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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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GENERAL COMMENT

First of all, we would like to thank the Editor, Associate Editor and Reviewers for doing excellent work and providing very useful observations and comments, which truly helped us in improving the overall quality of the presentation of our work. The revised manuscript that we are going to resubmit for possible publication is the proof of our deep appreciation for the useful and constructive indications and suggestions provided by Reviewers. We detail in the remainder of our reply how we incorporated all Reviewers' suggestions and comments in the revised manuscript, which, in our opinion, has an improved readability and represents an original contribution to the comprehension of the statistical behaviour of rainfall extremes for the study area. A theme of both reviews is the supposed strong similarity between our manuscript and the study by Brath et al. (2003). Prior to the detailed discussion of Reviewers' comments, we would like to remark here the differences and improvements between our study and the analysis described in Brath et al. (2003). By following an approach originally proposed by Alila (2000), Brath et al. (2003) identify a set of generalized depth-duration-frequency equations for the estimation of design storms for storm duration from 1 to 24 hours and test the equations' reliability through a jack-knife cross-validation. We believe that, with respect to previous studies (e.g., Schaefer, 1990; Alila, 1999; Brath et al., 2003), our manuscript presents new data, new concepts and ideas and different tools. For the first time in this study area, our study utilises an updated and significantly enlarged dataset with respect to Brath et al. (2003), which includes sub-hourly rainfall extremes (storm duration of 15 and 30 minutes) and is presented and analysed for the first time (new data). This is acknowledged by one Reviewer (see Bernardara, 2005, p. S1078). We model the relationship between statistical properties of rainfall extremes and mean annual precipitation (MAP) using a Horton-type curve (new concepts and ideas). We show that the curve is statistically significant for all duration considered in the study through an original extensive and objective Monte Carlo simulation experiment that we specifically designed for this purpose, as acknowledged by one Reviewer (see Bernardara, 2005, p. S1079). On the basis of these relationships, we develop a regional model for estimating the rainfall depth for a given storm duration and recurrence interval in any location of the study region (different tool). Perhaps the first version of our manuscript did not present these original contributions clearly enough. Therefore, the changes incorporated in the revised manuscript mainly aim at improving the description of the physical reasoning underlying the development of the analysis, addressing in particular the identification of climatically homogeneous regions. Also, we put more emphasis on the description of the model developed and on the technique used to assess his reliability as suggest by a Reviewer (see Bernardara, 2005, p. S1078). Finally, we included an additional analysis in the revised manuscript (described in a new section 5.3) in order to quantify the sensitivity of regional L-Cv and L-Cs estimates for ungauged sites. In particular, we performed a jack-knife resampling procedure that enabled us to quantify the uncertainty of regional rainfall quantiles for T = 100 and 200 years. These further analyses show that the estimation of the index-storm is the critical step for the application of the proposed regional model to ungauged sites.

The remainder of our reply, after a summary of the revised manuscript structure, addresses the comments raised by Dr. Peter Molnar (Associate Editor).

REVISED MANUSCRIPT STRUCTURE

We reorganised the revised manuscript as follow:

1 Introduction

2 Index Storm procedure

2.1 Growth factor estimation

2.2 Index storm estimation

3 Study area and locale regime of rainfall extremes

4 Regional model

4.1 Climatically homogeneous pooling-groups

4.2 Empirical regional model for estimating the L-Cv and L-Cs

5 Design storm estimation in ungauged sites

5.1 Application of the regional model

5.2 Index storm and MAP at ungauged sites (section 4.2 in original manuscript)

5.3 Uncertainty of the regional estimates (completely new section)

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6 Conclusion

Appendix (Homogeneity test)

REPLY TO ASSOCIATE EDITOR (Dr. P. MOLNAR)

The Associate Editor summarized the major objections in four points that are briefly recalled hereafter, along with our reply (see also Molnar, 2005, p. S1360-S1365).

