

Interactive comment on “Introducing a rainfall compound distribution model based on weather patterns sub-sampling” by F. Garavaglia et al.

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Detailed response to the comments of referee 1

We want to thank referee 1 for his accurate and helpful review of our manuscript. In this author comment, we list how each of the remarks provided by the referee was addressed. The comments made by the referee will be referred as RC and printed in bold; the authors' comments and answers as AC.

1. Concerning the general comments

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AC: Your general comments may be addressed by four main topics.

Reference to the Extreme Value Theory. For us it was a way to link our work to the contemporary framework of the Extreme Value Theory. We fully agree with your reserve on its applicability in the hydrological field. As engineers our approach is essentially pragmatic and we may or may not use parts of this theory, always with statistical checks (robustness and accuracy) on actual data. We have to admit that these checks were insufficiently detailed in the submitted paper.

Compliance with i.i.d hypothesis. In relation with the Extreme Value Theory or not, it has been a main concern of our study. However we fully agree with the extreme difficulty to assess the homogeneity of our heavy rainfall samples. Our weather patterns classification provide a discriminating variable that allows seasonal rainfall records to be split into more homogeneous sub-samples, at least in term of meteorological genesis. Thus the homogeneity hypothesis can be checked indirectly:

- *A priori*, considering the discriminating power of the WP classification, it should be checked that the chosen classification minimizes deviation within classes, and maximizes it between classes. Another numeric criterion has been added in the revised paper (section 2.3 and Table 2): sum of intra deviation within classes / total deviation.
- *A posteriori*, regarding the strong variability of rainfall asymptotic behaviours induced by the WP sub-sampling (scale parameters in Table 3). On the contrary inappropriate sub-sampling would have produced randomly parsed samples of the whole record, with a rather uniform scale parameter for each sub-sample.

Comparison with a simple exponential model. We agree that systematic comparison with a simple exponential model will make the paper clearer.

Evaluation of our model in term of robustness and accuracy. We agree with the fact that a single example is not sufficient. Besides, the advantages and limitations of

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this approach are not sufficiently exposed (our initial purpose was to detail this pro and cons questions in a companion paper). We propose to improve this paper with statistical results computed on a wide dataset.

In summary reference to Extreme Value Theory and i.i.d hypothesis should not be seen as a justification of our work, but rather as a set of hypothesis we want to confront to our data. In this regard our study has two main objectives: firstly to present a model which, as you say, should be evaluated mainly by its results. Secondly precisely to show the limits of a naïve application of the EVT to solve hydrological engineering problems (in our case: difficulty of estimating the asymptotic behaviour of heavy rainfall samples).

1.1 RC: The idea is not new, but examples of its application are seldom presented in scientific papers.

AC: We moderate the connotation "new" in the revised paper.

1.2 RC: Nuances should be introduced in the theoretical justification of the approach. The fact that the sub-samples are "statistically homogeneous" cannot be demonstrated. It should be clearly presented as a hypothesis. Moreover, even if so, there is no good reason to think that the statistical distribution of each isolated sub-sample will better fit the predefined simplistic statistical models - if possible exponential - than the distribution of the compound sample. Extreme value theory is not a question here (see detailed comments).

AC: As mentioned above our approach is essentially pragmatic. The homogeneity hypothesis is in background of our work but does not condition explicitly our results. This is more a concern to be kept in mind than a justification for our study (see authors' comments above).

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1.3 RC: The efficiency or robustness of the proposed approach can not be demonstrated on a single example. If the paper is based on one single example of application, judgements on efficiency and robustness should be removed.

AC: We agree with your comment. We improved this paper with statistical results computed on a wide dataset of 478 rainfall chronicles (Figure below) spread on the southern half of France. We focus on robustness and accuracy with simple tests while comparing the MEWP distribution with the exponential and Generalized Pareto distributions.

