We thank Dr Dongkyun Kim for his careful reading of the paper and his perceptive comments.

Here are some responses to the points made by Dr Kim.

(1) Dr Kim first points out that the model equations suggest that, when parameter α is reduced, the tail of the distribution of cell intensities becomes thinner, which will reduce the estimates of extreme values for given return periods. This in turn should therefore

improve the model's fitting ability to rainfall characteristics with "more regular" behavior.

Ans: First, we would like to point out that the variance of η is rather: $Var(\eta) = \frac{\alpha}{\nu^2}$. This does not, however impact the point made by the referee, which is an interesting one: if α is smaller, the variance of the distribution of the mean cell intensity in a storm, namely μ_X, is decreased, and this Gamma distribution will therefore have a thinner tail. This might indeed mean that the model is better designed to reproduce the 'regular behaviour' of rainfall.

This would also however seem to imply that it performs less well in terms of extremes, which is not the case. I think that it is probably difficult to draw conclusions insofar as the value of parameter ν also changes with the value of α.

(2) Dr Kim, who has access to the same data set we used in the paper, points to an apparent discrepancy in the observed annual maxima.

The observed annual maxima shown in Figure 11 and Figure 12 seems to be lower than the actual value. According to my calculation, the observed annual maximum of daily rainfall goes upto 90+ mm while the values shown in the figure goes upto only 70mm. I guess this discrepancy came from the way to estimate the annual maxima. In my case, I ran the moving window of a given aggregation interval throughout the 5-minute timeseries over one year to get the maximum value, while the authors aggregated first and then took the maximum

Ans: As the referee indicates, this discrepancy is due to the fact that we have considered daily maxima, while he has been working with 24-hour (moving-window) maxima. We chose not to use moving-window aggregation in order to be consistent with that in Kaczmarska et al. (2014), where the same Bochum dataset was used (see Figure 8 in Kaczmarska et al. (2014)). It is interesting though to note the discrepancy that is obtained, and in principle, we could also include a figure in which we compare the 24-hour extremes from observations and model simulations. Given the fact that the paper is already rather long, and that some additional material will need to be included to address other referee comments, we opt for not including this. In so doing, we are following standard practice.

(3) Dr Kim raises a very important point about the proportions of dry periods, about which we have not said anything in the paper.

The parameter estimation process does not seem to have considered rainfall intermittency (e.g. equations for proportion of dry/wet period). If you put the parameter values of Table 4 for the equation of proportion of dry period, the value is almost 0, which means it rains all the time

Ans: It is indeed correct that the proportions of dry periods (proportions dry) have not been included in the model calibration. That makes them prime candidates for model validation according to the standard practice of stochastic model validation: this involves distinguishing between properties used in the calibration and properties used in validation (rather than splitting the observed data set into a calibration and a validation period).

The proportion of wet periods plots the referee shows in his review indicate some substantial overestimation of the proportions of wet periods, i.e. an underestimation of the proportion dry, by the model over a range of time-scales. This is in fact an issue that we had noted in carrying out model simulations and that can be discussed in both theoretical and sampling aspects.

According to the theoretical form of the proportion dry given in Rodriguez-Iturbe et al (1988) (see equation (2.5)) and its approximation given in Wheater et al. (2006) (see equation (B48) at page 412), we note that the constraint for α is $\alpha > 1$. The constraint for α is however $\alpha > 0$ in the new RBL2-sM-NC model, and, as summarised in Table 4 in the manuscript, the α values we obtained are mostly smaller than or very close to 1. Therefore, the theoretical proportion dry can hardly be derived using the approximate equation given in Wheater et al. (2006).

This issue can however be better addressed through sampling. We had found that the underestimation of the proportion dry is due to the generation of many tiny amounts of rainfall which are not significant for any hydrological application. If we therefore look rather at the proportion of near-dry periods (with rainfall below a small threshold of 0.01 mm per 5-min) the problem disappears at hourly and sub-hourly scales. A comparison is given in Figure 1 of proportion dry statistics derived from 250 simulations of RBL2-sM-NC, RBL2-sM and RBL2-bM models, respectively. As can be seen, the new RBL2-sM-NC can better reproduce proportion dry statistics at 5-min and 1-h timescales than RBL2-sM and RBL2-bM models. However, the RBL2-bM model start to outperform the other two models at multi-hour timescales.

