Reply to anonymous referee #3

February 29, 2012

1 General comments

Reviewer comment: This paper contributes to the problem of fitting stochastic point process models to data. These models are important in a range of hydrological applications and so the paper would be worth publishing after suitable revisions. In general, the paper fits in with the scope of HESS, although the paper is long and could be significantly shortened (in my view). For example, some of the technical details on the optimisation algorithms might be omitted, as these are presumably available elsewhere and also of less interest to the hydrologist than the rest of the paper.

Reply: Since the paper mainly focuses on the comparison of various optimization algorithms, it seems appropriate (to our opinion) to elaborate on the technical details of these algorithms. The comprehension of the ins and outs of these algorithms contributes to the interpretation of the results. Furthermore, the algorithms are optimized before they are applied to the MBL model. This section requires that the reader is aware of the meaning of the optimized algorithm parameters, so it would seem inappropriate not to discuss them in advance.

Reviewer comment: The paper is on various fitting methods for the MBL model, and yet there is little (or no) comparison of the actual parameter estimates obtained. At least the final estimates for the MBL model using the different methods should be given in a table. Do the methods give similar final parameter estimates? Also, in this sort of study, surely it would be more appropriate to use known parameter values a priori and then see how well the different methods recover these?

Reply: The final estimates for the MBL model will be added to the paper. Also, in reply to comments made by Chris Onof, a section will be added where the ability of the algorithms to recover known parameter values is assessed.

Reviewer comment: Given that the paper is on comparing different fitting procedures, some which are designed to handle local optima, surely the constraints on the parameter estimates should be much wider than those in Table 1? Also, because the estimates are not given (re point A above), the reader cannot know whether they are close (or on) the bounds in Table 1. According to the authors, the simplex method is judged inadequate because it produces estimates on these artificial bounds. But given that the bounds are relatively tight this seems an unjustified conclusion. Furthermore, since the underlying parameter values are not known (point A above), we cannot deduce that this method is failing.

Reply: We fully agree with the fact that the location of the optimum is unknown and that parameter boundaries must be chosen wide enough to diminish the odds of the optimum being outside the boundaries. However, to our opinion, the boundaries are chosen large enough. The parameter space was constrained based on physical considerations. For example, λ, which is the parameter for the storm arrivals (according to a Poisson process), ranges between 0 and 0.1. So, at the upper boundary, the mean interarrival time between storms is equal to 10 hours. κ

and ϕ, which can be related to the average number of cells per storm (see Introduction) are also chosen sufficiently wide. Figure 1 shows the average number of cells per storm in function of κ and ϕ. Similarly, α and ν can be related to the average cell duration, which is shown Figure 2. Taking this into consideration, it seems unlikely that the optimum would be outside the preset boundaries. So, the reason why the DSM converges on a boundary is most likely caused by instability of the simplex at these boundaries in combination with a randomly chosen starting point near the outer boundaries. To surpass this, and in response to comments by anonymous referee # 1, the DSM with single starting point is replaced by the DSM with 30 starting points. When such DSM with multiple starting points is used, these issues do not present themselves and the performance of the DSM can be judged more fairly.

2 Specific comments

1. *Reviewer comment: Abstract - This could be shortened. For example, the first two sentences are essentially "motivational" and not really needed in an abstract. My suggestion would be to state the objective first followed by the proposed solution etc. Also, ".. widely acknowledged" may be acceptable in an abstract but this should be backed up in the introduction. On the "issue of subjectivity" - can it be assumed that readers will know what this is? Also, is it necessary to introduce multiple acronyms for the different fitting procedures into the abstract?*

   Reply: The abstract will be adjusted as follows:

   "The calibration of stochastic point process rainfall models, such as the Bartlett-Lewis type of model, suffers from the presence of multiple local minima which local search algorithms usually fail to avoid. To meet this shortcoming, four relatively new global optimization methods are presented and tested for their abilities to calibrate the Modified Bartlett-Lewis Model. The list of tested methods consists of: the Downhill Simplex Method, Simplex-Simulated Annealing, Particle Swarm Optimization and Shuffled Complex Evolution. The parameters of these algorithms are first optimized to ensure optimal performance, after which they are used for calibration of the MBL model. Furthermore, this paper addresses the choice of weights in the objective function. Three alternative weighing methods are compared to determine whether or not simulation results (obtained after calibration with the best optimization method) are influenced by the choice of weights."

