Reply document for paper egusphere-2024-2148

— round two —

An object-based and Lagrangian view on an intense hailstorm day in Switzerland as represented in COSMO-1E ensemble hindcast simulations

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We thank Reviewer 2 for suggesting to accept the paper as is, and Reviewer 3 for their constructive comments, which helped us to further improve the clarity of our study. In the following, we carefully address all comments of the referees. They are shown in **blue** and our replies in **black**. Mentioned references are listed at the end of the document.

Reviewer 2

Summary

I thank the authors for this revision, which has improved it so much. I'm therefore happy to accept the manuscript for publication.

Reply: Thank you for your positive evaluation of our manuscript.

Reviewer 3

Summary

The authors investigate a severe hailstorm episode over Switzerland, using a high-resolution ensemble. While the analysis is carefully carried out, using suitable tools, I thought the analyses were superficial and did not lead to much new insight. This is reflected by the summary in line 442: As a main result, the authors mention "impressive updraft velocities, overshooting tops, and the intricate cloud structures associated with severe hailstorms (Fig. 5)," while Fig. 5 does not even show any details (all I see is a coarse outline of a convective cloud). I also have a few comments regarding the interpretation of the results. Overall, my comments probably fall into the major category.

Reply: We sincerely appreciate your detailed and insightful review of our manuscript. Your feedback clearly highlighted the importance of a more focused and comprehensive analysis to substantiate our conclusions and to make our conclusions more specific. We genuinely hope that the revised version meets your expectations and that you now regard some of our results as insightful.

Concerning the specific critique about the sentence in the conclusions (L442 in the previous version), we agree that this formulation was not ideal (Fig. 5 does not show "intricate cloud structures") and we improved this formulation as well as a few other formulations in the first part of our conclusions. We however think that, overall, our conclusion provide a balanced view on the novelties of our study (e.g., combined Eulerian and Lagrangian approach, use of ensemble to obtain robust model representations of an observed severe hailstorm) and of its limitations – see in particular the last 3 paragraphs of the paper.

General comments

Reviewer Comment 3.1 — The analysis is based on a convection-allowing simulation, which is understandable because this is what is available operationally. Still, at ∼1 km grid spacing, the convective cells are not well-resolved. I think the authors should acknowledge more clearly that the storm-scale processes are only crudely represented and perhaps resist the temptation to report e.g., updraft speeds and cell sizes (which are almost certainly inaccurately represented).

Reply 3.1: Our answer has two parts. We first fully agree with you that 1 km grid spacing is not sufficient to capture all relevant details of deep convective storms and we see a great value in other studies that investigate convective storm dynamics with higher-resolution models. However, to the best of our knowledge, such models are currently not run operationally at numerical weather prediction centers. Our study is part of a project, which is partly driven by stakeholder needs — we would like to use the currently available modeling technology to provide relevant insight into hailstorms in Switzerland. At least in Europe, the model setup we have available (ensemble with 1 km grid spacing and HAILCAST parameterization of hail) is unique and at the forefront of operational NWP. Therefore, in our view, it is still relevant and important to understand how these hailstorms are represented in such a simulation. As this simulation (and models with similar resolutions) are used operationally to forecast hail, it is of value to understand how the model realizes hailstorms, in order to judge whether key processes are adequately accounted for. Additionally, such an analysis can serve as a benchmark for future studies performing hailstorm simulations with improved models (higher resolution and/or improved representation of hail microphysics).

Reviewer Comment 3.2 — Line 252 ff.: How/where was CAPE and SRH etc. calculated? In the inflow region? How far from the storm? Or is it an average?

Reply 3.2: This information is detailed in the caption of Fig. 4. We've improved the description to disambiguate it and it now reads: "The inflow region encompasses a $20 \times 20\,\mathrm{km}^2$ square box, centered 20 km ahead of the storm." To increase readability, we also include this information as a footnote in L252.

Reviewer Comment 3.3 — When considering the evolution of the CAPE available to a storm centered in the 50x50 grid point subdomain (L345), can you exclude that no other storms are present in the domain? For instance, CAPE might decrease as outflow of neighboring storms enters the domain. So the reported values may not be representative of the CAPE available to the storm in the domain center.

