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

This manuscript investigates the relationships between the parameters of Universal Multifractal (UM) models applied to rainfall time series and temperature measurements. These UM models are applied to rainfall observations at a very fine temporal resolution over the period 2018-2025, and recorded at three different locations near Paris, France. The manuscript is well structured, and the authors have provided a summarised description of UM which is not too technical and focuses on the interpretations of the results (parameters, scaling properties). I must acknowledge that I am not an expert in UM. It seems that the description of the relationships between the UM properties and temperature has never been done before, and I imagine that this study is of interest for readers who are well versed with this type of model. However, in my opinion, the authors miss the opportunity to reach a more general audience because these results are difficult to interpret in comparison to other approaches. My recommendation is thus to extend the end of the study. What is missing is an interpretation of the UM results in terms of rainfall intensity (return levels), at different durations (see major comment #1).

Thank you for your positive comments on the manuscript.
With regard to the second part, see answer to MC#1.

In addition, I did not understand if these results can be related to the Clausius-Clapeyron relationship. There is a very short subsection dedicated to this question (subsection 4.3) but no results are provided (e.g. application of Eq. 9). Additional results illustrating how UM properties can be related to the Clausius-Clapeyron relationship, i.e. if they confirm the CC scaling discussed at l. 18-21 (see major comment #2) are needed.

See answer to MC#2.

MC#1: The manuscript put a very strong emphasis on rainfall extremes (title, abstract, introduction, etc.). The abstract 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. Typically, return periods are expressed in years (e.g. intensities occurring once every 10 years on average). At l. 59-61, it is indicated that the aim of the study is to properly characterize the link between rainfall extremes and temperature. However, the rainfall series are rather short (6 years, <1 year and 2.5 years) and the rest of the study does not provide results on large return levels. The beginning of the manuscript is thus misleading in that respect. As I understand, the UM perspective considers that the properties of rainfall extremes can be derived from the scaling behaviour of rainfall from a very fine temporal resolution (30 s) to aggregated time steps. What is very difficult is to understand what the results shown in Figs. 5 and 7 mean in broader terms, for readers unfamiliar with UM, e.g. in terms of rainfall intensities at an hourly or daily time scale, and for return periods of 2, 5 or 10 years. I also mention this aspect because the trends for alpha and C1 are not easy to interpret in both Figures (opposite trends in Fig. 5, weak trends in Fig. 7) and the trend for gamma_s is very weak in Fig. 7 (r2=0.084). At the end of the section “Results”, it is indicated that this study provides “a framework to explain changes in increase of rainfall extremes with temperature according to scale (mainly from hourly to daily) which are reported in previous studies”. In my opinion, additional results must be shown to demonstrate that this coherence exists, e.g. the rate of increase of rainfall extremes per degree, for a given return period, at an hourly and daily time scales, for the three locations, derived from the UM, and in comparison to standard approaches using longer time series (precipitation and temperature) nearby. I certainly understand that this asks for additional work but in my opinion, the current manuscript is too much 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 and temperature.

With regard to rainfall extremes and IDF curves. The paper aims at exploring rainfall extreme variability through UM framework. The latter can be used to characterize IDF curves, but this is outside the scope of the paper. A paragraph was added in the conclusion to clarify this as a relevant perspective (authors fully agree that it is indeed a topic to be fully explored in a dedicated paper!): "Practitioners are typically used to work with IDF curves and how they evolve with climate change. In order to further link the results obtained in this paper with previous findings, often expressed in terms of return periods, it would be needed to continue exploring the theoretical links between UM parameters and IDF curves. This is a topic currently under investigation, relying on initial elements to be further developed from \citet{bendjoudi_interpretation_1997} or \citet{Langousis_2009}. Once this is achieved, the trends in UM parameters could be simply input in the theoretical shape of IDF curves and results quantitatively compared with existing work. This corresponds to stimulating work to be carried out."
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 sentence in the introduction were also updated to avoid any confusion.
With regard to the robustness of trends. Following your comment and some of another reviewer, a additional analysis relying on samples binned by temperature intervals was done and added to the paper (new Fig. 6).

MC#2: There is almost no discussion of the results in broader terms at the end of the paper. In particular, the CC relation is discussed in depth in the introduction and I was expecting an interpretation of the results in terms of CC relation at the end of the paper: is the UM framework in coherence with the CC relation or does it lead to a super-CC, a sub-CC? I also miss a general discussion about the impact of climate change on the relation between rainfall extremes and temperature for different types of rainfall events (https://doi.org/10.1007/s40641-015-0009-3).

The analysis carried out do not aim at determining whether a super-CC or sub-CC is observed. Following your comment, this was clarified in the beginning of section 4.4 (formerly 4.3). With regard to rainfall type, some analysis focusing only on highest rainfall was carried and did not yield different results. Splitting the temperature range was also explore, as well as seasonal effect. A new subsection was added to present the results. Thanks for the suggestion of citation that was incorporated in this new subsection.

Minor comments:
l. 2: "convection permitting model outputs" -> or more generally climate model outputs: https://doi.org/10.1029/2019GL082908.

The sentence was updated in the abstract.
Thanks for the suggestion of paper. It was added.

l. 52: "a" -> an

Thanks for your careful reading ! This was corrected.

l. 128: I suggest replacing "as it can be seen on Eqs. 1 and 2" by "Eqs. 1 and 2 also mean that" because I do not think many readers will see that.

Thanks for your suggestion which we implemented.

l. Eq. 3: missing dot at the end of the equation.

This was updated.

l. 153: is it H<0.3 or H<0.4? Or maybe rephrase as H is roughly smaller than [0.3, 0.4]?

This was updated following your suggestion.

l. 182: /2 h -> 30 minutes.

This was updated following your suggestion.

Table 3: Why is the slope for alpha so different for the SIRTA campaign? It looks like the linear model failed (r2 for alpha equals to zero), could you comment on this in the manuscript?

A filtering for potential snowfall was also added in the larger scale analysis and values of alpha and C1 and now very similar for the three campaigns.