2. *Reviewer comment: l5, 9709: The Neyman & Scott (1958) reference does not seem particularly relevant or useful here, since it is not on rainfall but on spatial modelling of galaxies.*

   Reply: This reference will be replaced by Kavvas and Delleur (1981).

3. *Reviewer comment: l.5-10. The distinction between the NS and BL models can be made in the third moment and proportion dry but not in the second order properties. Hence, empirical results for the two models are expected to be very similar; with the possible exception that the NS model may generate marginally more extreme values due to cell overlap since the distribution of cell origins after a storm origin is not uniform. The paragraph could be strengthened to make the point that the results may be applicable to both models (which could also be mentioned in the conclusions).*

   Reply: This will be adjusted as suggested.

4. *Reviewer comment: l1, 9709: probably you mean "point process rainfall" rather than "point rainfall"? Are they based on the "generation of rectangular pulses"? Basically, they are "marked point processes" (e.g. see Cox and Isham, 1980).*

   Reply: Indeed, this should be "point process rainfall" in stead of "point rainfall". The sentence will be rephrased as: "... use of stochastic rainfall models dates back ...".

5. *Reviewer comment: l23-30: Unless you are referring to simulated data, in the model "storm arrivals" occur in a Poisson process (not "generated by").*

   Reply: This refers to the model and should indeed be adjusted.

6. *Reviewer comment: 9710, l2: This last sentence needs correcting. What do you mean by superposition here? It is the aggregation of the continuous time stochastic process that results in a discrete rainfall time series.*

   Reply: What is meant here is that the final continuous time series (before aggregation) results from the superposition of the generated cells. Basically, at any given point in time, the modelled rainfall depth is equal to the sum of the depths of the active cells at that moment.

7. *Reviewer comment: 9710, l3-7. Should be "observed sample properties" (not just "moments", since autocorrelation and proportion dry may be used) of rainfall "depth" (not "intensity"). Also, should be the model is fitted to the data (rather than the other way, i.e. the observed properties to "those obtained by the model").*

   Reply: This will be adjusted in the paper.

8. *Reviewer comment: 9710 (second paragraph): Too much detailed information for an introduction.*

   Reply: The detailed description of the model will be moved to a separate section.

9. *Reviewer comment: 9710, 27-9: "... suffer from a few shortcomings". Obviously, models do not always fit the data well, but the point is usually whether they fit well enough for the intended application. For the same reason, I am not sure the MBL model is "flawed", so I suggest you reword (or delete) this paragraph.*

   Reply: This paragraph will be omitted.

10. *Reviewer comment: 9711, last three paragraphs. Again some rewording is needed.*

    Reply: These paragraphs will be adjusted as follows:

    "The calibration of the BL models has proven to be a cumbersome task because of the presence of multiple local minima (Verhoest et al., 1997). Traditional local search techniques sometimes fail to avoid these local minima, resulting in a suboptimal solution to the optimization problem.

Secondly, the calibration result is influenced by the choice of weights in the objective function. Different approaches exist, but it is not clear which leads to better simulation results. To address these issues, this paper proposes to use relatively new optimization methods as they are expected to be more robust than more traditional local search methods. Furthermore, three different approaches to the weighing of the objective function are compared in order to shed some light on their advantages and disadvantages in terms of model performance and practicality. For these purposes, data recorded at the Uccle-site of the Royal Meteorological Institute (RMI) in Brussels (Belgium), are used. The data set consists of 105 years of recorded rainfall at an aggregation level of 10 min (De Jongh et al., 2006)."

11. *Reviewer comment: 9711, discussion of GMM: As for comment for 9710,l3-7 above, not just moments but other sample properties are used.*

    Reply: This will be adjusted.

12. *Reviewer comment: 9712, 10-14: W is a matrix of weights (not "weighing matrix"). The value of f as a measure of "fitness" is for the parameter estimates at the solution (not the parameters x).*

    Reply: The matrix W will be referred to as the matrix of weights.
    f is the objective function, which can be calculated for any given x, hence it reflects the fitness of any given possible solution x to the optimization problem, not only the final parameter estimate. This sentence will however be rephrased to avoid misinterpretation.

