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

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.
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.
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.
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.

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.
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.
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.
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.
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.

- 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)
- https://wcd.copernicus.org/articles/6/645/2025/
  Ensemble model study of June 28, 2021
- 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)
- 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)
- https://egusphere.copernicus.org/preprints/2024/egusphere-2024-3924/ (accepted)
  Influence of Sahara dust on hailstorms in Europe
- https://journals.ametsoc.org/view/journals/mwre/152/2/MWR-D-22-0350.1.xml
  Topographic effects on supercells, including experiments with varying CAPE
- https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2024JD042828
  Modeled hail climatology of Europe

- 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)
- https://www.nature.com/articles/s41612-023-00352-z
  Observational reference of severe thunderstorm types – including how many supercells have hail and vice versa
- https://egusphere.copernicus.org/preprints/2025/egusphere-2025-918/ (in press)
  Changing hailstorm environments with climate change
- https://journals.ametsoc.org/view/journals/apme/62/11/JAMC-D-22-0195.1.xml
  Hail trends based on environmental changes (ERA-5)
- https://www.researchsquare.com/article/rs-6196143/v1
  Very large hail trends
- https://www.sciencedirect.com/science/article/pii/S0169809520311224
  https://www.sciencedirect.com/science/article/pii/S0169809515002719
  Supercell climatology Germany
- https://www.sciencedirect.com/science/article/pii/S0169809516306020
  Hailstorms and supercells in Germany
- https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019RG000665
  Review on relevant processes for hail formation and hail in a changing climate
- https://link.springer.com/article/10.1007/s00382-024-07227-w
  Modeled European hail climatology
- 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)
- https://wcd.copernicus.org/articles/2/1093/2021/wcd-2-1093-2021.html
  Expected changes in summer lapse rates

Line-by-line remarks:

- 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)
- Line 10: format issue with unit[400]%
- Line 63: Acronym ICON not defined / only defined later in line 73
- Line 73: 1 km is not fully convection-resolving, ideally convection-permitting should be used.
- Line 76: acronym 2MOM is only used once, please consider replacing with double-moment
- 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.
- Fig. 1: What was measured in Villingen-Schwenningen and how does it relate to the simulations?
- Fig. 2: The colorbar for precipitation should be cropped around 60
- Line 156: CIN was already defined. Please check this for all acronyms, there are multiple inconsistencies.
- 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.
- 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.
- 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?

- 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.
- Line 204-214: All descriptions of methodology / computations should be contained in the methods section.
- 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?
- Fig. 6: Given the constraint on xmax for the derived hail size distribution, how does this affect the possibility to meaningfully calculate the 95^{th} percentile of the HSD?
- Lines 245-257: Assuming the same method is used to obtain Fig. 6, this methodological explanation should be earlier, ideally in the methods section.
- 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.
- 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.
- Line 289ff: Can you visualize the precipitation associated with the front and with convection to support your hypothesis?
- 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.
- Line 361: Here the smaller sensitivity of larger hail to melting is mentioned, but this should be consistently discussed throughout the manuscript sections.
- 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.
- Line 386: Is there a specific reason to choose the unit mm / 30 min?
- 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?
- Line 405: Consider replacing convection-resolving with km-scale or convection-permitting
- 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.
- 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?
- 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.

- Line 447: This should be rephrased to reflect the limitations of the PGW setup and the nature of case studies.