already produced model output. Regarding the conclusions: To me, they read more like a summary than a conclusion, as many aspects are repeated and not presented in a concise manner with clear take-home messages.

   We agree with the reviewer that the result section should not start with results that are not shown. As suggested, we added a table with CAPE and CIN values and also added more text on what to expect from these indices when a PGW approach with uniform temperature modification is applied. The figures 7 and 8 were also combined. The small hail sizes are due to the averaging over larger domains. Reviewer No. 2 also had questions regarding the hail sizes, please see our reply to his/her major comment 3. In short, the hail class has an upper limit for water mass of

0.005 kg, which corresponds to hail diameters of 23 mm. Larger hailstones cannot be simulated by ICON. Maps of the dominant hail size of each grid point demonstrate that even though larger hail can be simulated, the most frequent ones do not occur at the larger end of the distribution, and the domain average will be smaller. The main statement that we want to make here is how the size distribution changes with different CCN concentrations and higher temperatures. This trend in size is described and explained, also regarding the different effects of melting due to changes in the surface-to-mass ratio. We hope that the restructuring and text modifications make it clearer to the reader.

Concerning the introduction of the PGW approach: We believe that at this point, it is sufficient to state that initial and boundary conditions are modified. Later in the section with the ICON setup and modifications, we explicitly state that besides the atmospheric temperature, soil temperature, and soil surface temperature are adjusted as well by applying the same temperature increments as imposed on the atmosphere.

We also added some text about the convection invigoration theory in the introduction as suggested: *"However, this theory remains highly debated: while some studies report stronger storms under polluted conditions, others find weaker or negligible effects depending on model setup and environmental factors (Altaratz et al., 2014a; Fan et al., 2018; Igel and van den Heever, 2021). Moreover, Barthlott et al. (2022a) showed that ICON simulations do not confirm a systematic invigoration effect. This ongoing debate highlights the need to analyse aerosol impacts in parallel with thermodynamic changes."*

Changes in the number concentration of the hydrometeors and latent heating are both interesting aspects. However, we believe that we already cover a large number of convection-related and microphysics-related parameters in our manuscript, and these additional sections would blow up the paper unnecessarily. Due to the extensive changes already made to the manuscript, we will refrain from these additional analyses for the time being.

The last section is called "Summary and conclusions" and therefore presents both a summary and some conclusions. We added some text and made a number of text modifications to highlight the results more clearly and also to take into account the limitations of our method.

Minor comments.

1. Line 4: the acronym ICON is missing.
   Done.
2. Line 5: aerosol effects on what? from context it is clouds, but it should be specified
   We have rephrased and clarified the text by specifying that the aerosol effects refer to their influence on the simulated clouds and precipitation of the storm events.
3. Line 20: "the three-day period around 23 June 2021" could be explicitly stated: Is it from 22-24? Or 21-23?
   We have clarified the time period in the revised manuscript and now explicitly state that the hailstorms occurred from 22 to 24 June 2021.
4. Line 23: A reference would be nice for this event on 23 June 2021
   The reference originally cited in the preceding sentence applies to both statements. We added the reference again in the sentence for the 23 June 2021 event.
5. Line 86: it is not a triangular grid, but a icosahedral grid, which is trisected forming the triangles
   We thank the reviewer for pointing this out. We have specified the grid in a more precise way as an icosahedral-triangular Arakawa C grid.

6. Line 87: What is the height of the model top? This is an important information to have a better understanding of the vertical resolution.
We have added the information about the model top to the manuscript: "The vertical coordinates contain 100 terrain-following levels with a model top at a height of 22 km."

7. Line 88: SLEVE coordinates were introduced by Schär et al. 2002, so the original source should be cited
Done.

8. Line 108: Surface temperatures are adapted accordingly in what way? Is there a formula behind it, or is the, e.g., +1 K imposed? A clearer explanation would be great.
To avoid strong temperature gradients between the surface and the lowest atmospheric model level, the surface temperatures were adapted in the same way as the atmospheric temperatures, i.e., by applying the respective temperature increment. We have clarified this in the revised manuscript.

9. Line 112: I believe that 1700 cm^3 CCN are rather on the high side, as also shown by Schmale et al. 2018. It is fine to use the hardcoded values in the two-moment microphysics scheme, but I believe the chosen concentrations should be better contextualized. As far as I know, these concentrations emerge from Segal and Khain, 2006, and are based on some rather early measurements of CCN.
We rephrased the text and included another citation to make it clearer: four different CCN concentrations are available in the Segal-Khain scheme, and 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. As our focus lies on aerosol-cloud interactions, we believe it is fine to use the full possible range from low to very high CCN concentrations.