Reply 3.3: We appreciate your concern regarding the potential influence of neighboring storms on the CAPE evolution within the 50×50 grid point subdomain. As stated above, the inflow region in our analysis encompasses a 20 km wide box located 10 km ahead of the storm. Given the average storm motion of approximately $12\,\mathrm{m\,s^{-1}}$ and the inflow moving towards the storm at $\sim 5\,\mathrm{m\,s^{-1}}$, the air within this inflow box would reach the storm in roughly 10 minutes.

If a secondary storm were to enter this inflow region and reduce CAPE, it would indeed influence the available energy for the storm under consideration. However, we would argue that this is not a limitation of our approach but rather an inherent feature of real storm interactions, which our analysis aims to capture. Storms in convective environments do not evolve in isolation, and the impact of neighboring convection on CAPE is a relevant dynamical process. Our methodology, therefore, reflects the natural complexity of storm environments rather than imposing an artificial constraint that isolates a single storm from its surroundings.

Moreover, our results remain consistent with expectations based on storm dynamics, suggesting that while external influences such as neighboring convection can locally modulate CAPE, the overall evolution of the storm remains physically interpretable within the framework of our analysis.

We've added the following sentence to L347: "It should be noted here, that neighbouring storms might influence/reduce CAPE in the inflow box."

Specific comments

Reviewer Comment 3.4 — L22: The main characteristic of supercells is their rotating updraft, not necessarily their size (there exist quite small realizations of supercells, e.g., in rainbands of tropical cyclones).

Reply 3.4: We've altered the sentence accordingly. The text now reads: "Distinguished by their towering vertical reach spanning the troposphere and rotating updraft, supercells surpass the typical scale of single-cell storms."

Reviewer Comment 3.5 — L36: Isn't there a distinction between short-term forecasting and nowcasting?

Reply 3.5: We agree and did not intend to suggest otherwise. We slightly changed our formulation, which now reads "At short lead times, radar-based nowcasting serves to inform about hail occurrence, where the future state of hailstorms is extrapolated from current radar observations and movement vectors of convective cells (e.g., Hering et al., 2004; Trefalt et al., 2023). Predictions with lead times beyond 1–3 h must rely on numerical weather prediction (NWP) models (e.g., Sun et al., 2014; Nerini et al., 2019)."

Reviewer Comment 3.6 — L83: The seminal paper on supercell propagation is by Rotunno and Klemp (1982, MWR)

Reply 3.6: Thank you, we've included a reference to this seminal paper in our manuscript. The sentence now reads: "Browning (1977) and Rotunno and Klemp (1982) explored storm propagation mechanisms, while Rotunno (1993) analyzed the three-dimensional airflow structure of supercell thunderstorms."

Reviewer Comment 3.7 — L84: Davies-Jones (1984, JAS) is perhaps more relevant.

Reply 3.7: Thank you for mentioning this study, we've included it appropriately. The text now reads: "Despite foundational studies like Browning and Ludlam (1962), Browning (1964), Davies-Jones (1984) that investigated airflow patterns within severe local storms, recent literature predominantly emphasizes hailstone trajectories and over air parcel trajectories in convective systems."

Reviewer Comment 3.8 — L117: Was ERA5 used for the boundary conditions?

Reply 3.8: Information was added accordingly: "The ECMWF global ERA5 reanalysis (Hersbach et al., 2020) was used in this study to characterize the large-scale atmospheric conditions and provided boundary conditions for the regional weather simulations with COSMO."

Reviewer Comment 3.9 — L123: Is the microphysics scheme single- or double moment? Also, which subgrid-scale turbulence scheme was used?

Reply 3.9: Thank you for pointing out this missing information. We've added the corresponding details to the manuscript. The text now reads: "Parameterizations represent unresolved subgrid-scale physical processes, including a single-moment bulk microphysics scheme (Lin et al., 1983) with five species (cloud water, cloud ice, rain, snow, and graupel) and schemes for shallow convection, boundary layer turbulence, radiation, and land-surface processes. The turbulence parameterization is adapted from Mellor and Yamada (1982) with a prognostic equation for the turbulence kinetic energy, including effects from subgrid-scale condensation and evaporation. It is applied to the bottom boundary of the atmospheric model to calculate surface-layer fluxes (Baldauf et al., 2011). The parameterization of radiation follows the scheme of Ritter and Geleyn (1992)."

Reviewer Comment 3.10 — L145: Why were only the 10+ mm embryos considered?

