

Interactive comment on “Effects of runoff thresholds on flood frequency distributions” by A. Gioia et al.

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General comments

The paper proposes an analytical concept for the estimation of flood frequency distribution. The main objective is to explain the high skewness in flood distribution, which is attributed to the assumption that the derived flood frequency distribution is based on a combination of two different threshold mechanisms of flood generation. The authors divide the floods to ordinary and rare and assume that in humid conditions, ordinary floods occur when an infiltration rate threshold is exceeded. This type of flood generation is considered to be produced by a relatively small contribution area. The rare events occur when a soil storage reservoir is filled and thus larger areas contribute to the flood generation. The authors validated their approach using data

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from 10 basins in Italy. The proposed model is applied to gauged catchments and validation is based on a visual comparison of the derived flood frequency distribution with the TCEV model and corresponding plotting positions. Finally the authors show the relation between selected model parameters and some physiographic catchment attributes.

Overall the estimation of flood frequency distributions and regionalisation of their parameters is a relevant topic which definitely fits within the scope of HESS. However presented manuscript does not sufficiently and clearly demonstrate the value of proposed concept and the interpretations and conclusions made are not always adequately supported by the presented results. The idea of analytically derived flood frequency distribution is interesting, but just visual comparison of proposed model with another model and/or plotting positions is, in my opinion, not enough. There are plenty of probability distributions and fittings methods available and I'm not sure why one should use the proposed approach? Therefore, I would strongly suggest to state more explicitly (e.g. in the introduction) in which context may be the application of analytically-derived flood frequency distribution beneficial (e.g. in prediction for ungauged sites). Within this context a clear quantitative validation should be performed and presented (e.g. considering gauged sites as ungauged performing a jack-knife crossvalidation).

The readability of the manuscript should be improved. The authors should be more precise in the formulation of basic assumption beyond their model and conclusions made. They are mixing the terms of arid and humid basins, ordinary and rare events, flood generation process and the conceptualisation of the process using a probability distribution. From the context of the manuscript, one may have a feeling that the ordinary floods are attributed only to the small contribution area or that the rare floods occur only in the arid basins. Similarly the conclusion that the study focuses on the dynamic of flood generation processes is slightly misleading, because the results show more a statistical fitting to the observed floods (based on some assumption) than a real analysis of particular flood generation processes. I would recommend to stress more

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explicitly that the proposed concept is based upon some simplifications/assumptions and validated over a specific region, which may not completely capture the real variability in flood generation process and may be not valid for some other regions. This is very important especially for the regionalisation of model parameters to other regions. As is documented e.g. in Parajka et al. (2008), in many catchments situated in Slovakia and Austria, the 'ordinary' floods are generated by the snowmelt process, which may not correspond to the proposed concept of small contributing area presented here.

I recommend this study for publishing in HESS after a revision. Authors should, in my opinion, improve the readability of the manuscript and provide more detailed evaluation of the benefits of proposed concept. The regionalisation of model parameters should be discussed in more detail and the predictive accuracy should be assessed quantitatively.

Specific comments

p. 904 (Abstract): I would suggest to put the threshold values found in the analysis into the abstract.

p. 907, l. 10: The following statement is not clear to me and I found it too specific for general goals description. 'The goal is to improve the descriptive properties of theoretically derived distributions with particular attention on their ability of coping with the Matalas condition of separation.' Please consider to revise it.

p. 907-911 (Section 2): Please consider to condense the description of the IF model.

p. 908: Please explain in more detail the estimation of the routing parameter. How sensitive are the results with respect to this parameter?

p. 916: Please provide more detailed information about the estimation of mean runoff coefficient and the permeability index. This is important in the context of parameter

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regionalisation.

Figures: Figures 2 and 3 are not necessary. This information is already available in Table 1 and Figure 1. Figure 5 is difficult to read.

Some sentences and paragraphs are difficult to follow. Please consider an English proof before publication.

References:

Parajka J., S. Kohnová, R. Merz, J. Szolgay, K. Hlavcová, G. Blöschl (2008) Seasonality of hydrological characteristics in Slovakia and Austria. Submitted to HESSD.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 903, 2008.

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5, S451–S454, 2008

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