

Review

The paper “Radar-based assessment of hail frequency in Europe” is targeted on climatology of severe convection storms (SCS) based on 10 years of radar data covering a large European region. The topic is very important and suitable for NHESS. The analysis covers a relatively large number of years (considering typical radar records) and geographic domain.

My main concern is if we are really sure these are hail cases. The paper, as it is now, is not clear about this. The text some times refer the data set as SCS and sometimes as hail cases.

On one hand, it seems some validation has been done: “Tests with long-living SCS tracks were compared with hail reports archived by the European Severe Weather Database (ESWD) operated by the European Severe Storms Laboratory (Dotzek et al., 2009) along the reconstructed storm trajectories to assess the reliability of CCTA2D (not shown). In most cases, the ESWD reports were located close to the center of SCS tracks.” (Lines 169-172). But, just next to it, at the beginning of the results section, there is a “disclaimer”: “Note that this climatology represents the spatial distribution of convective cells with high reflectivity, but not directly of hail. The term hail days used in the following refers to the exceedance of reflectivity, but not to confirmed hail observations” (Lines 180-182). I understand the first case refers to tracks while the second to the spatial distribution, but I think it would be better to clarify in a targeted section whether or not there is any validation that the analyzed storms are indeed hail events. Also, through out the text try to be more consistent in the use of SCS vs. hail events according to the level of assurance of the nature of these events.

Furthermore, validation seems quite crucial here, as without it, the data set may not represent hail events. So I encourage the authors to present the validation done against the hail reports from ESWD and if possible to extend it.

Other comments:

It would be good to provide a short background on hail formation, under what meteorological conditions we should expect hail events. This would help in understanding the interpretation of the presented results.

The data set includes radar data from two countries which goes some different processing procedures. To be sure this does not add any bias in results – is there any overlap region where analysis from both data sources can be compared?

Lightening filter: “If high reflectivity during a day occurs without lightning, the values at the affected grid points are set to zero.” (Line 127-128): why only during day? Are you sure this filter is not too aggressive? Can you provide any information on percent of hail storms that are not associate with lightening? if I understand correctly these storms will be filtered out from the analysis and it is important to verify their fraction is not substantial.

Split and merge: the authors write that “Special attention is given to cell splitting and merging” (Line 154). Why is that? I did not find in the results any consideration of the splits and merges that were detected.

Line 277: “As shown in Figure 5 the annual variability is very high and without any trend” – for 10 years of data I would not consider a trend for 10 years of data.

Line 283-285: “large-scale lifting (e.g., related to differential vorticity advection) could have led to an increase in convective available potential energy (CAPE) and a low convective inhibition (CIN). The combination of high moisture in the boundary layer, low CIN, high CAPE and lifting mechanisms may give rise to a substantial increase in SCS.” It is not clear if this is an assumption or analysis. Why not to check reanalysis data for CAPE, CIN, air moisture anomalies? without it, I think this sentence is too speculative.

General comment: the authors provide a very detailed description of the pattern shown in the figures. In my opinion this is too lengthy and could be shorten. I leave this however for the author decision.