Answers to both 1st and 2nd Referee's comments:

Answers to 1st Referee's comments:

General Comment: *The manuscript presents a stochastic approach to generate an ensemble of reference rainfall scenarios (giving a desired rainfall amount P, rainfall duration D and return period T). This approach is based on multi-fractal theory, which is parsimonious and easy to apply. While this research topic is generally relevant for the readership of HESS, there is still a bit of issue which needs to be addressed. Please find the attached comments. Those comments should be addressed before considering publishing this article.*

Response: We thank the referee for meticulously reviewing our manuscript and providing several constructive suggestions. We are especially grateful for the referee's positive feedback. In this document we provide our detailed response to the referee's supplementary comments and also mention how we have addressed these issues in the revised version of this manuscript.

Supplementary Comments:

Comment 1: *L7: research gap is not clear.*

Reply: It should be noted that this paper addresses the research gap between standard procedures for defining reference precipitation and the strong multi-scale intermittency of precipitation. It therefore proposes a procedure designed to tackle multi-scale intermittency head-on, based on extreme non-Gaussian statistics and scaling behaviour over a wide range of scales. The aim of this paper is that the baseline precipitation scenarios simulated by this procedure can be used as more realistic inputs into hydrological models for applications such as the optimal design of storm-water management infrastructure, including green roofs.

We have added the above explanation in the abstract (L7) of the revised manuscript to make the research gap that we address clearer right at the beginning of the paper.

Comment 2: *L21-23: it is not clear to me why 'uniform rainfall or … for such purposes' directly leads to the conclusion 'stochastic simulation of reference rainfall events is necessary'. At least one sentence (to discuss the lack of measurement data …) is missing.*

Reply: We agree and have improved the corresponding text in the revised manuscript. The sentence ‘Uniform rainfall … for such purposes’ has been shifted to L92, whereas L32 has been replaced by ‘Rainfall is quite commonly considered to be a stochastic variable due to the fact that rainfall process is complex and strongly dependent on initial conditions.’. We have also added a sentence (L34) mentioning the lack of high-resolution observational data following the referee’s suggestion.

Comment 3: *L23: Does it refer to several types of single-site stochastic rainfall models or space-time models? Event-based simulation or continuous simulation? Please clarify.*

Reply: When we mention several model types we refer to all these classes. To make this clearer, we have replaced this by ‘There have been several studies/attempts to stochastically produce rainfall time series and space-time fields as listed here: Simple point processes …’ in the revised manuscript (L36).

Comment 4: *L25: This paragraph is describing ‘several types of stochastic rainfall models’, but I am confused about why a model related to river discharge is described in detail. I get the point that one module in this ‘Hydrogram model' is to generate hyetograms. If it is the case, I am wondering whether it is better to directly state that one of those types is the Monte Carlo method instead of the 'Hydrogram model'.*

Reply: We agree, and have replaced L39 with ‘, and Monte Carlo method… All these four models are purely temporal’ in the revised manuscript.

Comment 5: *A general comment on this whole paragraph (L38) & Table 1: The idea of creating this table looks good. However, more references would be needed. The latest one mentioned in this ‘literature-based assessment’ is in 2004, which is almost 20 years ago. In references for several model types for example in hybrid processes-based models, all references mentioned were published 20 years ago. Although those reference papers mentioned by the authors are very useful, it is necessary to update the reference list.*

Reply: In the revised manuscript we have added more recent references in this part (L37-throughtout the paragraph) and in Table. 1 following the referee’s suggestion.

Comment 6: *L38: does it refer to spatial rainfall field or spatially averaged rainfall time series?*

Reply: For the sake of better clarity we have replaced this with '1) Heterogeneity: Spatial Heterogeneity – rainfall is extremely variable with spatial location, especially at small spatial scales and Temporal Heterogeneity or Intermittency – rainfall time series at a single spatial location is extremely variable with time, especially at small time scales' in the revised manuscript (L56).

Since more than one query of the referee seems to be regarding space-time vs. time modelling, we have clarified (L95 of revised manuscript) that:

i. this dichotomy is not as strong as usual for multifractal models because a time multifractal can be seen as a time cut of a space-time multifractal
ii. the aim of the present study is focused over a fixed (and rather small) space extension such as a building roof
iii. the large scale deployment of rainfall-runoff management technologies would instead require space-time models, obtained with the help of new and rather limited developments (see (i)).

Comment 7: *L38: Does 'modelling approaches' refers to those modelling approaches for getting a realistic rainfall field for most hydrological applications or specifically for sizing storm-water management infrastructures?*

Reply: It refers to those modelling approaches (mentioned in the previous paragraph) for getting a realistic rainfall field for most hydrological applications (one such application is the designing of rain-water management infrastructures). We have made this clearer in L54 of the revised manuscript.

