Review of manuscript egusphere-2025-3455 submitted to Natural Hazards and Earth System Sciences (NHESS)

Anonymous Referee #3, 19 Sep 2025

[answers in blue]

General comments

The authors make use of a high-resolution reanalysis dataset that represents a considerable effort in terms of length (37 years), spatial coverage (all of Italy), and resolution (4 km). This combination makes it a valuable resource for a national-scale study of changes in extreme precipitation. The availability of hourly rainfall data also makes the study relevant for analyzing the seasonality and spatial variability of hourly precipitation in Italy.

I consider this work useful for the scientific community because it brings together results that were previously limited to local or regional scales into a study covering the whole of Italy. This allows for a better understanding of the spatial variability of extreme precipitation event (EPE) characteristics and their changes, while showing consistency with earlier regional studies.

That said, I find that the Abstract and Introduction could be more explicit regarding the specific scientific contributions and aims of the work. The chosen methodology is interesting in that it introduces a structure-based perspective on hourly precipitation; however, some methodological aspects would benefit from clearer explanation or reconsideration.

Referring to hourly precipitation structures as “events” is, in my view, misleading. Since their temporal evolution (duration, displacement, deformation) is not considered, nor the total rainfall volume produced by multi-hour storm systems, the term “event” may create confusion and should be replaced with a more precise designation. Finally, the section addressing non-extreme precipitation should be better introduced, with its purpose and relevance clarified.

Overall, in my opinion, the manuscript is suitable for publication after minor revisions. These include adjustments in specific vocabulary, clearer argumentation, additional explanations to help reader understanding, and refinements in the methodology.

Specific comments

The 4 km resolution of the reanalysis is at the limit of what is usually called convection-permitting. This should be made explicit in the text, especially since the dataset paper uses “high-resolution” instead. I would keep the term “convection-permitting” for clarity but suggest adding a note that it is at the edge of the definition.

Thank you for this helpful suggestion. In the revised manuscript, we will adopt the term *convection-permitting* instead of *high-resolution* when referring to MERIDA HRES. As also discussed in our response to Reviewer 2, we acknowledge that the 4 km horizontal resolution lies at the boundary of the convection-permitting definition, and we will make this explicit in the text. This clarification will ensure consistency and transparency regarding the dataset’s capabilities and limitations (Viterbo et al., 2024; Cavalleri et al., 2024b).

Cavalleri, F., C. Lussana, F. Viterbo, M. Brunetti, R. Bonanno, V. Manara, M. Lacavalla, S. Sperati, and M. Raffa (2024). *Multi-scale assessment of high-resolution reanalysis precipitation fields over Italy*. Atmospheric Research, 312, 107734. https://doi.org/10.1016/j.atmosres.2024.107734

Viterbo, F., S. Sperati, B. Vitali, F. D’Amico, F. Cavalleri, R. Bonanno, and M. Lacavalla (2024). *MERIDA HRES: A New High-Resolution Reanalysis Dataset for Italy*. Meteorological Applications, 31(6), e70011. https://doi.org/10.1002/met.70011

Since about half of the figures in the Results section concern hourly rain structures and not EPEs, it is important to either (i) include non-extreme hourly rain discussion in the abstract as well as adapt the title accordingly, or (ii) introduce non-extreme hourly rain results as a necessary step before moving to extremes. Otherwise, the part on extremes, which is announced to the reader, takes too long to arrive.

We will include in the Abstract a mention of the findings related to non-extreme hourly precipitation structures (i) and, also following Reviewer 1 suggestion, we will make the motivation for including non-extreme rainfall analysis clearer (ii) at the beginning of that section, while at the same time shortening its discussion so that the focus on extremes—highlighted in the title and Abstract—emerges earlier in the Results section.

The Introduction and Abstract should state more clearly the purpose of the work and its scientific contribution.

In the revised manuscript, we will add a specific sentence in the Abstract to clarify the purpose of the study, namely to contribute to the discussion on extreme precipitation trends in Italy through an innovative approach based on hourly fields from a convection-permitting reanalysis. In addition, we will expand the relevant paragraph in the Introduction to more explicitly state the aim and scientific contribution of the work.

