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 2

Summary: This manuscript presents an number of simulation experiments discussing supercell and hailstorm intensity under different initial conditions, by changing the surface temperature and CCN content. Three case studies are selected and experiments are performed at 4 different warming levels and 4 different CCN concentrations. The authors conclude that supercells become more intense and produce larger hail in warmer conditions. Possible reductions in CCN content due to reductions in pollution further modify these effects. While this study targets a subject that is highly relevant, it comes with many limitations and the manuscript currently overstates the conclusions that can be drawn from this. Moreover, the work is not well-situated within the broader literature. Established process changes for severe convection under climate change are not discussed properly in the context of the presented results (e.g. how increasing temperature at constant RH implies increased CAPE and CIN). While the study addresses an important topic and includes interesting case studies from a recent field campaign, I recommend major revisions before it can be considered for publication. The paper has clear potential, but currently suffers from several methodological, interpretational, and structural limitations that should be addressed to strengthen its scientific credibility and clarity. The major points are listed below, as well as line-by-line comments. These revisions are essential to ensure the study provides a robust and realistic contribution to our understanding of supercell and hail processes and their sensitivity to warming.

Major comments.

1. Simulation setup: While the uniform vertical warming with constant RH meets the technical definition of a PGW setup, this is a highly idealized and simplified approach. Combined with the fact, that this manuscript targets 3 case studies, no general conclusions on convective severity in a future climate should be made. Therefore, the title appears overstated with respect to the results provided. Instability changes in Europe are complex and tied to far more than uniform vertical warming. Changes in lapse rates and relative humidity are key to appropriately reflecting future instability situations in Europe. As such, the experiments performed here do not necessarily reflect a future climate state, but a generically "increased CAPE and CIN" scenario. These limitations are listed almost as an afterthought, in the last paragraph of the manuscript, which is not an appropriate location to detail essential methodological constraints.
   We agree with the reviewer that no general conclusions about the future of convective storms in Germany can be drawn from these three case studies. Our primary goal was to investigate the possible effects of aerosols on clouds and precipitation in a warmer climate and to demonstrate case-to-case variability and agreement. For this purpose, we believe that the PGW simulation strategy with uniform temperature change is a valid approach for short 24-h simulation periods of convective storms. To better reflect our goal and not to overstate our results, we changed the title of the manuscript to:

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

Aerosol effects have not been studied at this high model resolution using a sophisticated double-moment microphysics scheme with an explicit hail class so far. This is the main unique aspect of our work. To better reflect that and the limitations of our simulation strategy, new text has been added in the model description, and the end of the conclusions has also been rephrased.

2. Validation of simulations: Despite picking case studies from a field campaign, no observational comparisons are conducted. Germany provides both a radar-based hail size estimate, as well as a crowd-sourced report database. Case studies from neighbouring Switzerland of the same day are not referenced, or discussed as observational reference. Precipitation data is also not used as a comparison.
We thank the reviewer for this comment. No systematic comparison with observations and no evaluation in this direction are carried out, as a model evaluation is not the goal of the present study. The RADOLAN data were only used to ensure that the simulations reproduce the general precipitation pattern of the selected day in the reference configuration. This study is designed as a sensitivity study, focusing on how the model responds to changes in the prescribed parameters.

   To make this clearer, the following paragraph was added in Section 2.3 (Analysed Cases): "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."

   We thank the reviewer for the references to the case of 28 June 2021, which have been added to the manuscript.

3. Discussion of hail size: The presented mean hail size distributions raise questions as, commonly, hail is considered to have a minimum size of 5 mm, which is the value where most distributions peak here. While Fig. 7 mostly serves to illustrate changes in distributions, commonly hail size distributions are more analyzed with respect to their larger hailstones, not just the mean. And even for the mean, these sizes appear very small. Especially for cases like June 28, 2021, where in Switzerland hail size reports exceeded 8 cm and reports, photos and sensor measurements indicate mean hail sizes well beyond 5 mm. Fig. 8 further shows dominant hailstone size, which still yields sizes well below 1 cm. Without any discussion of the observed hail sizes and a comparison to this, this poses the question, whether a) these cases are representative of damaging hail cases in southern Germany, or b) whether the modeled output sufficiently captures this.
In the double-moment scheme of ICON, there exist lower and upper limits for the water mass of each hydrometeor particle class. Hail has a lower limit of 2.6E-09 kg and an upper limit of 0.005 kg. The possible hail diameters that can be simulated then range between 0.2 mm and 23 mm, larger hailstones are not possible. This also means that hail particles in ICON can be smaller than 5 mm due to these particle mass limits. We have now also added the upper limit to the text. To compute the actual hail size distribution, we enlarged the upper limit and found identical distributions, but extending to larger diameters. This procedure has already been mentioned in the text. However, the modal value of the distribution did not change. An important point to consider is the fact that Fig. 7 presents average hail size distributions over the MOSES domain: for each grid point, the size distribution was calculated based on the simulated number and mass densities; in a second step, we calculate a domain-averaged size distribution for the respective domain. We then take the modal value of this mean distribution

and average over the simulation time. These values are presented in Fig. 8b,c,d. To illustrate

