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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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GENERAL COMMENT

Even though the reviewing process is not over yet and we are still waiting for the Associate Editor final decision on our manuscript, we would like to take advantage of the available on-line discussion forum to provide an off-the-cuff reply to Dr. Younes Alila's comments, some of which we deem to be critical and need to be discussed at this stage.

We believe that the Reviewer raised some very interesting points and provided useful observations and suggestions that will definitely improve the overall quality of the

presentation of our study. Nevertheless, the Reviewer is also rather critical on several issues. In particular, he questions the suitability of the study for publication in HESS (major comment) and identifies four specific comments as follows: [1] discrepancies of findings for the same study area between this study and a previous study (Brath et al. 2003), [2] authors misrepresented a paper published by Alila (1999), [3] use of incorrect terminology for describing the proposed regional model and [4] limitations in the applicability of the regional model.

Our short comment is structured as follows: in the next section we address the issues associated with the questioned suitability of our manuscript; in the following four sections we address the specific comments listed above (the order of our replies reflects the order of the comments in the original review); finally, in a summarising final section we comment on the points listed by the Reviewer in his “SUMMARY OF EVALUATIONS”.

SUITABILITY OF THE MANUSCRIPT

Concerning the suitability of the study for a possible publication on Hydrology and Earth Sciences System (HESS), we would like to remark that we were invited to submit this manuscript to HESS after the first author won the "Young Scientists’ Outstanding Poster Paper (YSOPP) Award" for the poster entitled: "A regional model for estimating the design storm in Northern-Central Italy" presented to the last EGU meeting. The poster was awarded based on (see http://meetings.copernicus.org/egu2006/ysopp_guidelines.html): (a) the evaluation of at least 3 judges during the Vienna Assembly and (b) the votes of a jury, composed of the Technical Chairs of the EGU Section of Hydrological Sciences among the top-ranked posters. As a result, these evaluations indicated (in principle) a suitability of the study for a possible publication on HESS. Nevertheless, we agree that the current version of the manuscript can be improved and ameliorated, and some of Alila’s indications will be of great help in pursuing this task. Regarding the manuscript suitability the Reviewer states: “the developed model is a tool but not new to the science literature.

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It would however be of help to professionals for estimating design storms in this study area in Italy"; "in my view there is very little science here". Instead, 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 new tools.

New data: with respect to the study by Brath et al. (2003) our manuscript presents new data including sub-hourly (15 and 30 minutes) duration rainfall extremes (first time in this study area). We believe that this is not a marginal improvement of our database. This is also acknowledged by the other Referee (see Bernardara, 2005, p.S1078).

New concepts and ideas: Our manuscript formalises the relationship between the L-moments and MAP using a single mathematical expression, which is proved to be statistically significant for all duration considered in the study. We believe that this is a fundamental step towards a better understanding of the process that controls the physics of precipitation extremes. In particular, instead of proposing a plethora of mathematical equations, each one of them valid for a single storm duration (see e.g. Alila, 1999), we used a Horton-type curve to describe the relationship between MAP and L statistics for all duration.

New tools: in order to avoid a "blind reliance" on the identified mathematical equation and to evaluate its reliability, we proposed an original extensive and objective Monte Carlo simulation experiment (as acknowledged also by the Reviewer, see Bernardara, 2005, p. S1078). Perhaps the current manuscript does not present the original contribution of the study clearly enough and this deficiency will definitely be fixed in the revised manuscript.

