We would like to thank you for your review. Please find below our answers to your comments. The manuscript was updated accordingly.

Summary: The paper studies the relationship between rainfall extremes and temperature using the Universal Multifractals (UM) framework. Using high-resolution rainfall and temperature data from 3 campaigns in France, the authors confirm robust scaling behavior at both the event scale (30s to 1h) and the synoptic scale (up to ~11 days). They further show that the maximum observable singularity tends to increase with temperature. The study argues that UM-based analyses provide a convenient, scale-invariant approach to understand the temperature dependence of rainfall extremes, in a way that is consistent with earlier findings based on Clausius–Clapeyron scaling.

Critical assessment: The application of the UM formalism to study the link between rainfall extremes and temperature is relatively new. The observation that the maximum observable singularity may be temperature-dependent is potentially interesting, though similar ideas have already been presented in other UM/rainfall studies in the broader climate context. The paper does not advance the UM methodology itself; it simply applies an existing framework to a new dataset and context. The study is strongly limited by its geographical scope (only 3 sites in France) and by the modest length of the underlying time series. Moreover, the practical implications of the findings are not obvious to me, and the authors do not really articulate them well in the paper. The paper is full of typos and difficult to read. Several parts of the methodology are poorly written and hard to understand, even for specialists (see comments below). The introduction provides a good summary of prior CC-based studies, but overall I get the feeling that the authors overstate the relevance of their results. As far as I understood it, the UM framework does not really provide any new insights into the rainfall-temperature link. Please correct me if I am wrong. If the results are just consistent with what is already known, what's the added value from a scientific point of view? What are the pros and cons of this framework, and what issues/questions remain open?

Results offer a new insight into existing ones by focusing on the extreme variability which was not previously addressed. This was clarified (see also detailed answer below). Perspectives were added in the conclusion.

Major comments:
(MC1): The paper contains many typographical mistakes and grammatical errors. A thorough proofreading and, ideally, professional language editing would be highly beneficial to improve clarity and readability. See Typos for some examples.

The manuscript underwent a thorough proofreading.

(MC 2): Instrumental/observational uncertainty is not quantified or discussed. Please provide rough estimates of the uncertainties affecting your data and discuss how this might affect modeled quantities (see MC 3).

Following your comment a paragraph was added in the data section mentioning recent references addressing the issue of instrumental limitations with disdrometers. Addressing these issues and the influence of the associated uncertainty on the multifractal analysis carried out would be an interesting topic, but it remains outside the scope of this paper. It is now clarified in the same paragraph.

(MC 3): No error bars, confidence intervals, or uncertainty estimates of estimated UM parameters (α, C1, H, γs) are presented. This makes it difficult to judge the significance of the observed trends. Please provide rough estimates of uncertainties and how they might affect your conclusions.

Following your comments, rough estimates on the uncertainty were computed for UM parameters alpha and C1. They are discussed on the ensemble analysis at both event and large scale. There are very limited with regard to discussed trends so they do not affect our conclusions. This is now clarified in section 4.1 and 4.2.

(MC 4): The description of the different quality control mechanisms and methods for filtering our bad data (e.g., due to solid precipitation, small sample sizes, negative temperatures etc..) needs to be extended. In the Data (or Methods) section, please provide a clear step-by-step description of all the filters that were applied. Currently, the information is scattered across the Results.

Following your comment, the filtering / preparation of the data (temperature <2°C, event selected, sample selection…) was clarified and all moved to data section. Only the comments on samples / events with poor scaling were kept in the results section because they correspond to results.

(MC 5): The paper would benefit from a more thorough discussion about the limitations of the proposed approach. For instance, the assumption of stationarity (seasonal and diurnal variability) and the treatment of mixed precipitation types (rain vs. snow, briefly mentioned on page 11, line 224) could be addressed in greater detail and with a more critical perspective. Similarly, the strong reliance on surface temperatures without any consideration for vertical variability constitutes another limitation. Not all precipitation extremes are generated by the same physical mechanisms, and not all events at a given temperature are comparable from a physical point of view. The paper should clearly acknowledge and discuss the critical assumptions underlying such an analysis.

With regard to the treatment of mixed precipitation types, only rainfall events are considered and this was clarified in the data section. A comment on the use of only surface temperature was added in the introduction.

(MC 6): The paper would benefit from a short, additional analysis of scaling rates of the 95% and 99% quantiles of rain rates with temperature at a few key time scales. This analysis could be added to Section 4.3 (Link with other studies) or presented at the start of the Results section, to provide more context and better understand how the new findings from the UM framework complement traditional CC-scaling analyses.

Following your comment, a subsection was added to present results according to seasons (mentioned in MC5) and rainfall intensities. Given the limited number of events available, only the 90% quantiles were used for rainfall intensity and this is explained in the text.

(MC 7): The reference list contains numerous formatting inconsistencies (e.g., journal names, DOIs, URLs) and requires careful revision (see technical comments at the end of the review). A thorough check against the journal's style guide would improve consistency and readability.

This was corrected.

