

Review for

Ambient conditions prevailing during hail events in central Europe

by Kunz et al.

Summary:

In their study, Kunz et al look at the ambient conditions during hail storm events in Central Europe. Different data sources are included in the analysis: Radar composites are used to track storm cells; the ESWD data base provides the hail diameter during storms; and the ERA-Interim reanalysis is used to characterize the environmental conditions at hail storm (HS) locations. The manuscript is well written, the scientific argumentation solid, and the balance of text and figures well motivated. Still, some aspects of the study should be addressed before it can be considered suitable for publication in NHESS. The major points are listed below (Major Concerns), some minor comments then follow in 'Minor Comments'.

Major concerns:

1. Resolution of ERA-Interim: Severe convective storms have a small spatial scale and can be triggered by small-scale topographic or other environmental features. How does this fit with the rather coarse spatial and temporal resolution of the ERA-Interim dataset? Of course, I notice that the authors are aware of this issue and they mention it at several places in the manuscript. Still, there remains the uncertainty whether the DLS (deep-level shear), SRH (storm-relative helicity) and SLI (surface lifted index) in ERA-Interim are really representative for the exact location where hail is recorded? I wonder whether

- the authors should show some results of the high-resolution analysis data set that is mentioned in section 5.1, which however is not shown in the paper.
- a limited time period (1 year?) could be chosen and the analysis of the manuscript be repeated based on, e.g., high-resolution ECMWF analysis data.

I certainly don't expect the authors to repeat the whole analysis with higher-resolution data; but it would be nice to see how well the environmental conditions are really represented in ERA-Interim. I can imagine, for instance, that ERA-Interim does not capture the environmental conditions very well over the Massif Central, or over the Black Forest. A critical discussion, optimally complemented by a reduced analysis at higher resolution, should be included in the manuscript.

2. Identification of fronts: On P5, L118-120 some details about the front identification are given. The methodology to do so is well established and fine also for this study. Nevertheless, some aspects of the identification might be worthwhile to carefully address in the manuscript: (i) First, only cold fronts are considered, i.e. any convective storm associated with a warm front is not included? Is there a specific reason why the analysis is restricted to cold fronts? (ii) Stationary or slowly moving fronts (< 3 m/s) are not included in the attribution, based on the (fair) assumption that synoptic fronts associated with

extratropical cyclones typically move with reasonable speed? Still, the application of this criterion also might be a little artificial. (iii) Thermal fronts, or fronts associated with land-sea contrasts, are explicitly excluded. I guess that there are good reasons why to do that, but they should be more clearly discussed in the paper. (iv) Finally, pure geometric conditions are used (e.g., minimum length of 500 km) to separate 'significant' fronts from more 'fragmented' ones. I wonder, however, whether this also brings in some bias, because I can imagine that frontal fragmentation can be favored as the 'pure' and elongated synoptic fronts originating in the North Atlantic move in over Europe and move land inward. Frontal fragmentation might also be enhanced over orography, e.g. the Massif Central or the Black Forest. I am aware that objective identification of fronts is a difficult matter. But at least I would appreciate if the authors carefully address these limiting factors in the discussion.

3. SCS vs HS; frontal vs. non-frontal: The focus of this study clearly is on the HS, whereby these HS are classified according to their hail diameter based on the ESWD reports. That's fine, but brings in also some subjectivity. More specifically, how reliable/robust is the analysis if it is based on the maximum recorded hail size? Furthermore, Figure 1 also shows that the number of HS is small compared to the objectively identified SCS. Of course, I see that exactly these HS are at the heart of the manuscript's analysis. But, at some stage while reading the manuscript I 'defined' what I considered for myself as the most interesting part of the study. And, honestly, it is not the categorization according to the hail size and the track length. To me, the most interesting aspect is the distinction between frontal and non-frontal storms! I then wondered whether this would not be a better storyline than the existing one. Basically, the story would start with a climatology of the SCS over Central Europe; then, the next question addresses the fact whether the frontal tracks are associated with front or not. If this attribution is done, a next section could deal with the different characterization of the frontal vs the non-frontal storms. Here, the track length, the track duration, and also the presence of hail or not (including its size) could be discussed. Of course, I know that this storyline kind of reverses what the authors do now. One advantage of restructuring would be that the weakest part -- at least in my opinion --, i.e. the hail diameter, does not stand at the beginning, but only appears at the end of the chain. I would certainly not enforce the authors to reorder everything in the manuscript, but they might give it a thought.

