Authors' responses in red italics

Submitted on 05 Sep 2023
Anonymous referee #1

We thank the reviewer for taking the time to review the manuscript again and for their constructive comments and feedback in the previous round.

Submitted on 28 Sep 2023
Anonymous referee #2

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:

We thank the reviewer for reading through the revised manuscript and providing further constructive feedback. We provide a point-by-point response below.

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.

We have added additional text introducing mesocyclones and their link to hazards (see L57–60 of tracked changes manuscript). Thank you for the suggestion.

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.

We have added the following line after the quoted line. "The nature of cells section will therefore provide novel insights into how convective hazard climatology varies depending on the distance from the front." (see L79–80 of tracked changes manuscript) Thank you for the suggestion.

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.

We feel the link to hazards is already covered in the conclusions. The results using the 55 dBZ threshold is mentioned several times as well as lightning, and mesocyclones. We remind the reviewer that convective cells (defined by 46 dBZ) are a hazard themselves, the corresponding rain rate could cause a flooding hazard. Furthermore, the forecasting of hail for example is not possible without first identifying where the convection will initiate in the first place.

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.

The goal of k-means clustering is to separate similar data into clusters by minimising within-cluster variance and maximising between-cluster variance. Minimising within-cluster variance can usually be achieved by increasing the cluster number. However, the results become less interpretable and there could also be more clusters with similar features (low between-cluster variance). Lowering the cluster number could increase the between-cluster variance at the expense of increasing within-cluster variance. Running the algorithm with 30 clusters then removing 6 clusters seems to provide a good balance between low within-cluster variance and high between-cluster variance for the remaining 24 clusters. The 6 removed clusters have high within-cluster variance, i.e., they contain fronts with different shapes and orientations, so we don't see any reason to include this as supplementary material. We have however produced plots for cluster numbers 15, 35 and 50 and put them in appendix as the reviewer suggests.

In summary, there is of course always potential for further optimisations with such machine learning algorithms, but one must also be pragmatic. We hope this settles the reviewer's concerns.

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)".

   Thank you for the suggestion, we have amended the manuscript.

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.

The means are calculated using all instances that an ERA5 grid point has a certain front distance and is not weighted by individual timesteps. We have added this line to the revised manuscript.

The variables are normalised to compare the typical magnitude of each variable at different distances from the front. This was already briefly mentioned, but we have now broken it down to a separate sentence so that it is clearer (see discussion between L219–225 of tracked changes manuscript).

Thank you for the suggestions.

3. Section 3.1.2 / CAPE. What type of CAPE is this? Most unstable CAPE? Surface CAPE?

Most unstable CAPE. This is mentioned in the last line of the Figure 3 caption.

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.

Amended. Thank you.

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.

We appreciate the suggestion, but we feel adding additional lines would make the plot too crowded.

Submitted on 19 Sep 2023
Anonymous referee #3

The authors have thoroughly addressed my comments and I did not find anything else to improve. Great work!

We thank the reviewer for taking the time to review the manuscript again and for their constructive comments and feedback.

One minor comment is below:

line 220: Perhaps add a reference supporting your point that this is a typical slope for cold fronts?

We have added the following reference. Thank you for the suggestion.

Bott, A.: Synoptische Meteorologie: Methoden der Wetteranalyse und -prognose, Fronten und Frontalzonen, p. 397, Springer Berlin Heidelberg, Berlin, Heidelberg, https://doi.org/10.1007/978-3-662-48195-0_11, 2016b