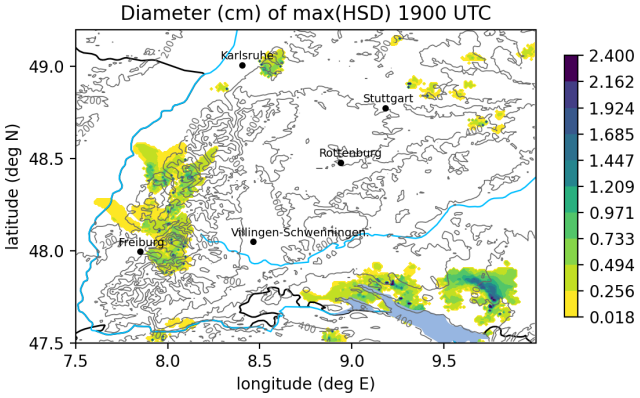

Figure R.1: Diameter of most frequent hail size on 23 June 2021 at 19:00 UTC.

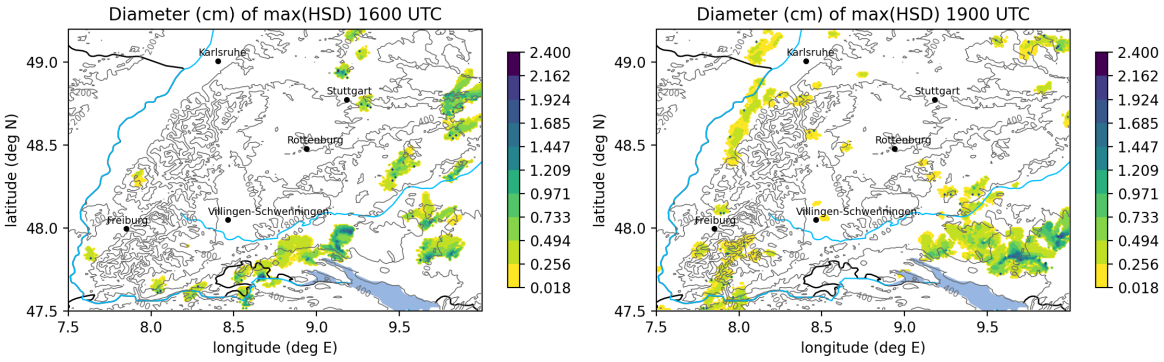

Figure R.2: Diameter of most frequent hail size on 28 June 2021 at 16:00 UTC (left) and 19:00 UTC(right).

that, we extracted the diameter of the most frequent hailstone size (the modal value) for each grid point. The examples for 23 June 2021 (Fig. R.1) and 28 June 2021 (Fig. R.2) demonstrate the simultaneous occurrence of smaller and larger hailstones, leading to comparatively smaller values when averaged over a larger domain. In both of these examples, dominant hail sizes up to the maximum possible values are simulated. The discrepancy to the mentioned observed large hail diameters can be explained by the fact that (i) maximum hail size is limited in ICON, (ii) the tail of the observed distribution cannot be compared to the most frequent one (the modal value), and (iii) the averaging over space and time in Fig. 8. This can explain the differences in the observed hail sizes. The important point is that ICON can simulate hail formation at all, even if the maximum hail size is limited for numerical stability reasons. The discrepancy in observed hail sizes is of minor importance. The main statement that we want to make here is whether and 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.

4. Discussion of hail size and climate change: In different parts of the manuscript, the combination of increased updraft strength and melting is discussed inconsistently. It is established, that higher-CAPE and hotter environments are expected to sort for larger hail sizes. Increased updraft strength and size increases the potential for larger hailstones. These are less affected by melting than the smaller ones. Hence, smaller stones are more likely to melt and decrease in occurrence at the surface, whereas large ones increase. Both of these aspects lead to shifts in the hail size distribution and do not contradict each other. This is not discussed coherently throughout the manuscript and not embedded in the current state of literature.