10. Line 150: wrong chapter reference → should be 3.2?
Done. We also changed "chapter" to "section".

11. Line 174: "To identify ..." sentence is doubled with the next sentence of the new paragraph.
To avoid redundancy, we have rephrased the second sentence. It now reads: "Helicity changes across the different temperature simulations are assessed by computing the mean values above this threshold and the number of grid cells that exceed it (Fig. 3)." This removes the duplication while keeping the intended meaning clear.

12. Figure 6 caption: what is meant by mm for the rain and hail? Is it a precipitation rate?
The values in mm for rain and hail refer to the daily accumulated amount (24-h totals), not precipitation rate. We have clarified this in the caption of Fig. 6, which now reads:
"Percentage change of the daily amount (24-h accumulated) of ... and the change in the area affected by daily totals of rain > 45 mm and hail > 10 mm ...".

13. Figure 8: Adding the numbers to the single tiles of the heatmap would help to grasp the figure in a faster way.
We thank the reviewer for this suggestion. However, we decided not to add the numbers to the individual tiles of Fig. 8, as we find that the colour shading already conveys the values clearly and adding numbers would likely reduce the readability of the figure.

14. Line 292-294: These two sentence basically say the same things, and can be combined.
We agree that the two sentences partly overlap and have therefore combined them into a single, more concise sentence. The revised text now reads: "This indicates that while CC scaling explains part of the observed precipitation increase, additional dynamical and microphysical

processes must also contribute to the deviations beyond the thermodynamic expectation for cases 1 and 3, necessitating further investigation to identify the mechanisms driving these super-CC precipitation trends."

15. Figure 10 caption: the domainS ARE defined ...
    Done.

16. Figure 11 caption: The ending in the caption, saying the reference simulations are with C3 does not make sense to me here. All CCN variations are shown in the plots for all integrated quantities, or am I misunderstanding something?
    All points represent deviations from the continental CCN assumption; therefore, the C3 deviation point is always at zero. Furthermore, all data points refer to the reference temperature scenario, as already mentioned in the text. We modified the caption text to make that clearer. Please note that in the version with tracked changes, this modification is not marked as blue due to technical reasons. The new caption reads:
    "Percentage deviations of spatiotemporal averages of autoconversion (AC), accretion (ACC), evaporation (EVAP), rain freezing (RF), melting (MELT), total riming (RIM), deposition (DEP) (left column) and of total column integrated cloud water (tqc), rain (tqr), ice (tqi), snow (tqs), graupel (tqg) and hail (tqh) amounts (right column) from the respective reference run C3 with continental CCN assumption. All points refer to the reference temperature scenario without increments. The Germany domain was used, which is defined in Fig. 1"

17. Figure 12: Where are these vertical profiles coming from? Are these based on mean values? The hail content seems very low.
    These vertical profiles represent averages over both time and space. As a result of this averaging, the hail content values appear relatively small. We have clarified this in the caption, which now specifies that the profiles are domain- and time-averaged.

18. Line 342: what is the 1 in (Fig. 11j, 1)?
    This was a typographical misunderstanding. The character is a lowercase "l," referring to subplot 11l, and not the number "1." Unfortunately, l and 1 look very similar and can easily be confused.

19. Line 364: I am wondering if the conclusion of "larger hailstones are more likely to reach the surface under low CCN concentrations" is circular, because under low CCN, the hailstones are larger. What is more important? So, the question is how does the melting rate change with CCN? From Figure 11, it looks more like that the melting rate is rather independent of the CCN concentration. I do not think that the sentence here is then fully correct.
    We thank the reviewer for raising this point. We agree that the conclusion in the original version was phrased too strongly and could give the impression of being circular. To address this, we have revised the text to clarify that the reduced melting efficiency of larger hailstones in low CCN environments is an interpretation based on their lower surface-to-mass ratio and on the vertical profiles of hail content. We also explicitly note that the model output only provides combined hail and graupel melting, so hail-only melting cannot be directly quantified. The revised paragraph now makes clear that the evidence for larger hailstones reaching the surface under low CCN conditions comes from the analysis of hail size distributions at the surface (Fig. 8), while the reduced efficiency of melting is presented as a consistent physical mechanism rather than a direct result of Fig. 11.

