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

The manuscript aims to study the link between precipitation extremes and temperature with a scale invariant framework based on the Universal Multifractal. The analysis is carried out on 3 high resolution time series of precipitation available in the Paris region, France.
I am not an expert of it but the theoretical UM framework seems to be well established and to allow for such an analysis. I see however some limitations in the analysis that should be likely fixed or at least discussed to strengthen the potential impact of this work and make it suitable for publication in HESS.

Thank you for your suggestions to strengthen the discussion part of this paper. See our answers below.

The innovation with respect to previous works is not clear for me. It has to be clarified in the introduction. What results (findings, robustness of findings, multiscale coherency of results ?) are allowed by this UM based analysis that could not have been presented in other previous works – especially with respect to the Temp/PrecipExtremes relationship.

In this paper, we suggest to address the same issues as previously mentioned authors and to not focus on a few single observation scales independently as usually done, but to investigate how rainfall extremes and more generally rainfall variability across scales, change with temperature. This will enable to get more robust results in the sense that they are valid across a given range of scales.
Introduction was updated to better clarify this.

The dependency to temperature is explored here with observations. Observations are obviously key for this. Other high resolution time series of precipitation are available worldwide. The work would really gain value and generality if other stations, from other climate contexts could be integrated in the analysis. The 3 stations considered here belong to a same and very small meteorological region and one would likely have some comparative results / findings of analyses for other contexts.
For me also, the authors should also recognize / discuss the interest of climate simulations for this T/Pextreme exploration (with strengths / limitations compared to analyses based on observations), especially those produced from convection permitted models. Works based on climate model outputs are numerous to quantify the impact of climate change on rainfall extremes. Models come of course obviously with a number of limitations but they give the opportunity to explore a much larger “meteorological/climatic domain” than those available from observations. Using models may also allow to explore the importance of the limitations mentioned in the introduction ln 23-24 (especially those relative to possible change in circulation regimes). I would strongly suggest to include a discussion on those issues, at least to mention them.

Yes you are right that the studied time series are short and come from a small geographical area. We agree that it would be interesting to carry out the analysis on other time series. The use of climate model outputs would also be very interesting, we agree. Following your suggestion, the conclusion was updated to include in more details these suggestions as interesting future work.

To estimate possible evolutions of extreme with climate change, an alternative to climate models is indeed that of the statistical approaches mentioned ln 55, where precipitation characteristics are regressed against temperature. As statistical approaches, they obviously present also a number of limitations that should be acknowledged. The most critical one is likely the assumption that the relationship identified from observations will be still valid in a modified climate (stationarity assumption). This assumption will likely not hold in a number of regions, especially (but not only) as a result of changes in circulation regimes. Different evolutions of this relationship may likely exist depending on how circulation regimes will change (this is likely to be shown with climate experiments with different climate models). This issue should be likely commented, at least mentioned in the introduction or elsewhere.

Indeed the assumption of stationarity is a strong limitation of the approach. Following your comment (which was also pointed out by another reviewer), this is now mentioned in the introduction.

For the introduction and perhaps discussion, I would thus suggest to put in a larger perspective the issue targeted in the manuscript, clearly identifying what we know / do not know for the present / future climates, what data / tools we have to explore this issue, what are the knots and challenges for scientists there, etc…

The paragraph introducing the work was updated to improve clarity.

In the introduction, the authors have a long discussion on the Clausius-Clapeyron relationship and on previous analyses of its interest to support observations of changes in precipitation extremes worldwide. How results of the present work confirm / contradict this CC influenced behavior of extremes for the considered stations ? Is there any dependency on time resolution ? on season, weather/rainfall type ? on the spatial integration area of real interest for extremes (local extremes are of little interest for most "impacts"). Those points would be worth a section in the discussion. The discussion should be likely strengthened. It would be worth to better put in perspective the results of the work with those of other studies / other approaches. Are the identified trends similar / smaller / larger than in the other works ? What is the added value of the present work ? The discussion should also discuss the limitations of the work

Results confirm the influence of temperature on rainfall extremes, as expected from CC relationship. The scale invariant framework used enables to have this result obtained for various time resolution. This is clarified in the text (section 4.4).
With regard to season effect, a section has been added to do discuss this following your comment (as well as one from another reviewer).
For quantitative comparison with existing work, a full theoretical shape of IDF curves with UM parameters would be needed. This is an ongoing work. Following your comment, a paragraph was added in the conclusion as perspective.

The perspectives of the work would be also worth to be mentioned in the conclusion.

Following your comment, conclusion was significantly expanded to include more discussion on potential perspective of this work.

Detailed comments.
Equation 9 : the % increase is valid for all return periods ? how does it compare to other works where the % increase has been sometimes found to depend on T (e.g. Chagnaud et al. 2025)

No dependency was found on temperature range in this study. This issue is now discussed in the new section 4.3, which was added following your remarks.