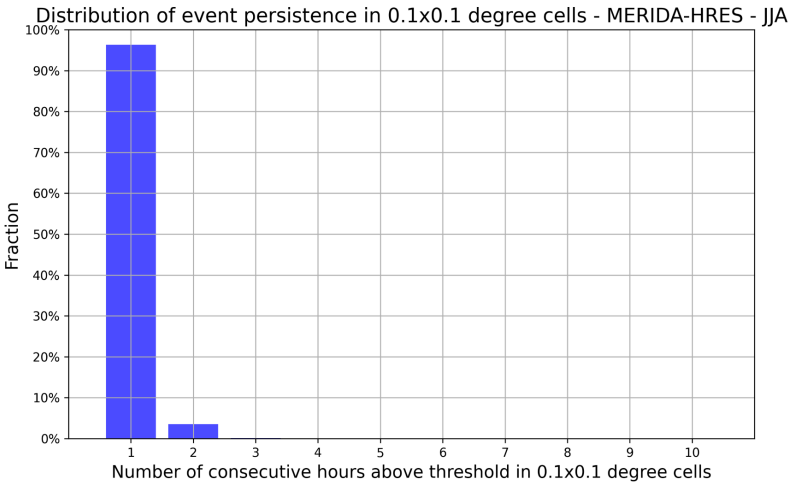
The HOPE-X dataset is a collection of hourly rain structures, not “events” in the usual sense of storm systems or local rain events. For instance, a precipitation system lasting two hours can be counted as two “events,” even though it is the same system. Similarly, a moving

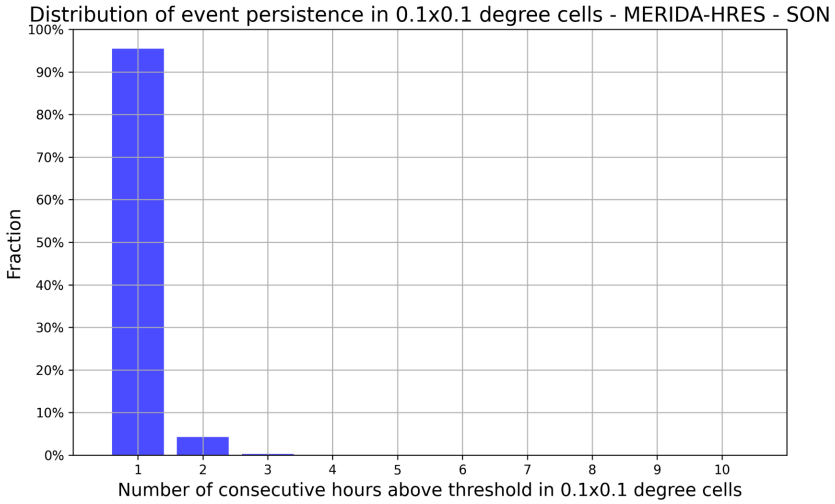
storm can be counted as several “events” in different areas as it displaces. For this reason, the use of the term “events” is inappropriate. The title is also misleading, since the study is not, in my opinion, event-based. The same applies to the dataset name HOPE-X. This does not invalidate the study, but the terminology issue is central and should be revised. I strongly recommend using the term “hourly rain structures.” If the term “event” is retained, it should not appear in the title, abstract, or in any part of the text where the definition is not clearly introduced yet (line 164: “Hereafter, the term ‘event’ denotes the precipitation structures identified using this method”). In that case, it should also be stated that the term “event” is used for readability purposes only.

We thank the Reviewer for this valuable comment and agree that a more precise terminology is appropriate for describing the dataset of all identified structures. In the revised manuscript, we will therefore refer to them as *Hourly Precipitation Spatial Structures (HPSSs)* instead of “events,” and use *Hourly Precipitation Extremes (HPEs)* rather than “extreme precipitation events (EPEs).”