1) ASSOCIATE EDITOR COMMENT:

“The authors use a statistical test to evaluate the degree of homogeneity in stations grouped by MAP. In the original test of Hosking and Wallis (1993), it is assumed that in a homogeneous region the data come from the same parent distribution. Weighted (with record length) average L-moment statistics are used to find the “best” parent distribution, from which samples of the same record lengths and site number are drawn, and the between-site variability of sample and generated L-moments are then compared. Hosking and Wallis (1993) propose several measures V to come up with the heterogeneity measure H, of which the authors use two (p.2401). The authors should add the formulas that are behind these two measures H(1) and H(2) in the text or an appendix. It is important here that in the method proposed by Hosking and Wallis (1993), the observed variability in L-moments is compared with the mean variability of the simulated data, e.g. like in the application by Alila (1999). From the description of the homogeneity testing in the manuscript on p.2401 it is not clear whether the authors follow through in the same way. It appears that H was not computed by comparing observations with simulations because no simulation is mentioned. This has to be clarified and/or corrected. Of course, the authors apply the homogeneity test in a slightly different way, they choose a moving window of sites ordered with the site MAP, and test the homogeneity of this subset of sites. The choice of the number of sites included in the subset is rather arbitrary, as was pointed out by one of the reviewers in his comment (P. Bernardara). The authors should consider addressing the issue of the number of sites in the revised manuscript. By increasing the number of sites one necessarily increases the heterogeneity. In the limit, when all sites are included regardless of MAP, one arrives at the heterogeneity measure of the whole region. The whole study region in the manuscript is "certainly heterogeneous" from the point of view of lower order moments as can be seen from the caption of Figure 6. This should also be mentioned in the text of the manuscript because it underlines the advantage of using MAP as a regionalisation variable."

We agree with the Associate Editor, we included the formulas that are behind H(1) and H(2) in an appendix. We also included in the revised manuscript this comment (at the end of section 4.1): "This analysis (see Figure 5) shows that the subsets identified according to the MAP value are generally acceptably homogeneous, whereas the H(1) value for the whole study region is equal to 3.41 while the H(2) value is equal to 1.73. Also, Figure 5 shows that the H(1) and H(2) values, quantifying the homogeneity degree, are significantly MAP independent. This result underlines the advantage of using MAP a surrogate of geographical location." Concerning the choice of the number of sites included in the subset see reply we commented about our choice in the revised manuscript (Section 4.1): "The different numbers of raingauges considered for the homogeneity testing (i.e., 15 and 30 for H(1) and 30 and 60 for H(2)) reflect two different aspects. First, higher order L-moments tend to be more homogeneous in space than the lower order ones (see e.g., Hosking and Wallis, 1997), therefore pooling-groups of sites for which the homogeneity is assessed in terms of L-Cv and L-Cs (i.e., use of H(2)) may be lager than pooling-groups for which the homogeneity is assessed in terms of L-Cv only (i.e., use of H(1)). Second, heterogeneity measures such as H(1) and H(2) are better at indicating heterogeneity in large regions, while have a tendency to give false indications of homogeneity for small regions, therefore pooling groups should be as large as possible."

2) ASSOCIATE EDITOR COMMENT:

"The empirical regional model is formulated and fit to the site L-moment ratios (L-Cv and L-Cs) as a function of MAP for different precipitation duration. I believe it is important to state here which optimisation procedure was used. For instance, because the uncertainty of the L-moment estimators is dependent on sample size as was pointed out by one of the reviewers (P. Bernardara), it may be wise to weight the site L-moment ratios in fitting the empirical model by the site sample size. Could the authors please comment on that? The authors describe in length how the reliability of the empirical model was assessed through Monte Carlo simulations. This appears to me to be a standard way of generatine confidence limits by simulation. I ask that the authors clarify whether this is the case, or what the novelty is here that I miss. In fact, Hosking (1990) and others have shown that L-moments are remarkably distribution independent, that is asymptotically (with large n) L-moment estimates converge to a multivariate normal distribution, where only the variance is a function of the underlying distribution. Figure 7 suggests that the generated confidence limits are symmetric, so perhaps normally distributed. In this context I also ask that the authors explain the need for the results in Table 4. If the data are iid then using the procedure above at the significance level of 5% one would expect on the average that 5% of the sample values lie outside of the interval, same is true for 10%. What is the significance of the fact that the values reported in Table 4 are not exactly coincident? If this is found not to be significant, please drop the table."

