Answer to editor

We would like to thank you for your review. Please find below our answers to your comments. The manuscript was updated accordingly and this helped a lot to improve the manuscript. Hopefully, you will be satisfied. Please let us know if further clarifications are needed.

Dear Auguste Gires and Yann Torres,

I have now received the reports from three referees, which provide a mixed set of recommendations: one suggests acceptance as is, another suggests minor revisions, and the third suggests rejection of the manuscript. The main reasons given for rejection are the lack of clarity in the presentation of the results (particularly in relation to rainfall intensities) and the lack of coherence between the early framing of the manuscript around rainfall extremes (title, abstract, and introduction) and the actual results and conclusions. Given this diversity of views, I also undertook an extensive review of the manuscript myself.

After reading the paper carefully, I believe the manuscript could potentially be accepted for publication in HESS, but substantial additional work is required to improve its quality and clarity. First, I tend to agree with the reviewer who noted that the link to rainfall extremes is not straightforward and is not presented clearly. Second, the novelty of the paper, and how the findings relate to common practices in extreme-rainfall analysis (in other words, the implications of the findings for hydrology and for studies dealing with rainfall extremes), is not well-positioned or clearly explained.

My main suggestion would be to revise the paper so that it is framed around rainfall variability, which is, in my view, what the multifractal method primarily addresses. I realize that the introduction is long and detailed in its discussion of the extreme rainfall–temperature relationship, but I do not think that most of this material is necessary or directly relevant to the manuscript in its current form. The rainfall–temperature relationship could likely be summarized in a single paragraph. Moreover, as I understand it, the multifractal framework is conditioned on temperature for all rainfall, not only for extremes. It is, therefore, somewhat misleading to discuss only the extreme rainfall–temperature relationship; instead, the rainfall–temperature relationship should be discussed in a more general sense.

Related to this point, the manuscript opens by invoking CC scaling as the key framework for the extreme rainfall–temperature relationship, which is in itself fine. However, I have difficulty seeing how the present work connects to that framework (especially as explained in Section 4.4). First, from my reading, the manuscript is not really about extremes, but rather about rainfall temporal variability. Second, the physical connection between rainfall and temperature is mainly relevant for heavy, short-duration rainfall events, whereas the present analysis includes all rainfall intensities (most of which are not extreme) and time scales that extend beyond those typically associated with CC theory (i.e., beyond daily scales). Third, the manuscript appears to assume that temperature during rainfall is representative of the rainfall-generating conditions, whereas, in many cases, the temperature prior to the event is more relevant for convective cloud development. In that sense, linking, for example, 3-day mean temperature directly to 3-day rainfall is not well justified. As currently written, the manuscript reads as if the CC-scaling concept is being incorporated into the UM framework in a way that is not sufficiently supported. To be clear, I think it is a good and potentially useful idea to test temperature as a covariate (or explanatory variable) within the UM framework. However, I would recommend avoiding a direct linkage to CC scaling and instead presenting the study for what it appears to be: an investigation of the relationship between rainfall temporal variability and temperature using multifractal theory. If the manuscript is reframed in this way, then the long introduction on CC scaling is largely unnecessary, especially since it is not well connected to the findings.

Additionally, even after the previous revisions, I still found the text unclear and not sufficiently fluent in many places. There are also numerous grammatical and English-language issues that need to be corrected. Please note that HESS no longer provides extensive English proofreading (i.e., English language editing of the manuscript, including spelling, grammar, and sentence structure, is not provided at any stage by HESS), so these issues will not be corrected unless you address them yourselves.

Below, please find my specific comments, listed in manuscript order. Some of them directly relate to the broader points summarized above. I look forward to receiving your revised manuscript.

Sincerely,
Nadav Peleg

Thank you for taking the time to read carefully the paper and write precise suggestions. The introduction was significantly updated to reduce reference to CC and highlight more the study of the link between rainfall variability and temperature. The paper was also carefully proofread.

1. The title is not well written ("... and variability, and temperature").

The title was updated and the word extreme removed to reflect better the content of the paper.