Nevertheless, we would like to retain a reference to “extreme events” in specific contexts, as this terminology is widely used in the literature and reflects different methodological approaches to defining rainfall events. For instance, some studies follow the motion of precipitation systems or clusters in a Lagrangian framework, whereas others adopt a fixed-area perspective, leading to a more local Eulerian definition (e.g., Ignaccolo & De Michele, 2010). Our study follows the latter approach: we do not track the temporal evolution of precipitation systems nor attempt to follow individual storms as they move. Instead, we adopt the viewpoint of a fixed spatial reference (i.e. the averaging window) and focus on the spatial structures occurring at that location.

When focusing on extremes, the HPSSs exceeding the extreme threshold (and so defined as HPEs) are typically short-lived: it is rare for the threshold to be exceeded locally for more than one hour. This is already partly illustrated in the mean persistence analysis (Fig. 15). To further support this point, we now also provide the distribution of HPE durations within 0.1° windows for JJA and SON:





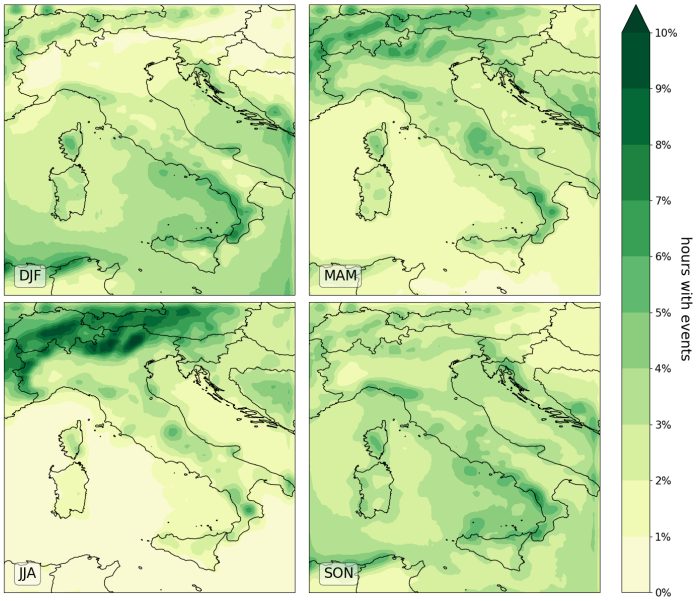
This underscores that HPEs almost always last only one hour and can therefore still be meaningfully associated with the concept of an “extreme event” in an Eulerian framework.

In the revised manuscript, we will (i) consistently replace the term “event” with Hourly Precipitation Spatial Structure (HPSS) when referring to the full dataset, (ii) use Hourly Precipitation Extremes (HPE) instead of “extreme precipitation events (EPEs),” and (iii) clarify the methodological framework and definition of “extreme event” in the Introduction and Methods sections, not using the word “event” before this explanation (not in the title nor in the abstract).

Ignaccolo, M. & De Michele, C., 2010. A point based Eulerian definition of rain event based on statistical properties of inter drop time intervals: An application to Chilbolton data. *Advances in Water Resources*, 33(8), pp.933–941. doi:10.1016/j.advwatres.2010.04.002

Following the previous comment, in maps such as Figure 6 the number of “events” (N) can reach ~300 per season in a 0.5×0.5 window. This N reflects a mix of the number of hours with a rain structure and the number of surrounding structures at each hour, rather than distinct events. For example, an hour with many small rain structures and an hour with one large rain structure could produce the same hourly rainfall total in that 0.5×0.5 area, yet N would differ greatly. It should therefore either (i) be clarified in the text what the analysis of N is intended to represent if it is something other than the amount of rain, (ii) reconsider the counting methodology, or (iii) include additional analysis showing that N reflects the actual volume of hourly rain.

Thank you for raising this important point. The purpose of this analysis is to identify the locations where rainfall structures occur most frequently, by counting the number of centers of mass falling within each grid window. This metric is therefore distinct from total rainfall volume, which is investigated through the mean intensity analysis. To explore alternative approaches, we also tested the frequency of wet hours (i.e., the percentage of hours in which at least one structure was detected within the window), which helps disentangle the mix you mention in your comment. The resulting spatial patterns were very similar to those obtained in Figure 6:



For the sake of simplicity, and to keep the analysis more directly aligned with the increase in the number of extreme events—which we find conceptually more tangible than frequency—we chose to present results in terms of the number of events (N) rather than wet-hour frequency. This point will be clarified explicitly when introducing Figure 6 in the revised manuscript.

