Round two: Author's response for paper egusphere-2025-918

Insights from hailstorm track analysis in European climate change simulations

by Killian P. Brennan, Iris Thurnherr, Michael Sprenger, and Heini Wernli

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Reviewer 1

Overview

I thank the authors for their detailed responses. My main comments are resolved with the additional discussion included in the new manuscript. I only have a few optional suggestions remaining. Congratulations for this excellent article.

Reply: Thank you for your positive feedback and for acknowledging the revisions made. We are pleased to hear that the main comments have been satisfactorily addressed. Below, we respond to your remaining optional suggestions and indicate how we have incorporated them into the revised manuscript where applicable.

Reviewer Comment 1.1 — I agree that you used the current state of the art, the comment was not meant as criticism about your approach. Just to expand on this aspect, I still believe you underestimate the limitations with respect to under-resolved updrafts. Adams-Selin (2025) and Fischer et al. (2025) recently emphasized how sensitive 3D hail trajectories are to updraft characteristics. I at least see it as a possibility that trends of certain hail sizes cannot be represented in your study because of the limit in realistic updrafts. Also, I'm not sure Prein et al. (2021) can be used as a strong support here. As you mention, they found that updraft characteristics continue to improve at resolution < 4 km and they did not look in detail at hail production or hailstorms in particular, which tend to be non-MSC. However, that's just my opinion, I'm ok with the manuscript additions above if you think it makes the limitations clear enough.

Reply 1.1: Thank you for the clarification. We agree that under-resolved updrafts remain a fundamental limitation in convection-permitting models and acknowledge the relevance of recent work by Adams-Selin (2025) and Fischer et al. (2025) in this context. Our intention was not to downplay this issue but to contextualize it within the current modeling capabilities. While we maintain that the manuscript sufficiently communicates the key limitations, we recognize the merit

of your perspective and appreciate the opportunity to reflect on this aspect more deeply. While Adams-Selin (2025) was already included in the discussion on limitations (L480) we've expanded the limitations with a reference of Fischer et al. (2025). Please also refer to Reply 1.3 in this document. We are of the opinion that the current state of the limitations section (L477–501) adequately addresses all points raised by the reviewers concerning model resolution, updraft realism, and associated implications for hail simulation.

Additional comments

Reviewer Comment 1.2 — Line 6 and throughout: Is the italics for numbers intended?

Reply 1.2: Thank you for pointing this out. The use of italics for numbers was not intentional and has been corrected throughout the abstract.

Reviewer Comment 1.3 — Lines 57-58: Would it make sense to put this shorter part before (iii)? At least to me it seems logical to end with the approach you are using. Just a suggestion.

Reply 1.3: Thank you for the suggestion. We agree that this ordering improves the logical flow and have swapped points (iii) and (iv) accordingly.

Reviewer Comment 1.4 — Lines 303-311: As mentioned in line 123, hailcast does not take horizontal advection of hail into account so the landing position relative to the updraft might not be realistic. Fig. 5a also indicates this as hail mostly falls directly under the track. In other words, the relative horizontal transport of hail and rain (size sorting) is not fully represented. Yet here you seem to take a roughly correct position of hail relative to rain as granted. See radar studies or 3D hail trajectory simulations for more realistic fall locations of hail. I think you can still include this part given you look at relative differences. However, the caveat should I think be mentioned in the context of this paragraph.

Reply 1.4: Thank you for this thoughtful comment. As stated on line L275f: "Graupel is explicitly included in the COSMO microphysics and is subject to horizontal advection, which, however, results in only a slight offset of the graupel maximum from the storm center to the left relative to cell movement (Fig. 5a). The location of the graupel maximum provides an upper limit on potential hail advection, as graupel has a lower terminal velocity than the smallest hailstones, allowing more time for horizontal advection." The location of the graupel relative to the storm's center offers an upper limit to the expected horizontal advection of the hail within our simulations — in a storm-resolving simulation, it might be possible that hail (and graupel) is transported over larger distances. We believe no additional changes to the manuscript are necessary.

Reviewer Comment 1.5 — Lines 497-501: I think it should also be mentioned that you only simulated a 10-year period, so changes in high-end hailstorms might not be represented, especially not at a regional level.

Reply 1.5: Thank you for the comment. We've added a sentence to the limitations to reflect this (L502): "In addition, the analysis is based on two 11-year periods, which may be insufficient to robustly capture changes in rare, high-end hail events, particularly at regional scales."

