Authors replies to reviewer comments RC2

Review for EGUSPHERE-2023-2639

Characterizing hail-prone environments using convection-permitting reanalysis and overshooting top detections over south-central Europe

Antonio Giordani, Michael Kunz, Kristopher M. Bedka, Heinz Jürgen Punge, Tiziana Paccagnella, Valentina Pavan, Ines M. L. Cerenzia, and Silvana Di Sabatino

We want to thank the reviewer for his/her assessment of our manuscript. In the following we give our answers to the comments and recommendations that have been raised. Reviewer comments RC are **bold**, our replies AR are in *italic*.

General comments:

RC: The study addresses the challenges associated with reliably observing and simulating hazardous hailstorms. The authors propose an approach that combines information from different sources, including remote sensing instruments, observations, and numerical modeling, to enhance the understanding of the spatial and temporal patterns of severe hail occurrences in south-central Europe. The methodology involves developing a proxy for hail frequency by integrating overshooting cloud top (OT) detections from the Meteosat Second Generation (MSG) weather satellite with convection-permitting SPHERA reanalysis predictors describing hail-favorable environmental conditions.

While the paper is already quite robust, there are a few shortcomings which should be addressed by the authors to enhance the readability and importance of their work.

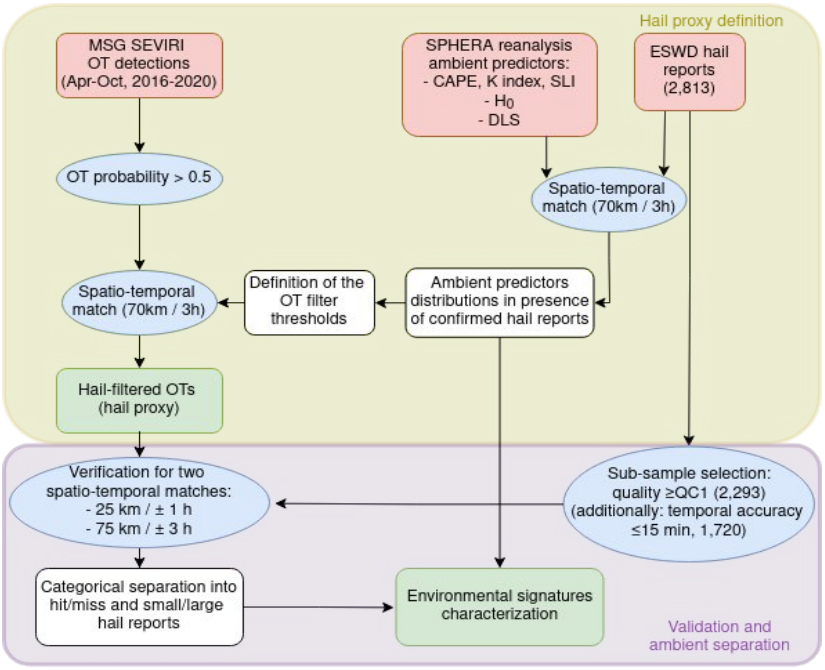
I recommend major revision with consideration of the specific comments listed below.

Specific comments:

Section 2

RC: While the methodology is generally well-described, ensuring greater clarity would enhance the paper's accessibility. Providing, for example, a flowchart for data processing and analysis could be beneficial.

AR: We thank the reviewer for the very nice suggestion. In the revised manuscript a flowchart has been added (reported below) which summarizes the analyses contained in the manuscript. We hope that this could enhance the readability and clarity of the paper.



Section 2.2

RC: I am wondering if you considered using the 5-minute rapid scan data from MSG, which may improve the quality of this paper, since you state in lines 538-544 that the low temporal resolution is a limitation of your work.

AR: We agree with the reviewer that the usage of 5-minute rapid cloud-top scans from the MSG could have enhanced the efficacy and significance of the work. Unfortunately, at the time of the analysis those higher-resolution data were not easily accessible in the necessary format (McIDAS AREA) via the Data Center of the University of Wisconsin Space Science and Engineering Center needed to run the Khlopenkov et al., 2021 algorithm for automatic OT detection. Further, ordering many years of rapid scan data from the EUMETSAT archive is a very time consuming and inefficient process, so this was a further limiting factor. Hence, the best available option was to rely on MSG scans data with a frequency update of 15 minutes. In the revised manuscript an explicit statement has been added to justify this choice.

Section 2.3:

RC: Why hasn't the low-level moisture been added to the filter? You clearly and rightfully state that it is an important factor for hail formation, so I don't really understand why the moisture wasn't considered for the filter.