The revised manuscript comments (Section 3) on the criteria reported in Table 1 and their possible effects on the reliability of sample L-moments: "As Table 1 shows, we considered all available series of sub-hourly rainfall with at least 5 years of measure (N ≥ 5). This criterion reflects our intention of incorporating into the analysis as much information as possible. The raingauge network for sub-hourly storm duration is more recent and sparser than the network for hourly and daily storm duration (see Table 1). In order to reduce the negative effects of sampling variability for short series we characterised the statistics of rainfall extremes locally at each site through sample L-moments estimators, as they tend to be less biased than sample estimators of traditional statistical moments for orders 2 and higher and small samples (e.g., Hosking, 1990; Hosking and Wallis, 1997). For the same reason, as suggested for instance in

Hosking and Wallis (1997), we characterised the frequency regime of rainfall extremes at a regional scale (i.e., for a group of raingauges) by weighting each sample L-moment proportionally to the sample length.”

Concerning the Monte Carlo simulation we comment in the revised manuscript (Section 4.2) on the importance of the cross-validation procedure and the importance of Table 4: “The Monte Carlo simulations test the null hypothesis that the model (6) is able to reproduce the statistical behaviour of rainfall extremes at the 5% and 10% significance level. Table 3 summarises the results obtained with the Monte Carlo analysis, reporting the percentage of sample L-Cv and L-Cs values lying out of confidence intervals. The results indicated that the null hypothesis could not be rejected at 5 and 10% significance level as the percentage of L-Cv and L-Cs values lying out side the confidence intervals is less than 5 and 10% respectively.”

3) ASSOCIATE EDITOR COMMENT:

“In the abstract and in the conclusions the authors state that “the proposed model is able to reproduce the statistical properties of rainfall extremes observed for the study region”. However the authors do not analyse the performance of the model in this paper in the context of reproducing precipitation extremes. For example, it would have been relatively straightforward to conduct a Monte Carlo analysis to show if the regional model produces more accurate estimates of the design storms than at-site GEV distribution fits, e.g. similar to the analysis of Alila (1999). Another option would have been to estimate the accuracy of the regional model on ungauged sites by jacknife analysis, as the authors did in Brath et al. (2003). In my opinion such an analysis is acutely needed in a paper that presents a new method/model/dataset for prediction purposes. Can the authors please comment on this? An additional interesting element was mentioned by one of the reviewers (P. Bernardara), in that the uncertainty in the predictive model of h(d,T) in equation (1) comes from uncertainty in the index storm and in the growth factor. Which of these two uncertainties are more important? Is it possible to quantify them relative to each other? Finally, it is mentioned in section 2.2. and shown in section 4.2 how the uncertainty in the spatial interpolation of the index storm may be determined; and an example for 1-hour, 24-hour precipitation and MAP with two kriging approaches is given. Like one of the reviewers (P. Bernardara) I do not see why the MAPr method which accounts for the influence of elevation should perform worse than the MAP method with ordinary kriging. The authors state rather vaguely that this is due to the "geographic area considered herein and the particular raingauge network of our study". I believe an attempt to explain this better would be beneficial."

