Review for "Pseudo-Global Warming Simulations Reveal Enhanced Supercell Intensity and Hail Growth in a Future Central European Climate" by Lucas et al., ACPD

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

- **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.

- **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?

- **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.

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.

Minor comments

- Line 4: the acronym ICON is missing.
- Line 5: aerosol effects on what? from context it is clouds, but it should be specified
- Line 20: "the three-day period around 23 June 2021" could be explicitly stated: Is it from 22-24? Or 21-23?
- Line 23: A reference would be nice for this event on 23 June 2021
- Line 86: it is not a triangular grid, but a icosahedral grid, which is trisected forming the triangles
- Line 87: What is the height of the model top? This is an important information to have a better understanding of the vertical resolution.
- Line 88: SLEVE coordinates were introduced by Schär et al. 2002, so the original source should be cited
- 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.
- 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.
- Line 150: wrong chapter reference → should be 3.2?
- Line 174: "To identify ..." sentence is doubled with the next sentence of the new paragraph.
- Figure 6 caption: what is meant by mm for the rain and hail? Is it a precipitation rate?
- Figure 8: Adding the numbers to the single tiles of the heatmap would help to grasp the figure in a faster way.
- Line 292-294: These two sentence basically say the same things, and can be combined.
- Figure 10 caption: the domainS ARE defined ...
- 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?
- Figure 12: Where are these vertical profiles coming from? Are these based on mean values? The hail content seems very low.
- Line 342: what is the 1 in (Fig. 11j, 1)?
- 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.

- Line 368: A reference to the figure 11 should be made again from the melting rates.
- 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.
- 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.
- 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.
- Line 416: The results do not demonstrate any increase in CAPE and CIN as the authors decided to not show this. I elaborated my reasons for not doing this above.
- Line 448: The sentence “This intensification ...” is circular. Removing the last part “due to the heightened ...” would remedy that.

Editorial comments

- Line 10: wrong / incomplete latex command for the unit
- Line 173: plotting is colloquial and should be avoided
- 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

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.