

We would like to acknowledge the reviewers for their insightful comments and valuable suggestions regarding the scientific quality and relevance of our paper. We have taken the concerns raised by the reviewer seriously and have made significant revisions and amendments to our methodology and manuscript in line with their recommendations. We believe these revisions have significantly enhanced the scientific quality and value of our paper and we hope that our point-by-point answers to each of their major concerns will sufficiently address the issues they have raised.

I. Reviewer 1

The paper reconstructs tracks of past major derecho events over France and examines the prevailing environmental conditions in terms of daily mean sea level pressure and near-surface temperature, total precipitation, and wind fields using ERA5 reanalysis. Based on the obtained event set, pressure analogues were estimated as 29-day mean fields for two 30-year periods. Changes in the considered meteorological fields are attributed to climate change. For most of the events studied, the authors found increased precipitation and temperature, while the pressure pattern remained largely unchanged. They also investigated the possible influence of natural climate variability on these changes, based on ENSO and AMO parameters.

I struggle with the scientific quality of the paper and its value to other researchers. My main concerns - among others - are the very small sample of derecho events (see major revision point 1), and that the authors relate the occurrence of derechos solely to near-surface fields. However, while the pressure fields provide the general setting for convection, both MCS and derechos are initiated and maintained by mechanisms in the middle and upper troposphere (e.g., potential vorticity PV anomalies, jet stream-related divergence) and by thermal instability, for which the 2-m temperature is not a reliable proxy (major revision point 2).

See below a list of major and minor revision points and a (short) list of edits.

Major revision points:

1. The authors used severe weather reports, mainly from Keraunos, to determine their list of past derecho events. However, in their 30-year study period from 1983 to 2022, they found only 11 derechos, i.e. 37 events per year, but only one in the first 20-year period. I assume that there is either a substantial underreporting of the severe weather reports during the first period, or that the criteria used for the detection failed. For example, Gatzen et al. (2020) identified 40 derechos between 1997 and 2014 that at least partially affected Germany. Therefore, I doubt that the sample is representative for a larger time period. Moreover, I wonder why the authors did not use station data in addition.

We addressed the concern of limited sample of derecho events by detecting past events in France between 2000 and 2022. We decided to focus on warm-season events as one motivation for our study was the derecho of 18 August 2022. We used station data from the French national weather service Météo-France to detect days with concentrated area of severe wind gusts reports and used the PyFLEXTRKR algorithm to automatically detect and track potential associated mesoscale convective system, using satellite imagery (NASA Integrated Multi-satellitE Retrievals for Global Precipitation Measurement (IMERG) dataset). We also used ESWD data to account for reports in other countries. We inspired from the methodology of Gatzen et al (2020) although we didn't use

radar data we think the use of satellite data is a practical approach to study MCS given their global coverage and ease of access, although this induces some limitations that are highlighted in the manuscript. Using this methodology, we significantly increased the sample size to 29 events for the warm-season only.

2. Derechos (or MCS/MCC) are not triggered by surface lows as stated several times (the same applies to T2m). This fact is acknowledged by the authors when they state that “there is no one-to-one correspondence between large scale low pressure systems and the occurrence of derechos” (L60/61). Nevertheless, they quantified composites of analogues based on daily mean sea level pressure. Evans and Doswell (2001), Burke and Schultz (2004), or Coniglio and Stensrud (2004), for example, considered the 500 hPa geopotential height in their investigations of derechos, while Gatzert et al. (2020) additionally considered model-derived PV anomaly and wind shear and stability parameters (e.g., CAPE) from proximity soundings. I strongly recommend considering parameters directly related to strong wind gusts (see, for example, empirical wind gust models, such as those from Wolfson, 1990; McCann, 1994; Nakamura, 1996; Geerts, 2001; or Dotzek und Friedrich, 2009).

Based on the reviewer’s suggestions, we have used the 500 hPa geopotential height as a proxy of mid-level atmospheric flow for the analogues search and we included the analysis of changes in convective instability as measured by daily maxima of CAPE and deep layer shear (DLS) defined as 0-6 km wind shear, which are the most common environmental parameters used as predictors for severe convection.

3. The authors rely on ENSO and AMO as proxies for natural climate variability and state that changes in the meteorological fields for episodes negligibly affected by these two modes of climate variability can be attributed to climate change. But what about other low-frequency modes of variability, such as teleconnections (NAO, Scandinavian or East Atlantic patterns) or SST? The reference to Nobre et al. (2017) and their finding that ENSO is important in some regions of the continent is only half the truth. They actually concluded: "that positive and negative phases of the NAO and EA are associated with more (or less) frequent and intense seasonal extreme precipitation over large areas of Europe. The relationship between ENSO and the occurrence and intensity of extreme precipitation in Europe is much smaller than the relationship with NAO or EA, but still significant in some regions".

