

Interactive comment on “Calibration of the modified Bartlett-Lewis model using global optimization techniques and alternative objective functions” by W. J. Vanhaute et al.

Anonymous Referee #3

Received and published: 10 February 2012

1 General comments

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.

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Two main points, both relating to the model parameter estimates, that I think need to be considered in a revision are:

1. 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?
2. 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.

In addition, to the above comments, some more detailed comments are given below to help towards a suitable revision.

2 Specific comments

1. 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,

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".. 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?

2. I5, 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.
3. I.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).
4. I1, 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).
5. I23-30: Unless you are referring to simulated data, in the model "storm arrivals" occur in a Poisson process (not "generated by").
6. 9710, I2: 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.
7. 9710, I3-7. 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").

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8. 9710 (second paragraph): Too much detailed information for an introduction.
9. 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.
10. 9711, last three paragraphs. Again some rewording is needed.
11. 9711, discussion of GMM: As for comment for 9710,I3-7 above, not just moments but other sample properties are used.
12. 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).
13. 9712, 15-19. Again, the same problem – only the first two are "moments".
14. I21 "[I" correct to "J)". Also, you say alpha must be greater than zero and then set the bound to $\alpha > 1$. Why not just set $\alpha > 0$?
15. 9713, I1-4 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.
16. 9714-9723 (Section 3). This section could be shortened, because it is more generic to optimisation than to hydrology (and HESS).
17. 9723-9728 (Section 4). The use of a penalty term in the objective function at "infeasible points" (e.g. 9725, I6-7) is a cause for concern. As mentioned above,

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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.

18. 9729, I16. "..., 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?
19. 9780, I5-7: Again boundary difficulties noted for DSM, but this argument is unconvincing.
20. 9744, Table 3. Check journal standard for scientific notation (usually journals do not accept the "E" notation for powers).
21. 9746, Table 5. As above, except such very small values, e.g. 1E-241, are better labelled as zero.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 9707, 2011.