Minor, technical comments:

- The terminology in Section 3 is confusing. The authors use "fields" to refer to time series. Yes, the theory of UMs is applicable to any type of stochastic process (including spatial processes), but this

paper only deals with time series. Therefore, “time series” or “stochastic process” would be more appropriate.

Fields can be 1D, 2D or more in general; hence we prefer to keep the standard terminology in UM literature. Yet, you are correct that only time series are studied in this paper. Therefore, following your comment, we added a sentence: “The word "field" can refer in general to processes in 1D, 2D or more; yet in this paper, only 1D processes corresponding to time series are analysed.” in section 3.1 to avoid any confusion.

- Page 2, line 28: The assumption that extreme precipitation rates should increase at the same rate than predicted by CC also relies on the assumption that surface temperatures are a good indicator of total precipitable water in a column of air. This may not be the case for all types of rainfall extremes, especially at daily and longer time scales where atmospheric dynamics and large-scale circulations play a much more important role than temperature. Please reformulate the text accordingly.

The corresponding paragraph was updated to account for your comment.

- Page 2, ll. 30-34: in the study by Lenderink and Meijgaard (2008), it is important to mention that the 2CC scaling only holds for a particular temperature range, and only for the higher quantiles during the warm season.

This was clarified.

- Page 2, line 36: you could mention the reply by Haerter & Berg (2009) to the paper by Lenderink and Meijgaard (2008), in which they labeled the 2CC scaling a "statistical" artifact. Haerter & Berg argue that 2CC scaling is not physical. It arises from the superposition of two different rainfall regimes (stratiform and convective), both of which exhibit CC scaling on their own, albeit with different magnitudes. Because the ratio of stratiform to convective rain gradually decreases with increasing temperature, the net scaling rate can reach 2 CC over a limited range of temperatures. Reference: Haerter, J., Berg, P. Unexpected rise in extreme precipitation caused by a shift in rain type*?* Nature Geosci 2, 372-373 (2009), https://doi.org/10.1038/ngeo523

Thank you for pointing this paper which we had missed. It was added in the corresponding paragraph to strengthen the discussion.

- In the introduction, the scale break at higher temperatures (e.g., decrease in scaling rate of precipitation extremes beyond 26°C potentially leading to zero or negative scaling) should be mentioned and discussed in more depth. There are plenty of studies that have looked at the sensitivity of scaling rates to the choice of the temperature range. Please pick a few and include them into your literature review. In Europe, temperatures above 25°C are often associated with high-pressure systems, which inhibit convection. At higher temperatures, other crucial factors such as the dew point temperature and atmospheric stability are therefore needed to understand the relationship between peak precipitation rates and temperature. The highest temperatures you consider during the event analysis seems to be around 25°C. It would be interesting to know what happens beyond that.

Thanks for the suggestion. We added a paragraph in the introduction. No clear effect of potential temperature regimes was found and this is now mentioned in the new subsection.

- Page 5, line 100: “In a first step, analysis analyses are carried out up to synoptic scale, which corresponds to the typical duration of a meteorological situation at planetary scale”.
This sentence is very confusing. Synoptic scale usually refers to phenomena that last 2-7 days (cyclones, fronts etc..) and extend over spatial scales of 100-1000 km. Planetary scale refers to phenomena that last for weeks to months and extend over much larger spatial scales (jet stream, planetary waves etc..). Please reformulate to clarify what you meant.

This was clarified following your comment. For better clarity, this regime is now denoted “large scale” in the paper with an upper limit corresponding to typical synoptic scale.

- Page 5, ll. 105-107: The definition of a “rain event” could benefit from further clarification. As it stands, events could overlap in time, with starting times potentially differing by only a single time step. However, since Table A2 reports only a few hundred events, it appears that some procedure may have been used to avoid overlapping events. Please clarify the procedure you used for identifying and selecting events.”

In order to avoid any confusion, this was clarified following your comment.

- Page 5, ll. 111-112: Please specify how the starting times of these samples were determined. Diurnal variations in rainfall intensity/variability may impact the results depending on how the data were resampled.

The process is initiated at the beginning of the available data without accounting for potential effect of diurnal variations. This was clarified in section 2.2 of the updated manuscript.

- Page 5, ll. 111-113: The procedure for selecting sub-events with a fixed length of 128 samples needs further clarification. The phrasing is awkward and the illustration in Figure 3 does not really help understand how the method works. If I understood correctly, for each rain event, you try to partition the event into as many non-overlapping samples of length 128 as possible. You then look for the partition that contains the heaviest rainfall chunk. Please clarify to avoid any misunderstandings! Also, please explain what to do in case two or more partitions have the same max rainfall value.

Yes, you are correct. The paragraph was updated to improve clarity following your comment.

- Page 7, line 142: The notion of a conservative versus non-conservative fields should be explained earlier. Also, the meaning of the operator <> should be explained. Since many readers may not be familiar with this notation, I suggest to use the standard, expected value operator instead.

We believe that it is easier for the reader to introduce first the UM framework on conservative fields and address in a second stage the topic of conservative vs. non-conservative fields. This topic can be updated if needed.
Following your suggestion the meaning of <> was explained at its first use in Eq. 1. We prefer to keep this notation, which is commonly used in UM literature.

