Hydrol. Earth Syst. Sci. Discuss., 6, S78–S84, 2009
www.hydrol-earth-syst-sci-discuss.net/6/S78/2009/

Hydrology and Earth System Sciences Discussions

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

L. Gaál and J. Kyselý

Received and published: 12 February 2009

We acknowledge the comments of Referee #3 and large majority of his suggestions for improving the manuscript; they will be adopted in the revised version. In this comment, we would like to respond to several points raised by the referee that concern mainly the design of the study, methodology applied, and data used/available.

GENERAL COMMENTS

REFEREE: "In terms of originality, this manuscript provides little advancement to the ROI technique. As a matter of fact, the manuscript appears to be a repetition of a previous research work conducted in Slovakia, only applied here to a new study area (Czech Republic)."

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

REPLY #1: We do not think that the manuscript is purely a repetition of our previous work in different geographical settings. We attempted to improve the ROI technique applied in Gaál et al. (2008) in several points, in order to avoid the main drawback of the original approach, i.e. many subjective decisions (parameter values) that were necessary to be set. The added value of the present work consists mainly in (i) incorporating a regional homogeneity test into the process of constructing the pooling groups (not a new idea in focused pooling, cf. Zrinji and Burn, 1994; Castellarin et al., 2001, but a clear progress compared to Gaál et al., 2008), and (ii) combining and preserving the beneficial aspects of two different strategies for constructing the pooling groups, termed 'forward' (e.g. Castellarin et al., 2001) and 'backward' (e.g. Zrinji and Burn, 1994) approaches (described in detail in the manuscript). We tried to emphasize the improvement of the ROI method also in the title ('... by improved region-of-influence method'); perhaps a slightly modified title which better reflects the goal of the study - which is not the application of the same procedure as in Gaál et al. (2008) to a new region - should be considered in the revised version.

REFEREE: "I strongly recommend the authors to expand the current scope of the manuscript. One promising way to do that would be to analyze more (shorter) rainfall durations with the aim to ultimately develop ROI-based regional rainfall-duration-frequency models for the study area."

REPLY #2: We completely agree with the referee's recommendation that it would be useful to analyze shorter durations. However, the lack of available sub-daily precipitation measurements makes this impossible at the moment; (i) there is a very limited number of rain-gauges with reliable (and sufficiently long) continuous recordings, and (ii) majority of the data from those rain-gauges has not been digitized and corrected yet. We have consulted the up-to-date availability of data with the staff of the Czech Hydrometeorological Institute; the longest series of hourly precipitation amounts begins in September 1996. That is why building up a regional rainfall-duration-frequency model has not been considered in this work and seems to be, at this time, unfeasible

HESSD

6, S78–S84, 2009

for the area under study.

SPECIFIC COMMENTS

REFEREE: "I think what was (mostly) analyzed in this manuscript is not precipitation, but rainfall. One would want to avoid mixing solid and liquid forms of precipitation in the analysis. It would also be beneficial to run a seasonal frequency analysis and separate convective rainfall events from frontal rainfall events."

REPLY #3: At p291/14 we comment that 88% of one-day annual maxima occur in warm season (April-September). The remaining 12% that is observed in October-March may appear both in the solid and liquid forms (with increasing fraction of solid precipitation at higher elevated sites). That is why we refer to 'precipitation' and not 'rainfall' in the analysis of annual maxima, although it is mostly rainfall that produces the extremes. We think that the (suggested) separation of rainfall events of different origin (frontal / convective) is not a straightforward task. In principle, one can delineate warm season (April-September) when convective events dominate and cold season (October-March) when precipitation of frontal origin prevails, and run seasonal frequency analyses. Obviously, such an approach is by far not perfect since different atmospheric processes that generate rainfall do mix in both seasons. A disaggregation of frontal events from convective ones, based on additional data from synoptic observations (SYNOP reports), is possible only at sites where these observations are available (around 15 in the examined area with digitized data available since 1982; compared to $>$200 stations and data since 1961 in the present analysis), and needs quite large and time-consuming effort (which includes also validation of proposed algorithms). However, first attempts to disaggregate precipitation of predominantly convective and frontal origin at the stations with synoptic observations have recently started; the main aim is the analysis of trends/variability but the results may be useful for the frequency analysis in the future, too (but note that the number of sites where the disaggregation may directly be made is very limited, and the time series are much shorter).

HESSD

6, S78–S84, 2009

REFEREE: "Section 2.1 states that a few errors in the dataset were corrected. Why just a few? Can all be corrected? Also, how did you define the 3-month threshold value for the exclusion of sites from the analysis?"

REPLY #4: The formulation was ambiguous and not precise; 'a few' is used in the meaning of 'not many' but 'all' - all identified errors were corrected in the revised dataset. The threshold of 3 months was used as the maximum time span in which data were supplemented by interpolation from nearby measuring sites (details on the procedure are given in Kyselý, 2008). This will be reformulated in the revised manuscript.

