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Extreme hazard cyclones – concept and ERA5 climatology

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General comments:

The authors introduce the concept of extreme hazard cyclones, defined according to the number of extreme precipitation and surface wind events associated with individual cyclones along their tracks. Using 75 years of ERA5 reanalysis, they investigate the geographical and seasonal distributions of these cyclones at global scale, considering two timescales (1 and 72 hours). The analysis subsequently focuses on the North Atlantic, examining cyclone characteristics, life cycles, dynamical structures, and differences among the four extreme-hazard categories.

Overall, I find the manuscript well written, clearly structured, and supported by informative figures. The topic is relevant to the Weather and Climate Dynamics community. However, I have several concerns regarding the definition and interpretation of the extreme hazard cyclone concept and the potential sensitivity of the results to methodological choices. Given that the authors aim to introduce a framework that could be applied in future studies, I believe these aspects deserve particular attention. My main concerns relate to (1) the distinction between tropical and extratropical cyclones, (2) the dependence of the ranking on cyclone identification and attribution methods, (3) the definition and interpretation of long-duration extremes, and (4) the physical interpretation of the differences between precipitation- and wind-related extreme hazard cyclones. Addressing some of these concerns may require additional sensitivity analyses, while others could be resolved through more detailed methodological explanations and a careful discussion of limitations. I therefore recommend **major revision** before the manuscript can be considered for publication.

Major comments:

1. Scope of the study: tropical versus extratropical cyclones

I find the scope of the analysis somewhat ambiguous regarding the cyclone types considered. The title suggests a general framework applicable to all cyclones, whereas the Introduction focuses predominantly on extratropical cyclones and their associated surface extremes. However, the methodology is applied globally, and the results indicate that many of the most extreme hazard cyclones originate in the tropics and subsequently undergo extratropical transition. This raises an important question about the physical interpretation of the global ranking. Tropical and extratropical cyclones differ substantially in their formation mechanisms, dynamical

structures, geographical distributions, and lifetimes. Combining these systems in a single ranking could therefore obscure differences in the processes responsible for their associated precipitation and wind extremes. I appreciate that the authors subsequently restrict the North Atlantic analysis to extremes occurring poleward of 30° N. Nevertheless, this geographical restriction does not necessarily guarantee that all the associated cyclones have completed extratropical transition or are dynamically extratropical at the time of the event.

I suggest that the authors clarify the intended scope of their framework and consider whether separating tropical cyclones, extratropical cyclones, and systems undergoing extratropical transition would provide a more physically meaningful comparison. At a minimum, a sensitivity analysis excluding cyclones of tropical origin from the North Atlantic composites would help establish whether the main conclusions are robust. Alternatively, the authors could explicitly frame the study as a global cyclone-hazard classification encompassing different cyclone types and strengthen the discussion of the implications of combining these systems.

2. Definition of extreme hazard cyclones and sensitivity to cyclone identification

A central methodological concern is the dependence of the proposed concept on the definition of the cyclone area. The authors use the outermost closed sea-level-pressure contour to construct cyclone masks and subsequently attribute local surface weather extremes to individual cyclones. Although this is a well-established approach, alternative cyclone-tracking methods identify cyclone centers without necessarily defining spatial footprints in the same way. Consequently, the number of extremes attributed to a cyclone may depend substantially on the selected cyclone detection method, particularly on the extent of the cyclone mask. This is important because the proposed ranking is intended to provide a general framework for characterizing extreme hazard cyclones. Its applicability across different cyclone-tracking algorithms, datasets, and model resolutions is therefore not immediately evident.

I would encourage the authors to assess the sensitivity of their results to the definition of the cyclone area. For example, an alternative approach could attribute extremes within a prescribed distance of the cyclone center. Hawcroft et al. (2012) used fixed-radius regions and assessed the sensitivity of cyclone-associated precipitation to this choice. A comparison between the original cyclone masks and one or two fixed-radius definitions, even for a representative subset of cyclones, would help determine whether the rankings are robust to the attribution geometry. An additional issue is that the ranking counts spatially distinct grid-point extremes accumulated along each cyclone track, rather than directly measuring the maximum intensity of the associated hazards. Large, long-lived cyclones may therefore accumulate more extreme grid-point occurrences simply because they cover larger areas or travel longer distances.