2. The abstract is very informative. You conclude by stating that "... the scale invariant maximum observable singularity increases on average with greater temperature, which provides a framework to interpret previously observed trends". What does this mean? What have you actually found, and what is the connection between rainfall variability, extremes, and temperature? How can this be used to "interpret previously observed trends," and what does that mean exactly? How does your analysis contribute to existing knowledge?

The abstract was updated to reflect more accurately the content of the paper.

3. Lines 17-18. Note that CC refers to sub-daily to daily rainfall extremes. Longer durations should not be connected to the CC relationship due to rainfall intermittency.

We agree and we believe that this point is explained in the three last sentences of this paragraph.

4. Lines 20-21. This may have been thought in 2014, but later studies showed that this is not generally true.

The sentence was removed and replace by “This rate tends to decrease with increasing temperature”.

5. The introduction is mostly focused on CC scaling and rainfall extremes, but it hardly relates to rainfall variability or to the connection with rainfall fields that is discussed later in the text. Moreover, it is too long and includes too many specific details from papers dealing with different

aspects of CC scaling. Are all of these details needed and relevant to your analysis?

Following your comments, the specific details from papers dealing with different aspects of CC scaling were removed and only a short paragraph on them kept. Some generic content on CC was kept because we believe it is relevant background to introduce the study of relation between rainfall variability (which includes extremes) and temperature. This was clarified.

6. Lines 72-74. These sentences would fit better in the discussion section, where the limitations of the work are discussed, rather than here.

Following your remark, the paragraph was removed and the comment on stationarity moved to the conclusion.

7. Lines 78-81. The novelty of the study, the research gap, and what you aim to address are not clear. First, I can point to multiple papers in which scaling is computed for a wide range of percentiles (or return periods), so the first claim you make is not correct. Second, you suddenly introduce a new "topic" of rainfall fields and rainfall variability without clearly explaining what you mean or what you plan to address. In fact, you are not analyzing rainfall fields, but rainfall temporal variability (rainfall field = 2D; temporal variability = 1D time-series analysis). This is very confusing and should be corrected here and later in the text. Is the paper about rainfall variability or rainfall extremes? This needs to be better focused and clearly explained.

The beginning of the paragraph was fully rephrased to better account for your comment and better highlight the purpose of the paper. Only rainfall variability is mentioned at the beginning and link with rainfall extremes is incorporated in a sentence at the end of the paragraph to avoid any confusion.

8. Lines 88-91. This is not really needed and can be removed.

We believe that keeping this short outline is helpful for some readers.

9. Figure 1. Please change the coordinate system to lat/lon (WGS1984). A north arrow is not really needed, but it would be useful to show topography in the background. Is this figure critical for understanding the work? If not, I would move it to the supplementary material.

The map was updated following your suggestion. We believe it is interesting for the reader to locate the place where the measurement campaigns took place.

10. Figure 2. This can be moved to the supplementary material.

We believe that the readers could be interested in having a visual look at the data that is studied in the paper, so we would prefer to keep the figure in the main document.

11. Lines 158-161. I found the definition of "field" to be very misleading, and it may confuse many readers. A rainfall field is, by definition, a 2D structure. Since you are ultimately conducting a 1D time-series analysis, I would avoid using "field" in the multifractal context, even if this terminology was used in previous papers.

We do not really agree with you and consider “field” would be an appropriate word. Yet, if you consider it could confuse HESS readers, we changed if to “time series” because indeed in this paper, only time series are studied.

12. Lines 239-240. You use expressions such as "excellent agreement" (and similar qualitative descriptions) several times in the text. Please indicate quantitatively how good the fit is (e.g., provide a goodness-of-fit metric).

In this case, we believe that a visual inspection is enough, and the sentence was updated to clarify this. With regard to quality of scaling, the qualitative statements are always back up by the quantitative indicator r2.

13. Line 247. You already use T associated with Lambda in Line 159. Please define one of them differently.

Thank you for your careful reading ! We had missed this issue. It was changed in the definition of lambda to keep T for temperature for clarity.

