Review

Derecho-favoring atmospheric environments in Finland: characteristics, identification criteria, and increasing frequency

Summary: This manuscript presents a number of derechos over Finland and discusses the synoptic conditions under which they occur. Subsequently two derecho-criteria sets are investigated, and a third one is developed, tailored to the Finnish cases. All three criteria sets are evaluated against the case sets in Finland. By applying the criteria to the ERA5 reanalysis, trend analyses are performed, concluding that derechos are increasingly likely over Finland in a warming climate. Additionally, it is hypothesized that the same should apply to the rest of Europe, giving similar warming trends.

General remarks: The premise of the study is very compelling, looking at a rare, severe convective phenomenon at high latitudes and discussing the occurrence conditions in depth. However, the following analyses lack justification and discussion depth. The choice of criteria sets is not well justified, and neither is the development of the tailored index. Why were some variables considered and others not? Was this a comprehensive analysis of the parameter space, that can yield the optimal constraints? Moreover, the analysis of trends in ERA-5 is split into 2 periods. The earlier period (with a lesser warming trend) has a relatively high derecho occurrence, whereas the later period (with a greater warming trend) begins from a lower occurrence frequency and increases approximately to the level of the earlier period's average. Over the entire period, barely any criteria sets yield a significant trend. The authors argue a link to the warming trend in the second time period. However, decadal variability and the influence of other low-latency phenomena is not discussed. Given the rarity of the event and the large interannual variability, I would not necessarily expect a trend to emerge, in line with what the data shows. At this point in time I recommend major revisions.

Major remarks:

1. Method of obtaining DEF: How exactly were these parameters selected and optimized? Looking at the cases, it seems like some other approaches may be more promising for a spatial match, e.g. focusing on the strength of CAPE and shear gradients, rather than just absolute values.
2. Why is there such a strong focus on the 6 and 10 km criteria sets, when they were not developed for Finnish derecho environments and appear to perform rather poorly? Why are the trend analyses continued with these, if they do not capture the activity well?

3. Decadal variability vs. climate change trend: I am missing a discussion of other components than just a long-term temperature trend contributing to derecho activity variability. Literature on lightning activity in Western Europe discusses a period of lower lightning occurrence after 2000 owed to anomalous occurrence of NAO patterns. Similar teleconnection patterns and decadal variability clusters may be at work here.
4. The climate change discussion could be expanded in more depth from a physical processes point of view. Systematic environmental shifts contributing to instability, convective organization and severe wind events should be discussed in this context. Moreover, the whole trend analysis hinges on the assumption that proxy criteria remain stationary in a changing climate. This limitation should be mentioned and discussed somewhere.
5. Structure and length: The main body of the manuscript focuses on 2 case studies in more detail, but other cases in the supplement are referred to very frequently. This to the point that the manuscript doesn't fully stand on its own without the supplement. Perhaps including one more case study in the main material could reduce this dependency. Contrastingly, the parameter and trend discussion includes all 3 criteria sets even after showing that the first 2 do not represent Finnish cases particularly well. This part could be trimmed down to sharpen the messaging – or the inclusion of all 3 parameters should be justified more.

General remarks on figures:

Most figures have very small text labels. Moreover, the colors of the 3 criteria sets keep changing between the plots for no evident reason. Streamlining the figures to be more coherent among each other would improve the readability of the manuscript.

Minor remarks:

1. Line 22: Several modeling projects now have km-scale resolution, allowing for explicit analysis of convective storms. In light of the development of the past few years in this area, this should be rephrased.
2. Line 25: Given the breadth of detected and expected convective trends in Europe, I would recommend broadening the referred literature a bit beyond Rädler et al., 2019.
3. Line 31: Given the vastly different spatial scales of a tornado and a hurricane, a single event generally has a very different hazard scale. On a global level, severe convective storms now rival the damages that hurricanes cause, but not tornadoes in isolation.

4. Line 89 (and in other locations of the manuscript): The citation format should be fixed, so that parentheses are not occurring inside of parentheses
5. Fig 1: Please homogenize the fonts on the figure labels and inside of the plot
6. Line 113: are the heights above ground level or sea level?
7. Line 121ff: Please express the parameter calculation in an equation.
8. Line 127: Please refer to MetPy with a reference, including the package version.
9. Line 133f: Given that ERA5 data is available hourly, why was it evaluated in 3-hourly steps and only in the afternoon? Overall, MCS and derechos are also known to occur at night and, more rarely, do also occur in morning hours. Given the small sample size of events over Finland, I am not confident that excluding all other times of day is a valid choice.
10. Line 166 (and other places): please avoid double parentheses.
11. Line 174ff: There are a number of papers discussing synoptic weather patterns during severe weather outbreaks in Europe, finding similar features such as southwesterly flow and being at the interface between a trough and a ridge. At least some of these should be mentioned here.
12. Section 3.2: CAPE has a clear latitudinal dependency (or also temperature dependency) in its climatological distribution. High values for Finland would not necessarily be high elsewhere.
13. Line 161: Additionally, the shear is distributed differently – in the US, a much greater amount of shear occurs near the surface
14. Section 4.1: Please already refer to Table 2 for an overview of the criteria sets. Is the area threshold for a connected area or any spatial arrangement of gridpoints exceeding the criteria?
15. Section 4.2: It is not clear, why exactly these criteria were chosen. How was it tailored or optimized? Why were other parameters not considered? Is this a complete evaluation of the parameter space?
16. Figure 5: It is hard to see the DEF criteria in the top panel – perhaps the plotting order could be reversed, so that the lightest color is on top? Or different line thicknesses could help.
17. Line 346ff: This makes me wonder if spatio-temporal gradients may be more reliable for derecho-characterization.
18. Line 362: In general, most convective proxies are overactive. They contain little information about actually realized convective initiation or triggering.
19. Section 5: Why not focus this section more on DEF?
20. Line 389: why was twice per day chosen here?
21. Line 409: Especially for DEF, but also for the other 2 criteria, looking at the 7-year moving mean, it does not appear that the second period is systematically more

active than the first. There is also no significant increasing trend over the whole period for the 6km and DEF criteria.

22. Line 420: This would be a good place to include a discussion of decadal variability and teleconnection patterns.
23. Line 465: This would be a good place to discuss the limitations of assuming a stationary proxy.
24. Line 488ff: This discussion on climate change needs to be deepened. Especially regarding the behavior of the jet with climate change, there are contradicting studies and the future uncertainty is large. Some literature on the expected change in severe wind events would also be warranted here. I do believe that the conclusion of assuming an increased frequency of derechos in future based on the analysis here is overstretching the conclusion.

Recommended literature:

- https://journals.ametsoc.org/view/journals/clim/38/1/JCLI-D-23-0633.1.xml -> derechos and climate change over the US
- https://journals.ametsoc.org/view/journals/bams/102/2/BAMS-D-20-0004.1.xml -> environmental trends over Europe (and the US)
- https://www.nature.com/articles/s41558-023-01852-9 -> expected changes in convective winds in warmer climates
- https://meetingorganizer.copernicus.org/ECSS2023/ECSS2023-87.html / https://presentations.copernicus.org/ECSS2023/ECSS2023-87_presentation.pdf -> QLCS climatology over Europe, including derechos
- https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/qj.5070 / https://wcd.copernicus.org/articles/6/1089/2025/ -> synoptics of severe convection over Europe in general
- https://link.springer.com/article/10.1007/s00382-022-06589-3 -> convection permitting climate modeling over Scandinavia
- https://iopscience.iop.org/article/10.1088/2752-5295/ad22ec/meta -> synoptic situation of clusters 4, 11 and 13