This is not necessarily a limitation of the proposed concept, but it should be clearly distinguished from a ranking based on hazard intensity. The implications of this choice deserve further discussion, particularly when comparing cyclones with substantially different sizes and lifetimes. Finally, because this framework may be useful to the wider research community, I strongly encourage the authors to provide the code used for cyclone-to-extreme attribution and ranking, or at least a sufficiently detailed and reproducible implementation.

3. Definition and attribution of 72-hour extremes

The selection of 1-hour and 72-hour timescales is motivated by differences in the potential impacts of short- and long-duration extreme weather. I agree with the general rationale, but the specific choice of 72 hours would benefit from further justification. Is this duration motivated primarily by the timescale of particular hazards, by the typical evolution of extratropical cyclones, or by previous studies? Would the main results remain similar if an intermediate duration, such as 24 or 48 hours, were selected?

A more important concern relates to the attribution of 72-hour extremes to individual cyclones. According to the methodology, a 72-hour extreme is linked to a cyclone whenever its mask overlaps the event location at least once during the 72-hour period. This criterion seems potentially permissive. A cyclone might occupy the location for only a few hours while most of the accumulated precipitation or sustained wind conditions occur during the passage of another cyclone.

The authors acknowledge that a single 72-hour event can be assigned to several cyclones and later discuss cyclone clustering. Nevertheless, I wonder to what extent this attribution choice influences the resulting rankings and the interpretation of the differences between short- and long-duration extremes. Would it be possible to test a more restrictive attribution criterion, such as requiring a minimum number of hours of overlap or considering the fraction of the 72-hour accumulation occurring during cyclone passage? Such a sensitivity analysis would strengthen confidence in the conclusions and help distinguish prolonged influence by an individual cyclone from extreme conditions associated with a sequence of cyclones.

4. Physical interpretation of cyclone dynamics and temporal evolution

The comparison of cyclone structures across the four hazard categories is one of the most interesting parts of the manuscript. However, I believe some of the dynamical interpretations could be supported more convincingly. In particular, the authors interpret the stronger upper-level winds associated with the W1 and W72 categories as evidence of stronger baroclinicity and dry dynamical intensification. While this interpretation is plausible, upper-level wind speed alone does not provide a direct quantitative measure of baroclinicity.

Furthermore, the comparatively weaker lower-tropospheric PV anomalies associated with wind-related cyclones do not necessarily imply that diabatic processes played a minor role in their previous intensification. Latent heating may have contributed to cyclone development at an earlier stage, while the wind extremes occur later in the cyclone life cycle. This leads to a broader question concerning the timing of precipitation extremes, wind extremes, latent heating, and cyclone intensification. The manuscript presents composites at selected times, but the temporal evolution of the dynamical structures would provide additional insight into whether the diagnosed differences reflect distinct intensification mechanisms or different stages of cyclone development. I suggest considering composites at several time lags relative to maximum cyclone intensification, minimum SLP, or the occurrence of the largest number of extremes. Even a relatively simple analysis at intervals of 6 or 12 hours could help clarify the physical interpretation. At a minimum, I recommend distinguishing more carefully between conclusions directly demonstrated by the diagnostics and plausible interpretations that would require additional analysis.

Specific comments

Introduction

Line 27: The expression “both over continents and within the storm tracks” is somewhat ambiguous. Storm tracks can also extend across continental regions, so the intended distinction is not entirely clear. Are the authors referring to the main climatological storm-track regions, which are predominantly oceanic, versus continental regions farther away from these regions? I suggest clarifying the terminology.

Line 33: The expression “a large fraction” is rather vague. If available from the cited study, providing an approximate percentage or range would make this statement more informative.