20. Line 368: A reference to the figure 11 should be made again from the melting rates.
    Done.

21. Line 385: The ratio of warm and cold rain and its dependence to the precipitation intensity is discussed in the next section, but the authors already mention it here, without giving much context or justification. I would move that to later.
This is a good suggestion, we merged the analysis of the cold and warm rain processes with the precipitation efficiency into a section now called "Rain formation processes and precipitation efficiency". We also added some more analysis text for the cold-to-warm rain processes, which was kind of short in the previous version.

22. Line 404: I disagree with the statement that this study investigates convective storms in Central Europe, because it does it only for Germany. This also then connects to the title, which in my opinion is misleading, as in principal Germany was looked at. The authors should consider adapting their title to that.
The entire simulation domain does not only cover Germany, but also parts of neighbouring countries (see Fig. 1). As we restrict the evaluation domain to Germany or the southwestern part of it, we agree with the reviewer that central Europe alone is not suitable here anymore. We changed the sentence to "in Germany". Also, the title of the paper was changed to "Aerosol effects on convective storms under pseudo-global warming conditions: insights from case studies in Germany".

23. Line 408: I also disagree with the statement that the chosen CCN concentrations are typical for Germany, which is not the case. These concentrations are coming from Segal and Khain, as elaborated above.
Please see our reply above. We study aerosol effects on clouds and precipitation and therefore use the full range of possible CCN concentrations available in the Segal-Khain scheme. What CCN concentration is typical is of minor importance here, but we provide two references stating that the continental assumption is typical for Germany. As this sentence is not essential here, we removed it and only mentioned the investigated range of different CCN concentrations in the sentence before.

24. Line 416: The results do not demonstrate any increase in CAPE and CIN as the authors decided not to show this. I elaborated my reasons for not doing this above.
We thank the reviewer for this comment. As requested, a table of CAPE and CIN values has now been added to the manuscript.

25. Line 448: The sentence "This intensification ..." is circular. Removing the last part "due to the heightened ..." would remedy that.
We agree with the reviewer that the original wording was circular and have revised the sentence accordingly. It now reads: "This intensification raises the risk of flash floods and poses a greater threat to property, agriculture, and infrastructure."

Editorial comments.

1. Line 10: wrong / incomplete latex command for the unit
Done.

2. Line 173: plotting is colloquial and should be avoided
Done.

3. Line 208: it is unusual to me to denote mixing ratios with r, maybe the authors can consider opting for a more common writing style
We agree that different notations exist for mixing ratios. To stay consistent with the definition of density potential temperature given in Hirt et al. (2020), which we explicitly cite here, we have retained the use of r to denote mixing ratios. We also note that this notation is commonly

used in the English-speaking literature.

References:

Schär, Christoph, Daniel Leuenberger, Oliver Fuhrer, Daniel Lüthi, and Claude Girard (2002). "A New Terrain- Following Vertical Coordinate Formulation for Atmospheric Prediction Models". In: Monthly Weather Review 130.10, pp. 2459–2480. ISSN: 1520-0493, 0027-0644. DOI: 10.1175/1520-0493(2002)130<2459: ANTFVC>2.0.CO;2.

Schmale, Julia, Silvia Henning, Stefano Decesari, Bas Henzing, Helmi Keskinen, Karine Sellegri, Jurgita Ovadnevaite, Mira L. Pöhlker, Joel Brito, Aikaterini Bougiatioti, Adam Kristensson, Nikos Kalivitis, Iasonas Stavroulas, Samara Carbone, Anne Jefferson, Minsu Park, Patrick Schlag, Yoko Iwamoto, Pasi Aalto, Mikko Äijälä, Nicolas Bukowiecki, Mikael Ehn, Göran Frank, Roman Fröhlich, Arnoud Frumau, Erik Herrmann, Hartmut Herrmann, Rupert Holzinger, Gerard Kos, Markku Kulmala, Nikolaos Mihalopoulos, Athanasios Nenes, Colin O'Dowd, Tuukka Petäjä, David Picard, Christopher Pöhlker, Ulrich Pöschl, Laurent Poulain, André Stephan Henry Prévôt, Erik Swietlicki, Meinrat O. Andreae, Paulo Artaxo, Alfred Wiedensohler, John Ogren, Atsushi Matsuki, Seong Soo Yum, Frank Stratmann, Urs Baltensperger, and Martin Gysel (2018). "Long-Term Cloud Condensation Nuclei Number Concentration, Particle Number Size Distribution and Chemical Composition Measurements at Regionally Representative Observatories". In: Atmospheric Chemistry and Physics 18.4, pp. 2853–2881. ISSN: 1680-7324. DOI: 10.5194/acp-18-2853-2018.