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Interactive comment on "A simple weather generator for applications with limited data availability: TEmpotRain 1.0 for temperatures, extraterrestrial radiation, and potential evapotranspiration" *by* Gerrit Huibert de Rooij

Anonymous Referee #2

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This is a concisely written paper covering a suite of models, about which the right amount of detail is generally provided. I have some general comments first.

The rainfall model that the authors use is a variant of a well-established model, the Modified or Random Parameter Bartlett-Lewis Rectangular Pulse model. The authors refer to the first paper in which this model was developed (Rodriguez-Iturbe & al., 1988, hereafter RCI88) which followed upon a seminal paper by the same authors the previous year (Rodriguez-Iturbe & al., 1987) which marked a step change in the approach to rainfall modelling. Previously, modellers had applied a variety of stochastic models

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to the discrete time-series of hourly or daily rainfalls: these would however typically not perform well at other time-scales. The idea of modelling the unobserved continuous-time process in such a way that the statistics of the aggregation of this process to different time-scales could be derived analytically formed the basis the work in RCI88 in which the authors chose the Bartlett-Lewis point process as the basis of their new model, and this opened up a whole area of hydro-meteorological research into the use of Poisson-cluster processes (Bartlett-Lewis or Neyman-Scott) for rainfall modelling which is alive and well today.

On this topic, Reviewer 1 (R1) provides statistics of the number of papers and citations for the two well-known processes of that category, the Bartlett-Lewis and the Neyman-Scott. It is not clear what is being claimed with these figures, but there is a suggestion that comparisons of numbers of citations are reliable guides to the scientific quality of the papers. If that is the claim he is making, it is a self-defeating one: his review illustrates the (perfectly understandable) practice of reviewers drawing attention to their work which will then subsequently be cited by the authors in their revised paper. The number of citations therefore clearly depends on factors that are independent of scientific quality.

R1 argues that the authors should have included a reference to some paper using the Neyman-Scott process. This could indeed have been included by the authors, but in my view, it is not required here (and certainly not on grounds of historical precedence as I explained above). There are in fact many other approaches to rainfall modelling, aside from the very similar Neyman-Scott point process modelling approach: one could argue that these other approaches should have been included, had the authors carried out a proper review of approaches to rainfall modelling. Here, I'm thinking in particular of another approach that has a strong tradition extending as far back as the early development of Poisson-based approaches, but which differs in its fundamental philosophy. These are (multi-)fractal models (Schertzer and Lovejoy, 1987), typically random cascades in which the (multi-)scaling properties of the observed rainfall signal

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are modelled explicitly. Within that broad category there are also a range of options whose differences between them, and from the Point process approach, are more scientifically interesting than the minor difference between Neyman-Scott and Bartlett-Lewis process approaches. These are the issues of whether one should use bounded or unbounded cascades, macro-canonical or micro-canonical cascades (Menabde & Sivapalan, 2000), and about whether claims of universality for a certain type of multi-fractal approach are substantiated (Schertzer and Lovejoy, 1997). This approach to modelling is equally alive and well today (see Raut et al., 2018, and references therein – this is specifically a space-time model but the methodology is applicable to a purely temporal model). The question now is: should the authors have carried out such a proper review of stochastic rainfall modelling? I would argue that this paper is not the place to do that, given that the rainfall model is but a component of a larger modelling strategy and that it is this combination of models is what is of importance and arguably novel here.

I say 'arguably' because it is important to flag the following: this approach is not novel in its outline at least, since the idea of modelling rainfall, then using the generated rainfall to model temperature and potential evaporation is at the heart of a well-known approach that is the UKCP09 weather generator (Kilsby et al., 2007). On this, I fully concur with R1's comment: a reference to this work is essential. The question then is whether the proposed suite of models under review is still sufficiently novel to warrant publication.

I have looked into the details of the UKCP09 weather generator to compare it with the method in the paper under review. I note the following:

(1) The rainfall generators are similar insofar as Neyman-Scott and Bartlett-Lewis processes are largely equivalent. But of course, there are different types of models under each heading whose mutual differences are often greater than those between these two approaches. It is therefore of interest to see whether the model chosen on the basis of a recent paper by Pham et al. (2013) has something to offer, e.g. with re-

spect to extreme value reproduction which is often a problem for long return periods. This by itself, however, is not sufficiently novel as the Neyman-Scott approach use in Kilsby et al. (2007, hereafter K07) appears to perform well in terms of reproducing daily extremes (at least at the locations for which results are shown).