1.4 RC: The example is not particularly well chosen: the highest observed rainfall values seem to belong essentially to one class (pattern 4) and one season (autumn). Why are so many efforts done to isolate these extremes when the standard approach consisting in selecting annual maxima or POT values would provide more or less the same sample? Can clearer examples be selected where the sample of the largest values has a clearly non-exponential distribution - which is not the case here - and where the distribution of sub-samples can be well approximated by exponential distributions?

AC: To address your comment some modifications has been introduced in the revised paper: Another example has been added (St-Etienne en Dévoluy, Alps) with nuances on the interpretation of MRL plots. Explicit comparisons with the exponential distribution have been added each time it was appropriate (for instance in Fig. 9). However in the presented case of Lyon, the scale parameter of an exponential distribution fitted above the same threshold (20.5 mm) jumps from 15 mm/24h (season-at-risk - Fig. 5B) to 19 mm/24h (WP4 in season-at-risk - Fig. 5c) justifying in this case the WP pattern sub-sampling. Anyway these figures were insufficiently explained in the initial paper.

1.5 RC: Confidence bounds are terribly missing, especially if robustness is an is-

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sue. Is there not possible to use Bayesian-MCMC inference techniques to assess inference uncertainties, especially for the proposed approach which is based on a larger set of data but involves also more parameters: the parameters of the sub-samples distributions but also the occurrence of each class! This increase of the number of parameters is a priori not a factor of robustness...

AC: We agree with your comment and followed your suggestion. We add the confidence bounds in the revised paper. The confidence intervals are computed using the bootstrap non-parametric method (Efron, 1979). In order to take into account the variability of the occurrence of each WP in the computation of bootstrap interval of confidence, we modeled this occurrence with a Poisson law. So for every bootstrap simulation, we extract randomly from a Poisson law the occurrence of the WPs. Concerning the MEWP distribution, the number of underlying parameters (within a season, a scale parameter for each 8 WP sample and eight WP relative frequency) may be view a priori as an issue for robustness. More results (confidence interval and robustness tests) will show that this is not the case.

1.6 RC: Moreover, I would appreciate more objectivity in the presentation of the method and results. The approach is interesting even if it partly fails. It is not necessary to demonstrate at all cost that it is revolutionary.

AC: Our paper surely reflects excessively our enthusiasm! To gain in objectivity, some elements to assess the strengths and the weaknesses of our method have been added, and the text has been reworded in the paper, especially section 4.2 and 4.4. Some important elements are underlined : precautions in the interpretation of MLR plots, one additional example, statistical diagnostics (confidence intervals, robustness and accuracy on a wide dataset).

1.7 RC: The comparison in Fig. 9 is not fair. There is no reason to fit a GEV dis-

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tribution to the whole data set and exponential distributions to the sub-samples according to the mean residuals life plots (Fig. 5) and to the empirical distribution (Fig. 9).

AC: In the Fig. 9, a GPD has been fitted on the Lyon seasonal rainfall data, not a GEV, and not on the whole data. Our original text (last lines of section 4.4) was confusing. The reference to GEV has been removed from the modified text. We agree that the MRL plot of Fig. 5B does not suggest us to use a GPD ($\xi \neq 0$) for the Lyon autumn rainfall. However, the purpose here was a sensitivity analysis of the estimated daily rainfall quantiles to maximum value recorded, with two different models. Obviously, the exponential model has to be part of this comparison, and it has been added in the Fig. 9.

1.8 RC: The compound distribution is only defined for intensities greater than 18 mm/day. This should be better explained and indicated on the figures. It does not reproduce the "bend of the empirical distribution in the range of observable frequencies" in the present case. The interpolated part of the distributions should be removed.

AC: According to your suggestion the interpolated part of the distributions has been removed in the figures 8 and 9 of the revised manuscript. Without it, i.e. over the greatest threshold value of all WP sub-samples, a bend of the MEWP distribution is still noticeable (see answer comment 1.4), and even more for the new example that has been added to the article.

