

Review of egusphere-2024-2978

‘Environments and lifting mechanisms of cold-frontal convective cells during the warm-season in Germany’

by George Pacey et al.

Manuscript in review for *Weather and Climate Dynamics*

Summary

To characterize the environments in which convective cells in cold frontal regions form, the manuscript combines convective cell tracking and detection with automatic front detection. The thermodynamic conditions and different lifting mechanisms for (i) convective cells, (ii) cell environments, (iii) and non-cell environments are compared using ERA5 data. Moreover, the results are binned in different categories based on their distance to the cold front location which facilitates to discriminate between post-frontal, frontal, and pre-frontal convection. Their results indicate that environmental conditions favorable for pre-surface-frontal and post-frontal convection differ. For example, pre-surface frontal convection appears to be favored by large CAPE values, while for post-frontal cells mid-level relative humidity appears to be relevant for convection to be present. The manuscript addresses open questions concerning the initiation of convection near cold fronts, and the applied methods are suitable to address the research questions. Generally, the manuscript is well-structured and the figures are presented clearly. The research questions are clearly stated and of relevance for the readership of *Weather and Climate Dynamics*. I recommend publication of the manuscript in *Weather and Climate Dynamics* but I have several comments that should be addressed prior to publication, which are outlined in detail below.

1 General comments

1 General structure of the manuscript

Overall, the manuscript is well-structured. Yet, the Results Chapters include individual phrases and paragraphs that rather belong in the Introduction or Methods or Discussion section. I would ask the authors to make sure that the information is consistently placed in the appropriate sections and revise/streamline the Results Chapter accordingly. This does not pertain to the overall structure of the manuscript (which I think is good) but rather to several individual sentences/paragraphs. For example, Section 3.4 starts with one paragraph of introduction to Q-vector convergence which could be substantially shortened by moving the content to the Introduction and/or Methods Sections. Similarly, I would ask the authors to make sure that their discussion of results is consistently placed in the Discussion Section (and not in the Results Section).

2 Introduction of data set

The presented study follows up on a study published by Pacey et al.

(2023). I think the reader would profit from summarizing the key results from the predecessor study (Pacey et al., 2023) in more detail.

3 Quantification of uncertainty

Most figures show only mean values binned in distances from the front. I would appreciate if a measure of uncertainty (e.g., standard deviation as shading, percentiles, etc.) could be additionally shown to illustrate the associated uncertainty and variability. I appreciate that significant differences are emphasized in the figures, however, including a measure of uncertainty would be beneficial, for example, as shown by the boxplots in Fig. 5. Figure 5 also shows that the distributions are not Gaussian, i.e., it may be useful to show the median in addition to the mean for some variables that are not normally distributed.

4 Definitions of CIN (vs CAPE)

The authors apply only one (of many definitions) for CAPE but several definitions of CIN. I would ask the authors to better justify why different definitions are used for CIN (but not CAPE). I'm not sure the manuscript profits substantially from the other definitions of CIN (i.e., the authors could consider moving panels (b) and (c) of Fig. 5 to the Appendix to streamline the manuscript), as the key conclusions do not depend on the detailed definition of CIN. Instead, I would be interested to see a comparison of the CIN distribution of convective grid points with the CIN distribution of the convective environment (and non-convective environment). Yet, the authors have mentioned that the computation of CIN for that many grid points is computationally demanding, and I understand if this is not feasible here.

5 Conclusions

In the conclusions, the authors summarize the most relevant factors for convective initiation by discussing each variable separately (e.g., CAPE, RH, Q-vector convergence, etc.) following the structure in the Results Section. I would find it more helpful if the authors could cluster the key factors and differences for pre-surface-frontal, near-700hPa-frontal and post-700-frontal as substantial differences between these categories are presented (i.e., similar to the structure in the Discussion Section). Finally, the authors have clearly defined three research questions in the introduction, which could be addressed again in the conclusions.

6 Writing style

This is only a minor point, but I would ask the authors to capitalize the first letter if a specific section or chapter is referenced (e.g. Section 1, instead of section 1). Moreover, please double-check the usage of ";" throughout the text (see also specific comments below).

2 Specific comments and technical corrections

1. 5 "At other front relative regions": At this point in the abstract, it is unclear what "other front relative regions" refers to. Please rephrase or add additional explanations in the abstract.

2. Introduction

3. l. 15: "; primarily due to convective permitting models (CPMs) at increased resolution": This does not read like a complete sentence. I would suggest to avoid the usage of ";" in the text (please also check later occurrences in the manuscript). I also think that it is common to use "convection-permitting" (instead of "convective permitting").
4. l. 28: Please remove "etc" and specify.
5. l. 84: Please avoid double-brackets ") ("). See also l. 129, 258, 357.
6. l. 32 ff: This reads a bit colloquially, please rephrase.
7. l. 43 f: "The literature would benefit from studies quantifying the relevance of frontal lifting at different regions relative to the front, especially during the warm-season.": This sentence appears a bit out of context, I'm not sure if it necessary here.
8. l. 51 ff: The role of wind shear should be discussed together with other factors relevant for convection, i.e., in the paragraph starting in l. 27. Moreover, following General Comment 1, I would ask the authors to include the background information provided in the Results sections in the introduction. Overall, the introduction would benefit from more clearly summarizing and structuring key variables relevant for convection, targeting specifically convection embedded in the frontal environment.