The use of seasonal thresholds to adapt to the intensity scales of each season is well justified and explored. However, the choice to let the seasonal threshold vary spatially with the local 50th percentile of 1 mm+ hourly rains requires stronger justification. As a result of threshold methodology, identical rain structures may be included in HOPE-X in one region but not in another. While such area-relative thresholds are understandable when defining extremes (e.g. EPEs) due to the definition of rarity being potentially region-based, they are harder to justify for non-extreme rain structures. Choosing fixed threshold across the domain for each season would be more correct. If the authors choose to go with a justification of the current methodological choice of non-homogeneous threshold, consider also that in areas with very little rainfall, the threshold based on wet hours 1mm+ may also be computed on a very small and potentially unrepresentative sample, so this would need to be tested.

Thank you for this detailed and thoughtful comment. The choice of threshold for identifying coherent precipitation structures was extensively discussed among the co-authors, reflecting the diversity of perspectives within our team. The final decision followed a series of sensitivity tests, some of which are included in the Supplementary Material. These tests compared fixed thresholds (0.5, 1, and 2 mm) with percentile-based thresholds (50th, 75th, 90th, and 99th percentiles), computed both from all values and from values above 1 mm.

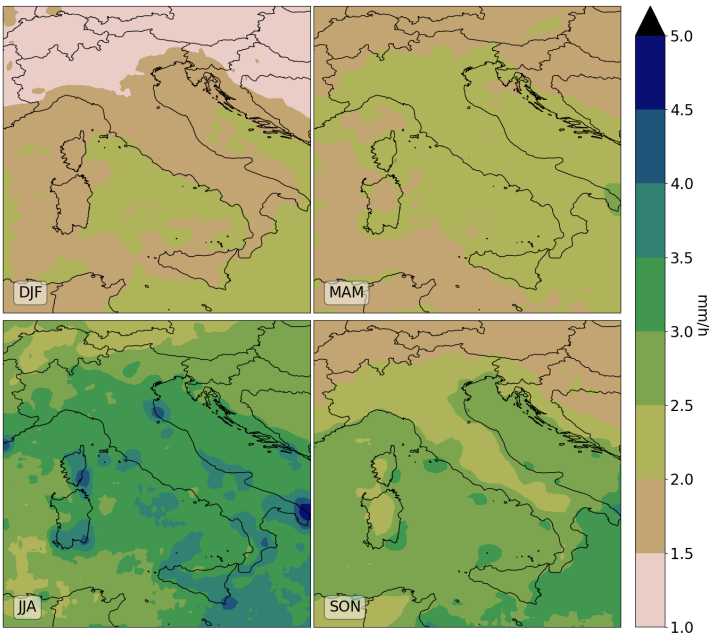
We opted for spatially varying thresholds for the same rationale as for seasonal thresholds. Precipitation regimes in Italy differ markedly across both seasons and regions, due to the country's complex geography (latitudinal gradients, mountain ranges, coastlines, inland areas). Even within the same season, distinct precipitation processes coexist in different regions. A threshold that varies seasonally and spatially therefore ensures that the identified

structures are meaningful for the local climatology and meteorology. The aim of this step is to retain precipitation objects of sufficient relevance for the season and region in question. Spatial modulation also improves the identification of coherent structures and helps separate nearby systems—for example, distinguishing individual thunderstorms within a mesoscale cluster in areas where median precipitation is higher—thus avoiding their artificial merging into a single object. These aspects will be clarified in the revised manuscript.

Regarding regions with very little rainfall (e.g., over the sea in summer), we acknowledge the reviewer's concern. Our full-dataset analysis (Section 3.1) indeed shows a scarcity of identified structures in such areas. When extreme events are subsequently filtered, these regions are excluded from the trend calculations (grey areas in Fig. 13), reducing the risk of drawing conclusions from unrepresentative samples.