We thank the reviewer for this valuable comment. We have revised the manuscript to provide a more consistent and coherent discussion of how updraft strength and melting jointly affect hail size distributions under climate change. Specifically:

- **Section 3.2.1 (Hail sizes):** We clarified that stronger updrafts promote the growth of larger hailstones, which are less susceptible to melting, while simultaneously the rise in the freezing level promotes melting of smaller hailstones. This combined process results in a surface hail size distribution with fewer small stones and more large ones, consistent with Raupach et al. (2021).
- **Section 3.4.1 (Microphysical processes):** We added a parallel discussion showing that the same mechanism applies to changes in CCN concentration. In low CCN environments, larger hailstones form and are less affected by melting, while smaller hailstones are more likely to melt. We further noted that this mechanism is comparable to that observed in warmer environments, thereby unifying the interpretation of both temperature and aerosol effects.
- **Section 4 (Conclusions):** We included an explicit summary statement highlighting that stronger updrafts and melting processes act together to shift the hail size distribution toward larger stones, embedding our findings in the current state of literature Raupach et al. (2021).

We believe these changes address the reviewer's concern by making the discussion of hail size and climate change mechanisms consistent across the manuscript and better connected to existing literature.

5. Discussion of super-CC scaling: A brief discussion of where / how the additional precipitation falls would be very desirable in this context. Is it tied to more or longer lived convective storms? Is it tied to trailing stratiform precipitation, perhaps associated with the approaching fronts? Given that cell tracking is already performed on the data, this would provide meaningful additional information and reduce the remaining hypotheses in the manuscript.
We believe that we answer this question already in the manuscript, at least partly. The precipitation increase can mainly be attributed to larger CAPE values in a warmer climate, even if CIN is slightly rising as well. This has been documented in sections 3.1 and 3.2. The cell tracking revealed more numerous, but shorter-lived cells for higher temperatures, as outlined in section 3.3. Increased rain rates for higher CAPE and, as a consequence, faster depletion of CAPE were mentioned as reasons for that behaviour. The exact reasons for super-CC scaling of cases 1 and 3, however, cannot be given because of the non-linear character of microphysics and the stochastic nature of deep moist convection. We already presented possible reasons for super-CC scaling and references for them in the introduction. Now, some more text has been added in section 3.2.2 reflecting the difficulties of extracting reasons for super-CC scaling in short-term simulations of deep convection.

6. Discussion of INP vs CCN: The modeling experiments only touch upon the sensitivity towards CCN concentration. However, for hail formation pathways, INP concentration is a key element. E.g., hail seeding experiments aim to target INP concentration, rather than CCN concentration. At least a discussion of the conditions assumed for INPs / freezing processes should be included.
We focused only on CCN effects, because recent studies showed a smaller impact of INP concentrations on total precipitation than CCN concentrations (e.g., Wellmann et al., 2020). Furthermore, the investigation of INP effects in different CCN and global warming scenarios would have drastically increased computational costs. To make clear which INP concentrations were used, we added this information in the model description part of the manuscript.

Wellmann, C., Barrett, A. I., Johnson, J. S., Kunz, M., Vogel, B., Carslaw, K. S., and Hoose, C.: Comparing the impact of environmental conditions and microphysics on the forecast uncertainty of deep convective clouds and hail, Atmos. Chem. Phys., 20, 2201–2219, https://doi.org/10.5194/acp-20-2201-2020, 2020.

7. Differentiation of supercells from all convection: The study claims that supercells in particular are intensifying and producing more hail. From my understanding, most analyses were conducted in a set model domain and not stratified by storm type. This should either be clarified, or the interpretation should be adapted. Given the importance of shear for convective organization and supercell development, the shear conditions should also be discussed somewhere. I am aware that these experiments do not systematically modify the initial wind field, however it is implied several times that the synoptic forcing plays out differently in the warming scenarios, which would, in turn, also affect the vertical shear profile and the potential for convective organization.
   We thank the reviewer for this helpful comment. We have revised the manuscript to clarify the distinction between supercell-specific diagnostics and domain-wide evaluations. In the Introduction, we now explicitly state that while the case studies were chosen because they featured supercells, domain-mean analyses also capture the behaviour of other convective modes, such as single and multicells. To make this distinction clearer in the methods, we introduced a new subsection (Evaluation techniques), which emphasises that diagnostics such as cold pools, domain-mean precipitation, and convective cell numbers or lifetimes reflect the combined convective population within the model domain.

