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Hydrology and Earth System Sciences Discussions

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 2

We want to thank referee 2 for the 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: We agree with your general comments. First of all we moderate the connotation "new" in the revised paper. Furthermore we agree with the fact that the advantages and limitations of 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 of 478 rainfall chronicles (Figure 1 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.

2. Concerning the specific comments

2.1 RC: Page 317, line 3: How representative are these 54 rainfall gauges for France? It would be interesting to see the spatial distribution of the 54 gauges, eg in relation to the French topography, and to have a statement on the representativeness of French rainfall by these gauges. 54 stations seems a small number given the considerable spatial extent and variation in terms of rainfall for France. If the points in the left diagram of Fig. 1 are the rainfall stations considered, then the spatial distribution is strongly biased towards SE France. I am not familiar with the French climatology, however, I doubt that this distribution of stations gives an adequate picture of the country-wide rainfall.

AC : The rainfall stations are indeed the ones plotted in the left diagram of Fig. 1. We will respond on this comment in two parts: one addressing the question of the global location (SE France), one addressing the question of the number (only 54 gauges).
Global location of the gauges: it is true that our study area is oriented on SE France. This is due to the fact that our company (EDF) interests are mainly in the mountainous regions (Alps, Pyrenees, Massif Central) while speaking of rainfall and flooding risks analysis. We agree that it is not clear in the paper. We introduced a more precise presentation in the revisited paper.

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Only 54 chronicles: We voluntarily didn't get into details but this 54 rainfall chronicles are daily areal precipitations (it means each time the average of several rain gauges chronicles), already used in previous works (Obled et al 2002). Nevertheless, we agree to the fact that it would have been more natural to work on a wider dataset (for instance the 478 rain gauges mentioned above). But the fact is that this is the dataset we used initially (in 2004) to compute our classification. We computed later a new classification based on the wider dataset leading to slightly different results (neither better nor worst). We then preferred to keep our initial classification, mainly because we had begun to use it intensively in various studies and contexts (among them a PhD Thesis - Gottardi, 2009).

2.2 RC: Page 317, line 5: What does this statement exactly mean? Is a certain day identified as rainy day, if at least one gauge measured 5 mm?

AC: No. The global average computed on the 54 chronicles has to exceed 5 mm (it has to be a "rainy" day in a somehow spatial meaning). We have been more precise in the revisited paper.

2.3 RC: Page 317, line 16: I do not completely understand why steps 3 and 4 are necessary. I suppose that the result of step 2 is the classification of rainy days into seven clusters, i.e. in this step each rainy day is already assigned to one cluster. If this is true, why then step 3 and 4 with an additional assignment?

AC: The result of step 2 is indeed the classification of rainy days into seven clusters, but remind that this concerns only 21% of the days. A "non-rainy" day (with our criteria) may be an important rainy day for a specific rain gauge (in case of strong located precipitation). That's why we want to be able to classify all the days in our process and this is the main reason of steps 3 and 4. The second reason is that it is easier to rely on meaningful atmospheric homogeneous archives (such as the NCEP or ERA 40

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ones), than to rely on rainfall archives that are more difficult to keep homogeneous (for instance when one rain gauge is stopped). This is important when you want to keep your classification "alive" (update the values for 2009, 2010, etc...).

2.4 RC: Page 318, line 3: What does it mean: "...ratio of the mean WP to global mean precipitation...". Please give an unambiguous definition.

AC: A new redaction has been introduced : "Figure 2B shows the corresponding relative precipitation fields (i.e. ratio of WP mean to "all-days" mean precipitation) over Western Europe". For instance, a value of 4 for a given WP means that the average rainfall for this WP is four times greater than the global average rainfall. These figures are computed on "all days" (being rainy or not) of the concerned populations.

2.5 RC: Page 318, line 20: I recommend to rename this section. Instead of Discussion something like 'Suitability of the proposed weather pattern classification'.

AC: We agree with your comment. This section has been renamed in the revised manuscript following your suggestion.

2.6 RC: Page 320, Line 19: I am not completely convinced that the independence criterion is sufficient. If I understand the criterion correctly, it allows 2 days to be considered as independent, even if they are only separated by a rainfall day in between with smaller rainfall than these two days. Since circulations patterns apply over a few days, this criterion might be too weak. Please expand this issue.

AC: This central rainfall (CR) sampling is closely linked to the rainfall-runoff simulation process part of the SCHADEX method. This has been mentioned in the revised paper. However, to check the independence hypothesis of this kind of re-sampled rainfall time series, the first order lag autocorrelation coefficients were computed for

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a wide dataset (478 chronicles). The median autocorrelation coefficient is 0 for AM (annual maxima) and POT sampling methods, 0.07 for CR (Central rainfall) sampling method and 0.23 for daily time-series. Furthermore, we therefore select POT values of "central rainfalls", above the 70% quantile of each WP sub-sample. We could although have directly taken all the days (it means without the CR sampling step). This would have lead to a different POT threshold quantile (something like 83%). But for us it is important to keep close to our global rainfall-runoff simulation process (SCHADEX method) that will be explained in future papers (for more details see http://meetingorganizer.copernicus.org/EGU2010/EGU2010-5207-4.pdf).