Considering that the paper is already rather long, and we do not use proportion dry for model calibration, we opt for not including this in the main paper. However, we will add this in the supplement.

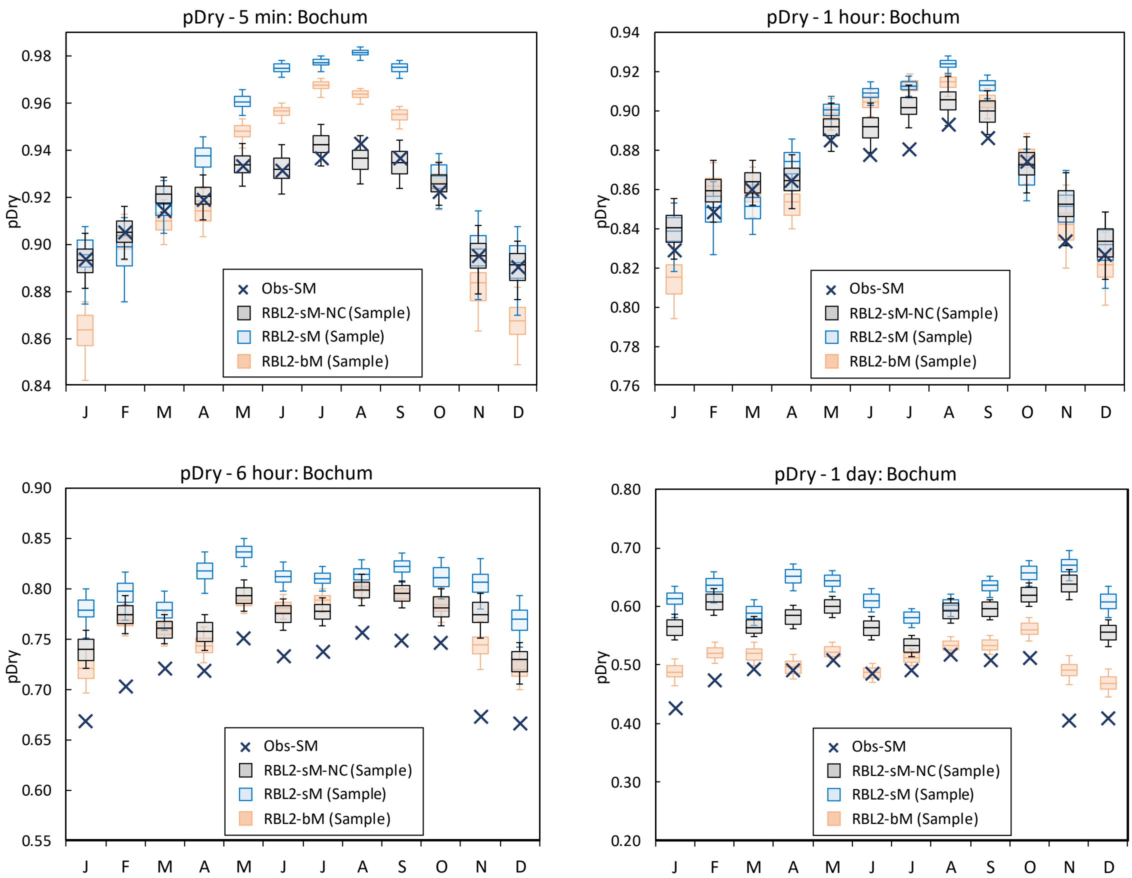

Figure 1: Proportion dry by month at Bochum: the observed vs. the fitted using RBL2 models with the original and the new solution spaces of α (RBL2-bM, light orange boxplots; RBL2-sM, light blue boxplots; RBL2-sM-NC, black boxplots).

(4) Dr Kim suggests specifying parameter units and objective function values in the tables.

Please specify the unit of the parameters in the tables. Especially, the parameter iota in the paper confused me because the original Bochum data is in the unit of cm and your iota is in the unit of mm. It may be also beneficial if you add the column of the objective function values in the tables for the reader's reference.