2. Concerning the specific comments

2.1 RC: In the introduction, a large emphasis is put on extreme value theory as if it was obvious that the distributions of the largest observed hydrological

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data, which according to the sampling methods and the depth of observation are mainly composed of "frequent" data, were necessarily well approximated by one of the extreme value distributions. Of course it is not the case. The extreme value theory is an asymptotic theory and data set heterogeneity is only one of the possible reasons for quantile underestimations, the first cited by Coles et al. (2003) being the ignorance of statistical model itself. Even if statistically homogeneous, there is no reason why the distribution of data sub-samples should be better approximated by a GEV distribution than the distribution of the whole sample. The justification of the approach based on extreme value theory is absolutely not convincing and really naïve to take the words of Coles et al. (2003). This introduction has to be reworded in a more conditional form: this work is a test of a method that may provide more accurate estimates for this and this reason...

AC: We try to answer to this comment with a new redaction of the introduction, which could take the following form :

"To produce correct estimation of extreme rainfall quantiles, several solutions based on the extreme value theory use an asymptotic model to describe the stochastic behaviour of extreme value processes. Standard methodology for modelling extremes is based on the hypothesis of independence, stationarity and homogeneity. According to Coles et al. (2003), a false assumption of model homogeneity is one of the reasons that can lead to a wrong estimation of extreme events probabilities. The standard approaches based on extreme value theory use generalized extreme value (GEV) distribution or generalized Pareto (GP) distribution, face the difficulty of locally estimating the shape parameter on the basis of point data (Koutsoyiannis, 2004). To deal with the lack of robustness of the local estimation of a shape parameter a solution is to use a regional approach.

In order to improve robustness without losing accuracy in extreme rainfall estimation, we propose an alternative approach using a classification of atmospheric circulation

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patterns. These weather patterns (WP) provide a discriminating variable that is consistent with French climatology, and allow seasonal rainfall records to be split into more homogeneous sub-samples, in term of meteorological genesis. An exponential POT model is used to fit the distribution of each sub-sample. The distribution of the multi-exponential weather patterns (MEWP) is then defined as the composition, for a given season, of all WP sub-sample marginal distributions. The statistical characteristic of each sub-sample and the compound distribution are analysed for two selected rain gauges. Some tests are ran on a wider dataset to assess the robustness and the accuracy of the proposed model."

2.2 RC: Line 30: how is it possible to evaluate that the sub-samples are homogeneous from a statistical point of view? It is, I think, not possible!

AC: See authors answer to referee general comments.

2.3 RC: Line 47: replace Lyons by Lyon also in the rest of the manuscript.

AC: Lyon replaced Lyons in all the manuscript.

2.4 RC: Line 60: the classification of weather patterns used is oriented toward the detection of heavy precipitation situations unlike what is said in the manuscript since it has been calibrated to provide quantitative precipitation estimates. The comments on lines 114-120 are very welcome.

AC: The meteorological variables (geopotential fields at 700 and 1000 hPa) and their analysis window (Fig. 2) have been optimised in works on quantitative precipitation forecast by analogue method. It is therefore legitimate to use these variables, well adapted to the explication of rainy situations over our area of interest, in our classification process. In this process (step 1 and 2), for the considered rainy days, local rain

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depth is normalized by the average precipitation of the day, as a way of considering the "shape" of the rain field more than its scale. In other words, rather than using the *how much does it rain* information, we use the *where does it rain* information in our process. So the classification is clearly oriented toward precipitation (chosen variables, analysis window, initialisation with HAC of rainy day) but not considering their *heavy* status. We propose a clearer text about this.

2.5 RC: Part 3.1: the whole presentation is disturbing me (see comment 1). Line 167: the selected observations, annual maxima for instance, can not be considered as "extreme" values, at least not in the sense of the extreme value theory.

AC : In the revised paper, "heavy precipitation" apply to the highest observed quantiles, instead of "extreme" ones. More generally, "extreme" has been considered as an asymptotic status, almost never observed.

2.6 RC: Line 183: replace "true" by "real".

AC: It has been changed in the revised manuscript.