Potential artifacts due to the threshold methodology chosen discussed in previous point: It is not clear whether some of the patterns in the maps (e.g. the higher JJA values over Ravenna in Fig. 8 AvIn and Fig. 9 PkIn) are genuine results or artifacts of the methodology filtering out structures with higher thresholds (higher thresholds over Ravenna in Fig. 1). Line 257: Are the "hotspots" results of higher local thresholds? Or an actual signal? Same for "isolated spots" line 278.

Thank you for raising this question. Some of the patterns you highlight, such as the JJA intensity signal near Ravenna, are not artifacts of the thresholding methodology but features of the reanalysis, as also documented in previous validation studies (Viterbo et al., 2024; Cavalleri et al., 2024b; Giordani et al., 2025). In these areas, summertime convection tends to be excessively triggerer, leading to large rainfall amounts. This mechanism, already discussed in the revised Discussion section of our manuscript, explains the presence of such localized "hotspots". The fact that this is not a result of varying threshold is also supported from the fact that an analysis analogous to Figure 8, but based on a fixed 1 mm threshold, produced very similar spatial patterns (figure S4 in Supplementary Material):



We will clarify this point in the manuscript to make explicit that these signals originate from the reanalysis itself rather than from the structure identification procedure.

Cavalleri, F., C. Lussana, F. Viterbo, M. Brunetti, R. Bonanno, V. Manara, M. Lacavalla, S. Sperati, and M. Raffa (2024). Multi-scale assessment of high-resolution reanalysis precipitation fields over Italy. *Atmospheric Research*, 312, 107734. https://doi.org/10.1016/j.atmosres.2024.107734

Viterbo, F., S. Sperati, B. Vitali, F. D'Amico, F. Cavalleri, R. Bonanno, and M. Lacavalla (2024). MERIDA HRES: A New High-Resolution Reanalysis Dataset for Italy. *Meteorological Applications*, 31(6), e70011. https://doi.org/10.1002/met.70011

Giordani, A., P. Ruggieri, and S. Di Sabatino (2025). Added value of a multi-model ensemble of convection-permitting rainfall reanalyses over Italy. *Atmospheric Research*, 328, 108402. https://doi.org/10.1016/j.atmosres.2025.108402

The role of the "minimum enclosing ellipse" is unclear. Its mention in the title of Fig. 2 and in the text (line 169) is confusing: are the variables in Table 1 computed on the ellipse itself or on the rain object it encloses? Is the ellipse only used for visualization of selected structures, or does it play a role in extracting the properties of the rain objects? This part should be clarified for better understanding.

Thank you for pointing out this source of confusion. The minimum enclosing ellipse is used to compute the maximum spatial extent of each rainfall structure: specifically, the major axis of the ellipse provides a linear measure of event size. This approach is motivated by both operational and research applications (e.g., Wernli et al. (2008); SAL method), where representing an event as an ellipse allows extraction of its key spatial characteristics in a consistent way. The ellipse is therefore not only a visualization tool but also plays a role in defining one of the descriptors listed in Table 1 (axis_maj). In the following comment on spatial scale definition, we further justify why we chose maximum event extent—rather than area—as the most informative metric for our purposes.

Line 169: for readers unfamiliar with Wernli et al. (2008), it would be helpful if the purpose of using this methodology were explained.

In the revised manuscript, we will add a short explanation of the methodology by Wernli et al. (2008) to guide readers who may not be familiar with it, clarifying its purpose and relevance for our analysis.

Following the previous points, it is unclear whether axis_maj refers to the major axis of (i) the rain structure itself or (ii) the enclosing ellipse. In both cases, the phrasing in line 264 ("summer events are generally smaller") is problematic, as are later formulations in the SpS analysis regarding size. If (i), and the aim is to characterize the size of the structures, the "area" variable would be more appropriate. For example, a thin elongated structure may be characterized as large with SpS even though its area of coverage is small, while a rounder

structure with the same SpS value could cover a much larger area, yet still be classified as the same “size” when using SpS. If (ii), the analysis would instead describe the size of influence of the structure rather than its actual size. It should therefore be clarified what SpS is intended to capture, and statements about “size” should be made with caution. Replacing the SpS analysis with an analysis of the area variable would make the interpretation more straightforward.