Reply 3.10: In initial tests, we saw that the size distribution of the largest hail embryos was more representative of the radar-observed hail size distribution from the mean diameter over all 5 HAILCAST embryos. Further, the correlation between the largest diameter and the mean across all 5 HAILCAST embryos is high, so most analysis would be interchangeably valid. — No changes were made to the manuscript regarding this comment.

Reviewer Comment 3.11 — L149: Isn't the main issue that the output time step is rather coarse, rather than the storms being small and moving fast (this is just a matter of domain size and temporal resolution).

Reply 3.11: You are right; however, there are technical limits such as storage to take into consideration, which govern the temporal resolution of NWP output in practice. To this end, the tracking algorithm must account for the limited temporal resolution. And, as far as we know, it is unusual to have 5-min output from an operational NWP ensemble — this was only possible thanks to our colleagues at MeteoSwiss re-running the simulation with "high-frequency" output, which amounted to almost 20 TB.

Reviewer Comment 3.12 — L154: Were these adjustments done manually?

Reply 3.12: The methodology around the adaptive threshold is automated and further described in the appendix (L529 ff.). — No changes were made to the manuscript regarding this comment.

Reviewer Comment 3.13 — L218: Perhaps state the number of storms considered as well?

Reply 3.13: The number of storms is already given at the beginning of Sect. 3.2. We additionally added the number of storms to L217.

Reviewer Comment 3.14 — L238: It is not clear what "the selected storm" refers to, and no specific storm was described earlier (unless I missed it).

Reply 3.14: In L238 we refer to Sect. 3.2 (L222) where we write: *"[...] one exemplary storm was selected from ensemble member 5, which shows a similar realization to the actual storm [...]"*. This storm is highlighted in red in Fig. 3: we've reiterated this in L222 and L238: "(red track in Fig. 3)"

Reviewer Comment 3.15 — L257: Not sure I understand why in your simulation SRH doesn't indicate the potential for storms to rotate. The background value certainly is enhanced within the storm's inflow, but one can still identify a representative "environmental" value that does indicate the supercell potential. Indeed, in Fig. 4, SHR does seem to correlate with the max updraft speed (e.g., increasing at 14:00 UTC).

Reply 3.15: Thank you for pointing out this inaccuracy, we've removed this statement.

Reviewer Comment 3.16 — L273: I suggest removing subjective qualifiers such as "impressive."

Reply 3.16: Done.

Reviewer Comment 3.17 — L287: I'm not sure what you mean by "graupel not activating freezing of cloud water" because cloud ice is present mostly above the homogeneous freezing level; suggest rewording.

Reply 3.17: We've reworded it to make clearer what we mean here. The sentence now reads "However, since the cloud ice is present mostly above the level of homogenous freezing, this suggests that ice introduced by graupel below this level is not activating the freezing of cloud water droplets."

Reviewer Comment 3.18 — L298 ff.: Suggest referring to individual panels in the figure (I wasn't sure if you were talking about panel a or b)

Reply 3.18: Thank you, this is helpful, we added panel specifiers.

Reviewer Comment 3.19 — L311: I think the size of the storm cores need to be taken with a grain of salt as the storms themselves are very poorly resolved. Yes, you state that at the very end, but why even report numbers if at the end you need to acknowledge that they are likely not accurate.

Reply 3.19: In our view, the limitations mentioned in the final section warrant reporting these numbers here. See also our reply to the general comment about model resolution. — No changes were made to the manuscript regarding this comment.

Reviewer Comment 3.20 — L324: Why does CAPE decrease as the storm passes? (I suspect that increasingly, anvil material is sampled, and at sufficient proximity to the storm, the outflow.) Also, why does CAPE not go to zero? Within the storm, the thermodynamic profile ought to be approximately pseudo-adiabatic.

Reply 3.20: The storm consumes some of the CAPE in its environment as it moves air vertically. However, the storm is not a perfect, idealized system — rather, it is a system with limited extent, mixing, etc. — thus, some CAPE remains. If the reviewer knows about an observational study that would reveal that the CAPE decrease should be substantially larger after the passage of the storm, then we would be happy to include it. — No changes were made to the manuscript regarding this comment.

Reviewer Comment 3.21 — L327: Earlier you stated that the horizontal placement of the hail is not well-represented by the HAILCAST model except for a fudge factor that describes horizontal advection. How confident are you that these results are realistic? If not, why even report them?

