Hydrol. Earth Syst. Sci. Discuss., 2, S1078–S1081, 2005
www.copernicus.org/EGU/hess/hessd/2/S1078/
European Geosciences Union

Hydrology and Earth System Sciences Discussions

Interactive comment on "Evidences of relationships between statistics of rainfall extremes and mean annual precipitation: an application for design-storm estimation in northern central Italy" *by* G. Di Baldassarre et al.

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Received and published: 24 November 2005

General comments The paper shows the evidences of relationships between statistics of extreme rainfall and means annual precipitations. This relevant scientific question is definitely within the scope of HESS. At the same time, a similar discussion on the same dataset is found in literature, (Brath et al. 2003) [1]. For this reason more emphasis could be put on the description of the model itself, the more original part of the work at my knowledge, and on the technique used to assess his reliability (see specific comments). The paper presents also a novel dataset, namely a set of rainfall time series of at 15 minutes resolution. I personally recognize the interest on a wide database analysis. It is very useful that the fellow scientists can compare their results or model hypothesis with the results obtained on other wide dataset. A careful description of the data is thus recommended (see specific comments). A more careful statement on the hypothesis related to the choice of extreme value theory is also recommended (see specific comments). Related to the wide range of duration analyzed, a comment, or at least a quotation of the scaling approach is recommended (see specific comments). I suggest a revision of the paper before the final publications in order to give the authors the opportunity to highlight the originality of the work versus the previous quoted papers and to fix some specific and technical comments.

Specific comments

 A wide range of duration (15min-24h) is investigated. This adds significant value to the work. A comment on the scaling properties of the estimated index could be very interesting. This topic was widely investigated in literature, in particular for the annual maxima of rainfall. (Burlando and Rosso, 1996) [2]. In this view we notice that the number of parameter of the model is (a,b,c) is 37. It could be very useful to look for scaling relation on the parameters. That could strongly reduce the numbers of parameters.

 The goal (1) as depicted at line 7-8 p2395 is reductive, I would say, more generally that the authors are interested in testing the hypothesis that the coefficients of L-variation and L-skewness depend on MAP. Moreover, the authors state ‘for example the coefficient of variation and the coefficient of skewness can be considered constant’ p2396 4-5. Note that the coefficients of variation and skewness are not actually the same that L-variation and L-skewness. The moment of order 3 (the skewness) for heavy tail distribution could not exist but the L-skewness always exist! The authors should comment on this difference.

 In section 2.1 “growth factor estimation” the authors should put the equation

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of to the L-moments statistics instead the equation of GEV parameters. Indeed the extreme value theory and the GEV distribution are only used to calculate the T-quantile of the annual maxima distribution, while, the empirical L-moments are finally the variable which is regionalized. Moreover the L-moments are distribution-free. This is important because the authors can claim that the result of their analysis on regionalization is not affected by the choice of an extreme value distribution.

 The authors say that 'severe regional recent analyses showed that the GEV is a suitable statistical model for representing the frequency distribution of rainfall extremes' p2396 18-21. I suggest they recall the hypothesis that lead to the derivation of the GEV following the extreme value theory. 'According to the theory of the extreme value , the largest value from a set of independent and identically distributed random variable tends to an asymptotic distribution, such as the GEV'. Thus the annual maxima are also supposed to be independent and stationary. These hypotheses are widely accepted in frequency analysis in hydrology, but I recommend recalling it.

 In Table 1 it is shown that for t=15 min the criterion for the selection of rain gauges is N$>$5 while for t=1day is N$>$30. This is obvious since the available daily series are usually longer than the 15-minutes series. But the confidence of the L-moments calculated on N=5 series is different from the confidence of the L-moments calculated on a N=30 series. The authors should comment on that.

 Lines 5-15 p2401. It is not clear why the authors calculate the H(1) on groups of 15-30 stations and the H(2) on groups of 30-60 stations. These choices have to be explained more clearly.

 The regionalization procedure is done on two components of the rainfall measures. The mean annual maxima and the extremes. The two of them are treated differentially (kriging vs. regionalization of parameter). The authors show the uncertainties related to each of the two part of the procedure. Unfortunately they did non comment about the influence of these uncertainties on the final estimation of h(d,T). In

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an ungauged site, the uncertainties related to the L-moments estimation on equation(7) are more or less important that the uncertainties on mt for the estimation of h(d,T)?

Tecnhical correction

 Line 23 p2399 I suggest to define the L-kurtosis as L-Ck in order to maintain the coherence between the formal representations of L-moments.

 The caption of figure 4 is not clear. The authors should clearly state that the moving weighted average is represented by the solid line.

 Line 6 p2404 it may be “at the two duration considered” instead of “and the two duration considered”

 The results plotted in figure 4 are plotted again in figure 5a and 5b. The authors should try not to show the same information twice if it is possible.

 At line 14 the authors stated that “the cross validation produces best performance indexes combining a linear regressive modelĚ..residuals”, but in figure 11 and Table 5 it looks like the uncertainties linked to MAPr index are larger then those related to MAP, and they remark at line 29 p2404 that “this indicatesĚĚ.. Performance”. The authors should clarify these apparently conflicting points.

1. Brath, A., A. Castellarin and A. Montanari, Assessing the reliability of regional depth-duration-frequency equations for gaged and ungaged stations. Water Resource Research, 2003. 39(12). 2. Burlando, P. and R. Rosso, Scaling and multiscaling models of depth-duration-frequency curves for storm precipitation. Journal of Hydrology, 1996. 187(1-2): p. 45-64.

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 2393, 2005.