   In addition, we have expanded the Results section to acknowledge the role of vertical wind shear in supercell organisation. We clarified that in the PGW framework used here, wind profiles are not systematically modified, so shear conditions remain close to the reference synoptic situations. Consequently, the changes reported for updraught helicity mainly reflect thermodynamic influences, while potential shear-related modifications are not represented in this study. To quantify wind shear and the potential for different storm types, we now include the deep-layer shear (DLS) of the reference simulation in the description of the cases. Domain averages indicate suitable conditions for supercell formation for all investigated days (case 1: 18 m/s; case 2: 20 m/s; case 3: 19 m/s).

   We believe these revisions address the reviewer's concern by making it explicit which results pertain to all convection versus those most relevant to supercells, and by embedding a clear statement on the role and treatment of vertical shear.

8. Clarity of results: While the study follows 3 case study, single cases are highlighted at different points and it is not quite clear, why which case is focused on where. Overall, the manuscript lacks a clear storyline and presentation, seeming more like a list of results. This does not require large content changes, but rather an adaptation of the framing to make the main points more clear to the reader. Personally, I would find it helpful to have all methods in the methods section, as opposed to the beginning paragraph of each results block.
   Most of the results are presented for all three case studies, but some results are only shown for individual ones if the other cases show similar behaviour for the sake of brevity (e.g., 2d-histograms of vertical wind in Fig. 4 only for case 2). We always mention that other cases show similar results in the manuscript. As suggested by the reviewer, we moved all technical details of our investigation to a separate part in the methods section. We believe that the readability of our manuscript is enhanced.

9. Missing literature discussion: A significant portion of recent severe convection literature is not

referenced or discussed in this study. A list of relevant studies is provided below, with a short note of their respective context. This contains a certain amount of Swiss literature, as the selected region is very close to the border and these studies generally extend into southern Germany, to the extent that the Swiss radar network provides coverage.
We thank the reviewer for this long list of suggestions and included most of them in our manuscript. However, we consider not all papers to be relevant for our study, and several unreviewed works have been omitted, which could be included at a later stage. Please see our individual comments below. Our reference section was already quite large and now even covers 96 entries.

References:

1. https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/wea.4306 Detailed case study of June 28, 2021 in Switzerland (synoptic discussion, radar data and hail reports include southern Germany)
   Thanks for this very useful reference, we cited it in the description of our cases.

2. https://wcd.copernicus.org/articles/6/645/2025/ Ensemble model study of June 28, 2021
   Has been included at the same place.

3. https://arxiv.org/abs/2503.07466 (in press) Modelled current and future climatology of supercells in Europe, including hail size and environmental analysis (see heterogeneous evolution of instability in Europe)
   Has been included.

4. https://www.nature.com/articles/s41558-023-01852-9 Wind gust and downdraft change with climate change (thermodynamical reasoning for increasing downbursts, relevant for Fig. 4)
   Given the already extensive list of references, we do not consider this citation to be necessary.

5. https://egusphere.copernicus.org/preprints/2024/egusphere-2024-3924/ (accepted) Influence of Sahara dust on hailstorms in Europe
   Sahara dust acts as INP and is therefore relevant for hail formation. In our study, we do not consider changes in INP concentrations and therefore do not see a compelling reason to cite this paper.

6. https://journals.ametsoc.org/view/journals/mwre/152/2/MWR-D-22-0350.1.xml Topographic effects on supercells, including experiments with varying CAPE
   The suggested study investigates the effects of lakes in mountainous terrain on the evolution of supercell thunderstorms and the influence of orography on both storm intensity and occurrence frequency. As we do not focus on convection-initiating mechanisms and geographic impacts, we do not include a reference to this paper.

7. https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2024JD042828 Modeled hail climatology of Europe
   We now cite this paper in the introduction.

8. https://www.authorea.com/doi/full/10.22541/au.173809555.59545480 Modeled future hail climatology of Europe, including associated precipitation and environmental changes (see also moisture trends)
   This is a pre-print which has not been peer-reviewed yet. May be included at a later date if it is accepted for publication.

9. https://www.nature.com/articles/s41612-023-00352-z Observational reference of severe thunderstorm types – including how many supercells have hail and vice versa
   Given the already extensive list of references, we do not consider this source citation to be

necessary.

10. https://egusphere.copernicus.org/preprints/2025/egusphere-2025-918/ (in press) Changing hailstorm environments with climate change
    This is still a pre-print and could be included at a later date.

11. https://journals.ametsoc.org/view/journals/apme/62/11/JAMC-D-22-0195.1.xml Hail trends based on environmental changes (ERA-5)
    Has been added in the introduction.

12. https://www.researchsquare.com/article/rs-6196143/v1 Very large hail trends
    This article has not been reviewed yet, we therefore prefer not to cite it. Could be inserted at a later stage.