(2) As regards the temperature model, K07 uses AR(1) time-series models, after removing seasonality by normalising the temperatures (using means and standard deviations of half-monthly periods), for both the mean daily temperature and the temperature range. There are different models depending on whether we are considering two consecutive wet days, two consecutive dry days or wet-dry day transitions. All variables are assumed normal. In the paper under review, the seasonal trend is modelled parametrically, using a sinusoidal shape with parameters depending on whether the day is overcast or not. The stationary signal is then also modelled using an AR(1), with one such model for a clear day and one for an overcast day. The probability of an overcast or clear day is then dependent upon the rainfall amount (using a staircase function) The temperature range is modelled by a log-normal variable. So, here, there are similarities insofar as an AR(1) model is involved for the mean temperature, but the way this is used and the way the range is represented differ. The scheme used for the temperature range in particular seems to be important for the Potential Evapotranspiration (PET) through the extremes it generates (see lines 449-451), so the fact that it is log-normal here rather than normal as in K07 is likely to make a significant difference.

(3) Vapour pressure, sunshine duration and wind speed are then generated using linear regressions upon daily rainfall, mean temperature, temperature range and one another (this is a multi-variate regression) in K07. From this, the PET is derived using a version of the Penman formula. In the paper under review, a modified Hargreaves formula was preferred (and the authors explain why). The extra-terrestrial radiation required in that formula is then obtained using work published in 2013, so postdating K07. Here, there are no similarities in the methods.

On the basis of this analysis, I think that the detail of the combined model in the paper under review is sufficiently distinct to be considered as a separate multi-model, although, as said above, it is based upon the same broad modelling idea of starting with the rainfall and conditioning the other variables upon it. It is therefore scientifically interesting to see how this different implementation of the same general approach performs (particularly given the significant differences in the PET scheme). Of course, and again I agree with R1 on this point, this calls for a comparison of the two implementations, but such a comparison cannot be required in this paper which contains enough material as it is.

Looking into the detail of the paper, I have the following comments:

LINE - COMMENT

118 The sentence is odd: 'other models (. . .) found that (. . .) models work well'. I think the first 'models' should be 'modellers'.

165 Formulae 6a and 6b contain products sin(xi) sin(xi) for cosâĄą and tan. These should be written as squares or is this a typo? Please check

210 It is not clear to the reader what 'adequate' might mean at this point, so a pointer to the further explanations in the paper would be helpful.

228 The authors specify that the model can be used to generate data for leap years. However, formulae 6a and 6c would seem to apply to non-leap years only. Please clarify.

230-250 These are issues of detail (e.g. how to generate random numbers from an exponential distribution) which could be moved to an appendix.

255-300 The procedure for selecting model parameters seems to involve a lot of choices, as the many 'set. . .' statements indicate. It seems that the idea here is to move away from a systematic calibration of the rainfall model, probably because of the difficulty of obtaining convergence of numerical optimisation schemes reported in the cited papers. But is it the case that the average non-rainfall specialist will have a clue as to how to set the required numbers to sensible values. The impression here is that too much is left to the expertise of the user. What about some guidelines as to what kind of values have been found reasonable by the authors? It may be the case that the information provided in the supplement and referred to in lines 352-355 addresses this. Please comment.

319-329 Here in the case of the temperature, useful guidance is provided for the user, so, referring back to my previous comment, the lack of it for the rainfall is all the more noticeable.

359-361 The reader will be somewhat unclear as to how rainfall parameters for the Sahara have been produced (i.e. the numbers in table 4, line 720), apparently without any data (?) This point is connected to the two previous ones. Please provide further explanations/guidelines here.

Finally, although the authors show many results of running the model suite. I am not clear overall, to what extent the model has been validated. There are comparisons between locations and seasons and comments about how the model produces what one might expect, but to what extent has the model been validated? This needs to be made clearer.

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