2.7 RC: Page 321, Line 26: You say that seasonal divisions may vary considerable from year to year. Should this interannual variability be taken into account? And if yes, how could it be taken into account?

AC : This sentence has been removed in the revised paper. We wanted to say that, in some places, the occurrence months of heavy rainfalls can show some variations, depending of the year of record. Anyway, on a consistent record (more than 30 years), the seasonal divisions have a true climatological sense, i.e. the highest quantiles of rainfall occur more likely during a period on several months we call "season-at-risk" (see comment below). This seasonal division has spatial coherence too.

2.8 RC: Page 322, Line 1: I do not understand this sentence: Why are the lowest quantiles between May and August? Please explain: What means "...Season-at-risk..." in Fig. 3. Similarly, give a definition of WP-at-risk (used at other locations in the manuscript).

AC: Thank you for this comment. 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. The season at risk is the season

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during which the highest rainfall quantiles are observed. So it is the period of the year with the higher rainfall risk. Similarly, the WP at risk is the WP during which the highest rainfall quantiles are observed. Concerning the MEWP probabilistic model, we can also define the WP at risk within a given season as the WP associated with the greatest scale parameter (see numbers in bold in Table 3). These definitions have been added into the text and within the caption of Fig. 3 and Fig 4 in the revised paper.

2.9 RC: This section is partly a repetition to text/arguments earlier in the manuscript. I recommend to concentrate the literature review on homogeneity of samples and on underlying processes in one section (early in the manuscript). Then sections 3.2 and 3.3 should be combined.

AC: We agree to concentrate the literature review in section 2.1. However, we prefer to keep the sections 3.2 and 3.3 separate. Indeed, the 3.2 section talks about what is usually done (seasonal sub-sampling), while section 3.3 propose a new level of stratification (WP sub-sampling).

2.10 RC: Page 324, Line 21-23: Please elaborate these two sentences. What exactly do you mean with '...provides a new view...'. What is the interpretation of the MRL plots? Does the comparison of the plots in Fig. 5 suggest that seasonal stratification within one weather pattern is superior to a non-stratified approach?

AC: 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

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sub-sampling. In other words, the asymptotic behaviour might be exponential, but a loose 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 loose sub-sampling might lead to an underestimation of the scale parameter: for instance Lyon scale parameter going from approximately 15 mm/24h (season-at-risk) to 19 mm/24h (WP4 in season-at-risk), in Figure 5. At this step of the paper we didn't want to speak in term of "superiority", but only in term of "difference". The possible superiority of the proposed stratification has to be checked on a wide dataset in terms of robustness and accuracy.

RC: If such conclusions can be drawn from these MRL plots, would it be interesting to see more comparisons (or on which criteria have you selected the examples in Fig. 5 and 6)? The MRL plots are central to the argumentation of the paper and the selection of examples and the conclusions drawn are not convincingly demonstrated.

AC: We agree with your remark. The section 4.2 has been expanded with more commentaries and with one more example. This example is not the "best one" in our dataset (nor the worst of course). We chose Lyon because it is the second largest French city, and this data is available on line so people may experiment this kind of treatment on it. A location like "Cuges-les-Pins" (one of the best of our dataset), although typical, would have been more difficult to publish ! Concerning the choice of the three WP in Figure 6, they are the three WP with the higher scale parameters (the more important for the asymptotic behaviour of the compound distribution).

RC: What exactly do you mean with "...parsimonious effect..."?

AC: This "parsimonious effect" is detailed in this sentence in the section 4.4: "For high and extreme quantiles (currently over 50 years of return period), the asymptotic

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behaviour becomes exponential, and is fully parameterised by the scale parameter λ_j^i and the relative frequency p_j^i of the WP at risk and the season at risk". But as mentioned above, the possible superiority of the stratification will be checked later in the paper on a wide dataset in terms of robustness and accuracy (see general comment). This sentence has been removed in this paragraph and discussed later.

2.11 RC: Page 327: The robustness of the MEWP is demonstrated by one example only (using the autumn sample). It is necessary to see other examples, in order to understand if the robustness of MEWP is consistently higher compared to the standard GP approach.

AC: See authors answer to referee general comment 1.

3.Concerning the technical issues

3.1 RC : Page 320, Line 18; Please displace " ...time space..." by "...time period..."

AC: It has been corrected in the revised manuscript.

3.2 RC : Page 320, Line 19: Please delete "... begin to..."

AC: It has been removed in the revised manuscript.

3.3 RC : Page 321, Line 13: Sentence ("...among which variation...") seems incomplete

AC: The sentence has been modified in the revised manuscript.