14. Figure 4 is too small. I zoomed to 150%, and I still could not read the text or clearly see the lines and points.

The font size was increased, as well as marker size.

15. Table 2. Two digits after the decimal point are sufficient. There is no need for more.

I let two digits after decimal points or last zeros after decimal point, except for r2.

16. Figure 5. The font size is too small to read.

Font size of the legend was increased.

17. Table 3. Reduce to two digits after the decimal point.

I let two digits after decimal points or last zeros after decimal point, except for r2.

18. Figure 6. The font size is too small.

Font size of the legend was increased.

19. Figure 7. The font size is too small.

The font size was increased, as well as marker size.

20. Tables 4-5. Please keep only two digits after the decimal point.

I let two digits after decimal points or last zeros after decimal point, except for r2.

21. Figure 8. The font size is too small.

Font size of the legend was increased.

22. Lines 340-341. What do you mean by "extreme variability"? You discuss "extremes" in the introduction and "variability" later, but "extreme variability" is not defined.

It was simply to refer to the fact that the variability of multifractal field is strong. We removed “extreme” to avoid any confusion.

23. Lines 341-343. Yes, rainfall–temperature scaling is higher for shorter durations. But how do you connect this to your findings?

Following your comment, the first two paragraphs of the subsection were updated and rephrased to clarify the link.

24. Lines 344-345. First, what you show is that the maximum observable singularity may increase with increasing temperature (the reported r values are lower than 0.5 in both experiments), but it may also remain at a similar level, given the relatively large spread seen in the figures. Second, the CC relationship concerns moisture-holding capacity and is used as a thermodynamic benchmark for short-duration rainfall intensification. If I understand correctly, singularity, by contrast, is an exponent-like, scale-based measure of intermittency/extremeness across scales, rather than a rainfall intensity itself. Therefore, stating “as expected with the CC relationship” may imply a direct theoretical prediction that CC scaling should lead to an increase in singularity. That link appears indirect and is questionable for the time scales explored here (up to 11 days).

The main point is that it is possible to explain the point reported by numerous studies that there is a scale dependence of the increase of rainfall extremes with temperature, i.e. that the increase is stronger in percentage for shorter durations, independently of whether it is related to CC relationship. The sentence you mention was removed and replaced by a new one.

25. Lines 351-353. I do not see this link. Could you provide a concrete example?

Following your comment, a sentence was added to clarify the paragraph.

26. Appendix. Please move all appendices to the supplementary information, provided as a separate file, following the author guidelines.

This was done.

27. Lines 379-384. These final sentences do not seem directly relevant to the paper. Why do you suddenly discuss IDF curves? I would remove this from the text.

It was added to answer to comment by a referee during previous revision. We removed it.

Answer to anonymous referee #1

Thank you for your positive feedback accepting the paper as is.

Answer to anonymous referee #2

The authors have made substantial improvements to the paper and addressed most of my previous comments. However, there are still a few minor issues that should be considered before publication.

Thank you for your review and positive feedback. Please find below our answers to your remaining comments. The manuscript was updated accordingly.

Introduction and Literature Review:

When discussing that short-duration extremes are more sensitive to temperature (ll. 75-80), it would be useful to also cite Schleiss et al. (2018), who also mentions this issue and how it relates to intermittency (https://esd.copernicus.org/articles/9/955/2018/).

Thank you for the suggestion. This was added in the rewritten shorter paragraph following the editor's comments.

Methodology;

The use of a fixed 2°C temperature threshold to remove snowfall appears questionable. Over land, snowfall can occur at surface temperatures up to about 4°C. A more conservative threshold (e.g. 5°C), as suggested by the National Snow and Ice Data Center (NSIDC), or a combined approach using temperature and Parsivel weather codes, may be more robust. If the authors don't want to change this threshold, they should at least include a short sentence to clarify that 2°C is not sufficient to confidently exclude snow/ice.

Following your comment, a sentence was added.