[1] "DISCREPANCIES OF FINDINGS FOR THE SAME STUDY AREA BETWEEN THIS STUDY AND ANOTHER PUBLISHED MANUSCRIPT"

We disagree with the Reviewer on this point. There are no discrepancies between our manuscript and the study by Brath et al. (2003) for the simple reason that the two study areas are different. A comparison between Figure 1 of our manuscript and Figure 1

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in Brath et al. (2003, p. 11-3) shows that we excluded the Tyrrhenian Region in our study, and this emerges also from Table 1, which reports the number of raingauges and annual maximum rainfall data. By analysing this table, one can see, for example, that the number of hourly raingauges is 125 in our manuscript, while is 132 in Brath et al. (2003). The Tyrrhenian Region was excluded in our study because this area reveals an atypical behaviour that was already pointed out (see e.g. Castellarin and Brath, 2002 and Brath et al., 2003). Although we agree that analysing this anomaly is interesting in principle, the available rainfall data in the Tyrrhenian Region (7 recording raingauges) do not enable us to carry out a sufficiently accurate analysis. It also would be more correct, from a phenomenological viewpoint, to analyse this area together with the Tyrrhenian coastal Region (Liguria), and this is out of the scope of our study. Maybe some confusion arises from the fact that we wrongly reported (Section 3) that study area is 37200 km2 (as in Brath et al. 2003) while the right size of the study area is 35800 Km2. The revised manuscript will reflect this correction.

[2] "AUTHORS MISREPRESENTED A PAPER PUBLISHED BY ALILA (1999)"

The Reviewer is right here. We wrote “L-Cv can be considered to be independent of the geographic location (or MAP) for d less than 1 hour, with different values for duration equal to 15 and 30 minutes (Alila, 1999)”. To be correct, Alila (1999) pointed out that L-Cv values can not be considered to be independent of the geographic location also for sub-hourly duration. We undertake to modify quotations in order to correctly describe Alila’s work in the revised manuscript, and to better identify congruencies and differences between this manuscript and Alila’s findings. However, it is important to underline here that the relationship between sample L-Cv and MAP for our study area and duration less than 1 hour (see Figure 5b) does not point out any significant dependence between the two measures. It is also interesting to remark, regarding this point, that our study refers to rather dense raingauge network with respect to previous studies: a) the study by Schaefer (1990) refers to Washington State (about 180000 km2) and considers a raingage network with a number of recording raingauges varying

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from a minimum of 112 for the 2-hour storm duration (on average 1 station every 1605 km2) to a maximum of 316 for the 24-hour storm duration (on average 1 station every 570 km2); b) Alila (1999) refers to Canada (about 10200000 km2) and analyzes the observations collected at 375 hourly raingauges (on average 1 gauge every 27200 km2) and 320 sub-hourly raingauges (on average 1 gauge every 31875 km2); c) our study region has an area of around 35800 km2 and the average network density is 1 station every 91 km2 for daily rainfall data, 1 station every 286 km2, for hourly rainfall data, 1 station every 192 km2, for 30 minutes rainfall data and 1 station every 235 km2 for 15 minutes rainfall data. Finally, it is important to remember again that our results are validated against an extensive and objective Monte Carlo procedure to test the validity of the assumptions, in order to avoid a "blind reliance".

[3] "USE OF INCORRECT TERMINOLOGY FOR REPORTING THE REGIONAL MODEL CALLED INDEX STORM APPROACH"

The Reviewer is right in the sense that the classical index flood (or index storm if reference is made to rainfall extremes) hypothesis is based on the most restricting assumption that L-Cs and L-Cv “do not vary with location”. Nevertheless, since the original model was introduced (see e.g. Dalrymple, 1960) several extensions and evolutions were proposed, which partly relax the fundamental hypothesis of constant statistics (e.g. L-Cv and L-Cs) within a simple geographical homogeneous region. An example is the hierarchical application of the index-flood model, where the statistics of increasing order are constant within a set of nested regions (the larger the order of the statistics, the larger the region), see e.g. Gabriele and Arnell (1991). Another relevant example of index flood evolution is the Region of Influence approach (see Burn, 1990; Castellarin et al., 2001) which introduces the concept of homogeneous pooling group of sites as opposed to homogeneous geographical regions. Also our model can be considered to be an extension of the index flood model, in the sense that a homogeneous region, within which L-Cv and L-Cs are constants, is a group of climatically homogeneous sites, as in Schaefer (1990) and Alila (1999). In this context climatic

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homogeneity means a very limited variability in terms of MAP for all sites. We acknowledge that our manuscript does not make this point clear, we are definitely willing to include these observations in the revised manuscript these observations.