Reviewer 2

Summary

The authors have made some changes based on my comments, but some were not addressed. The authors sometimes claim this is due to lack of data availability, but they could be more creative in this respect. I also find myself going between this study and the Cui et al. and Thurnherr et al. preprints a lot. In the current state, I think that it may be difficult for other scientists to follow the paper outside of their field, which I would find a pity. Nevertheless, I leave it up to authors on whether they wish to make further revisions to improve the quality of their study and make it accessible to a wider audience.

Reply: Thank you for your continued engagement and constructive feedback. We acknowledge that some of your earlier suggestions were not implemented, primarily due to data limitations, but we appreciate the encouragement to explore alternative approaches. We have reorganized Sect. 2.1 to make the methods more accessible.

Comments

Reviewer Comment 2.1 — Regarding assessing changes in CI frequency in current/future climate, it is said that no radar reflectivity data is available, however on L116 it is said that precipitation fields are saved every 5 minutes. Furthermore, Cui et al. (2024) build a lightning climatology in COSMO using the lightning potential index (LPI). Therefore, there are indeed data available to use as a proxy for CI. This would be a very basic analysis that will enable further interpretation of the results. In a response to a comment, the reviewers mention that a revised version of Thurnherr et al. (2025) (not available publicly) found changes in CIN in the region with decreasing hailstorm frequency. This should be mentioned in the manuscript.

Reply 2.1: Thank you for this constructive comment. While precipitation and lightning fields are available in the model output, we don't expect that they provide more insight on the question of convection initiation without applying a similar tracking algorithm as applied to the hail fields in this study. Without such a tracking, it is not possible to clearly differentiate between the effect of changing frequencies (i.e. changing convection initiation) or changing amounts of precipitation/lightning per convective storm. It is the strength of the tracking algorithm applied in this study that we were able to differentiate between such effects. And it is beyond the scope of this study to develop and verify a tracking of lightning and precipitation cells, but future studies could explore this utilizing our published hail tracking code. The following sentence was added (L209): "In regions where hailstorm frequency decreases — such as central France — CAPE is reduced and CIN increases in the seasonal Eulerian mean, consistent with less frequent convective initiation (Thurnherr et al., 2025)."

Reviewer Comment 2.2 — The authors' response to comment 2.16 confuses me. They now claim that their study and Wilhelm et al. (2024) are not comparable. Why are they then using the

study to support their conclusions? Please either remove this citation as support for their results or discuss further the nuances.

Reply 2.2: Thank you for raising this point. We agree that the comparison with Wilhelm et al. (2024) should be more clearly qualified. The section on L463f now reads: "The findings in this study align with past trend analyses in the northern Alpine domain (1959–2022, Wilhelm et al., 2024), which reported similar regional signals. South of the Alps, Wilhelm et al. (2024) also identified a positive past trend in yearly haildays. In our projections, both positive and negative changes in hailstorm frequency occur in close spatial proximity within the southern Alpine domain, with increases along the southern fringes of the main Alpine crest and a pronounced localized decrease in the Po valley (Fig. 2c). Observational studies such as Manzato et al. (2022) support this signal, showing a slight negative past trend in northeastern Italy, which is consistent with our projected localized decrease in hailstorm frequency in that area. These findings are further consistent with projections from studies such as Raupach et al. (2021), Thurnherr et al. (2025), and Feldmann et al. (2025), which report comparable regional trends throughout the remaining simulation domain."

Reviewer Comment 2.3 — L72: This will likely get changed by the typesetters, but it would be better to write "The simulations in Thurnherr et al. (2025)"

Reply 2.3: Thank you, we've changed this as you've suggested.

Reviewer Comment 2.4 — L138: "Be" is repeated.

Reply 2.4: Thank you for pointing this out. This has been rectified.

References

Adams-Selin, R., 2025: The Quasi-Stochastic Nature of Hail Growth: Hail Trajectory Clusters in Simulations of the Kingfisher, Oklahoma, Hailstorm. *Mon. Weather Rev.*, **153 (1)**, 67–87, DOI: 10.1175/MWR-D-23-0233.1.

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Fischer, J., M. Kunz, K. Lombardo, and M. R. Kumjian, 2025: Hail Trajectories in a Wide Spectrum of Supercell-Like Updrafts. *J. Atmos. Sci.*, **82 (7)**, 1403–1422, DOI: 10.1175/JAS-D-24-0222.1.

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Wilhelm, L., C. Schwierz, K. Schröer, M. Taszarek, and O. Martius, 2024: Reconstructing hail days in Switzerland with statistical models (1959–2022). *Nat. Hazards Earth Syst. Sci.*, **24 (11)**, 3869–3894, DOI: 10.5194/nhess-24-3869-2024.