Comment 8: *L40: All these characteristics are '… makes the simulations physically relevant/realistic' while 'physically based' is specifically mentioned again in the second criteria 'Physically-based – the simulations rely on physical principles. What's the difference between these two? At least, the meaning of 'simulations rely on physical principles' in the second criteria need to be clarified.*

Reply: For improving clarity, we have replaced 'physically relevant/realistic' by 'realistic' in L55. The term "physically-based" simply implies that these rainfall models represent the underlying process (at least abstractly) using physically meaningful parameters. This term is used in a slightly more generalized framework, because it is stochastic rather than deterministic, with fractional rather than integer derivatives. We have added this explanation in L59 of the revised manuscript.

Comment 9: *L56: The research gap is not ever mentioned until now. It would be much better to clearly state it in the manuscript.*

Reply: We have done this as mentioned in our reply to Comment 1 of the referee.

Comment 10: *L57: Single-site or multi-sites or spatial-temporal rainfall field or something else? please clarify.*

Reply: At present we simulate reference rainfall time series scenarios for different conurbations so single-site for three different sites (we have clarified this in L90). However, the advantage of the proposed simulation procedure is that since it is based on UM framework it can be easily extended in future to simulate spatio-temporal rainfall fields as well. This is mentioned in L98 of the revised manuscript.

Comment 11: *L59-61: Ok. this seems to be the research aim, which gives me the impression that the authors are implying that: Using any existing stochastic rainfall models, it is NOT possible to "simulate reference rainfall ensembles characterized by P, D, T while exhibiting temporal variability and intermittency close to that of observed rainfall data". If my impression is correct, the authors were taking this as a research gap. However, this gap was not discussed or mentioned in the introduction section. I suggest revising the whole section and adding more essential information to it to make this introduction section easier to follow.*

Reply: Yes, as mentioned in our reply to Comment 1 of the referee we have modified the abstract (L7) and this part (L81) in the revised manuscript.

Comment 12: *L78: resolution (space and time)?*

Reply: Just time, we have mentioned this clearly in Table. 3 and L116 of the revised manuscript.

Comment 13: *L120: are TM analysis and DTM analysis newly developed in this study? This is not clear to me.*

Reply: No, these are standard multifractal statistical analysis techniques and we have cited the corresponding papers in L163 and L179 of the revised manuscript. However, this study uses a slightly modified iterative DTM procedure (also already existing) as explained in Appendix A. We felt it would be a bit more convenient for readers unacquainted with these analysis techniques if we added brief explanations here.

Comment 14: *L170: Is scale-symmetry able to be represented by other model types? From Table 1 in this manuscript, at least the Radar-based method can represent this, which can directly give us a space-time field. Why choose the multifractal theory instead of a radar-based method (see Pegram's paper as mentioned by the authors)? It will be helpful to justify the choice and include this discussion (probably in the introduction section).*

Reply: The procedure proposed here needs only observational rainfall time series (not very data demanding) and is computationally simpler and parsimonious compared to the Radar-based bead method of Pegram. The current procedure can also be directly extended to obtain space-time fields as well. Furthermore, the idea of space-time complexity in the UM framework is somewhat more generalized than it is in the Radar-based bead model (spatial complexity and temporal complexity are dealt with separately rather than together). We have added a brief discussion about the Radar-based bead model and why we prefer the UM cascade model over it in the introduction section (L76) of the revised manuscript.

Comment 15: *L172: The authors mention that these types of models can be considered as a bridge between purely statistical and purely physical models. I am wondering whether these features have been in this cascade model used in this study: seasonality (or different types of storm event).*

Reply: As mentioned in L219-222 of the revised manuscript, this statement was made in the context of the UM cascade model using the physical concepts of energy transfer from large scales to small scales by random breakup of eddies to abstractly represent atmospheric processes underlying rainfall production.

Although multifractal (statistical) analysis of observed rainfall in the three conurbations chosen by this study do not display any significant seasonality (as there is no scaling break around a few months time scale), there is a clear evidence of a strong synoptic maximum (indicated by a scaling break around few weeks time scale) with corresponding changes in scaling behaviour. It is worth noting that this aforementioned absence of seasonality in multifractal characteristics means that the low frequency scaling regime's UM parameters are sufficient to represent seasonal variability (in cumulative precipitations), whereas together with the high frequency scaling regime's UM parameters they are sufficient for reproducing well the statistics of different storm types (either convective or stratiform). This requires some elaboration of the UM cascade process to guarantee good agreement between observed and simulated rainfall over the full range of time scales. This explanation has been added to L208 of the revised manuscript.