We addressed this comment by including additional analyses in the revised manuscript in a totally new section 5.3 and new figure 12. We used a jack-knife resampling procedure to quantify the sensitivity of regional L-Cv and L-Cs estimates for ungauged sites. In particular, this study enabled us to quantify the uncertainty of the regional rainfall quantiles for T = 100 and 200 years and to compare it to the uncertainty associated to the index-storm estimates. The results of these further analyses show that the estimation of the index-storm is the critical step for the application of the proposed regional model to ungauged sites.

The revised manuscript included a new section 5.3 (uncertainty of the regional estimates) that describe this analysis: "We evaluated the performance of the regional model through a comprehensive a jack-knife cross-validation (see e.g., Brath et al., 2001). The cross-validation procedure enabled us to compare the regional and resampled estimates of the design storm at all considered raingauges for two arbitrarily selected duration: 1 and 24 h and two reference recurrence intervals: 100 and 200 yrs, which are normally adopted in Italy for designing flood risk mitigation measures. Through this comparison we quantified the uncertainty of the design storm estimates that can be computed by applying the proposed regional model to any ungauged site within the study area. In particular, we compared the regional estimate of the dimensionless growth factor, h'(d,T), and design storm, h(d,T), with their resampled counterparts, h'jk(d,T) and hjk(d,T), respectively. We computed h'jk(d,T) and hjk(d,T) as follows: 1) one of the NS raingauges, say station i, and its corresponding data are removed from the set; 2) the parameter a, b and c of model (6) are estimated on the basis of the pluviometric information collected at the remaining NS-1 raingauges; 3) jack-knifed regional L-moments, L-CvRjk and L-CsRjk, are calculated for site i by using the recalibrated model (7) identified at step 2) and the jack-knifed MAP value (MAPjk) retrieved for site i from isoline MAP generated through ordinary kriging as described in section 5.2; 4) the jack-knifed parameters of the regional GEV distribution are estimated for site i through the method of L-moments on the basis of the L-CvRjk and L-CsRjk values estimated at step 3); 5) h'jk(d,T) is computed for site i as the T-year quantile from the GEV distribution estimated at step 4); 6) hjk(d,T) is then computed as the product of h'jk(d,T) and the jack-knife estimate of md, which is calculated as described in section 5.2; 7) steps 1-6 are repeated NS-1 times, considering in turn one of the remaining raingauges. The box-plot diagram of Figure 11 shows the distributions of relative errors for the estimation of the growth factor h'(1 h,100 yrs) and h'(24 h,100 yrs) and the design storm h(1 h,100 yrs) and h(24 h,100 yrs). Figure 12 shows the distributions of relative errors for the estimation of the growth factor h'(1 h,200 yrs) and h'(24 h,200 yrs) and the design storm h(1 h,200 yrs) and h(24 h,200 yrs). The figures show that the application of the proposed regional model to ungauged sites provides unbiased estimates of h'(d,T), for d=1, 24 h and T=100, 200 yrs. Also, Figures 11 and 12 illustrate that the absolute value of relative errors of the dimensionless regional growth factors is generally small and always lower than 10%. It is important to notice that if the local value of MAP is know (e.g., from observed daily rainfall data), then the relative errors of h'(d,T) estimates, resulting from the uncertainties in the parameters of model (6), become practically negligible ($<$1%). Concerning the estimation of h(d,T) the performance of the model is definitely lower as 30%. A comparison between the relative errors of Figure 11 and 12 points out rather clearly that the largest uncertainty in the application of the regional model to an ungauged site is associated with the index-storm estimates (see section 5.2). The presence of cross correlation between stations does not introduce a bias effect, but could increase the uncertainty of the estimates (see Hosking and Wallis, 1988). These effects are not quantified at this stage, but we plan to investigate this matter in the future.”