4. Application in forecasting: This final major concern is a more 'philosophical' one. The basic aim of the study is to identify the severe and damaging storms, whereby the severity is (obviously) a combination of hail diameter and storm track length (footprint). These storms are taken as the basis of the analysis, and then the environmental conditions of these storms are considered. From a forecasting perspective, the reverse questions is much more interesting. In short, if we have an environment with this and that environmental conditions, what is the chance that we get a storm with a severe footprint? Please note that this reversed perspective is not exactly the same as the one studied in the manuscript, and this should be carefully discussed. Stated in a more practical way: How can the results of this study be used in an operational weather forecast? How would be the performance of such a forecasting algorithm if it were based on the environmental parameters used in this study? I think that the authors do a first step in this direction by quantitatively looking in P13,L378-383 at the predictive skill of certain parameter combinations. This part of the text should, however, be more detailed. The remained rather unclear to me how the predictive skill is determined..

Minor comments:

- P2,L24: 'large hail' → Does this refer to large hail stones, or does it refer to intense hail? It is not completely clear.
- P2,L30: What exactly is meant by 'footprint'?

- P2,L56: What is the temporal resolution of the cell tracking algorithm, more precisely: what is the resolution of the input fields?
- P2,L68: "Does the propagation direction of hailstorms depend on hail size?" → This question comes a little out of the blue, i.e. it was not motivated by the preceding paragraphs of the introduction. Is there a good reason to assume that the storm movement's direction depends on the hail size? If so, discuss this in the introduction.
- P4,L103: The SRH is calculated based on the storms movement (cx,cy), and based on the ERA-Interim
- P5,L141: "Several studies have provided evidence that suggests this threshold to provide sufficient skill for hail detection" -> Rephrase in clearer way.
- P5,L148: Here, the SCS tracks are first introduced. Possibly, it would be nice to learn already at this place about the typical characteristics of these tracks? For instance, about their length and duration? This is, in my view, reasonable because it allows already to set this characteristics to set into perspective of the limited spatial and temporal resolution of the ERA-Interim dataset.
- P6,L156: A minor detail: Wouldn't it be easier to take the spherical distance between the frontal points and the storm points instead of this (approximate) formula? It is approximate because the latitude actually changes along the storm track and along the front.
- P7,L211: "The frequency of both SCS and HS events shows a large spatial variability with an overall increase from north-to-south, i.e., with the distance to the Atlantic" -> I Am not sure whether I see this effect as clearly as it is stated in the text. There is a distance-to-Talantic effect near the coast, but further land inward it is less clear to me...
- P7,L212: From Figure 1 it is not clear that the regional hot spots are downstream of the Massif Central or the Black Forest. It is not clear because we don't see in this figure the direction of the wind.
- P9,L250: "not the individual fronts are done here" -> Sounds a little strange! It becomes clear with the succeeding sentence.
- P10,L187: "fronts create hail-conducive conditions mainly through two effects: along-front advection of moisture at lower levels leading to larger CAPE, and higher wind speed aloft enhancing vertical wind shear." -> What about the propagation of the front itself? This will also induce vertical lifting, as does also the ageostrophic circulation associated with fronts? I would also assume that these effects depend on the intensity of the fronts. In short, a more comprehesnive discussion of the frontal effects would be nice -- as is actually done in the later discussion/conclusion sections of the paper.
- P10,L291: "fronts also largely determine the orientation of the tracks" -> Sounds a littel strange to me! Rephrase, e.g., to "the tracks are strongly coupled to the (typically eastward) propagation of the fronts.
- P12,L389: Based on the ERA-Interim winds, right-moving and left-moving supercells are distinguished. Can this reliably be done based on the coarse ERA-Interim winds? Note that the ERA-

Interim winds are representative for a approximately 80 x 80 km². The storm itself is, on the other hand, much smaller.

- P311,L314: "Considering the magnitude of DLS, it is found that the values are quite low with a mean of 12.5ms⁻¹ around the HS events. Several authors have shown that organized convection 315 capable of producing larger hail develops only in sheared environments above around 10m m/s" -> Just for curiosity, myself not being an expert on storm dynamics. I would expect that there is an intermediate range of DLS that favors an intense and sustained convective activity. Too weak, but also too strong wind shear would hinder the convective storm development. It would be interesting to discuss this aspect somewhat in the manuscript, if my 'naive' thought is correct.