General assessment:

This is a rather low quality manuscript. Its two main weaknesses are 1) lack of novelty and 2) poor writing/structure. The first relates to the fact that UM cascade have been used to analyze/simulate rainfall time series for a long time, including extremes. Therefore, the contribution of the present paper remains unclear. In terms of writing, I would say that the paper is lengthy and overly complicated. The text contains many digressions on other, irrelevant issues that are outside the scope of the paper and distract the reader from the essential. Also, there is a lot of jargon, which makes the research sound more complicated than it really is. At the same time, lots of important practical details about the actual implementation of the cascade are missing, which makes it impossible to reproduce the work.

My recommendation: major revisions.

Major comments:

a) The abstract does not state/summarize the most important results. It is too long and misrepresents the scope of the paper.

> Suggestion: rewrite the entire abstract. Describe what this paper is about, highlighting the novelty and contribution. Be concise and clearly mention the main results.

b) The Introduction is too broad and contains irrelevant information. This paper is about the simulation of rainfall time series using discrete UM cascades. Therefore, I do not see any need to dwell on spatial models and space-time models. The part about the 8 characteristics of rainfall fields on lines 53-67 is not necessary for the understanding of the paper, and so are Table 1 and Fig 1. Most of these aspects never come back in the results part of the paper.

> Suggestion: tighten the scope of the introduction/methods by focusing on time series only. Instead of wandering off topic, include more relevant background information about the current weaknesses/strengths of rainfall time series models, including their ability to reproduce extremes and IDFs. Highlight what the knowledge gap(s) is/are and how the methods proposed in this paper address it/them.

c) Figure 1 and Table 1 are not necessary for the comprehension of the paper. The computational complexity never comes back and none of the other methods are implemented/used.

> Suggestion: remove/shorten them or consider adding other methods to compare against.

d) Fig 1 and Table 1 are deeply misleading. Within a given category, many different implementations/flavors have been proposed. The complexity and number of parameters vary a lot depending on which publication you consider.

> Suggestion: to make the comparison fair, you should refer to specific papers (e.g., authors + year + name of method) or give a range of values for multiple publications.

e) The random cascade model implemented in this paper uses 4 parameters and not 2 as claimed in Table 1. Therefore, it is not objectively more parsimonious than many of the other methods mentioned in the introduction. According to your own definition in Fig 1, the model would not be labeled as highly parsimonious.

> Suggestion: do not label models as highly parsimonious, etc. Focus on explaining the differences in approach, and how much of the original complexity can be reproduced with a

given set of parameters. Depending on the application, different characteristics will be important, such as extremes, mean, variance, autocorrelation, intermittency etc. Clearly explain which characteristics are the most important to you.

Note: actually, the number of parameters is 5, because you also need to count the scale break (which needs to be estimated from the data).

f) There is crucial information missing about how the cascade models are implemented, and how the time series are generated. Because of this, the research is impossible to reproduce.

Suggestion: restructure section 4. Consider creating more sub-sections in 4.1 to explain the different parts, from the simulation itself (using the Lévy random variables) to the renormalization. Provide a step-by-step description and mention the software packages/tools used. If possible, provide documented example codes.

g) The Results/Discussion part is too short and too shallow. The outcomes need to be discussed in more details. The scores are not enough to understand/interpret the results.

Suggestion: extend the Discussion part. Include more diagnostic plots and critically discuss the pros/cons. If possible, compare the outcomes to what is possible to achieve with another of the mentioned simulation techniques (not UM based).

h) Be more critical with respect to obtained results. While reading the paper, I got the impression that the authors were very quick at praising the UM cascade model and how amazing it is. However, UM cascade also come with limitations and the whole approach relies on some pretty strong assumptions which need to be discussed.

Suggestion: objectively report on what the method can/cannot do and critically discuss the assumptions it relies on.

i) explicitly state what you actually mean by seasonality. Different characteristics of the precipitation process may have different seasonal patterns. For example the wet/dry spell lengths, the average precipitation amounts or the extremes. In addition, you don't actually need the UM framework to assess seasonality.

Suggestion: clearly define what seasonality means in the context of this paper and use traditional metrics such as the coefficient of variation (or related) to quantify the observed/simulated seasonality. Check whether the UM cascade can reproduce these quantities.

j) lots of self-referencing: Out of the 71 references, at least 27 refer to work done by people in the same group as the co-authors. This is a lot and could be qualified as excessive self-citation.

Suggestion: check whether all these self-references are really needed.

Minor comments:

- Throughout the paper: avoid using too many parentheses at the end of your sentences. This gets annoying very quickly and is bad writing. Just add a new sentence or consider combining the two parts using a comma.

- The TM and DTM methods have already been explained in great detail in other studies. You could save space by not repeating the theory and referring to the relevant papers.

- ll.46-47: *"[…] however they do make some non-physical simplifying assumptions […]"*

  Which ones?

- ll.53-67: when mentioning the 8 properties, you should better distinguish actual properties (as seen in observations) from model properties.

- l.60: *"[…] for instance fields are not presumed to be additive"*

  Please explain what you mean by this. Are you referring to additive errors?

- ll.63-64: *"[…] extreme rainfall values are more frequent than usual resulting into strongly non-Gaussian statistics."*

  Nonsense. By definition, extreme values are less frequent than usual ones. Just say that the distributions are positively skewed, with long right tails.

- l.145 Equation 2: Why not give the general expression with the H?

- l.151: *"Larger the sample size, better will"*

  Gibberish

- ll.184-185: *"Generally, this could be due to two different issues: […]"*

  The most plausible issue should also be mentioned here: that the data are not really multi-fractal. In other words: the assumption itself should be questioned (on top of how the parameters are estimated).

- ll.207-208: "These low values of MCI justify the aforementioned selection procedure"

  Well, maybe. But without context, this number does not mean much. What is an acceptable value?

- ll.221-222: *"[…] a property respected even by the Navier-Stokes equation used by state-of-the-art NWP models for operational forecasting"*

  Please add a reference here.

- l.223: "[…] can be considered as a bridge between purely statistical and purely physical models"

  Nonsense. A bridge is what you use to cross from one side to another. Here, you just have a method that combines the properties of both worlds. But that does not make it a bridge and does not tell us how to go from the physical to the statistical world.

- l.236: "[…] and suitable amplitude [...]"

make this part more explicit by stating exactly how the Lévy variable is simulated. See major comment (f).

- l.351: “[…] it can be seen that they are somewhat similar to another.”

Too vague. Provide the absolute and relative differences. Actually, one could make the point that since the UM parameters for different sites with different rainfall properties are very similar, they do not really offer a great physical interpretation. Otherwise, one would be able to see the differences just by looking at the parameter values. Is this because small differences in parameter values can have large differences in terms of patterns? Please elaborate.

- ll.360-364: “[…] thereby confirming that the simulations have reasonably realistic seasonality features.”

I don’t think that you have presented enough evidence to conclude this. The simple, subjective comparison with some ratios close to 0 is very sketchy and some more in-depth analyses and diagnostic plots are necessary to convince me of the realism of seasonal features in the simulations.

- l.374: […] “~~physically~~ statistically realistic reference rainfall ensembles”

- ll.377-378: “[…] seems to be the most reliable comparison metric.”

Reliable is a strange word in this context. Did you mean robust? Or adequate?

- ll.388-391: This is a strange way to conclude a paper. This paragraph would fit better in the Introduction, to justify the UM cascade model.