Review on “The climatology and nature of warm-season convective cells in cold-frontal environments over Germany” by Pacey et al.

I would like to thank the authors for considering all of my previous comments and revising the manuscript in an appropriate way. I especially appreciate the addition of Figure 3 which considerably strengthens the manuscript. However, in my opinion the authors could still improve the manuscript regarding two of my previous major comments:

Previously I wrote *“The first major comment concerns how well this study fits the scope of this journal and the broader context of the results. In the manuscript the link to actual hazards is weak and little emphasis is given to this aspect. Lightning is considered but relatively briefly. It should be clearer how the results of this study inform about meteorological hazards.”* Although this has been improved I still find the link from the convective cell characteristics to hazards weaker than it could be. Some specific comments related to this:

1. Line 64. Mesocyclones are rather suddenly mentioned at the end of the introduction with little background given to why the presence of these features would lead to more hazardous / extreme weather. Details could be added to the introduction on how does the presence of a mesocyclone related to a hazard.

2. Do the hazards (lightning, hail) associated with convective cells vary if they are on the pre-frontal or post-frontal side of the cold front? This is some what included in question 2 in the introduction and it is in the analysis but it is not clearly stated in the introduction that this is covered in the manuscript. Another place where the link between meteorological features and hazards could be strengthened is on lines 78 – 79 where it is stated that “For the nature of cells we investigate cell lifetime, propagation speed, organisation, lightning frequency, cell intensity, and mesocyclone frequency” → here text could be added explicitly stating that how hazards vary by distance from the front are investigated.
3. In the response the authors state “*We will also emphasise in the conclusion that this work improves understanding of convective hazard climatology*” but when reading the revised conclusions I see that details concerning the results from the new Figure 3 have been added but text about how hazards (hail, lightning) relate to fronts as identified from this study is still mainly lacking.

The second major comment that I feel the authors could do more to address regards the clustering. Additional details about the clustering have been added, which I appreciate, but I still feel the justification for using k=30 ***then*** removing 6 clusters is weak. In particular, I find it hard to understand why this is an more appropriate choice than using k=24. At a minimum the authors should show the 6 clusters that the remove from their analysis. Furthermore, Figure 9 could be reproduced in the supplementary material with a few different choices of the number of clusters so that a reader can see how sensitive the results are. In particular, there is a localised maximum in the Silhouette score at k=9 so this would be interesting to present – and if the results do not show something physically meaningful this would actually strengthen the authors choice of k=30.

Minor comments:

1. The caption in Figure 3 could be clearer regarding the description of the lines. Suggest using “...CAPE (dashed line), surface dewpoints (solid line), surface shortwave radiation (solid line with circular markers)”.

2. Line 226-227. The addition of Figure 3 makes many of the conclusions presented in this manuscript much more robust and I’m really pleased to see some evidence to support the commonly written claim that cold fronts have a slope of 1:100 – thank you. However, how

exactly has figure 3 been created? Does every front / convective cell pair contribute values at all grid points shown in this figure domain? e.g. for each front is the cross section extracted from ERA5 and then all of these averaged? Can a few additional details be added here? Furthermore, it is not clear how or why the normalisation has been done for CAPE, dewpoint temperature and solar radiation. Can these details also be added.

3. Section 3.1.2 / CAPE. What type of CAPE is this? Most unstable CAPE? Surface CAPE?
4. Line 255. Suggest to move "(straight dotted line in Figure 3) earlier in this sentence as it currently implies that cloud cover is plotted in Figure 3. Also see minor comment #1 above regarding line description.

5. Line 264 – 266. While I think it is now fine to state that the surface front is on average 300km ahead of the 700-hPa front (e.g. there is evidence for this in Figure 3), the authors still assume that all fronts are the same. This assumption could be supported by computing some estimate of variability in the parameters shown in Figure 3. e.g. what is the standard deviation of the convergence or the 25-75% percentiles of the CAPE values (could be shown on Figure 3). While this is not essential, it would strengthen the manuscript.