Furthermore, we have tried to compare seasonality (in a simplistic manner) in the simulated and observed rainfall as discussed in L355 of the revised manuscript.

Comment 16: *L176: It seems that the study presented here is to simulate temporal rainfall as the author mentioned in the abstract, but 'rainfall fields' convey a message that a time varying spatial field is generated. Not entirely sure if this word is widely used when time series is generated. If not, please change this word.*

Reply: We used the term field in a more generalized context since a time series can be considered as a one-dimensional field. However, for the sake of better clarity we have used 'rainfall time series' instead as suggested by the referee in the revised manuscript (L227 and several other places).

Comment 17: *L180: Does it always refer to temporal resolution?*

Reply: Yes, we have explicitly stated this in the revised manuscript (L231, L233).

Comment 18: *L225: Ok, I can see that these metrics might be useful. Besides these, I am wondering whether the autocorrelation of simulated events is well reproduced. This feature plays a critical role in dominating a catchment response to a rainfall event. In addition, since this approach is proposed for hydrological applications, I am wondering whether it is much more convincing to feed those simulated scenarios into a hydrological model (event-based) for better validating this approach?*

Reply: The Codimension Comparison Metric (CCM) already takes care of this issue and more. Autocorrelation or its inverse Fourier transform i.e. spectral density are generally just second order statistics. Comparing the scaling moment function $K(q)$ for $q = 2$ of observed and simulated rainfall is the same as comparing their respective spectra and therefore their autocorrelation. The CCM compares $c(\gamma)$ instead of $K(q)$ however they are just the Legendre transforms of each other. We have added this explanation in L330 of the revised manuscript.

It is true that the proposed approach is for hydrological applications such as designing green roofs for rain-water management, however, we need observational data of not only rainfall but also discharge from the green roof to validate the entire hydro-meteorological modelling approach. We are still working on setting up experimental green roof prototypes and monitoring protocols for this purpose, and the referee's query can only be addressed via a separate publication in future. We have added this part in the conclusions section of the revised manuscript (L379).

Comment 19: *L280: Has the author developed/improved a novel method in this study. Please clarify it.*

Reply: Yes (as mentioned in our reply to referee's comments 1 and 11). Even though several earlier studies have attempted to simulate rainfall using a Universal Multifractal (UM) approach, we are unaware of UM-based studies that have proposed procedures to simulate reference rainfall scenarios. We have added this explanation in the beginning of L363.

Comment 20: *Figure 6 and Figure 7: these two figures are not informative. Please consider simplifying/redesigning these or moving them into the appendix*

Reply: We feel the referee is referring to Figures 7 and 8. If so, we agree and have moved them into the appendix.

Answers to 2nd Referee's comments:

General Comment: *This paper presents the use of universal multifractal to generate ensembles of rainfall time series that recreates the Intensity (I), Duration (D), and Frequency (F) of rainfall time series, commonly used in the design of storm-water infrastructure. This paper may become an essential contribution to the literature body of stochastic simulations of rainfall time series. However, I found two pitfalls in the paper: 1) There is no clear definition of the research gap (including connections to previous works), and (2) Even though the paper assesses their methodology, the discussion about the results is almost non-existent. I hope my* *comments provide a road map to improve the important contribution done by the authors. See the attached file for a detailed description of my concerns.*

Response: We thank the referee for reviewing our manuscript and providing several constructive suggestions. We are especially grateful for the positive feedback. In this document we provide our detailed response to the detailed description of the reviewer's comments and also mention how we have addressed these issues (especially regarding research gap definition and result discussion) in the revised version of this manuscript.

Major Suggested Comments:

Comment 1: *The research gap is vague. The authors must address the following questions. Why is it important to explore universal multifractals in rainfall datasets? Have studies been using this technique in rainfall datasets before? What are the challenges of using stochastic techniques and/or multifractals to reconstruct rainfall time series?*

Reply: Following the referee's remarks and suggestions, we have specified the research gaps, which are of three kinds:

- a general discrepancy between standard procedures for defining reference precipitation and the strong multiscale intermittency of precipitation.
- missing procedure to adapt multifractal precipitation modelling to given partial statistical references.
- missing procedure to assess the accuracy of the method.