In addition to snowfall, potential hail contamination should be considered, particularly during intense convective events. While Parsivel disdrometers provide hail-related flags based on particle size and fall velocity distributions, these are not fully reliable and may produce false detections during heavy rainfall. Additional quality control or sensitivity analyses could help assess and reduce possible hail contamination, especially at higher temperatures.

Hail is very rare in these location, and filters for potential hail were not implemented. Following your comment, a sentence was added to mention this point.

Results:

The analyzed temperature range appears to be limited, with maximum values around 23°C. This restricted range may limit the robustness of temperature-scaling conclusions. It would be nice to mention this somewhere in the paper and provide some insights into the trends above 23°C (even if the sample sizes are not large enough).

Such data was not available. You are correct that it is a relevant point and it is now mentioned in the conclusion.

Using event-mean temperature may be debatable, particularly for long-duration events and/or nocturnal events, where temperature variability within the event could be substantial. A short comment mentioning this issue could be added.

This point in now mentioned in section 4.1

Figure 6 shows strange decreasing trends for C1 at lower temperatures that are not discussed in the text. Further discussion would be helpful, including whether this behavior could be the result of seasonality or changes in dominant precipitation type with temperature.

Only 1 and 4 samples were available for these two points compared to more than 14 for all the other bins. Your points are relevant and interesting but it is not possible to explore this issue with the available data. It is now mentioned explicitly in the text.

For the seasonal analysis in Section 4.3, providing temperature distributions (or at least minimum, maximum, and mean values) for each season would improve interpretability.

Following your comment, this information was added in section 4.3.

Section 4.4 is very brief. Equation 9 presents relative rates of increase from the UM model, but the discussion abruptly ends there. Some numerical examples and applications of Equation 9 would help readers better understand how these predicted rates compare with expected rates from the literature.

Following your comment as well as the editor’s, the section was partly re-written to improve clarity on what is done. Numerical examples were incorporated.

A large portion of the Results section focuses on diagnosing scaling parameters. While informative, this is clearly less interesting and novel than the temperature dependence of gamma_s and its scaling behavior. The last part of the results is comparatively short, and several open questions remain. In particular, the advantages and limitations of this approach relative to CC scaling are not clearly discussed. The 2CC scaling issue mentioned in the introduction does not come back, not the potential changes in scaling behavior at high temperatures (e.g. above 24°C). By linking back to open questions raised in the literature review, Section 4.3 would become much stronger.

Following the comments by the editor, the purpose of the paper was re-oriented to focus on the link between rainfall variability and temperature, mentioning less the link with CC scaling.

Table A3 shows an unequal number of samples across temperature bins. It would be helpful to clarify whether this imbalance was accounted for when fitting regression lines and estimating confidence intervals for slopes and correlation coefficients.

This was not accounted for and it is now clarified in the text.

Typos and English:

Line 36: "went twice as strong as than expected" → remove "as"; correct to "was twice as strong as expected".

Line 40: "stemed" → should be "stemmed".

Line 50: "increased twice faster than CC" → change to "increased twice as fast as CC"

Line 51: "three time faster" → should be "three times faster".

Line 71: "sationarity" → should be "stationarity".

Line 73: "exploring link between rainfall variability" → change to "exploring the link between rainfall variability".

Line 75: "fits in this overall context" → "fits within this overall context".

Line 85: "across wide range of space-time scales" → change to "across a wide range of space-time scales".

Line 109: "Temperature data was collected" → "Temperature data were collected".

Line 121: "analysis are carried out" → should be "analyses are carried out".

Line 126: "accounting for potential effect" → "potential effects".

Line 140: "number of events kept in analysis" → "kept in the analysis".

Line 150: "number of samples time 128" → "number of samples times 128".

Line 170: "with the help a scale invariant singularity" → "with the help of a scale-invariant singularity".

Line 175: "an homogeneous field" → should be "a homogeneous field".

Line 220: "maximum observable, They showed" → capitalization error after comma

Line 235: "Outcome of spectral analysis" → "The outcome of the spectral analysis".

Line 270: "withing a given bin" → should be "within a given bin".

Line 275: "Fig. ??" → unresolved figure reference

Line 280: "C1 and γs increases" → subject is plural; change to "increase".