We agree that other low-frequency modes of variability, such as teleconnections and SST, can also play an important role in extreme weather events. The focus of our study was to investigate the changes in meteorological fields for episodes negligibly affected by ENSO and AMO as these are well-known and widely used climate indices. However, we acknowledge the importance of other modes of variability and agree that they should also be considered in future studies. We have thus included more factors of natural variability in our analysis (PDO, NAO, EA and SCAND patterns, in addition to ENSO and AMO) as suggested by the reviewer. We included some other references discussing the roles of these different factors in Europe and revised the part where clarified the interpretation of Nobre et al. (2017) results.

4. ERA5 has too coarse a resolution for reliable estimates of convective gust wind speed and convective precipitation. It is not clear to me why the authors did not use available station data such as E-OBS for their study period.

We have taken into account the reviewer's advice as mentioned in the answer to point 1., we changed our strategy and used severe wind gusts reports from weather stations to detect and track derechos.

5. Without a detailed interpretation of the results, the discussion of the results is not very scientific. The fact that the 2-m temperature has increased is not a new result, but is stated in every single subsection. I would suggest summarizing the results, especially when they don't really differ (e.g. negligible pressure differences between the two periods found in almost all cases). The use of the same phrases and almost the same wording in subsection 10 is not very clever (e.g., "The analogous analysis is shown in Figure A4").

We appreciate the reviewer's comments and agree that a more detailed interpretation of the results was necessary in the discussion section. We have expanded and improved the analysis of the results and grouped all events (29) to interpret the climatology of detected events (section 2.1) and the results of the attribution study (section 2.3) instead of making separate treatment for each event. We still give a more detailed interpretation for the derecho of August 2022 (section 2.2) to explain how the results shown on Figures 4,5 and 6 are analysed.

6. Why are the derechos tracked using precipitation data? Derechos are defined as contiguous areas of high wind speed, but the relationship to precipitation is not that straightforward.

We have changed our strategy for detecting and tracking derechos as explained in answer to point 1.

7. Introduction: The discussion of how climate change is expected to modify the intensity and probability of convective storms is too general. There are several papers on this that the authors should refer to. The paragraph in the current version implies that there is not much research in this area.

We included a more precise and up to date discussion of the effect of climate change on the intensity and frequency of convective storms, adding more references.

8. Introduction: The objectives of the paper are very vague and imprecise.

We have reworked the introduction to better motivate our study and highlight the purposes of our work, which includes:

- a detection of warm-season derechos in the past 23 years and the analysis of their frequency and characteristics, in comparison with other countries.
- the identification of potential changes in synoptic patterns and environments associated with past warm-season derechos in France, along with a first assessment of the roles of climate change and internal variability.

9. The English writing is (almost) acceptable, but I'd suggest a native speaker / editing service to improve the writing.

We did our best to improve the level of English, taking in consideration the suggestions of the reviewer.

Minor revision points:

1. The term “attribution analysis” is misleading when only surface pressure and a few other meteorological parameters are considered.
2. L4: Why writing “in the satellite era” when no satellite data is used or shown? This is somewhat misleading.
3. L8 and elsewhere: What do you mean by “unprecedented”?
4. L11 “clear change in depth of the low pressure system trigger”; more important than the absolute minimum is the pressure gradient; trigger is not the right word either, because a low pressure system does not trigger derechos (otherwise you would have identified a much higher number of derechos)! The same goes for L13 “low pressure systems possibly leading to derechos”.
5. L15: Give a reference when introducing derechos
6. L16: “..serial downburstThis is somewhat misleading because derechos can be divided into serial and progressive derechos.
7. L17: “bow echo”: as this is a clear sign of a derecho, briefly explain the term and give a reference.
8. L19-20: “meso-depression”; do you mean a wake low? Again I miss a reference.
9. L28-29 “only a few derechos are registered each year in the world”. For example, Bentley and Sparks (2003) identified 230 derechos in a 15-years period in the US; Gatzen et al (2020) detected 40 events in 18 years in Germany. That's pretty much more than a few a year in the world.
- 10.L30-32: Sentence is unclear; what do you mean by better documented (more damage reports??) “where the available energy is important” is also unclear.
- 11.L34: “while the large scale conditions” should be specified
- 12.L40: Is this really true that the entire summer 2022 was governed by a high-low pressure dipole as stated???
- 13.Figure 1 is too simple. I miss the specification of the geopotential and the altitude, the jet stream, the location of the frontal systems, the warm air advection and so on, see literature. In the caption I do not understand the relation to the polar vortex. Besides, you should refer to the paper by Morris, 1986, who first used the term “Spanish plume”.
- 14.L49-50: Note that the predictability of MCS is much higher compared to isolated convection; can you underpin your statement with a reference?
- 15.Streamline the data section; in the current version, it's a mix of data and methods and also first results.
- 16.L85-86: “...consistency of a dataset...” You should mention that over the long term, both the instruments (e.g. radiosondes) and the assimilation of the data (e.g. satellite) have changed, affecting the reanalysis. Therefore, one must be very careful when using them for trend analysis.
- 17.Method section: Give some more details so the reader can really follow what you did.

