

Interactive comment on “Regional frequency analysis of heavy precipitation in the Czech Republic by improved region-of-influence method” by L. Gaál and J. Kyselý

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I basically agree with the comments of the other referees. As a matter of fact, the manuscript appears to be a repetition of a previous research work conducted in Slovakia (HESS Vol. 12, 2008). However I believe that the novelty in the dataset and region of study plus some variations in the methods could justify the publication, after revision, of the paper.

My concern is about the choice of the Lu and Stedinger homogeneity test. Lu and Stedinger's test, in fact, is based on the assumption that the "real" underlying distribution is GEV. This assumption has been criticized in recent literature, for example by Viglione

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et al. (2007) who state: "The necessity to preselect F implies that the test actually does not allow one to verify the homogeneity hypothesis alone, but the composite (homogeneity plus goodness of fit) hypothesis that the parent distribution is the same at each site, and has a predefined mathematical form F. As a consequence, the possible reasons why the test is not passed can be either that the region is heterogeneous, or that the adopted regional probability distribution F is inadequate." Is the choice of Lu and Stedinger test related to your Monte-Carlo simulation scheme, in which a GEV distribution is assumed as the "true" distribution? You could use instead the Hosking and Wallis test, which is also affected by this problem, but at a lower degree, assuming a more flexible 4 parameter kappa distribution. Please comment on this.

I would also ask to the Authors to comment on the selection of the pooling methodology (modified version of Castellarin et al., 2001). They say that "The essential issue of the Monte Carlo simulation is the way the unknown parent distribution (the "true" distribution) of the extremes is estimated. We decided to estimate the true at-site distribution by adopting the region-of-influence approach in which the similarity of sites is determined according to the statistical properties of the at-site data samples (abbr. ROIsta), as in Castellarin et al. (2001) and Gaal et al. (2008)." In my point of view, the fact of using basin characteristics, and not data statistics, to pool sites is not only done to include ungauged basins in the regions. The underlying objective of regionalization is to increase the information available locally. For example, a site could not have been interested by major events because of its limited historical record, so it is useful to use data from a "meteorologically/morphologically similar" site in which major events happened. The two sites, in fact, would have very different "statistics" because sample of variability, not because of heterogeneity.

Finally I agree with referee # 3 that a promising way to expand the current scope of the paper would be to analyse more (shorter) rainfall durations with the aim to ultimately develop ROI-based regional rainfall-duration-frequency models for the study area.

References: Viglione, A., F. Laio, and P. Claps (2007), A comparison of homo-

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