13. https://www.sciencedirect.com/science/article/pii/S0169809520311224 https://www.sciencedirect.com/scie
    Supercell climatology Germany
    Has been added in the introduction.

14. https://www.sciencedirect.com/science/article/pii/S0169809516306020 Hailstorms and supercells in Germany
    We do not consider this paper relevant to our study.

15. https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019RG000665 Review on relevant processes for hail formation and hail in a changing climate
    Has been added to the introduction.

16. https://link.springer.com/article/10.1007/s00382-024-07227-w Modeled European hail climatology
    We do not see a compelling reason to cite this paper.

17. https://amt.copernicus.org/articles/17/7143/2024/amt-17-7143-2024.html Normalization of hail size number distributions (possibly a useful reference for the conversion of microphysical output to HSD)
    In the proposed study, the double-moment normalization has been used to model the shape of a series of hail size number distributions collected by a network of automatic hail sensors. We, however, compute the hail size distribution from the simulated mass and number densities directly and outline the equations used in the methods section, together with references for them. We therefore do not see a reason to include this additional reference in our manuscript.

18. https://wcd.copernicus.org/articles/2/1093/2021/wcd-2-1093-2021.html Expected changes in summer lapse rates
    This reference has also been added to our manuscript.

Line-by-line remarks.

1. All figures: The standard color scheme used for all discrete color classes is not very color blind friendly. I would recommend to switch to something at least without red-green contrast, such as the IBM color scheme (https://lospec.com/palette-list/ibm-color-blind-safe)
   We thank the reviewer for this important suggestion. In the revised figures, we have replaced the original colour scheme with the Okabe–Ito colour universal design palette, which was specifically developed to be unambiguous for people with the most common forms of colour vision deficiency. This palette avoids problematic red–green contrasts while providing eight distinct, colourblind-safe hues. The palette is described in Okabe & Ito (2008) (Colour Universal Design (CUD): How to make figures and presentations that are friendly to colorblind people, https://jfly.uni-koeln.de/color/

2. Line 10: format issue with unit[400]%
   Done.

3. Line 63: Acronym ICON not defined / only defined later in line 73
   We removed the mentioning of the ICON model here and included the acronym definition at the first occurrence in the abstract as well as in the remaining text.

4. Line 73: 1 km is not fully convection-resolving, ideally convection-permitting should be used.
   Done.

5. Line 76: acronym 2MOM is only used once, please consider replacing with double-moment
   Done.

6. Section 2.2: While the methodology is laid out in detail here, its limitations need to be discussed more thoroughly. Overall, to embed the manuscript better in the literature, it may be beneficial to include a dedicated discussion section.
   We added some text here to address the limitations of our approach. In addition, numerous references were included throughout the manuscript as recommended by the reviewer.

7. Fig. 1: What was measured in Villingen-Schwenningen and how does it relate to the simulations?
   The reference to Villingen-Schwenningen has been removed, as measurements from this site are not directly relevant to the scope of the present study. To clarify the role of observational data in this work, a new paragraph has been added in Section 2.3 (*Analysed Cases*). The revised text explains that no systematic comparison with observations and no evaluation in this direction are carried out, since this is not the goal of the study. The RADOLAN data are only used to ensure that the simulations reproduce the general precipitation pattern of the selected day. This clarification also highlights that the study is designed as a sensitivity study, focusing on how the model responds to changes in the prescribed parameters. Furthermore, we improved Fig. 1b by showing fewer cities, removing the rivers, and enlarging the names of the mountains.

8. Fig. 2: The colorbar for precipitation should be cropped around 60
   There was a mismatch between the colorbar and the precipitation plots in the initial submission. This has been corrected so that the colorbar now accurately represents the plotted precipitation values. Therefore, no cropping of the colorbar is necessary anymore.

9. Line 156: CIN was already defined. Please check this for all acronyms, there are multiple inconsistencies.
   Done.

10. Line 160: CAPE and CIN changes depend heavily on the chosen experiment, especially on changing lapse rates and RH. Maintaining lapse rates and RH basically mandates an increase in both CAPE and CIN, as the moist adiabat steepens, while everything else remains more or less constant. Moreover, the dewpoint increases less than the temperature, leading to an increase in LCL.
    Thanks for this comment, we included more comments on that dependence in the manuscript. Moreover, a table with CAPE and CIN values has been added, as demanded by the other reviewer.