13. *Reviewer comment: 9712, 15-19. Again, the same problem - only the first two are "moments".*

    Reply: This will be adjusted.

14. *Reviewer comment: l21 "[" correct to "]". Also, you say alpha must be greater than zero and then set the bound to alpha > 1. Why not just set alpha > 0?*

    Reply: "[" will be corrected to "]". As for alpha, this was already mentioned earlier: "For the expected cell lifetime to be finite, *alpha* > 1" (See Introduction, p. 9710 l. 17). This approach was adopted directly from Rodriguez-Iturbe et al. (1988).

15. *Reviewer comment: 9713, l1-4 and Table 1. "... parameters can still take a wide range of feasible values ..". The bounds used in Table 1 are not especially "wide". Given that the paper is addressing the problem of local optima, such a priori restrictions on the parameter space implies the that results in the paper may not be conclusive or widely applicable, since at other locations there may be more than 15 cells per storm on average, and this is used as an upper bound in the table.*

    Reply: This issue has been addressed earlier. Some confusion has arisen due to an error in Table 1, the fourth parameter of the model is not μ_c but μ_x, this was indicated wrongly.

16. *Reviewer comment: 9714-9723 (Section 3). This section could be shortened, because it is more generic to optimisation than to hydrology (and HESS).*

    Reply: see previous comment.

17. *Reviewer comment: 9723-9728 (Section 4). The use of a penalty term in the objective function at "infeasible points" (e.g. 9725, l6-7) is a cause for concern. As mentioned above, some of these points may not be "infeasible". On the other hand, if they are "infeasible" then the use of an appropriate constrained optimization procedure, rather than an arbitrary penalty function, might be preferable. For example, we know the parameters have to exceed zero, so why not optimize against the log of the parameter? Similar transformations (e.g. the logistic function) can be used for other types of constraints, without the need of an arbitrary penalty function.*

    Reply: We fully agree that a log-transformation would be perfect for this specific situation. However, by not applying such transformation the article shows that these methods might be applicable to various kinds of optimization problems, and that they do not require a tremendous amount of expertise to operate. The fact that an algorithm is easily understood and fairly easy can be considered as an advantage.

18. *Reviewer comment: 9729, l16. "..., it seems that the DSM does not handle the constrained parameter space very well". Related to the previous comments, this may not be a fault in the DSM method: the constraints on the parameters may be too narrow and perhaps instead the results from the DSM method are an indication of this?*

    Reply: See previous comments.

19. *Reviewer comment: 9780, l5-7: Again boundary difficulties noted for DSM, but this argument is unconvincing.*

    Reply: See previous comments.

20. *Reviewer comment: 9744, Table 3. Check journal standard for scientific notation (usually journals do not accept the "E" notation for powers).*

    Reply: This will be corrected.

21. *Reviewer comment: 9746, Table 5. As above, except such very small values, e.g. 1E-241, are better labelled as zero.*

    Reply: This will also be adjusted.