- Page 8, line 154: “[…] and will not generate biased estimates”. This statement is too strong, as some bias will be introduced. I suggest to write: “without substantially biasing the estimates of alpha and C1.”

Indeed you are correct. This was updated following your suggestion.

- On Zenodo, please zip the csv data files. This reduces file sizes by at least an order of magnitude and will make it much easier for people to download and store the data.

Thank for your suggestion, this will be done at a later stage once the final version of the paper is available.

- Please pay more attention to verb tenses. In the Introduction, some present tense statements are mixed into a past-tense literature review. My recommendation: use past tense for literature review (except general truths). The methods section mostly uses present tenses, but a few sentences slip into past tense. Please use present tenses wherever possible. In the Results, you inconsistently use past tenses ("was found") and present tenses ("is found"). My recommendation is to use past tenses for findings, and present tenses for figure/table descriptions.

This was done, following your suggestions.

- Fig1: some information is missing on the map, such as geographical coordinates and/or names of departments/regions.

Following your suggestion, geographical coordinates were added as well as region names.

- A histogram of surface temperatures during rainy periods, as well as a scatterplot of rain rate distribution versus temperature for the events mentioned in table A1 and A2 would be useful, to get a sense of the temperature range over which precipitation occurred.

We are not sure to understand your suggestion, because the temperatures are already visible in Fig. 5 and 7. The scatterplot you have in mind would be for each time step or each event / sample (in that case should the cumulative rainfall or average or maximum rainfall be plotted ?) ?

Typos and grammatical mistakes:
Please be aware that this is not an exhaustive list!
Page 1
- Line 22: "Such statements relies" → **"Such statements rely"**
Page 2
- Line 33: "goes twice stronger" → **"is twice as strong as"**
- Line 34: "over Netherlands" → **"over the Netherlands"**
- Line 40: "Wettest 10 hours" → **"The wettest 10 hours"**
- Line 45: "CC relations holds" → **"CC relation holds"**
- Line 49: "Precipitations are complex" → **"Precipitation patterns are complex"**
- Line 52: "a increase rate" → **"an increase rate"**
Page 3
- Table 1: "End day" → **"End date"**
Page 4
- Line 95: "times series" → **"time series"**
- Line 96: "to match rainfall ones" → **"to match the resolution of the rainfall data."**
Page 5
- Line 105: "Analysis are also" → **"analyses are also"**
Page 6
- Line 120: "is power law" → **"is a power law"**

Page 7
- Line 150: “with regards to” → **“with regard to”**
Page 8
- Line 166: “is yields” → **“it yields”**
- Line 174: “this tools” → **“this tool”**
- Line 176: “natured-based solutions” → **“nature-based solutions”**
Page 10
- Line 210: “behavior” → **“behaviour”** (if keeping UK spelling)
Page 12
- Line 237: “tem-perature” → **“temperature”** (hyphenation error)
Page 14
- Line 253: “changes” → **“change”** (singular subject earlier in sentence)
Page 15
- Line 265: “campaign” → **“campaigns”** (plural, since referring to three campaigns).
Appendix
- Table A1 & A2: “# of sample” → **“# of samples”**, “# of event” → **“# of events”**.

Thank you for your careful reading, this was updated.

References:
- Several references have strange DOIs starting with <GotoISI>. Please check that the URLs are correct.

Thanks for your careful reading, this was corrected.

- Journal names are inconsistently formatted: some are full names (Journal of Hydrology) while others are abbreviated (J. Hydrometeorol., Wat. Resour. Res.). Please use consistent formatting and names.
- Several references contain URLs of the form “http://www.sciencedirect.com/…”. These should be replaced with the actual DOI of the article, as specified on the publisher’s webpage: For example: “https://doi.org/10.1016/j.advwatres.2012.03.026” instead of “http://www.sciencedirect.com/science/article/pii/S0309170812000814”.

Thanks for your careful reading, this was corrected.

- **Borga et al. (2014)** includes “climatic change impact on water: Overcoming data and science gaps” at the end, which is weird. Maybe some leftover text?

Thanks for your careful reading, this was corrected.

- **Douglas & Barros (2003) has d**uplicated journal name entries: Journal of Hydrometeorology, 4, 1012–1024, j. Hydrometeorol., 2003.

Thanks for your careful reading, this was corrected.

- **Haerter et al. (2010) the** DOI has redundant parts https://doi.org/https://doi.org/…

Thanks for your careful reading, this was corrected

- Masson-Delmotte et al. (2021, IPCC) has a double comma in “Yu, R., , and Zhou, B.”

Thanks for your careful reading, this was corrected

- Moustakis et al. (2021) includes repeated identifiers (e2020EF001824 2020EF001824).

Thanks for your careful reading, this was corrected

- Panthou et al. (2014) Title ends with an asterisk (*). Not sure why.

Thanks for your careful reading, this was corrected

- Parisi & Frish (1985) I believe that “Frish” is misspelled. Should be “**Frisch”**

Thanks for your careful reading, this was corrected

- Sharma & Mujumdar (2019) the DOI has redundant parts https://doi.org/https://doi.org/

Thanks for your careful reading, this was corrected