3.4 RC : Page 321, line 26: Why an "s" in "...Lyons...". (Similar at other locations

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in the manuscript.)

AC: Lyon does replace Lyons in all the manuscript.

3.5 RC: Page 322, line 8: '...A discrimination...': Use capital letter

AC: It has been corrected in the revised manuscript.

3.6 RC: Page 324, Line 10: Move '...respectively...' to the end of the sentence: "...within the autumn season, respectively...".

AC: It has been corrected in the revised manuscript.

3.7 RC: Page 326, Line 3: use "autumn" instead of "fall" as it is done at other locations in the manuscript

AC: It has been corrected in the revised manuscript.

3.8 RC: Page 329, line 5: "...A comprehensive...": Use capital letter

AC: It has been corrected in the revised manuscript.

3.9 RC: Page 338, Fig. 2: Figure is rather small; country borders are hard to recognize. Please give explanation of grey rectangle and the arrow in the figure caption (in addition to the explanation in the text).

AC: This figure and caption has been modified following the referee's suggestion.

3.10 RC: Page 340, Fig. 4: Please explain: What means "...WPs-at-risk..."?

AC: See authors answer to referee comments 2.8. The caption of this figure has been

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modified.

3.11 RC: Page 340, 341: I recommend to combine Fig. 5 and 6 in one figure.

AC: The Fig. 5 has been modified in the revised manuscript. The example of another station (St Etienne en Dévoluy) has been added to Lyon's example in Fig. 5. So we prefer not to combine the two figures.

3.12 RC: Page 342: Figure is too small, can hardly be recognized.

AC: This figure has been modified following the referee's suggestion.

References

Gottardi, F.: Estimation statistique et réanalyse des précipitations en montagne, PhD Thesis. Polytechnic Institute of Grenoble, Grenoble, France, 252 pp., 2009.

Obled, C., Bontron, G., and Garçon, R.: Quantitative precipitation forecasts: a statistical adaptation of model outputs though an analogues sorting approach, Atmos. Res., 63, 303-324, 2002.

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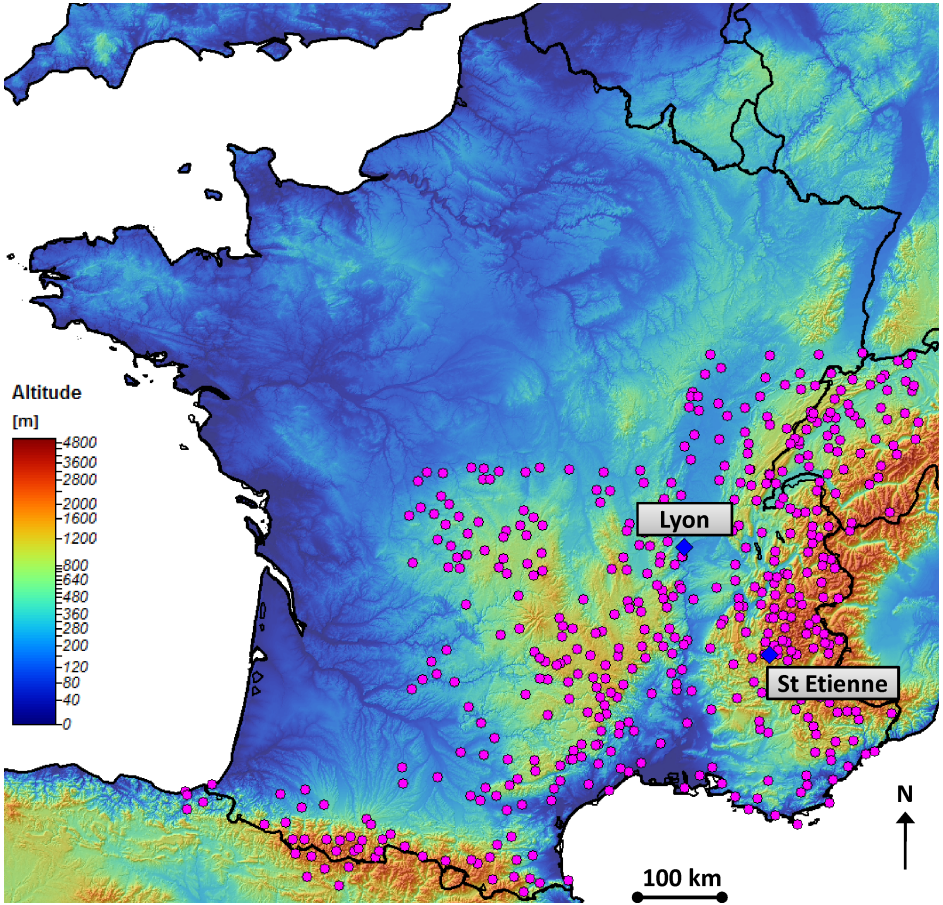

Fig. 1. Location of the 478 rain gauges used in this study (Lyon and St-Etienne en Dévoluy rain gauges are highlighted).

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