Line 285: "analysis were carried out" → should be "analyses were carried out".

Line 300: "in the individual events with bad scaling, i.e." → should be "individual events with bad scaling".

Line 315: "In a similar the others were classified" → change to "Similarly, the others were classified".

Line 330: "belonging in the 10 % upper percentile" → "belonging to the upper 10% percentile".

Line 365: "enables to explain" → "helps explain".

Line 375: "expand our understanding on the dependence" → "expand our understanding of the dependence".

Line 386: "python scrip" → "python script".

Line 390: "Authors declare they have no competing interests" → "The authors declare that they have no competing interests".

Line A3 table: Both rows are labeled "# samples in large scale". Please check that this is correct.

Thank you very much for your careful reading. This was corrected.

Answer to anonymous referee #4

The revised version of the manuscript still puts a very strong emphasis on rainfall extremes (title, abstract, introduction, etc.). While the term “variability” has been added to the title, it is in addition to the term “extremes”. The abstract still starts with a definition of rainfall extremes: “a given percentile or for a given return period” so that rainfall extremes must be interpreted as large intensities with a rare frequency. The authors failed to address the main comments made by several reviewers concerning the results shown in the manuscript with regard to rainfall extremes (comparison to other models, interpretations, generality of the results for other regions). The current manuscript is still focused on the application of the UM framework and does not show the general interest of this framework for studying the link between rainfall extremes (with actual rainfall intensities) and temperature. The authors respond that “The title was also updated to avoid any confusion. The abstract clearly states the scope of the paper [Here we investigate how rainfall extremes and more generally variability across scales change with temperature]. A few sentences in the introduction were also updated to avoid any confusion.” This response illustrates the general confusion between rainfall variability and rainfall extremes made in the paper. The manuscript addresses the relationship between rainfall variability and temperature via the prism of the UM framework but not rainfall extremes (basically the extrapolation of these relationships). Furthermore, all the results show UM parameters which are meaningless for the readers who are not familiar with the UM framework. I urge the authors to show results in terms of rainfall intensities which could be intelligible for any reader.

Following your comment as well as the editor’s, the focus of the paper was clearly shifted to variability.

In addition, I disagree with the new statement made in the conclusions “Practitioners are typically used to work with IDF curves and how they evolve with climate change.” IDF approaches are just one approach among others, the most common practice being frequency analysis (see, e.g., Klemeš, 2000), often using extreme value models, and often with a nonstationary setting (Westra et al., 2013). It is also possible to apply stochastic models (e.g. Cantet and Arnaud, 2014, Agilan et al., 2019) to obtain characteristic values. The authors did not provide any comparison with other approaches on the basis “The latter can be used to characterize IDF curves, but this is outside the scope of the paper.” In the conclusions, the authors seem to acknowledge that the UM framework cannot easily provide insights for return level estimates. As indicated later “it would be needed to continue exploring the theoretical links between UM parameters and IDF curves”. It seems to be an interesting perspective, although linking the parameters of non-stationary IDF models to UM parameters seems very challenging. However, it also means that UM models are not interpretable in terms of rainfall intensities associated with different frequencies (including rainfall extremes).

Agilan, V.; Umamahesh, N. V. Rainfall Generator for Nonstationary Extreme Rainfall Condition. Journal of Hydrologic Engineering 2019, 24 (9), 04019027. https://doi.org/10.1061/(ASCE)HE.1943-5584.0001821.
Cantet, P.; Arnaud, P. Extreme Rainfall Analysis by a Stochastic Model: Impact of the Copula Choice on the Sub-Daily Rainfall Generation. Stoch Environ Res Risk Assess 2014, 28 (6), 1479–1492. https://doi.org/10.1007/s00477-014-0852-0.
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Westra, S.; Alexander, L. V.; Zwiers, F. W. Global Increasing Trends in Annual Maximum Daily Precipitation. Journal of Climate 2013, 26 (11), 3904–3918. **https://doi.org/10.1175/JCLI-D-12-00502.1**.

This statement was removed.