- 18.L112: “The method...” Which method do you refer to? Only the comparison? I wouldn’t term this a method.
- 19.L114-116: This sentence is unclear; “making it statistically impossible to say whether climate change has made” in the context of an unprecedented event is confusing.
- 20.L118: “Westphalia floods”; the most severe floods in July 2021 were in Rhineland Palatinate (Ahr basin) and not in Westphalia.
- 21.“counterfactual world”: I wouldn’t use this world since the emission of GHG emissions started to increase at the end of the 19th Even if the largest changes in temperature have been observed since the mid-1980s, there were some signs of climate change before that. In this sense, the term “factual world” is not appropriate either.
- 22.L131: “daily averaged slp”: Does this mean you averaged over the 24 hours of ERA5?
- 23.L140: In what sense did you examine the seasonality?
- 24.L141: “using monthly indices” of what?
- 25.L182: “investigate the characteristics of the derechos”; you did not investigate that, but rather prevailing ambient conditions (see also major point 2)
- 26.L193: Severe hail for sure did not occur along a 1000 km axis.
- 27.L200: The 2m temperature is not directly related to the convective available potential energy (btw: include the term “available” for CAPE).
- 28.L204: Give some more details about the orographic influence
- 29.L250-252: Here the authors refer to mid-tropospheric levels; why not included in the analyses (see major comment 2). Besides, you should write here “(not shown)”.
- 30.L252 and several times later: the authors claim that the derechos originated from an MCC, without giving a reference or explaining how they determined the occurrence of an MCC (according to the extent of different satellite IR temperatures according to Maddox 1980?).
- 31.L260: “good” is a very subjective term. Can you describe / quantify what you mean by this?
- 32.L378: I do not agree that you did an in-depth analysis (see major comment 5)
- 33.L388: Again, low pressure systems do not trigger derechos
- 34.L408: Why did you mention the Arctic Oscillation at first? Is it really relevant for derechos in France? Do you have a reference for this? What about EA or SCAND?

Edits:

1. Authors should use the date (day) consistently; e.g., in L3, they write 18 August 2022, L189/190 August 18, 2022; all subheadings are in the form of dd-mm-yyyy.
2. If you are referring to something you have done once or so, you should use the simple past.
3. L3: of 18 August --> on 18 August
4. L8 reformulate “...that is no good analogues can be found...”; in particular specify “good”
5. L32 reformulate “the public opinion was shocked”
6. L33 “the related MCS formed...”
7. L36: fueled --> maintained or triggered, depending on what you mean; “hot water” for certain is an overstatement (you may specify SST)
8. L38 “there was immediate questioning” reformulate; by whom? Reference? Otherwise this is not really scientific...
9. L43 (and others): I’d prefer the term cut-off low rather than cold drop.
- 10.L46 use a more scientific formulation for “found guilty”
- 11.L48 is --> was
- 12.L50 statements --> analyses / assessments

- 13.L51 “these modeling difficulties”: be more specific
- 14.L51 “fate” is no appropriate expression
- 15.L71: The data section should be Section 2 and not 1.1; this affects, of course, all subsequent sections
- 16.L74: **convective** storm phenomena
- 17.L75: analysis --> analyses
- 18.L79: reconstructed
- 19.L81: “in Figure 2”
- 20.L81-83: include a reference for ECMWR/ERA5 already here
- 21.L113: delete “hurricane” as this is completely out of context
- 22.L117: “attribution protocol” reads somehow weird
- 23.L126: “natural **climate** variability”
- 24.L135: prohibit --> limited
- 25.L101 **an** MCS
- 26.L196 **Figure** 1
- 27.L225: of --> in
- 28.L230 reformulate “unstable context”
- 29.L265: were --> was
- 30.267: “...strong divergence that led to the development of...”; again an MCS
- 31.L286: Temperature was much warmer...
- 32.L310 use the abbreviation MCS
- 33.L323: upper-level minimum pressure (or geopotential height)
- 34.L367: a minimum **pressure** on

II. Reviewer 2

Review of “Changes in synoptic circulations associated with documented derechos over France in the past 70 years” by Fery and Faranda.