We agree with the reviewer that the definition of event “size” was not sufficiently clear in the original manuscript. The *axis_maj* variable refers to the major axis of the minimum enclosing ellipse (ii), which we use as a consistent measure of the maximum extension of each event. In the revised manuscript, we will therefore replace the term *size* with *maximum extension of the event*. As you suggest, what we aim to capture is the *size of influence* of the structure rather than its area. We chose this linear metric over area because atmospheric phenomena are commonly characterized in terms of linear spatial scales (e.g., meso-beta, meso-gamma) expressed in kilometers, rather than in square kilometers (Thunis and Bornstein, 1996). Formulations such as line 264 will be revised to read, for example: “summer events generally have a smaller maximum spatial extension.”

Thunis, P., and Bornstein, R. (1996). Hierarchy of Mesoscale Flow Assumptions and Equations. *Journal of Atmospheric Sciences*, 53, 380–397. https://doi.org/10.1175/1520-0469(1996)053<0380:HOMFAA>2.0.CO;2

Technical comments

- Line 10: “the most extreme component.” The word component is unclear here. I suggest rephrasing to: “The most extreme rain events (EPEs)…”

We agree that the wording was unclear. What we intended to convey was the most extreme subset of the identified rainfall structures. In the revised manuscript, we will rephrase this expression for clarity, adopting a formulation such as “the most extreme rain events (EPEs).”

- Line 111: “event-based approach.” As discussed earlier, this would be better described as an approach using hourly rain spatial structures. I also suggest adapting the phrase to be less generic and more explicit about how clustering helps address the limitations: “In light of these limitations, an object-based approach using a clustering technique was adopted to capture coherent hourly precipitation structures and reduce sensitivity to small-scale discrepancies between simulations and observations.”

In the revised manuscript, we will describe our methodology as an “approach based on hourly precipitation spatial structures”, rather than using the generic term “event-based approach.” We will also be more explicit about the clustering technique employed, while directing the reader to the Methods section for the detailed explanation.

- Line 146: “The event-detection … precipitation events…” The vocabulary of event throughout the text before line 164 is misleading, since line 164 specifies: “Hereafter, the term ‘event’ denotes the precipitation structures identified using this method.” Event terminology should not appear earlier without this clarification.

As clarified in our previous responses, in the revised manuscript we will consistently refer to the identified clusters as “Hourly Rain Spatial Structures”, rather than “events”.

- Line 296: “is conducted on a subset of EPEs.” A clearer phrasing would be: “a subset of the HOPE-X dataset.” (But HOPE-X should be renamed)

We agree that this phrasing is clearer. In the revised manuscript, the dataset will be renamed.

- Lines 321–322: The phrase “seasonal redistribution is likely driven by the persistence of summer-like convective activity into early autumn” is confusing, since the focus is on differences between summer and autumn. This cannot be justified by describing autumn activity as “summer-like.” A phenomenon-based explanation could instead highlight that the persistence of warm sea surface conditions beneath a cooler atmosphere creates instability favorable to convection. Autumnal convection is not “summer-like”. This is also reflected in Fig. 12, which shows differences in the “size” characteristics of hourly rain structures between summer and autumn.

We agree with your phenomenon-based interpretation, which will be incorporated in the revised manuscript. Confusing expressions such as “summer-like” will be removed, and we will highlight that the persistence of warm sea surface conditions beneath a cooler atmosphere creates instability favorable to convection. We will also use information on the maximum extension of events (Fig. 12) to emphasize both similarities and differences between summer and autumn convective activity. We appreciate this useful suggestion.

- Line 354: Instead of “alternating pattern,” I suggest: “spatial heterogeneity in the sign of the signal” or “patterns of alternating signs.”

We agree with this suggestion and will revise the manuscript to replace “alternating pattern” with “spatial heterogeneity in the sign of the signal” for clarity.