2.7 RC: Line 186: if heavy rains "most likely occur during fall", then a simple extraction of maximum values will predominantly contain fall season events and the calibrated extreme distribution will be controlled by these fall season events. What is then added value of a seasonal or by event type analysis (see also next comments)?

AC: Considering the Fig. 5, we agree that, in the case of Lyon, the exponential behaviour is noticeable in the MLR plots over a threshold around 20 mm for the global population (Fig. 5A), the fall sub-sample (Fig. 5B) and the WP4 within fall sub-samples (Fig. 5C). However, the corresponding scale parameters are respectively 13 mm/24h,

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15 mm/24h and 19 mm/24h. The "added value" of the proposed sub-sampling is not in this case the asymptotic behaviour detection (exponential in the three cases), but in the discrimination of the asymptotic level, expressed by the scale parameter.

Line 192: if we are interested in the extreme values, the separation in seasons must rather be based on the higher bound of the boxplot. The highest are observed between May or June and November. The lowest are not observed between May and August but in winter: please correct this.

AC: We agree on this. Two different seasons have been proposed: December-May and June-November. The sentence about the lowest quantiles is wrong. The lowest quantiles are not observed between May and August but in winter. This sentence has been corrected in the revised paper.

2.8 RC: Line 221: "according to the asymptotic theory... can be fitted with a GP distribution". I disagree totally with this statement. What element can be put forward to show that the asymptote is close???

AC: It was not a good formulation. Our proposition is :

Within section 3.1: "According to Coles (2001), a POT sample may be regarded as independent realizations of a random variable whose distribution can be approximated by a member of generalized Pareto distribution..."

Then the line 221 would be rewritten: "As mentioned before, asymptotic behaviour of POT values of a daily rainfall sub-sample of season i and WP j , may be approached by a GP distribution".

2.9 RC: Part 4.2: The conclusions drawn from fig 5 are completely subjective! The authors conclude what they want to conclude. The residual life plot is much

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more "horizontal" for the whole data set than for the sub-sets. It is particularly hard to consider it as horizontal for WP4, which is the data set which controls the shape of the distribution for the higher quantiles... An exponential model could also have been chosen for the complete set to be consistent.

AC: We agree with your remark (see comment 2.7) with the example of Lyon. A different example has been provided and the section 4.2 has been expanded with more commentaries. The Mean Residual Life (MRL) plot, is considered by various authors as an appropriate tool for the threshold selection. We agree that the graphical interpretation of an MRL plot may appear as subjective. However, in our study, it has been used to illustrate how the vision of the asymptotic behaviour of a given population may be dependent of the chosen stratification level (global, season, season and WP). The MRL plot is supposed to help to determine the asymptotic behaviour of the underlying distribution, but we see how far the final diagnostic can depend on the chosen sub-sampling. In other words, the asymptotic behaviour might be exponential, but a standard sub-sampling (e.g. records from whole year of whole season) might completely mask it. Furthermore, given the hypothesis of an exponential asymptotic behaviour, a standard sub-sampling might lead to an underestimation of the scale parameter.

2.10 RC: Line 249: the fact that the sub-samples are more "homogeneous", from a statistical point of view I suppose, is absolutely not demonstrated!:

AC: See authors answer to referee general comments.

2.11 RC: Line 250: the probabilistic model is more parsimonious is not linked to the proposed method but to the arbitrary choice of a shape parameter equal to 1.

AC: We agree with this. Anyway in our study, the choice of an exponential model for

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POT samples has not been arbitrary, but motivated by the MRL plots, robustness tests etc... Besides this can be seen as part of the method: only the WP sampling make it possible to fix a shape parameter equal to 1.

2.12 RC: Line 251: " To provide a continuous probabilistic description... the CDF of each sub-sample is extended below its threshold by a linear interpolation of empirical quantiles ". It is very important to have mentioned this fact. Strictly speaking, the fitted distribution does only hold for intensities exceeding 18 mm/day. It would have been preferable for the soundness of the paper to draw only this distribution on figures 8 and 9.

AC: See authors answer to referee general comments 1.8.