Line 35: The authors mention that extreme precipitation is often associated with warm conveyor belts. Given the previous discussion of frontal precipitation, it would be useful to clarify the relationship between these two mechanisms. Warm conveyor belts are closely associated with frontal structures, so these contributions are not necessarily independent. A brief clarification, ideally supported by the cited literature, would improve the physical context.

Line 38: I suggest expanding the explanation of how complex topography can lead to precipitation extremes at considerable distances from the cyclone center. Presumably, the interaction between cyclone-induced moisture transport and orographically forced ascent is important. Explicitly mentioning this mechanism

would make the statement clearer and better motivate the potential limitations of the cyclone-mask attribution.

Data and methodology

Line 106: The expression “constitutes the ideal basis” seems somewhat strong, particularly given the known limitations of reanalysis data in representing local precipitation and surface wind extremes. I suggest replacing “ideal” with “a suitable” or “a valuable” basis.

Line 111: The authors identify cyclone boundaries using closed SLP contours at intervals of 0.5 hPa. Could they clarify the rationale for this contour interval and whether it has been tested for sensitivity? Since the cyclone masks determine which extremes are attributed to each cyclone, differences in the detected footprint may affect the ranking.

Lines 113–114: The description of cyclone tracking raises the question of how cyclone merging and splitting are handled. I understand that the authors apply a filter to avoid unrealistic jumps in cyclone position, but merging and splitting are also physically meaningful processes during cyclone evolution. How are these situations treated by the algorithm? For example, if two cyclones merge, are their tracks joined, is one track terminated, or are they treated as separate systems? A brief clarification would be helpful because the resulting track continuity may influence the number of extremes accumulated over a cyclone's lifetime.

Line 125: The authors adopt a higher percentile threshold than Pfahl (2014), defining extremes using the local 99.9th percentile. What is the main rationale for this choice? I appreciate that the study focuses on particularly severe extremes, but the selected threshold may influence both the number of identified events and the geographical distribution of the associated cyclones. A short explanation, and potentially a sensitivity test using a lower threshold such as the 99.5th percentile, would strengthen the methodology.

Lines 127 and 216: As mentioned in the major comments, the rationale for selecting 72 hours is not entirely evident. I recommend explaining why this timescale is particularly relevant and discussing whether the results are expected to depend strongly on the chosen duration.

Line 132: The attribution of extremes based on the cyclone masks is a central component of the proposed framework. Please see Major Comment 2 regarding the dependence of the results on the cyclone-area definition and the potential value of providing a reproducible implementation.

Lines 135–138: The criterion requiring only one hour of spatial overlap during a 72-hour event appears potentially permissive. I suggest quantifying how often long-duration extremes are attributed to multiple cyclones and assessing how a more restrictive criterion might affect the rankings. Please also see Major Comment 3.

Line 143: The authors argue that only a small fraction of cyclone-related precipitation extremes are likely missed because most heavy precipitation occurs relatively close to the cyclone center. However, the Introduction also emphasizes the importance of frontal precipitation, and elongated fronts can extend beyond the closed SLP contours used to define cyclone masks. I would therefore be cautious about assuming that the missed fraction is necessarily small.

Global and North Atlantic results

Table 1: The table illustrates an important aspect of the global ranking: many of the highest-ranked cyclones have a tropical origin and subsequently undergo extratropical transition. I suggest emphasizing this more clearly when introducing the results, particularly given the predominantly extratropical focus of the Introduction. It would help readers distinguish between the global analysis and the subsequent North Atlantic investigation.

Lines 269–271: Restricting extreme-event locations to latitudes poleward of 30° N does not necessarily ensure that the associated cyclones are dynamically extratropical. This issue is particularly relevant for cyclones undergoing extratropical transition. Could the authors clarify whether cyclone type was assessed at the time of the extreme event, or whether some systems may still retain tropical characteristics? Please also see Major Comment 1.