The corresponding challenges addressed in this paper are:

- to tackle multiscale intermittency head-on, based on extreme non-Gaussian statistics and scaling behaviour over two subranges of time scales, due to the finite size of the earth. This requires a given adaptation of the multifractal modelling procedure.
- to define a renormalizing procedure for the multifractal model to make the simulations fit with these partial statistical references.
- to define multiscale metrics to assess distance between (closeness of) two time series (observed and simulated) across time scales.

This will enable us to provide baseline precipitation scenarios that can be used as realistic inputs into hydrological models for applications such as the optimal design of storm-water management infrastructure, especially green roofs.

We have incorporated the above explanation in the abstract (L7), and introduction (L81) to make the research gap addressed clearer right at the start of the paper.

Comment 2: *The ambiguity in the research gap is also reflected in the research objective: Line [57] states, "The objective of this paper is to simulate region specific reference rainfall scenarios which could be used as realistic inputs (..) to hydrological models for optimally designing storm-water management infrastructures." This needs to be more specific. What method will be used? Will this be compared to a reference? Also, the objective elucidates "hydrological models for optimally designing"; however, the paper does not address optimal design at all. Readers may believe that the paper explores the use of universal multifractal for optimal designs for specific water-related infrastructure. However, the paper covers a general procedure for estimating ensembles of rainfall time-series, but it does not cover applications and consequences for engineering design.*

Reply: We hope that the above clarification (Reply to comment 1) will dissipate any ambiguity on the goal of the paper. Moreover, we have further clarified that designing optimal storm-water management infrastructures (such as green roofs) is not an objective of this paper but a scope for applications of the reference rainfall simulation procedure (which makes use of the UM framework to properly take into account the temporal variability of rainfall) proposed here. This explanation has been added to L81-90 of the revised manuscript.

We would also like to mention that we are currently working to set-up experimental green roof prototypes (designed based on reference rainfall scenarios simulated using the method proposed in this manuscript) and establishing monitoring protocols, therefore the applicability of the present results is beyond a good wish and referee's comment will be further addressed via a separate publication in future. This explanation has been added to L379 of the revised manuscript.

Comment 3.1: *There is no discussion section! The authors go from results to conclusions, omitting key discussions that will strengthen the overall contribution of their work. Here are* *some general pointers that I feel are relevant to find in the discussion section. 1) How are the results compared with the previous literature? Are the scaling parameters similar to previous studies? What are possible connections in the difference of scaling parameters between the three site studies?*

Reply: We agree and have inserted a specific discussion section based on result discussion already present in the paper. We have added a discussion part in Line [336] addressing these queries.

Comment 3.2: *What are possible strategies to expand this methodology to spatial correlated rainfall datasets? What are potential limitations in the application of this methodology? How feasible is this methodology to represent rainfall structure in large spatial scales? How can the spatial scale and limitations on this methodology affect the design of water-related infrastructure?*

Reply: In the context of these queries regarding space-time vs. time modelling, we have clarified in L94 of the revised manuscript that:

i. this dichotomy is not as strong as usual for multifractal models because a time multifractal can be seen as a time cut of a space-time multifractal

ii. the aim of the present study (which mainly focusses on temporal scales) is focused over a fixed (and rather small) space extension such as a building roof

iii. the large scale deployment of rainfall-runoff management technologies/infrastructures would instead require space-time models (and space-time rainfall datasets), obtained with the help of new and rather limited developments (see (i)).

Minor Suggested Comments:

Comment 1: *[75] Add reference for MeteoFrance.*

Reply: We have added it in the revised manuscript (L118).

Comment 2: *[110] Small sample size? Be more specific. What would be an ideal sample size?*

Reply: In line [147] we have mentioned that a single sample is used, so here the small sample (effective) dimension is 1. Larger the sample size, better will be the estimate of spectral slope (better straight line fit). But increasing sample size with a fixed dataset length means that with more samples the length of each sample is smaller, implying that there is a reduction in the largest scale considered. This may in turn lead to a difference in multifractal characteristics. The TM analysis, on the other hand, does not have this disadvantage and the straight line fits are reasonably good and not too dependent on the number of samples. Therefore, TM analysis is simply more preferable/relevant compared to spectral analysis or the question of how many samples will be ideal when using spectral analysis. We have added this explanation in L151 of the revised manuscript.

Comment 3: *[Figure] Figure 1 is not mentioned in the document*

Reply: It is mentioned in Line [69] of the revised manuscript.

Comment 4: *The term "resolution" is ambiguous throughout the text. Does it refer to time or space? Probably time.*

Reply: The interest of the term "resolution" is that it is dimensionless because it is precisely defined by the ratio of the outer scale by the inner scale. As such, it is valid for time and/or space, but presently used for time. This has been specified in the revised manuscript to avoid any ambiguity.