Hydrol. Earth Syst. Sci. Discuss., 10, C4660–C4663, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C4660/2013/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "A copula-based assessment of Bartlett–Lewis type of rainfall models for preserving drought statistics" *by* M. T. Pham et al.

M. T. Pham et al.

minhtu83@gmail.com

Received and published: 5 September 2013

0.1 Response to general comments

We thank dr. Evin for his positive feedback on our paper. The suggestion that the Poison process may not be adequate for simulating drought spells may indeed be an additional reason. However we believe that the interplay with other processes is also of importance. However, changing the BL structures with another distribution will require the development of a new model and corresponding analytical expressions for the different moments, which is out of the scope of this paper.

0.2 Response to additional comments:

1. The title seems to put the emphasis on the use of copula for this study. I do not really find that application of copulas is a central point of the paper. Conversely, the analysis of drought properties is. I would suggest removing 'copula-based' from the title or rearrange the title.

We agree with your comment and will change the title into: An assessment of Bartlett-Lewis type of rainfall models for preserving drought statistics.

2. P. 7471, l.9: similar statistics as -> similar statistics to

We will correct it in the revised manuscript.

3. P. 7472: Only one time series is simulated for each version of the Bartlett-Lewis model. Why not simulating, say, 100 time series in order to assess the variability of the statistical properties. This would avoid also obtaining bad or good results just by 'chance'.

We agree that stochasticity in the time series may have an impact on the final results. There are too reasons why we prefer not to include such analysis: (1) the time series generated all well preserve the "classical statistics" which proves that the model is well calibrated. Furthermore the time series generated is quite long also accounts for letting some stochastic variability pass. (2) It will become very difficult to compare models as each model is represented by several time series. A decent statistical analysis could require that all models are compared to each other which will result in a tremendous number of analysis results, thus making it difficult to draw clear conclusions. However, the suggestion to validate the results of one model through several model runs is highly appreciated and will be included in future research.

4. How the authors calculate the severity S is well described. However, there are no details about the calculation of the duration D. For example, is there a threshold below which rainfall intensity is just considered as noise? Is the treatment different for

observed and synthetic data?

The calculation of duration D is based on the definition of drought event (page 7478, lines 2&3). Therefore D is the number of days during which the value of EDI index is continuously less than -0.70. The calculation method is applied for all observed and synthetic time series.

5. P.7482, l.26: “It is clear from the figure that all BL models simulate less lower EDI values” -> not so clear for models TBL and TBLG.

The figure seems to be unclear enough. The result will be clearer if the figure is used in conjunction with figure 1. We will rephrase the text to make it clearer.

6. P.7484, l.18: Consider removing “found in the fact”.

We will correct it in the revised manuscript.

7. P. 7487, l.19: closest -> close to.

We will correct it in the revised manuscript.

8. P. 7489, l.1-3: The probability of extreme events seems to be underestimated by all BL models (Fig. 3, 4 and 7), not overestimated. The conclusions are a bit confusing in terms of under/overestimation of the marginal distributions andunder/overestimation of the probability of extreme events.

It is a mistake, thanks for your important remark. We will rephrase the conclusions to make it clearer.

Important remark: During the period of public review and discussion, we have found that by using an improved approximation of the analytical expression of the variance, the problem of producing unrealistic model parameter sets, obtained when the 3rd order moment was included in the calibration, was solved; it seems no longer needed to include these 3rd order moments in the calibration. We therefore recalibrated the different models using the improved approximation and including only the first and sec-

ond order moments and the zero depth probability as classically done. This has minor impact on the results and the conclusion, but as the results are obtained through better approximations, we believe that these better calibrated models had to be compared. In the revised paper we then will also include these newly calibrated models, even though the results do not significantly change.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 7469, 2013.