AR: The amount of low-level moisture is implicitly included in the parameters entering the filter conditionally on the presence of instability as in the formulation of the K index whose 2nd and 3rd additive components (i.e., Td_{850}, dew-point temperature at 850 hPa, and $(T_{700} - Td_{700})$, difference between the ambient temperature and the dew-point temperature at 750 hPa) quantify respectively the low-level moisture content and the moist layer depth (as reported in Appendix A). The inclusion of the low-level moisture within the K index has been highlighted multiple times within the text (e.g. lines: 175/187/248/434). However, in the revised manuscript we attempted to be even more explicit and added a statement in Section 2.3 to make it clearer.

RC: Moreover, a recent study showed that CAPE above the -10°C-isotherm stood out as the best predictor for Europe (Battaglioli et al. 2023). Another recent study (Nixon et al. 2023), for the US this time, showed that the depth of the storm ("maximum parcel level") and storm-relative winds below the hail-growth-layer may play a key role in formation and size of hailstones. Hence, I would suggest rethinking the choice of ambient predictors.

AR: We thank the reviewer for the suggestions. As of today, there is not general consensus in the scientific community on which are the best thermodynamical predictors specifically to describe convective environments supporting hail development. The choice on the environmental predictors included in this work relied upon those parameters that are mostly used for the purpose in central-European regions, based on previous findings that mainly inspired this work (Punge et al., 2017), as well as literature that reported their quantitative added value over the common investigated area (Kunz 2007, Kunz et al., 2020, Jelic et al., 2020). Further, the referenced work (Battaglioli et al., 2023) was unpublished at the time of the proceedings of the present work. We would also like to point out that the filter approach proposed is not foreseen for positive hail predictions, nor for the definition of a robust climatology of potential hail events, but is mainly designed to remove non-hail occurrences based on the identification of the minimum environmental conditions necessary for supporting hail development. Hence, we believe that the chosen set of predictors is adequate for this purpose, owing also to the conservative nature of the filter designed (which ambient thresholds are defined to remove all those occasions which are unlikely to have produced hail by excluding a portion relative to the 5% of the whole single parameters distributions), and which could hardly benefit from a change of e.g. the formulation of the CAPE parameter. Furthermore, extracting the reanalysis fields including the information on this alternative formulation of the CAPE parameter is not a straightforward task and would require significant effort. Additionally, the potential inclusion of the the storm-relative winds below the hail-growth-layer, despite being very promising as demonstrated by recent research (e.g., Kumjan & Lombardo, 2020), we believe to be significantly complicated due to the necessary inclusion of the information on the hail-growth zone of the storm which is still very challenging to estimate, and is not possible to account for with the reanalysis data included in this work. That said, it is certainly interesting for possible future extension of the work to include other different parameters such as those proposed by the reviewer. In the revised manuscript we expanded the future outlook of the Conclusion section including that possibile future extension of the work could focus more on the analysis with additional environmental parameters to improve the identification of hail-producing environments

Section 5:

RC: I would suggest splitting the discussion and conclusion. The current content of this section is not well structured, and the take-home messages are not clearly outlined.

AR: We thank and agree with the reviewer. In the revised manuscript the Discussion (Section 5) is clearly separated from the Conclusion (Section 6).

RC: Providing context for the practical applications of the research would also highlight the significance of this paper (e.g., risk assessment, insurance).

AR: We appreciate the suggestion of the reviewer. In the revised manuscript we added a brief discussion in the future outlook of the Conclusion section highlighting potential downstream

applications of the presented analysis (such as its inclusion in risk assessment strategies or for insurance purposes) with the aim to enhance the relevance of this scientific work.

Figure 1:

RC: Large hail is defined with "≥ 2 cm" in the ESWD (and not "≥ 3 cm"). So, this error needs to be addressed throughout the manuscript to maintain coherence.

AR: We thank the reviewer for pointing out this mismatch between our analysis and the ESWD nomenclature. However, since the adjective "large" is arbitrary on the definition choice, and since the paper is self-contained and completely independent from the ESWD database, we do not believe that we should rename the hail size characterization used in the manuscript. Furthermore, as reported in the review of Raupach et al., 2021, the exact hailstone diameter defining "large" or "very large" hail is a matter of definition and varies in the literature, and their proposal is to refer to severe hail as that with hailstones of at least 2 cm in diameter, large hail as that with at least 3.5 cm diameter hailstones and very large hail as that with hailstones of at least 5 cm diameter. Anyhow, to enhance clarity and avoid possible misinterpretations, an explicit statment has been added at the beginning of Section 2.1 to point out the different nomenclature between the paper and ESWD.

Technical corrections:

RC: Line 203: "intrinsic"

RC: Line 519: "large hail-producing storms"

References:

Battaglioli et al. 2023 (https://doi.org/10.1175/JAMC-D-22-0195.1)

Nixon et al. 2023 (https://doi.org/10.1175/WAF-D-23-0031.1)

AR: References

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