

Interactive comment on “An application of the Bivariate Generalized Pareto Distribution for the probabilities of low flow extremes estimation” by W. Jakubowski

Anonymous Referee #3

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Review of manuscript: AN APPLICATION OF THE BIVARIATE GENERALIZED PARETO DISTRIBUTION FOR THE PROBABILITIES OF LOW FLOW EXTREMES ESTIMATION

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OVERALL ASSESSMENT

The manuscript presents an application of bivariate generalized pareto distribution (BGPD) to represent the joint probability distribution of two correlated low flow indices, the low flow deficit and duration.

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Despite the relevance of the topic addressed by the author, in my opinion the manuscript suffers from numerous fatal flaws that make the manuscript itself unpublishable on HESS. In particular, the manuscript miss to present and clearly illustrate the objectives, elements of novelty and innovations with respect to previous studies. The notation used and the mathematical expressions reported in the text are not clear enough. Also, the presentation of the study lacks references and, in particular, the manuscript quotes studies that are not up-to-date nor readily accessible. Finally, I rate the presentation of the comparison with a previously presented model (Zelenhasić and Salvai, 1987; see Section 6.3 in the text), which could actually be one of the most interesting parts of the manuscript, very poor. I also suggest a different and more recent model to use for the comparison (see point 4 of the Major Comments section)

Therefore, I recommend to reject the manuscript in its present form, but I also encourage the author to submit a new, radically revised and restructured manuscript that addresses all major comments reported below.

MAJOR COMMENTS

1. Presentation of the state of the art, objectives of the study and elements of novelty

The manuscript does not indicate the objectives and main innovations of the study. For instance, the first section of the manuscript (1. Introduction, 7-line long) should be totally re-thought and re-written, and used to illustrate the 1) state of the art, 2) objectives of the study and 3) original contributions. None of these points is addressed in the current version of the manuscript.

2. Notation and mathematical expressions

All terms used in the manuscript need to be defined and described thoroughly; see for instance the definitions presented at the beginning of section 2; eqn. 1, eqns. 9, 10 and 11, here the fundamental quantities DM and TM are not defined. No discussion of

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the BGPD parameters is provided (overall number, meaning). Also, I found the notation adopted in the manuscript to be awkward and confusing in several occasions; see for instance, once again, DM and TM, which probably differ from Dn and Tn defined in section 2. and for which the meaning of the subscript “M” is totally unclear.

3. Results of the study

There is a generalised lack of clarity in the presentation of results. In particular, the presentation of the comparison of the proposed model with the model suggested by Zelenhasić and Salvai (ZS, 1987) is sparse and incomplete (impossibility to trace the results, see section 6.3). Also, Figures 12 and 13 show that for two different test sites the ZS model cannot even reproduce the first order moment (central tendency) of the marginal distributions for deficit (Figure 12) or duration (Figure 13) of low flows. This result is puzzling, the author need to double-check the application of the ZS model and comment thoroughly on this outcome.

4. Referencing

The author should include additional references and, in particular, quote more accessible and up-to-date studies (see Minor Comments). Recent studies clearly pointed out that Copulas represent useful and effective tools for investigating the statistical behavior of dependent variables and for relating the marginal distributions of different correlated hydrological variables (see e.g. Favre et al., 2004; Salvadori and De Michele, 2004). I warmly recommend that the author refer to these statistical models, along with (or instead of) the ZS model, for evaluating the performance of the proposed BGPD model.

5. Language

The English language is poor.

MINOR COMMENTS

I report in this section a list of minor comments. These comments do not refer to fatal

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flaws, but the author may as well want to address them before submitting the new manuscript.

1. Tajvidi (1996)

Please, cite country and university in the reference. If possible, it would be better to quote a more accessible study reporting these results.

2. “threshold level method” (p.2 beginning of section 2)

Provide a reference or explain the method.

3. Misuse of words

“effect” is used instead of the verb “to affect”, “then” is used instead of “than” several times in the text.

4. Deficit and Duration (p.2)

Please, revise the definition of the two fundamental measures. Is t_{n0} really a low flow? Shouldn't it be an instant of time.

5. “other significant droughts” (p.5 last paragraph of section 4)

Please, clarify.

6. Mutually independent (p.5 beginning of section 5.1)

This statement seems to conflict with the statement reported in the first line of section 4, and with the general evidence that deficit and duration extremes are correlated.

7. Section 5.1

Please, summarise and discuss all parameters of the BGPD.

8. Reference to and inexistent Section 1.1 (p.7).

9. Clarify the last statement of Section 5.2.

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10. Provide references or describe within the text the Gauss-Jacobi quadrature method and the Lamba-Kolmogorov test.

11. Point 3 reported in the Conclusions needs a specific discussion in the body of the manuscript.

REFERENCES

Favre, Anne-Catherine; El Adlouni, Salaheddine; Perreault, Luc; Thiémonge, Nathalie; Bobée, Bernard, Multivariate hydrological frequency analysis using copulas, *Water Resour. Res.*, 40(1), 2004.

Salvadori, G.; De Michele, C., Frequency analysis via copulas: Theoretical aspects and applications to hydrological events *Water Resour. Res.*, 40(12),2004

Zelenhasić, E. Salvai, A., A Method of Streamflow Drought Analysis, *Water Resour. Res.* 23(1), 156-168, 1987.

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