We also modified the abstract: “Several hydrological analyses need to be founded on a reliable estimate of the design storm, which is the expected rainfall depth corresponding to a given duration and probability of occurrence, usually expressed in terms of return period. The annual series of precipitation maxima for storm duration ranging from 15 minutes to 1 day are observed at a dense network of raingauges sited in northern central Italy are analyzed using an approach based on L-moments. The analysis investigates the statistical properties of rainfall extremes and detects significant relationships between these properties and the mean annual precipitation (MAP). On the basis of these relationships, we develop a regional model for estimating the rainfall depth for a given storm duration and recurrence interval in any location of the study region. The applicability of the regional model is assessed through Monte Carlo simulations. The uncertainty of the model for ungauged sites is quantified through an extensive cross-validation.”

And the end of Introduction (Section 1): “Once assessed the applicability of the proposed mathematical expression through an original and objective Monte Carlo simulation experiment, we developed a regional model for estimating design storms for storm duration from 15 minutes to 1 day in any location of the study area and we quantified the uncertainty of the regional model for ungauged sites through an extensive cross-validation.”

4) ASSOCIATE EDITOR COMMENT:

“In conducting statistical analyses of the type of data used in this study it is commonly assumed that the data at the sites are independent and identically distributed. I do not find the issue of spatial dependence mentioned in the manuscript? Was spatial independence tested? Is this issue not important in the context of this paper? I would ask that the authors comment on that.” The Associate Editor raises a very important point, specifically in the context of this paper. We incorporated a discussion of this point

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in the revised manuscript (at the end of Section 5.3, see also above): “The presence of cross correlation between stations does not introduce a bias effect, but could increase the uncertainty of the estimates (see Hosking and Wallis, 1988). These effects are not quantified at this stage, but we plan to investigate this matter in the future.”

TECNICHAL SUGGESTIONS (see Molnar, 2005, p. S1365)

A) ASSOCIATE EDITOR TECNICHAL SUGGESTION: “p.2395, line 25: You mean to say that the index storm is site dependent?”

We corrected the sentence in the revised manuscript: Original manuscript: “The index storm, usually assumed equal to the mean of annual rainfall maxima of duration d, is site independent; while the growth factor is assumed to be valid for the entire homogeneous group of basins.” Revised manuscript “The index storm, usually assumed equal to the mean of annual rainfall maxima of duration d, is site dependent; while the growth factor is assumed to be valid for the entire homogeneous group of basins.”

B) ASSOCIATE EDITOR TECNICHAL SUGGESTION: “p.2397, bottom: Please mention some arguments why the L-moment method is superior to others, and therefore used here.”

We followed the Associate Editor suggestion as we incorporated a comment (Section 2.1): “We characterised the regional frequency regime of rainfall extremes over the study area using the L-moments as suggested by Hosking and Wallis (1997). The L-moments are analogous to the conventional moments, but they have the theoretical advantages of being able to characterize a wider range of distributions and, when estimated from a sample, of being more robust to the presence of outliers in the data. Hosking and Wallis (e.g., 1997) also point out that L-moments are less subject to bias in estimation than conventional moments.”

C) ASSOCIATE EDITOR TECNICHAL SUGGESTION: “p.2400, line 5: Please remove Figure 4 and the reference to it. The data shown in Figure 4 are also shown in Figure

5, the comparison with the empirical relationships of Alila (1999) are not relevant at this point."

We followed the Associate Editor suggestion as we removed Figure 4 from the manuscript.

D) ASSOCIATE EDITOR TECNICHAL SUGGESTION: "p.2411: I suggest to join Tables 2 and 3 into one."

We followed the Associate Editor suggestion, we joined Tables 2 and 3 into one in the revised manuscript. E) ASSOCIATE EDITOR TECNICHAL SUGGESTION: "p.2412: Remove Table 4 (see discussion point 2 above)" Maybe Table 4 (Table 3 of revised manuscript) is not essential, but the information included are important (see discussion point 2 above). F) ASSOCIATE EDITOR TECNICHAL SUGGESTION: "p.2418: Remove Figure 4" We followed the Associate Editor suggestion (see point C above).

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