Reply 3.21: The argumentation provided on L317 ff. corroborate the results on L327: *"Co-location of the hail and updraft maxima is to be expected, as HAILCAST does not account for horizontal advection of hailstones to other grid columns. In contrast, since graupel is explicitly included in the COSMO microphysics, it is subject to horizontal advection in the simulations. Only a small offset of the graupel maximum from the storm center exists (Fig. 6e). The location of graupel gives an upper bound on the potential advection of hail, as graupel has a smaller terminal velocity than even the smallest hailstones, giving more time for horizontal advection to take effect."* We think that this properly describes the limitations when using HAILCAST and why, in this case, we regard the results as still meaningful. — No changes were made to the manuscript regarding this comment.

Reviewer Comment 3.22 — L338: Suggest flipping Figs. 7a, 7b, and 7c as you first talk about panels b and c, and then panel a.

Reply 3.22: Thank you for your suggestion, we've arranged the panels in the order of their references in the text.

Reviewer Comment 3.23 — L353: You haven't mentioned yet how you calculated the inflow depth. Suggest at least pointing the reader to the trajectories in section 5.

Reply 3.23: Thank you for this comment, we've added a reference to Sect. 5 and a footnote reading: "Mean height above ground level of the inflow trajectories in the period −60 to −30 min before reaching the storm."

Reviewer Comment 3.24 — L354: Coffer et al. considered the low-level mesocyclone, not the total inflow into the updraft. Suggest e.g., Nowotarski et al. (2020, MWR, "Evaluating the Effective Inflow Layer of Simulated Supercell Updrafts").

Reply 3.24: Thank you for pointing this out. We've adjusted the text to: "Throughout the storm's lifetime, the bulk of the inflow originates from 330 – 900 m AGL. Previous studies found the inflow level height of simulated supercells to be between 1400 and 1800 m AGL (Thompson et al., 2007; Nowotarski et al., 2020), while in idealized simulations of supercell low-level mesocyclones, Coffer et al. (2023) found the inflow to be around 200 – 400 m AGL."

Reviewer Comment 3.25 — L362: You could offer a potential explanation for why w does not seem to be related with CAPE: Vertical pressure-gradient accelerations (e.g., textbook by Markowski and Richardson, 2010, p. 233) and precipitation loading.

Reply 3.25: Thank you for this suggestion, we've added a possible explanation to the manuscript.

Reviewer Comment 3.26 — L376: Were the parcels initialized in a box that had a ceiling of 5000 m AGL (or MSL?)? Or in a layer at 5000 m?

Reply 3.26: Thank you for pointing out this ambiguity. The height refers to altitude above mean sea level, we've specified this in the revised manuscript.

Reviewer Comment 3.27 — L396: In Fig. 10a, it seems that CAPE is pretty steady around 1100 J/kg, so I was unable to see the steady increase mentioned in the text.

Reply 3.27: Thank you for pointing this out, we've added the rate of change in CAPE and CIN during this period. Please refer to the track-changes file for the changes made regarding this comment.

Reviewer Comment 3.28 — L398: Suggest replacing "they" with "the parcels."

Reply 3.28: Thank you for pointing out this detail. We've changed this according to your suggestion.

Reviewer Comment 3.29 — L410: minimum

Reply 3.29: Changed as suggested.

Reviewer Comment 3.30 — L415: I think this is only approximately true (rain falling through an unsaturated parcel breaks the assumption of a closed system, so theta-e would not be perfectly conserved).

Reply 3.30: Thank you, we've added an appropriate footnote: "As the trajectories are not a closed system, there might be influence from, e.g., sensible heat transfer from colder rain falling into the warmer air beneath and removing heat energy, effectively reducing Θ_e in the parcel. However, this effect is expected to be minor, as the temperature difference between the parcel and the infalling rain is limited."

Reviewer Comment 3.31 — L424: I felt you were overselling the results a bit by claiming that you offered "detailed insights into the dynamic and thermodynamic processes." (You only spent one paragraph on this!)

Reply 3.31: **We agree, and we shortened the formulation as follows: "These findings form the basis for the summary and conclusions in the next section."**

Reviewer Comment 3.32 — Fig. 10: By "outflow" do you mean "anvil outflow"? If so, mention that somewhere, as "outflow" is commonly taken to be the "cool-air outflow" associated with the storm's cold pool.

Reply 3.32: Changed as suggested (L412).