Line 291: The unit “Bergeron” should be defined when first introduced. I recommend providing its conventional definition, including the latitude normalization, and specifying how the deepening rate is calculated in this study. This is especially important because the investigated cyclones span a relatively broad latitude range.

Cyclone composites and dynamical interpretation

Figure 6: I have two comments regarding this figure. First, could the authors clarify whether the cyclone-relative composites are constructed by translating the cyclone centers to a common origin only, or whether the fields are also rotated to account for the propagation direction of each cyclone? If the composites are not rotated, variations in cyclone orientation could smooth the frontal signals, particularly those associated with warm and cold fronts. It might be informative to consider whether rotating the cyclones according to their propagation direction changes the spatial patterns, although I recognize that such an approach introduces its own limitations.

Second, there appears to be an inconsistency between the figure caption and the panel titles. Panels (c) and (d), as well as panels (g) and (h), appear to have their hazard-category labels reversed in the caption. The panels identified as containing hatched regions should also be checked for consistency.

Figure 7: The vertical PV composites are particularly interesting. Would it be possible to complement the absolute PV fields with PV anomalies relative to an appropriate background state, analogous to the upper-level PV anomalies shown in

Figure 8? This could help distinguish the cyclone-associated PV structures from background PV variations and provide further insight into differences among hazard categories. It could be considered as supplementary material if including it in the main manuscript would make the figure too complex.

Lines 398–399: The authors infer strong baroclinicity from the presence of intense upper-level winds associated with the W1 and W72 categories. However, it is not immediately evident from Figure 8 how baroclinicity itself is diagnosed. While strong upper-level winds may be consistent with stronger vertical wind shear and horizontal temperature gradients, they do not directly quantify baroclinicity. Could the authors provide an additional diagnostic, such as the lower-tropospheric horizontal temperature gradient, vertical wind shear, or Eady growth rate, to support this interpretation? Alternatively, I suggest rephrasing the statement to indicate that the jet structure is consistent with, rather than direct evidence of, enhanced baroclinicity.

Lines 409–410: The interpretation that W72 cyclones are primarily associated with strong baroclinicity and dry dynamical intensification may be too strong based on the presented diagnostics. Could these systems also have undergone substantial latent heating during earlier stages of their development, contributing to lower-tropospheric PV production and cyclone intensification before the occurrence of the most widespread wind extremes? The relatively weak PV tower in the composite at the selected time does not necessarily exclude an earlier contribution from diabatic processes. I suggest discussing this possibility or using a time-evolving diagnostic to distinguish between dry and diabatic contributions.

Conclusions

Lines 504–505: The authors conclude that cyclones associated with many surface wind extremes reach lower minimum SLP and intensify more rapidly than those associated with precipitation extremes. This is an interesting result, but I wonder whether part of the difference could be related to the timing of the processes involved. Latent heating associated with precipitation can contribute to lower-tropospheric PV production and subsequent cyclone intensification. Consequently, intense precipitation may occur before the cyclone reaches its maximum intensity or strongest surface winds. Could the differences between precipitation- and wind-related cyclones partly reflect their respective stages of development?

It would be interesting to examine the evolution of precipitation and wind extremes relative to cyclone intensification and minimum SLP. For example, composites at time lags of ±6 or ±12 hours, or a longer life-cycle analysis, could provide useful evidence. This would help determine whether the identified differences reflect fundamentally distinct cyclone dynamics or the different timing of precipitation and wind extremes during cyclone development.

Code and data availability: The authors indicate that key cyclone tracks and extreme-event locations will be made available upon acceptance. Given that the

manuscript introduces a methodological framework intended for application to other regions and climate simulations, I would strongly encourage the authors to make the corresponding cyclone-to-extreme attribution and ranking code available as well. This would substantially improve the reproducibility and transferability of the proposed framework.

References

Hawcroft, M. K., L. C. Shaffrey, K. I. Hodges, and H. F. Dacre (2012): How much Northern Hemisphere precipitation is associated with extratropical cyclones?, Geophys. Res. Lett., 39, L24809, https://doi.org/10.1029/2012GL053866.