

Interactive comment on “Region-of-influence approach to a frequency analysis of heavy precipitation in Slovakia” by L. Gaál et al.

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Received and published: 6 September 2007

General Comments:

The paper addresses a topic of relevance to the hydrologic community as well as to the engineering community since heavy precipitation estimates are often at the basis for design values for hydraulic works. Methods that deliver possibly more reliable results in that respect are therefore very valuable.

The paper is clearly written, gives a good introduction and overview of the approach, represents a very thorough analysis considering all sorts of alternatives and options related to that kind of regional frequency analysis and the individual steps can be reproduced.

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The positive comment on the thoroughness of the analysis has also another side of the coin: there were so many models (combinations of alternatives and options) analysed and commented on, that the sheer wealth of statistical analyses was hiding to me at a certain point of reading the paper the question and aim of the paper: at the end I could not completely get rid of the impression that the paper runs a bit the risk to be a statistical analysis to its own end and not serving as a means to get an answer to a question.

As to its originality: I would put it rather at the lower end of the originality scale since it collects methods and approaches that have already been used in other studies, areas and domains and applies it to the Slovak study region. This does, however, not limit the value of being published in HESS and being brought to the attention of the scientific community.

What limits a bit the practical value is twofold: First the best performing approach relies on statistical at-site characteristics (actually alternative 1, chapter 3.1.1., and alternative 2, chapter 3.1.2, require rainfall data for calculating the distance metric); thus, it is of no use for ungauged sites (the authors are aware of it and mention it in the paper). Second, the analysis addresses only the growth curve, but does not enter into the question of how to estimate the index storm.

What I would have liked is a critical comment on the very strong assumption made for the evaluation that the "true" at-site quantiles are identical with the at-site sample quantiles. Of course this is an unavoidable and wide-spread procedure in that kind of analysis but it is somewhat critical (the quantity that the regional estimation approaches strive to improve is actually taken as reference value when evaluating the regional estimation approaches) and has implications on the significance of the findings and conclusions.

The analyses associate equal weights to the used attributes for the definition of similarity. Although I understand the reasoning provided by the authors I would add some

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further comments that attention has to be paid when selecting the attributes such as examining that the chosen attributes are independent and not correlated or carry redundant information.

technical corrections: page 2367: in the second paragraph (line 9) the block maxima approach is mentioned but no reference given. page 2367 line 12: seasons without s page 2371 line 5: characteristics without s page 2382 line 5: "...resulted in _*the *_delination of...." page 2382 line 7 "...one of the most experienced....."

recommendation for further work: if there is the intention to make the step from the scientific community to the engineering community and transfer the findings to practical applications I would recommend trying to enlarge the "information-pool" (i.e. the set of gauging stations) underlying the whole ROI-analysis by gathering and adding stations from beyond the Slovak borders (I guess that the gain in getting better estimates at least in the close-to-the-border areas would be higher than by trying to find a better ROI-setting).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 2361, 2007.

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