In this paper, the authors survey 11 past derecho events that affected France in the satellite era and construct reanalysis-based analogue datasets in order to place these derecho events in the context of two eras with different warming profiles (1950-1979 and 1993-2022). They find two of the events to have no good analogues, but for the events for which analogues can be found, they find that increased precipitation due to higher temperatures is most relevant.

The study puts together a useful dataset of events and analogues, and generally the methods used for the attribution study are sound. My greatest concern is with the emphasis and context of the paper and some of the interpretation (especially early in the paper) that somewhat overpromises based on the relationship between derechos and broad synoptic-scale SLP patterns. With some restructuring, I believe this paper will be a strong addition to the literature.

MAJOR COMMENTS

1. I am finding it difficult to agree with a lot of the analysis presented here mainly because fundamentally low-pressure systems are being used as a proxy for derecho occurrence. While the authors do acknowledge in Lines 60-62 that this is indeed a very weak proxy

relationship, I think some more space in this manuscript should be dedicated to describing the limitations of the use of this low pressure-derecho relationship. What other factors contribute to derecho formation? Might it be possible to see an overall increase in intensity of analogue low-pressure systems but a meaningful decrease in derecho frequency and/or intensity? As I'm sure the authors are aware, one of the substantial difficulties in evaluating the impact of climate change on midlatitude convective storms is that these storms are heavily impacted by mesoscale (and even smaller-scale) processes that are not resolved by the bulk synoptic-scale analogues identified in this manuscript. While I don't disagree that there is very likely a connection between the analogue lows and more intense convective events, I believe this manuscript overstates the certainty in that connection. More time should be spent describing the limitations of equating deeper lows with more derechos and perhaps also improving and emphasizing the arguments for that connection in a regional sense. I would encourage the authors to reevaluate the organization of this manuscript as a whole, especially in the introduction and conclusions; this is less of a methodological concern and more one of emphasis and context. Fundamentally this is a study of synoptic circulations changing with climate change; the connection to derechos is more tenuous (especially given the very small sample size of 11 events and the lack of comparison to strong low-pressure events that did not result in convection).

We would like to address the major comment regarding the use of low-pressure systems as a proxy for derecho occurrence. To address the concerns raised by the reviewer, we have reformulated our introduction and the presentation of our methodology to clarify our goals and better stress the limitations. Specifically, we have removed the emphasis on low-pressure system and better presented the link between atmospheric circulation patterns and severe convective weather while underlining the importance of sub-synoptic scale phenomena in their development. We also have tempered the interpretation of our results accordingly. We have changed strategy following Reviewer 1's suggestion to consider 500 hPa geopotential height pattern as a proxy of atmospheric circulation and we have included common environmental parameters (CAPE and 0-6 km wind shear) to better investigate changes in the environment and its potential impact on convection.

MINOR COMMENTS

Lines 40-48: While a helpful overview, this paragraph is missing some key citations for the information being presented.

Figure 1: I am a little unclear as to what is being represented by the thermometers to the right of the warm air, since the icons indicate middling temperatures.

Figure 2: The caption for 2i) has the words "tracked between" that don't seem to belong there. In addition, "cumulated" precipitation should be replaced with either "cumulative" or "accumulated".

TRIVIAL/TYPOGRAPHICAL COMMENTS

Line 28: This editorial "as we can see" comment is not necessary and can be removed in place of a more objective description of derecho frequency.

Line 30: The word “the” at the start of the sentence may be omitted. In addition, “Great Plains” is typically capitalized when referring to the region in the United States.

Lines 31-32: “the public opinion was shocked” is an awkward sentence construction. Perhaps “For these reasons, the violence and widespread destruction of the derecho which hit Corsica in summer 2022 garnered a great deal of public attention”?

Line 51: “IPCC reports do not” rather than “does not”.

Line 78: “source that documents” rather than “source that document”.

Line 87: “this dataset” rather than “this datasets”.

Line 89: “reanalysis datasets” rather than “reanalyses datasets”.

Line 101: “partially addressed” rather than “partially address”.

Line 196: Should read “is given in Table 1”.

Line 204: “casts” should be “coasts”.