11. Fig. 3: and corresponding analysis: The value of 75 m2/s2 from Ashley et al., 2023, was established on 4 km resolution data. Updraft helicity is very strongly impacted by horizontal resolution. To have the same criteria, this value needs to be adjusted in relation to the resolution (this is also mentioned in the methods of Ashley et al., 2023). If the same threshold at a higher resolution is desirable, it should be justified differently.
    The sentence in the manuscript has been revised to provide further clarification. While the

threshold of 75 $m^2 s^{-2}$ in Ashley et al. (2023) was established on 4 km resolution data, the same value is retained here despite the higher resolution of 1 km, since the focus of this study is not on the exact threshold itself but on the relative changes. In addition, an analysis of the 99.95th percentile, similar to the approach of Wang et al. (2022), yielded values close to 75, which further supports the use of this threshold in the present work.

12. Fig. 4: I do not fully understand what is depicted in Fig. 4. What exactly is evaluated in the reference and warming simulation? And how are the percentage changes computed?
Figure 4 illustrates the relative changes in updrafts and downdrafts between the reference simulation and the warming experiments. More precisely, it shows frequency differences of vertical velocity as a function of height and wind magnitude for different warming scenarios with respect to the reference temperature evaluated over the MOSES domain and the full 24-h simulation period. We have adapted the Figure caption to make it clearer. Due to technical reasons, this modification does not show up in the version with tracked changes. Positive values indicate an increase under warming, and negative values indicate a decrease.

13. Line 199: Why is the CCN sensitivity not shown? At least this should be included in an Appendix or Supplement. The following description of the vertical structure is not very clear without any supporting material.
We have now included the CCN sensitivity plots in the main text (new Fig. 5), which provide supporting material for the description of the vertical structure.

14. Line 204-214: All descriptions of methodology / computations should be contained in the methods section.
We thank the reviewer for this comment. All methodological descriptions have now been moved to the Methods section to ensure consistency in the manuscript structure.

15. Line 228: Considering that the experiment does not include changes to the large-scale dynamics, could you please elaborate how the synoptic forcing is expected to change?
The text has been revised to clarify that the imposed warming experiments do not modify the large-scale dynamics, and that the prevailing synoptic environment of each analysed day remains unchanged. In addition, a short discussion was added on how synoptic forcing is expected to change in a warmer climate (e.g., poleward shifts of storm tracks and jets, and changes in cyclone and frontal characteristics; Harvey et al., 2020; Priestley and Catto, 2021).

16. Fig. 6: Given the constraint on xmax for the derived hail size distribution, how does this affect the possibility to meaningfully calculate the 95th percentile of the HSD?
There seems to have been a misunderstanding: Fig. 6 does not show the hail size distribution but rather the daily amounts (24-h accumulated) of precipitation in mm. To avoid confusion, the caption of Fig. 6 was revised accordingly and now explicitly states that daily totals of rain and hail are analysed, including the 95th percentile of these accumulated amounts.

17. Lines 245-257: Assuming the same method is used to obtain Fig. 6, this methodological explanation should be earlier, ideally in the methods section.
As clarified above, Fig. 6 does not present hail size distributions but rather daily accumulated precipitation amounts (rain and hail in mm). As these quantities simply represent daily accumulated precipitation amounts, we believe that no additional methodological explanation is needed, and therefore, a revision of the methods section is not required.

18. Line 261: Enhanced melting does not immediately mean shifting the distribution peak towards smaller sizes, as it affects small stones more than large stones and can skew the HSD towards the tail. Moreover, hotter conditions are often related to greater instability and greater updraft

speeds, especially in this simulation setup, where RH is kept constant and higher temperatures basically just increase CAPE.

We thank the reviewer for this helpful clarification. We agree that the original formulation was misleading, as it implied that enhanced melting necessarily shifts the hail size distribution peak toward smaller sizes. In the revised manuscript, we have removed this statement and instead clarified that a rise in the freezing level primarily enhances the melting of smaller hailstones, while larger hailstones produced in stronger updrafts are less affected. This distinction avoids the previous confusion and is now consistent with the subsequent discussion of hail size distributions and with the current state of the literature (e.g., Raupach et al., 2021).

19. Line 283: Is a vapor content increase close to CC in line with climate projections for central Europe? While moisture trends have much greater uncertainty than temperature trends, a number of models suggest a drying throughout much of Europe, including southern Germany (e.g. Fig. 4 in Thurnherr et al., 2025). This needs to be discussed somewhere, as a constant RH can not immediately be presumed to be representative of a future climate state.

