Weather Clim. Dynam. Discuss., referee comment RC2
https://doi.org/10.5194/wcd-2022-27-RC2, 2022

Comment on wcd-2022-27

Anonymous Referee #2

Referee comment on "Supercell convective environments in Spain based on ERA5: hail and non-hail differences" by Carlos Calvo-Sancho et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-27-RC2, 2022

Title: Supercell Convective Environments in Spain based on ERA5: Hail and Non-Hail Differences

Authors: Calvo-Sancho et al.

RECOMMENDATION: Rejection

The purpose of the paper is to compare the environments conducive to supercells with and without hail to identify differences. The analysis is performed over Spain using ERA5 reanalyses over 10-years. The study is potentially interesting, but I do not think the quality of the paper in the present version is sufficient to justify the publication in a high-quality journal. I have serious concerns about the methodology, the analysis of the results, the quality of the figures. The paper is difficult to read and appears mainly as a very preliminary, disorganized draft that would have still needed a lot of revision before submission. In the following, I would not focus on the style since there is a lot to be done on the content. So, I recommend a complete rearrangement of the paper and improved analysis, although I encourage resubmission.

Introduction:

- the focus of the paper is on the comparison between hail and non-hail supercells, so

you should remain focused on that also in the introduction. Also, the studies you mention in the introduction are almost exclusively focused on Spain, while you should extend your comparison of hail vs non-hail supercells to the whole Mediterranean and possibly other regions;

- Line 45: "supercells in Europe tend to be smaller, both horizontally and vertically, than those formed in the US": do the papers you refer to include quantitative estimation of the reduction in horizontal and vertical extent? I think this is a difficult task to assess, so I would be curious if there are some statistics supporting this statement. Similar considerations apply to the "reduced rotation and shorter life spans": is there any statistics to support your sentence?

Section 2:

- Has the supercell dataset been validated somehow? For example, did you make a comparison with the hail occurrences as reported in ESWD or in local datasets?
- Line 83: you mentioned earlier that the medium-high confidence events are detected in radar images but without direct observation; here, you mention that thanks to volunteers, 20.5% of the medium-high confidence supercells were confirmed by two-dimensional radar images. Sorry, but I am confused.
- Line 143: I think it is interesting that the only parameter changing with time is CIN, possibly as a consequence of the change in the environmental conditions after convection is triggered.
- Line 146-147: I disagree with this point. Once convection is triggered, the environment should be "contaminated" by the vertical redistribution of temperature consequent to the vertical motion, thus the profile at tc would be less representative of the environment conducive to supercell development than that at the earlier stage.
- Line 148-149: I do not agree that the information related to WS06 is more important than that on MUCIN. The fact that MUCIN is different reveals that the environment has substantial differences between tc and t0, i.e. before and after convection is triggered (you wrote that other buoyancy terms can be evaluated, but I do not see which ones you consider here).

Section 3:

- Line 164: I do not understand why the eastern half of Spain should be special from the point of view of upper-level forcing for ascent.
- Line 170: in other Mediterranean areas the peak of hailstorms occurs in June (e.g., Manzato, 2012), due, as expected, to a combination of strong diabatic heating and cold air intrusions, more frequent in late spring-early summer. Why does Spain behave differently?
- Figures 3, 4, 5: To highlight the differences, I suggest showing the SP-HAIL fields and the differences compared to the SP-NONHAIL fields. In the present version, it is difficult to detect the rather small differences. In addition, in Figure 5: contour lines are very difficult to identify, differences are not clear, coastlines can be hardly identified.
- Line 197: do you mean short or small in amplitude?
- Line 205 and elsewhere: moisture, not moist;

- Line 208 and elsewhere: easterly winds, not eastern;
- Line 209-210: I would rather say that the difference in DWPT is mainly a consequence of the different dominant seasons in the two supercell datasets.
- Lines 211-213: "The high elevations reduce the role of convective inhibition, which is also met by the convergence of southwestern and eastern surface winds": what do you mean??? do you mean that the orography forces the air parcels to be lifted above the LFC?
- Line 223: "This maxima omega area matches with positive Q-vector divergence values … and convergence of Q-vectors": I do not understand: do you mean that maxima omega values are superimposed with both divergence and convergence areas???
- Line 225: "higher values of maxima omega in SP-NONHAIL at 850-500 hPa": why do you consider in the following analysis only the maxima omega vertical velocity at 700-400 hPa thickness in SP-HAIL and not the maxima omega in SP-NONHAIL at 850-500 hPa?
- Line 227: wind convergence does not enhance and reinforce convection, rather favors triggering.
- Line 231-232: I am very confused, I see values of order 30 m/s, never below 20 m/s, in Fig. 5.
- Line 245: 90-th percentile with respect to what?
- Line 247: what do you mean with "a better buoyancy distribution"?
- Line 248-249: I do not see where the value of CAPE is reported, and where you show that the CAPE values are larger at t0 than at tc;
- Line 250: "The CIN in SP-HAIL increases from t0 to tc": where do you show this increase? also, it is very hard to physically understand why CIN increases: should not the convection remove progressively the inhibition?
- Line 252: -137 J/kg is a rather extreme value for CIN, I do not believe that convection can develop even in the presence of mountains with such a value; conversely, a value for about -50 J/Kg as reported by Taszarek et al. (2020b) (Line 317) appears more reasonable.
- Line 254-255: "a higher LCL is related to the width of the deep convective updraft, resulting in a wider, deeper, and faster vertical velocity": I do not understand how general this result is and the physical reasoning for that;
- Line 257: why is it relevant to have large WS values "above" the updraft height?
- Line 260: "The evolution from t0 to tc depicts a reduction in WS for SPHAIL": where do you show this point?
- Line 261: what does "contrary to the SP-NONHAIL episodes" refer to?
- Line 266: "Different distributions can be seen in Figure 7": I would say this is not relevant, it is rather a consequence of the different dominant seasons in the two categories.
- Line 266: what do you mean with "bimodal distribution"? I do not see it in Figure 7.
- Lines 272-276: I think the differences in humidity are mainly due to the different seasons prevailing in the two categories and not to the different wind features;
- Line 284: what do you mean "with a lower amplitude"?
- Lines 287-288: I would rather state that MLCAPE is very close to SBCAPE.
- Line 304: you cannot compare values in high-resolution models with those in reanalyses.
- Line 305-307: "the CAPE values found in our study would correspond with those for tornadic storms … finding SBCAPE values higher than 400 J kg-1 in tornadic storms": here you find that 75% are below 400 J/kg, so they do not correspond.
- Figures 8, 9, 10: what time do the figures refer to?
- Lines 319-324: the causes you address for the high CIN would be relevant in case you consider soundings at times distant from the development of the cell, while it is very strange that you have such a high CIN during or in the proximity of convection.
- Line 336 "the median MLLCL in SP-HAIL events is greater": this may be due to the presence of the mountains.
- Line 337: how do you interpret physically the higher LFC for SP-HAIL? I would rather

expect that a lower LFC would favor deeper updrafts and then stronger hail formation!

- Line 339: values of MLLFC higher than 2000-3000 m appear extremely high (you even obtain values of 5000 J/kg!): how do you explain them?
- Lines 350-355: Figure 10 shows the opposite compared to what you wrote, i.e. wind shear is higher for SP-NONHAIL.
- Line 363: about SHR01 "the median value of SP-NONHAIL is higher than SP-HAIL": this appears counterintuitive: any explanation for that?

REFERENCE: Manzato, A., 2012: Hail in NE Italy: Climatology and bivariate analysis with the sounding-dervied indices, J. Appl. Met. Clim., 51, 449-467.