Ln 51 – 54 : the rationale / scope of the paragraph is not clear. A reformulation would be worth.

We are sorry, but we are not sure to understand what you mean.

Ln. 59. How is it possible to "properly" characterize the link ? Are we sure that the link exists ? that it is strong ? I guess this is not always the case. What about the significance of the link ?

Following your comment, the sentence was rephrased and qualified to avoid any confusion.

Ln 64. "Another limitation" : what is the first one ?

It was corrected to "A limitation"

Ln 102 and 107. The number of years considered should be given in the main text.

This information was added.

Ln 112-113. Can you clarify "point ii)" ? I did not understand what is done / why this is done.

This was clarified.

Fig. 3. I do not understand why a sequence of 4 hours of rain should be split into 3 different subevents. Is it relevant to consider that the temperature predictor can be considered with a so small resolution (i.e. that changes in temperature from one 3h time step to the other can have some explanatory potential on precipitation extremeness ?) Can you clarify ?

This is done to have a unique scale range for all the event to have consistent analysis on the UM analysis. In event analysis, the samples of a same event are analysed together and the average temperature considered is the one for the whole event. This was clarified in section 4.2.

Ln 119. What is a conservative field and ln 142 : what is a non conservative field ??? For me, rainfall is by nature conservative. (at least observations). Can you define "conservative" ?

Following your comment, the paragraph was updated, and an additional reference containing details for interested reader cited.

Ln 212 and 231 I do not understand how a field can be non-conservative. Please clarify.

It corresponds to fields with long range correlation. It was clarified in the text.

Ln 128. How is defined a "singularity"

A "singularity" can be seen as a scale invariant threshold. More precisely, a multifractal fields behaves as lambda^singularity. The singularity remains the same across scales while the value of the field changes with scales; which is typically the case for rainfall. 20 mm/h does not have the same meaning over a time step of 30 s and 1 day. Following your comment, this was clarified in the text.

Ln 130. I am not sure I agree. Is it valid for all multiplicative random cascades processes ? microcanonical ones ? canonical ones ?

UM are limit behaviour of all multiplicative random cascade processes. It comes from a generalization of the central limit theorem. The precise meaning is described in details in the reference from 1997 which is already cited. This was clarified in the text with a clear reference for the interested reader.

Ln 130. I am not sure multiplicative random cascades have been defined previously.

Indeed, but we believe that it is not needed to add more details for this paragraph which remains very generic. This can of course be updated if you believe it is much needed.

Ln 154. “in case of greater H, epsilon should be used”. Should be used to do what ?

To implement TM and DTM technique. This was clarified.

Ln 177. “synoptic scale” : warning, this is only the temporal scale. All your analysis are on local data (then no synoptic in spatial dimension)

Yes you are correct. Following your comment and also one by another reviewer, this was clarified in the data section. More generally, the term “large scales” for the regime 30 min – 11 days is now used to avoid any confusions.

Table 2. There is some large difference in the coefficients between stations (while the 3 stations belong to a same very small “climate” region). Can you comment ? Is it expected ? large ? small ? reasons for this ?

Values were recomputed after adding a filtering on snowfall also for long samples and they are now very similar.

Ln 197 and 223 : “individual sample with “bad” scaling”. I do not understand why you expect “good” scaling behavior for all events. The variability of rainfall temporal patterns is potentially huge and if I understand that a scaling behavior is expected in average considering all events, I guess there is no reason to have it for each event. Then, how do you deal with those events which do no have a scaling behavior ??? I fear that disregarding them may lead to too naïve interpretations of the existence / strength of the temperature / scaling relationship. Can you clarify, discuss this point.

Scaling being an average behaviour, it is expected that some samples will not exhibit a good scaling behaviour. And they were incorporated in the ensemble analysis. Yet, adding their assessed parameters in the individual analysis would not be relevant because they are not reliable so they could bias results. This was clarified in the text.
Yet, following your suggestion, an additional type of analysis was carried out, enabling to account for all the events. It consists in performing a UM analysis on ensembles of sample / event binned within temperature intervals. In enables to get more robust results. So thank you for your stimulating question !

Ln 202-206. Can you comment more divergent results on C1 and alpha ? Could you have some equifinality issue here ? can you precise why taux-s is a better variable to study the relationship ?

Divergent results on alpha and C1 are common, and already reported in Royer et al. (2008) for example. It is not an equifinality issue. gamma_s combines the influence of both which is why it is interesting to use. Section 3.2 was updated to clarify this.

Ln 231 : what is the significance of the estimated trends ?

A statistical test to assess the significance of the retrieved trends was added in the paper and is now discussed.

Chagnaud et al. 2025. How fast is the frequency of daily rainfall extremes doubling in global land regions, *ERC.* https://doi.org/10.1088/2515-7620/ad9f12

Thank you for your suggestion. This paper is now discussed in the text.