2.13 RC: Line 270: "Being the WP... ". This holds for autumn but not for the summer which is not too far from the autumn season (fig 8). Is there not a problem in the proportions indicated in table 3? Or is a distribution fitted for each WP and each season, which would reduce significantly the usefulness and meaning of WPs?

AC: This sentence ("Being the WP...") has been suppressed from the revised text. To be clearer, only two seasons has been distinguished (see answer to comment 2.7). Proportions in Table 3 have been computed according to the new seasons. With this new presentation of the Lyon example, the discrimination brought by both seasons and WP within season is clear.

2.14 RC: Line 295: the bend is significant for return periods significantly lower than 1 year, generally not taken into account in statistical adjustments and neither in this case since the real threshold is 18 mm/day (return period of about 1 year). Since this bend corresponds to the range of values for which "a linear

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interpolation of empirical quantiles" is used, the proposed approach does not really represent it!

AC: See authors answer to referee general comments 1.8.

2.15 RC: Line 307: The proposed adjustments should be compared with the adjustment of an exponential distribution of the maximum annual values or POT values exceeding 18 mm/day. The "robustness" - I do not like this ambiguous term- of the approach is not linked to the approach but to the constraint introduced in fixing the shape parameter value of the GEV equal to 1 (i.e. imposing an exponential model). The four adjustments can not directly be compared.

AC: An exponential model has been added to this comparison (see answer to referee general comments 1.7).

References

Coles, S. An introduction to statistical modelling of extreme values. Springer, London (2001).

Coles, S., Perricchi, L. and Sisson, S.: A fully probabilistic approach to extreme rainfall modelling. *Journal of Hydrology*. 273, 35-50 (2003).

Efron, B.: Bootstrap methods: Another look at the jackknife. *The Annals of Statistics*. 7, 1-26 (1979).

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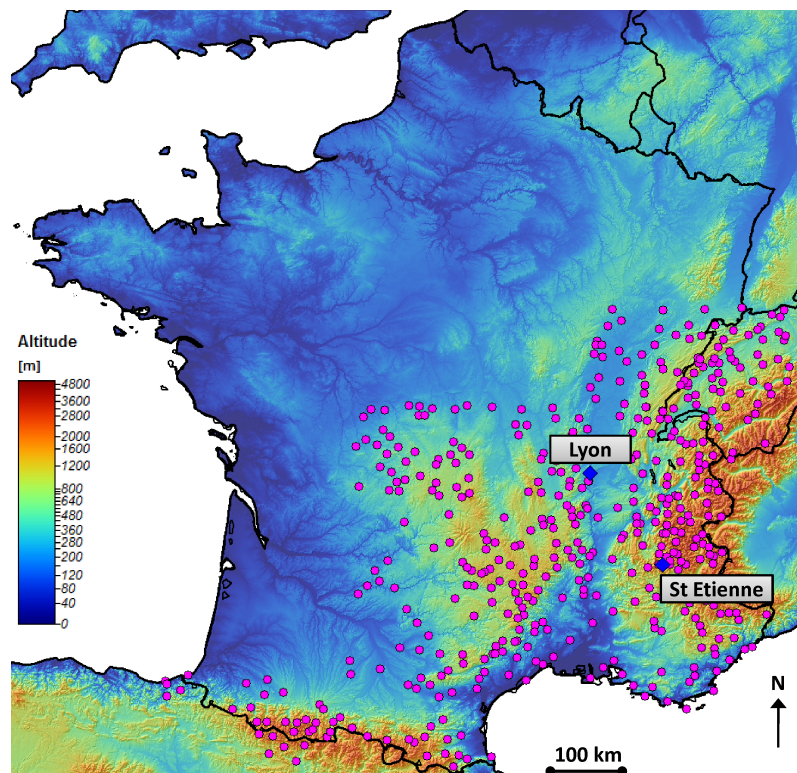


Fig. 1. Location of the 478 rain gauges used in this study (Lyon and St-Etienne en Dévoluy rain gauges are highlighted).

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