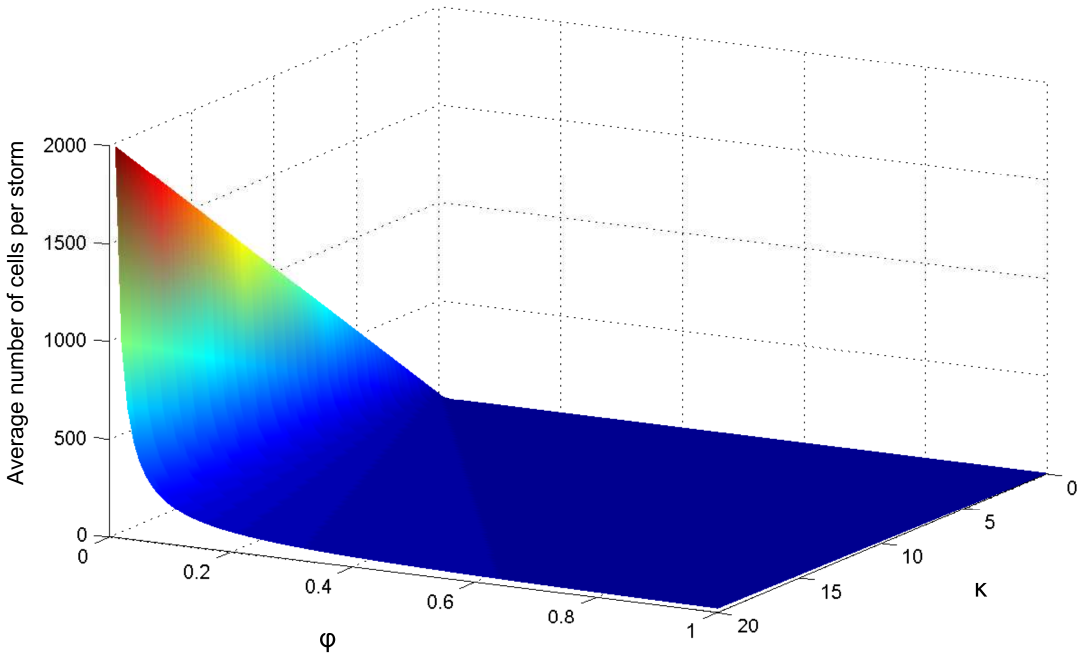

Figure 1: Average number of cells per storm in function of parameters κ and ϕ.

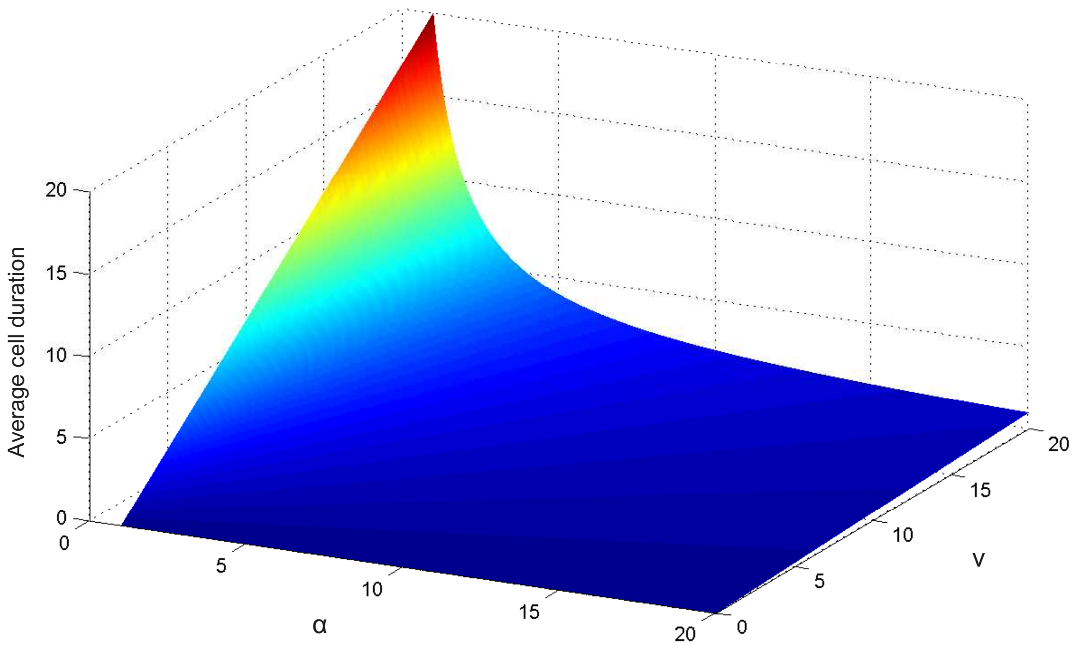

Figure 2: Average cell duration in function of parameters α and ν.

Bibliography

Kavvas, M. L. and Delleur, J. W.: A stochastic cluster model of daily rainfall sequences, Water Resources Research, 17, 1151–1160, 1981.

Rodriguez-Iturbe, I., Cox, D. R., and Isham, V.: A point process model for rainfall: further developments, Proceedings of the Royal Society of London Series A-Mathematical Physical and Engineering Sciences, 417, 283–298, 1988.