9. Methods

10. l. 85: I would ask the authors to include one explanatory sentence on the TFP equation.
11. l. 106: "The process is repeated 30 times to remove any local-scale features.": Please explain here, why specifically "30 times" was chosen. I would ask the authors to elaborate on why this specific smoothing method was applied (in contrast to other methods)?
12. l. 109: "than some previous studies": Please include those studies here.
13. l. 118 f: "This is also supported by the mean maximum climatological surface convergence in ERA5 data (Pacey et al., 2023; their Figure 3)". Please add some additional information on how this figure supports the statement, such that the reader does not have to read the mentioned publication themselves.
14. l. 132 ff. Please label the criteria following your approach above (i.e., "(A)", "(B)", etc.), and add text describing the criteria.
15. l. 142: Does the time of first detection correspond to the first exceedance of 46 dBZ? Would it be possible to track these cells also before they reach maturity, i.e. in their developing phase with lower reflectivity?

16. l. 139 ff: "Since some cells have a lower area than the grid size the bounds are increased by 0.125 degrees (half a grid point)": I find this difficult to understand, please rephrase.
17. Figure 1: This figure nicely illustrates the definition of the three categories. For further illustration, the authors could additionally include the position of the surface cold front as well as the defined pre-frontal, pre-surface frontal and post-frontal regions.
18. l. 152, Table 2: I appreciate that detailed numbers of grid points in each category are provided in Table 2, yet it would be easier to comprehend if the numbers were shown as an additional figure.
19. l. 161 ff: The choice of variables should clearly be motivated in the introduction, such that referring to literature in this short paragraph is not required anymore.
20. l. 166: As mentioned above, it is not fully clear to me why different definitions of CIN, but not of CAPE are used.
21. l. 168: "So that a CIN value is present for all grid points": Please rephrase (e.g., "To ensure that ...").
22. Equation 3: Please define the symbols/abbreviations in the text.
23. l. 193 f. I would ask the authors to elaborate on the smoothing method and how the number of smoothing cycles was exactly determined (see also previous comment above)?
24. **Results**
25. l. 203: It could be helpful for the reader to show and discuss the occurrence frequency of convection (Fig. 12) before Sub-sections 3.1, 3.2, etc. to familiarize the reader with the dataset.
26. l. 207 ff: The authors emphasize the differences in dewpoint between convective and non-convective environments. Did the authors also consider the 2-m temperature distributions? Are the differences in dewpoint related to differences in humidity (i.e., dew point depression) or to differences in the background temperature? I would be interested to see Fig. 2 for 2-m temperature.
27. l. 236-241. This is repetitive. Please streamline and/or move to Methods and Discussion Sections.
28. l. 241 f: Please rephrase and avoid using ";".
29. l. 259: I assume the two numbers have been swapped? I guess post-700-frontal should have lower absolute CIN values.
30. l. 262: "by forecasters" is not necessarily required.
31. l. 263-271: Please streamline this paragraph.

32. l. 279: "shift to the left on the plots": Please rephrase.
33. l. 297 f: The authors show mean vertical velocity for cell grid points of on average a few cm/s. I would ask the authors to relate those numbers (in the Discussion) to typically observed convective updraft velocities and discuss its implications for using ERA5 vertical velocity for studying convection. Is vertical velocity normally distributed and are large outliers present?
34. Caption Fig. 7: Please correct the typo: "Postive"
35. l. 335-336: This sentence is not fully clear to me, please rephrase.
36. l. 351: I would appreciate if the authors could include a more original reference (in addition to EUMeTrain).
37. l. 352: Please rephrase "condensation" by "cloud formation" or similar, as condensation inherently implies that precipitation only forms from warm-phase cloud processes.
38. l. 354 ff: Fig. A2 suggests that a substantial number of identified cells occurs in an environment with large cloud cover, and thus, may be embedded in a larger precipitating cloud system (see Fig. 11), such as the warm conveyor belt (which has also been mentioned). The Discussion Section could profit from including studies on precipitation characteristics and distribution in the warm conveyor belt airstream compared to pre-frontal convection.
39. l. 361-364: I'm not sure if I agree with the conclusions in this paragraph. Assuming ERA5 would (at least partially) represent convection, the precipitation signal would show up in "Large-scale precipitation", and not in the parameterized convective precipitation. In this case, I would expect a difference between the three categories in Fig. 11a (which is very small, in particular pre-frontal). Instead, the parameterized "convective precipitation" differs between categories (Fig. 11b), suggesting that convection cannot explicitly be represented in ERA5 (and needs to be parameterized). Apologies, in case I mis-understood this paragraph. I would ask the authors to rephrase this paragraph.
40. l. 366 ff: Please streamline this paragraph, this information has been repeated several times.
41. l. 371 ff: I appreciate that the authors discuss and summarize the relevant factors for (i) pre-surface-frontal cells, (ii) near-700hPa-frontal cells, and (iii) post-700-frontal cells. While the overall structure is good, this section could profit from (even more thoroughly) comparing the presented results to previous studies, and e.g., pick up literature that has been mentioned in the introduction.
42. l. 408: typo: include "°" in "16 C".
43. **Conclusions**
44. l. 401: Please remove "etc" and specify.

45. l. 408-426: In general, I think it is ok to use bullet points to emphasize key conclusions. Yet, I would reduce the number of bullet points and more strongly aggregate the relevant information.
46. l. 422 f: It is expected that convective cells are associated with positive vertical velocity. In Section 3.5, the authors have briefly mentioned the implication for consistently seeing this signal in ERA5 data. I think the manuscript would profit from a more thorough discussion on ERA5 and its ability to (at least partially) represent convection and convective precipitation. The last sentence of the manuscript brings up this open question, yet it could be discussed more thoroughly.
47. Have the authors considered including a schematic that illustrates and summarizes the relevant factors that discriminate between convective and non-convective conditions in different regions of the front?