    The mentioned Thurnherr paper seems not to be published yet; we only found a preprint which is not citable. However, we found a new publication by Feldmann et al. (2025) that apparently uses the same data set. They compare a current climate simulation with a pseudo–global warming +3°C global warming scenario and find that the future climate simulation shows an average increase of supercell occurrence by 11%. However, there is a spatial dipole of change with strong increases in supercell frequencies in central and eastern Europe and a decrease in frequency over the Iberian Peninsula and southwestern France. In central Europe, 2-m specific humidity increases, so that 2-m relative humidity only slightly decreases, which would be similar to our approach with constant relative humidity. We like to point out that our focus lies on case studies on aerosol–cloud interactions with sophisticated microphysics, including a separate hail class for a detailed process understanding. We included this reference and some remarks on why we cannot study long-term impacts on humidity at the end of section 3.2.

20. Line 289ff: Can you visualize the precipitation associated with the front and with convection to support your hypothesis?

    We changed the sentence from "a pronounced synoptic front" to "in the presence of moderate to strong large-scale synoptic ascent supporting convection initiation (see Fig. 2e)."

    It is not possible to separate the precipitation associated with the front and with convection as they are connected. The precipitation distribution resembles more a squall line than stratiform frontal precipitation and agrees quite well with Radar observations (Fig. R.3).

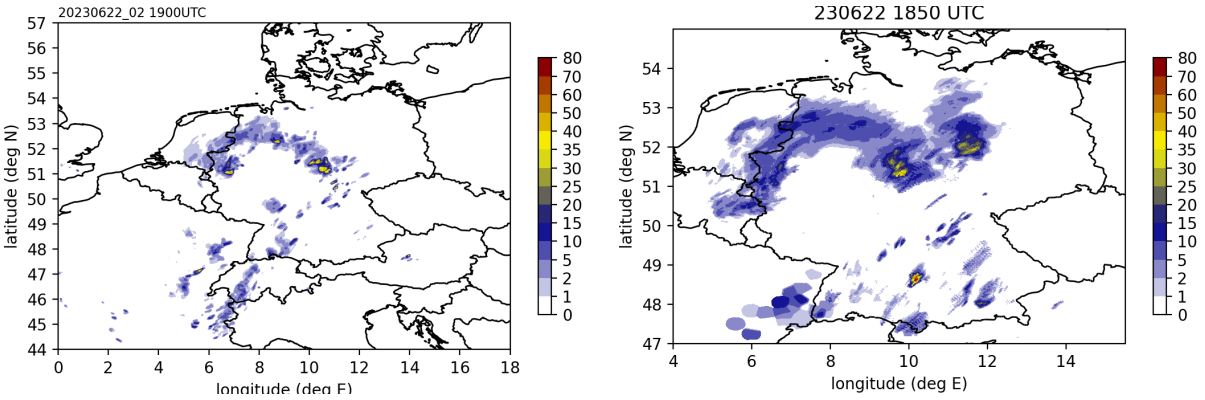

Figure R.3: 30-min precipitation rate from ICON reference run (a) and radar-derived 1-h precipitation amount (b).

21. Section 3.3: The tracking methodology should be detailed in the methods. How are splits and mergers handled, how are they "counted" for the cell number? Tracking algorithms can be quite sensitive to this and hence also the total cell number. The mentioned behavior of faster convective lifecycles may apply to multicell and single-cell thunderstorms, but should not apply to supercell thunderstorms (the initial main focus of this study), as they normally propagate along long tracks, as long as the encountered environment remains favorable. While the listed explanation may apply to less severe storms occurring in the model domain, this differentiation should be made clear.
The description of the Tobac tracking methodology has now been moved to the Methods section, and additional detail has been added regarding how splits and mergers are handled. Specifically, Tobac links features between consecutive time steps using a nearest-neighbour and overlap-based approach. When a feature splits into multiple successors, the trajectory continues along the largest successor while the others are recorded as new tracks. In the case of mergers, the trajectory of the largest predecessor is continued, and the additional features are terminated. In this study, the number of convective cells, therefore, refers to the number of unique tracks initiated during the simulation period.

    We agree with the reviewer that supercells usually propagate along long tracks, as long as the encountered environment remains favorable. In our tracking of convective cells with Tobac, we do not differentiate between different types of convection; all cells are tracked, independent of their lifetime or track length. By doing this, we find on average a reduction in the lifetime of cells. We also restricted the computation of the averages to lifetimes greater than 120 minutes and found the same systematic behaviour. So our statement that convective storms on average have shorter lifetimes is valid. Furthermore, the formation of supercells can be strongly dependent on the aerosol load, as different CCN concentrations can determine whether a supercell is successfully simulated or not (Barthlott et al., 2024).

    We modified our text to make it clear that this finding is valid for all detected convective cells and not specifically for supercell storms.