[4] "LIMITATIONS IN THE APPLICABILITY OF THE REGIONAL MODEL"

The proposed regional model was developed through a statistical optimisation procedure so that the model itself can be applied to: a) storm duration from 15 minutes to 1 day; b) return periods less than 100 years and c) sites located within the study area. A careful application of the regional model should also consider that the regional model was developed for raingages located below 1500 m a.s.l., while the study area can locally exceed 2000 m a.s.l. Finally, the spatial interpolation of rainfall extremes or MAP adopted in our study is unable to reproduce micro-climatic effects such as rain shadow effects, and can only provide an overly simplified representation of differences existing between leeward and windward sides of the same mountain depending of the particular spatial interpolator adopted in the study. We agree with the Reviewer, and the revised manuscript will include these comments.

SUMMARY OF EVALUATIONS:

Using the list reported by the Reviewer as a reference we address and summarise here, for the sake of completeness, all Reviewer's comments.

1) Does the paper address relevant scientific questions within the scope of HESS?

Reviewer: "No: Because the reporting of regional model for estimating design storm at gauged and ungauged locations using very well established methodologies and concepts is conducted with no discussion of processes or reflections on the physics of precipitations extremes".

We disagree with the Reviewer because our study introduces the relationship between the L-moments and MAP using a single mathematical structure. It is an essential step in order to better understand the process that controls the physics of precipitation

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extremes. In particular, instead of proposing a series of mathematical equations, each one of them valid for a single storm duration (see e.g. Alila, 1999), we used a Horton-type equation to describe the relationship between MAP and L statistics for all duration (see general comment in “SUITABILITY OF THE MANUSCRIPT”).

2) Does the paper present novel concepts, ideas, tools, or data?

Reviewer: “The developed model is a tool but not new to the science literature. It would however be of help to professionals for estimating design storms in this study area in Italy.”

We believe that the manuscript presents new concepts, new ideas, new tools and new data (see general comment in “SUITABILITY OF THE MANUSCRIPT”).

3) Are substantial conclusions reached?

Reviewer: “No - because the same relationships between L-statistics and MAP have already been reported in Brath et al. (2003).

We disagree with Alila because this is a completely different study and the proposed relationships between L-moments and MAP are totally new, and some of them conflict with previous studies (L-Cv and MAP for sub-hourly duration, see point [2] of our comment).

4) Are the scientific methods and assumptions valid and clearly outlined?

Reviewer: “Not applicable - In my view there is very little science here”.

This is provocative. No comment.

5) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Reviewer: “NO - miss use of the index storm approach”

See point [3].

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6) Should any parts of the paper be clarified, reduced, combined, or eliminated?

Reviewer: "There is no discussion or interpretation of the results in the context of processes and physical reality of the precipitation extremes in this manuscript. The manuscript is heavy on crunching a large data set through statistical tests and procedures with no reference to process understanding and physics of precipitation extremes".

This comment sounds out of the context of question 6). Also, we strongly disagree with the Reviewer because:

a) we believe that there is as much "discussion or interpretation of the results in the context of processes" as in other previous studies proposed by the scientific literature (see e.g., Alila, 1999). For instance as far as the results are concerned (i.e. statistical relationships between L-moments and MAP) Alila (1999) does not comment nor analyse from the physical viewpoint the hypothesis of a constant L-Cs for all of Canada (and we obtain an opposite result for a much smaller region) as the variability of L-Cv with the MAP. Probably (and we agree with the Reviewer if this is the case) our original manuscript lacks a detailed climatic characterisation of the study area. The revised manuscript will incorporate this comment.

b) Instead of proposing a plethora of different mathematical expressions (see e.g. Alila, 1999) we use the same mathematical expression for all duration.

c) The suitability of this particular relation is assessed through a series of extensive and objective Monte Carlo experiments (see also point [1] and [2]), and we believe that defining these numerical experiments "dataset crunching procedures" is reductive, to say the least.

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