Ans: We thank Dr Kim for this suggestion. The units of the parameters will be added to the tables. In addition, a new table (similar to the one below) will be added, summarising the minimum objective function values.

Note that the minimum objective function values of the RBL2-bM model in Kaczmarska et al. (2014) (see Table 2) are given here (in grey font colour). This is to demonstrate that the minimum objective function values obtained in our work are similar to those obtained in the previous research. It is also worth noting that the minimum objective function values of the RBL2-sM-NC model are much lower than those of the RBL2-sM model.

Model	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
RBL1-bM	85.6	66.8	89.4	93.3	127.9	105.8	107.6	126.6	114.2	92.1	102.9	83.8
RBL2-bM	39.5	30.1	52.1	56.2	73.0	65.2	65.6	72.8	60.4	47.0	41.0	36.6
RBL2-bM*	39	22	46	63	74	76	92	74	68	47	23	26
RBL1-sM	227.5	176.7	192.1	169.1	221.9	328.5	180.3	620.3	323.9	110.1	280.4	410.0
RBL2-sM	145.0	76.7	117.6	173.6	174.3	315.6	96.5	478.4	241.6	61.2	244.6	280.5
RBL1-sM-NC	186.5	169.9	192.0	149.4	221.9	328.5	180.3	620.3	323.9	107.5	104.0	348.8
RBL2-sM-NC	37.4	23.7	75.7	60.9	43.7	59.1	8.2	32.9	8.5	32.4	109.2	142.6

* The minimum objective function values are obtained from Table 2 in Kaczmarska et al. (2014)

(5) Dr Kim indicates that using another numerical method, better parameters can be obtained.

I could estimate the better parameter values with the particle swarm optimization algorithm (less underestimation of variance and skewness, and the P0 aligning to 1:1 line)

Ans: Again, we thank Dr Kim for his work in reviewing this paper, which amounts to a very thorough and useful investigation. There are two issues here. First, as Dr Kim points out, the objective functions obtained when calibrating these Poisson-cluster rectangular pulse models are highly non-linear and are likely to have many local optima. He is therefore right to point out that non-traditional numerical methods such as Particle Swarm optimisation are likely to be very useful to avoid an iterative algorithm converging to a non-global local optimum. Second, however, the statistics used in the fitting by Dr Kim are different from the ones we used – and in particular, they are likely to include the proportion dry. Aside from the issue of improved reproduction of the proportion dry, we would have to look at whether the other statistics are significantly improved.

Based upon Dr Kim's results, we agree that, in this case, Particle Swarm optimisation is a better solver than our numerical method that, as described in the paper, combines the Simulated Annealing and the downhill simplex Nelder-Mead algorithms. We are keen to try the Particle Swarm optimisation method in our future work. However, we would like to highlight that the main contribution of this paper is the re-investigation of the key parameter constraint for the RBL models and the associated new formulation; and we believe that the impact of this change in preserving sub-hourly extreme statistics is likely to be more significant than that resulting from a better numerical solver.

(6) Dr Kim raises an interesting point about the fact that there seems to be a difference in the role that the proportion dry and the proportion wet would play in the objective function, when we use the weights that are recommended for these generalised methods of moments.

Let's say that we consider the proportion of dry period (P0) in the calibration process. The interannual variability of P0 will be very small because it is one minus small value every year (e.g. 0.998, 0.980, 0.950, etc.). Therefore, it will have very high weight. Let's say we consider the proportion of wet period (PW) in the calibration process. The interannual variability will be greater than the first case (e.g. 0.002, 0.020, 0.050, etc.)

Ans: I think there is a misconception here in thinking that the variability of the proportion dry will be different from that of the proportion wet. Indeed, we always have $Var(1-X) = Var(X)$ so if X is the proportion dry, then $1-X$ is the proportion wet, and they both have the same variance, and will therefore be given the same weight in the objective function.

(7) Dr Kim makes a point about the block estimates

Regarding the block estimation, the mean of the block values are the estimates of the true statistics, which we can get easily, so I think the parameter estimation should always be performed based on the true statistics

Ans: We agree with the last part of the statement if 'true statistics' means 'best available estimates of the population statistics' in some sense of best (probably including non-biased, maybe also with minimal variance). But the block estimation method introduces some bias for instance.

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