REFEREE: "The target site should not be included in the formation of pooling groups."

REPLY #5: This is an important comment which points to the regional rainfall/flood frequency analysis that aims at estimating growth curves/T-year return values at ungauged sites. In this sort of analysis, it is obvious that the target location cannot be included in its own pooling group (in general there are no measurements available). The target location is usually omitted during a process of cross-validation, when ungauged conditions are simulated; in this case, the pooled growth curve of the target site is only estimated by using the data from all the sites in the pooling group except for the target one (e.g. Castellarin et al., 2001; Brath et al. 2003). Nevertheless, our study does not focus on the estimation at ungauged sites (although this may be the ultimate goal) but attempts to compare the regional/pooling approaches and improve reliability of the estimates at the target site. That is why we do not see reasons for excluding the target site from the pooling group in this application of the ROI method (which is analogous, as to the inclusion of the target site into ROI, e.g. to Castellarin et al., 2001, Eq. 7, p. 273); it is the data from the site itself that contain the most important piece of information for the estimation of high quantiles at the given site. This will be better clarified in the revised manuscript.

REFEREE: "It is not clear if the size of the pooling groups varied according to the target return period, as it should." [...] "It seems to me the 100-year pooling groups were also

HESSD

6, S78–S84, 2009

used to estimate the 200-year events. In any case, 200-year design storms are very rarely used in engineering design and should not be reported in the study."

REPLY #6: This is another useful comment. The formation of the pooling groups was only governed by a fixed minimum number of sites (11) that corresponds to the return period T = 100 years according to the 5T rule. Consequently, the pooling groups for the return periods 5, 10, 20 and 50 years are perhaps oversized, while the size of the pooling groups for the return period 200 years are rather underestimated. We will consider the alternative approach in the revised manuscript, and omit the estimation of the 200-year quantiles.

REFEREE: "Did you do any sensitivity analysis on the number of Monte Carlo simulations?"

REPLY #7: Yes, we carried out a sensitivity analysis (which should have been commented in the manuscript - will be supplemented in the revised version); there are negligible differences between results of Monte Carlo simulations based on 1000, 5000 and 10000 repetitions. The choice of 5000 is a reasonable compromise: the number of repetitions is sufficiently high and the time demands still allow accomplishing the simulations on PC.

REFEREE: "The pooling groups are rather large. What significance level was used in the homogeneity testing? Perhaps a higher significance level should have been used instead."

REPLY #8: In homogeneity testing, we used the regional homogeneity test of Lu and Stedinger (1992) in a standard form, i.e. with the significance level of 0.05 (see Appendix B, p297/4). We did not change the significance level of the homogeneity test in order to avoid relaxing ad-hoc the standard settings of the test to get better results.

REFEREE: "Are you sure the BIAS and RMSE values in the tables are already in [%]? They seem to be extremely small for indirect (regional) estimates."

HESSD

6, S78–S84, 2009

REPLY #9: The values of bias and RMSE are indeed in [%]; the results of the Monte Carlo simulations have been multiplied by 100. The low values occur due to the fact that the pooling groups are rather large.

REFEREE: "I would like to see a table that would summarize the size, degree of homogeneity, and the average spread of sites for the different pooling approaches."

REPLY #10: The table will be supplemented in the revised manuscript.

We do not reply herein to the TECHNICAL CORRECTIONS suggested by the referee; they will be implemented directly in the revised manuscript.

REFERENCES

Brath, A., Castellarin, A. and Montanari, A.: Assessing the reliability of regional depth-duration-frequency equations for gaged and ungaged sites. Water Resour. Res., 39(12), 1367, doi:10.1029/2003WR002399, 2003.

Castellarin, A., Burn, D. H. and Brath, A.: Assessing the effectiveness of hydrological similarity measures for flood frequency analysis. J. Hydrol., 241(3-4), 270-287, 2001.

Gaál, L., Kyselý, J. and Szolgay, J.: Region-of-influence approach to a frequency analysis of heavy precipitation in Slovakia. Hydrol. Earth Syst. Sci., 12, 825-839, 2008.

Kyselý, J.: Trends in heavy precipitation in the Czech Republic over 1961-2005, Int. J. Climatol., early view, available at: http://www3.interscience.wiley.com/journal/121566539/abstract, doi:10.1002/joc.1784, 2008.

Lu, L.-H. and Stedinger, J. R.: Sampling variance of normalized GEV/PWM quantile estimators and a regional homogeneity test. J. Hydrol., 138(1-2), 223-245, 1992.

Zrinji, Z. and Burn, D. H.: Flood frequency analysis for ungauged sites using a region of influence approach. J. Hydrol., 153(1-4), 1-21, 1994.

HESSD

6, S78–S84, 2009

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 273, 2009.