Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-406-RC3, 2019
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Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Modelling rainfall with a Bartlett–Lewis process: New developments" *by* Christian Onof and Li-Pen Wang

Anonymous Referee #3

Received and published: 25 September 2019

The stated contribution of this paper is twofold: first, to clarify an issue regarding the range of acceptable parameters for a class of stochastic rainfall models that has been in widespread use for some 30 years; and second, to highlight a potential problem with the calibration of these models due to the way that empirical properties are calculated from precipitation data. It is, arguably, quite a specialised topic and the mathematical presentation is quite dense in places, although this on its own should not prevent publication.

Unfortunately however, as far as I can tell the authors' arguments in support of *both* of their main points are flawed. If I understand correctly, their first point can be paraphrased as "there is a potential problem with previous mathematical derivations for these models" (lines 157–158) "so that earlier reported results may be incorrect" (lines

178–181)", but after doing some lengthy mathematics there isn't a problem after all" (e.g. the expression at line 210). Moreover, I have checked the mathematics for their "RBL1" model and found that in fact the 'established' derivation is fine: it seems that the present authors have actually *introduced* the non-problem due to their own approach to the derivation, which is in itself questionable. I haven't checked the other derivations in the present paper: however, the apparent errors for the RBL1 model cast doubt on the credibility of the other results. I also think that the arguments can be simplified substantially.

The second issue, relating to calculation of empirical properties, again seems to be due to an error on the authors' part — or, at least, to their use of non-standard procedures for their empirical calculations. I agree with them that the expressions they're using should not be used, but I am quite surprised and worried to discover that anybody is using them at all.

More detailed justification follows.

Mathematical derivations

The authors' concerns, about potential problems with previous mathematical derivations, are centred around the convergence of integrals that appear in the derivations. However, the original proponents of the RBL1 model (Rodriguez-Iturbe et al. 1988) did not use these integrals in their own derivations. In fact, the present authors appear to have made an error in lines 151–152 where they claim that moments for the model are obtained by multiplying the corresponding expressions for the OBL model by the density of $\Gamma(\alpha, 1/\nu)$ and integrating. This would be correct if there was a single value of η for an entire realisation of the process. In the model structure however, each storm has a different value of η. In Rodriguez-Iturbe et al. (1988), the derivation of the variance and covariances does *not* make use of separate integrals as claimed in the present paper: it just uses the expectation of $\exp(-\eta\phi\tau)/\eta$ where τ is a temporal lag;

and (correctly) notes that this expectation exists only when $\alpha > 1$. It therefore looks to me as though the apparent problem noted by the present authors may be an artefact of an incorrect — or, perhaps, needlessly complicated — approach to the derivation.

Apart from the error in lines 151–152, the authors' reporting of previous results with "non-valid" estimates for the α parameter (lines 178–181) should have made them stop and think more carefully. The reason is that the model fits are obtained by minimising an expression involving the theoretical model properties. Earlier authors must have calculated the properties for these values of α, therefore; but this wouldn't be possible if the integrals diverged (or the algebraic expressions would have produced results that are obviously wrong, such as negative values of E(X^2)).

Although I haven't checked *all* of the derivations in the present paper, I suggested above that the arguments could probably be simplified. This is potentially relevant to properties such as the third moment and skewness, which were not presented by Rodriguez-Iturbe et al. (1988) and may indeed require the evaluation of numerous integrals as claimed by the present authors. In view of this, it may be helpful to note that the 'problematic' integrals are all of the form $T(k, u, l)$ in the authors' notation as defined in their equation (5):

$$T(k,u,l) = \frac{\nu^\alpha}{(\nu+u)^\alpha}\frac{\Gamma(\alpha-k, l(\nu+u))}{\Gamma(\alpha)} \ ,$$

where the last term is the ratio of an incomplete to a complete gamma function. The authors' concerns about convergence are all focused on the situation where $l = 0$, because this is where the integrand can become infinite. In this case however, the final numerator in the expression above is a *complete* gamma function so that the expression can be written as

$$T(k,u,l) = \frac{\nu^\alpha}{(\nu+u)^\alpha}\frac{\Gamma(\alpha-k)}{\Gamma(\alpha)} \ .$$

But if $k > 0$ is an integer (which I think it is throughout the paper), we have $\Gamma(\alpha)/\Gamma(\alpha - k) = (\alpha-1)(\alpha-2)\ldots(\alpha-k)$ *providing* $\alpha - k$ isn't a negative integer (if it is, then $\Gamma(\alpha - k)$ is undefined). Thus

$$T(k,u,l) = \frac{\nu^\alpha}{(\nu+u)^\alpha(\alpha-1)\ldots(\alpha-k)} \ ,$$

which is obviously finite providing none of the terms in the denominator is zero. Unless I've missed something, this seems to resolve the convergence issue much more simply.

Calculation of empirical properties

The authors' second main point relates to the calculation of "block" statistics used for model calibration with uncertainty. They claim that the block estimators of variances and other quantities are biased (e.g. lines 257-260). However, the expressions that they give for these estimators are incorrect because there is no adjustment for degrees of freedom in the denominator in either case: the denominator in the first expression should be $N_y N_{m,h} - 1$ and that in the second expression should be $N_y(N_{m,h} - 1)$. In fact, Section 5.1 of their Jesus and Chandler (2011) reference (cited on line 244) discusses the need for careful treatment of small-sample biases: that discussion would probably be relevant to quantities such as the skewness coefficient, discussed by the present authors at lines 291-294. Jesus & Chandler did not discuss the variance specifically: I assume that this is because the form of an unbiased variance estimator is well-known so they didn't think it needed mentioning. If the variance expressions given by the authors are indeed in standard use, this is worrying: a decent journal is probably not the best place to draw attention to such a basic error, however. The bottom line is that there isn't necessarily a problem with block estimators *per se*; but (as with any other sophisticated technique) if you're going to use them then you need to do it carefully.

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