22. Line 361: Here the smaller sensitivity of larger hail to melting is mentioned, but this should be consistently discussed throughout the manuscript sections.
Such a statement is now written at all suitable places, i.e., in section 3.2.1, 3.4.1, and in the summary.

23. Line 378: Could the relevance of the synoptic forcing be verified by providing a spatial visualization of the cold-to-warm ratio? The presence of the forcing is mentioned in many hypotheses throughout the manuscript, without attempt to verify them.
We thank the reviewer for this helpful comment. The reference to synoptic forcing in this context was based only on an assumption and may not be directly related to the frontal system. To avoid misinterpretation, this statement has been removed from the manuscript. We particularly like the idea of a spatial visualization of the cold-to-warm ratio, which will be pursued in future work.

24. Line 386: Is there a specific reason to choose the unit mm / 30 min?
Yes. The model output is available at 30-minute intervals. Consequently, precipitation amounts are reported in mm per 30 minutes, which directly reflects the model's temporal resolution.

25. Section 3.4.3: It is a bit unclear to me, which point exactly is being made here. If precipitation efficiency increases, but generation decreases and hence total precipitation decreases, why are not all 3 terms discussed in combination here? Why does precipitation efficiency require a separate analysis?

Section 3.4.3 has been revised to clarify that precipitation efficiency (PE) is not an independent measure but a diagnostic linking total precipitation (P) and hydrometeor generation (G). We also merged this section with section 3.4.2 about the cold and warm rain processes, as the same processes are analysed there. This was also a suggestion from the other reviewer. The combined text now explains that PE is discussed separately because it provides insight into how efficiently generated condensate is converted into surface precipitation, highlighting microphysical processes such as evaporation and melting. We also make explicit that the decrease in precipitation under higher CCN concentrations results from the combined effect of decreasing G and increasing PE.

26. Line 405: Consider replacing convection-resolving with km-scale or convection-permitting
Done.

27. Line 416: Increasing temperatures vertically homogeneously, while maintaining constant RH, basically mandates an increase in CIN and CAPE. This is inherent to the simulation setup and not a finding. The CAPE and CIN conditions, as well as their changes are not shown in detail in the first place, results that are not shown should not constitute a main point of the conclusion.
A table containing CAPE and CIN values has been added to the manuscript to provide a clearer basis for this discussion. While the general increase in CAPE and CIN is indeed inherent to the PGW setup with vertically homogeneous warming and constant RH, the results also show case-to-case variability and nonlinear interactions with other processes. For this reason, we included these analyses to test and illustrate how such interactions manifest in the simulations.

28. Line 418ff: Changes like this are highly regionally dependent and should not be generalized to this extent. What is exactly is meant by stronger storms? Which variable?
To avoid overgeneralization and to be more precise, the sentence has been revised. The new formulation emphasises that simultaneous increases in CAPE and CIN imply the need for stronger triggering mechanisms to initiate convection, which in turn favours the development of fewer but more intense convective systems, rather than broadly referring to "stronger storms."

29. Line 421ff: Supercell frequency and intensity are two different matters and should be discussed separately. Frequency overall cannot be addressed by case studies, as the frequency of supercell-favorable days cannot be deduced from this. The simulations have the data necessary to identify whether UH is increasing in intensity per storm, increasing its area per storm, or if there are more storms. Ideally this should be addressed quantitatively and not left to speculation.
We agree with the reviewer that no trends regarding the future occurrence of supercells can be derived from our work as the number of days with suitable atmospheric conditions can not be assessed. Simulations in climate mode are necessary for that, e.g., the study of Feldmann et al. (2025), which is also cited in our manuscript. The question about more storms has been answered by tracking each convective cell with the Tobac tracking tool in section 2.4.3. However, we did not differentiate between different types of convection and included averages of the number of detected cells and the mean lifetime over all tracked elements. Focussing on longer-lived storms did not change the characteristic dependence; please also see our reply to the comment on Section 3.3. We present only mean values of helicity and state that the increase in the mean value can be caused by more supercells, more intense storms, or a combination of both. We believe that this statement is valid in its current form. We also find increased rain intensities in the warming scenarios, which supports this statement. To assess the UH of each individual storm is a difficult task and would require selecting the thermodynamic environment of each storm during the entire lifecycle. Since our focus is more on the interactions between aerosols and clouds in a warmer climate, this comprehensive work would go beyond the scope of the present study. We therefore limit ourselves to the previous statement that the UH increase

can be caused by more supercells, more intense storms, or by a combination of both.

30. Line 447: This should be rephrased to reflect the limitations of the PGW setup and the nature of case studies.
We rephrased the end of the conclusions to better reflect the limitations of our simulation strategy.