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Hydrology and Earth System Sciences
Discussions

Interactive comment on "On the quest for a pan-European flood frequency distribution: effect of scale and climate" *by* J. L. Salinas et al.

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Response to Anonymous Referee 4

The authors are thankful for the very detailed and structured review the referee has done. Corrected manuscript(s) will be uploaded within the next days, including the changes cited below. The original referees comments will be formatted in *italics*, and the authors' response in **bold**.

Following the reasons exposed in the response to Referee F. Laio (a considerable amount of additional analysis has been performed on the "first part" of the paper, in particular a new set of Monte Carlo simulations taking into account, among others, the effect of sample length; new plots, tables and subsections

are included in the new manuscript that could lead the reader to lose attention in the final part), the authors have decided to split the manuscript in two parts, which are more balanced in length and content:

Regional parent flood frequency distributions in Europe – Part 1: Is the GEV distribution a suitable pan-European parent?

Regional parent flood frequency distributions in Europe – Part 2: Climate and scale controls

The authors think that now the two parts have even more differentiated and direct science questions with independent conclusions and take home messages.

From the technical point of view (following instructions of the Ms Topfer from the Copernicus editorial team), the HESSD discussion of the paper that is being reviewed now will continue, and as "post-referee review corrected manuscript", the two parts paper will be submitted. The final decision will be taken by the handling editor.

1. Title: I found the title a bit too general given the actual content of the article. Only mean annual precipitation is investigated as climate descriptor. Maybe the title could be more specific, e.g. writing "effect of catchment size and mean precipitation"

This refers to the title of Part 2 paper. Even though the authors agree on the fact that mean annual precipitation (MAP) might be a lumped parameter, there are several reasons why this could act as an aggregated surrogate for a variety of climatic controls on flood regimes. As stated in references included in the paper (Bloschl and Sivapalan, 1995; Sivapalan and Bloschl, 1998; Sivapalan et al., 2005), mean annual precipitation acts as control of probabilistic behavior of floods through its effect on antecedent soil moisture conditions and is also well correlated with precipitation extremes . Even in Merz and Bloschl (2005), in a multiple regression approach, MAP is preferred as explanatory variable (not

always, but in some cases) to mean daily maximum precipitation. Overall, the authors prefer to keep the "scale and climate" notation, and explain better in the text why these have been chosen, in particular MAP as aggregated surrogate for a set of climatic controls.

2. p. 6324: Write: "Two Component Extreme Value (TCEV)" → **Corrected**

3. p. 6326, section 2.1: The homogeneity of the samples on the 4105 sites could be shortly commented, especially in terms of sample length.

Following the suggestions of the referee F. Laio, a more detailed investigation of the effect of sample length on the data vs. simulations was carried out. Also, a histogram of the sample lengths distribution is included and commented in the corrected version of the manuscript.

4. p. 6326, section 2.2: Maybe the information of advised distributions in each country could be added to Table 1, to give a more systematic overview.

Another manuscript is currently under preparation, which focuses exactly on the survey details, concerning among others the advised distributions and methodologies in each country is being prepared. The authors think that including this information in either one of the two revised manuscripts is not necessarily functional nor critical for addressing objectively whether the GEV can be accepted as a pan-European parent. Since the manuscript under preparation has not been submitted yet (it will be shortly, though), we preferred not to cite it, but we are certainly open to cite it as Kohonova et al. 2013 [manuscript under preparation for Hydrological Science Journal] if the Handling Editor recommends doing so.

5. p. 6327, l. 5: Is there any reference on the DIST software? → **The reference to the DIST software has been removed from the text, as it was not necessary or helpful.**

6. p. 6333: The authors could shortly explain why these three countries were selected

here.

The choice of this subset of the entire database is due to the minimum requirements set when compiling the full European database, where only an agreement on sharing the L-moment-ratios was reached. This means that even the very basic catchment descriptors were not available for all the countries, at least for all the stations with L-moments provided. In the second part of the paper, the subset from the countries of the authors' core team is used, as they could provide, at least, MAP and area. A short explanation is added.

7. p. 6333: If available, it would be good to have a bit more information on the catchment sample used, especially in terms of hydrological regime. Which proportion of catchments are Mediterranean? Snow dominated?, etc.

The reviewer raises a good point here, brief information on the dominant hydrological regimes of the subset of catchments considered for the analyses will be included in Part2 of the manuscript.

8. p. 6333: Do the authors have information about the median altitude of catchments and fraction of snowfall in total precipitation? If yes, this could be added in Table 3. If snow plays a major role in some catchments, do the authors have information on the uncertainty linked to the MAP estimates (MAP may be underestimated in snowy catchments due to snow under-catch and/or low raingauge density in high altitudes).

The future lines of research will surely include other catchment descriptors, but for the moment the authors prefer to analyse the effect of area and MAP and acknowledge the influence of other factors like snow (via e.g. elevation and temperature) which are of interest in the given database, where the presence of mountain catchments in significant. An explicit reference to this research outlook is included in the conclusion of the corrected manuscript.

9. p. 6333, l. 14: Why only 282 gauges from Italy were used while Table 1 indicates

that 373 were available?

These reductions correspond to the fraction of Italian stations were both area and MAP were provided at the time of the analysis. This discrepancy is commented with a sentence in the corrected manuscript.

10. p. 6334: The choice of catchment area and mean annual precipitation could be shortly justified. What motivated the authors for this choice (a priori hydrological relevance? previous studies? descriptor availability?). Actually one may question the use of MAP since other descriptors of rainfall may be more relevant (precipitation quantiles, quantiles of cumulated precipitation over a given time window, seasonal variability of precipitation, etc.). Was this analyzed in a prior study?

This question is linked with the answer to point nr.1; the two catchment descriptors were chosen fundamentally for their easy accessibility, but also because there seems to be a general agreement in the scientific community on the very important morphological control of scale (via catchment size) and of climate (via MAP, correlated with both precipitation extremes and maybe more important to antecedent soil moisture conditions) on flood regimes. These facts are already acknowledged at the discussion section and will be explicitly highlighted also in the section that describes the dataset in revised Part2 of the manuscript.

11. p. 6334: As rightly mentioned by the authors, the categories made according to size and mean annual precipitation are relative to the sample used. Therefore the terminology may be less ambiguous if the authors used relative terms: "smaller" / "larger" and "drier" / "wetter". This would avoid confusion with more absolute scales based on worldwide classifications.

The reviewer offers a good solution here, to the nomenclature of the categories. Names are changed to "smaller" / "larger" and "drier" / "wetter" in text and figures of the corrected manuscript Part2.

12. Discussion: Would the graphs provided by the authors bring additional information if they had been done on sub-samples made by typical climate conditions (Mediterranean, continental, snow-dominated, etc.)? Would this help further explaining the quite large variability shown on some of the graphs?

This is a very valid point, with which the authors agree. The possibility of stratification with respect to hydrological regimes, when available, is commented in the corrected manuscript.

13. Conclusion: The authors could more precisely discuss which other specific climate or physical descriptors could be useful to consider, if available, to better characterize flood generating process. For example, could the occurrence of some weather patterns based on geopotential be useful to consider?

Connected to the reply on point nr.8, the future lines of research will surely include other catchment descriptors, which have been found to have a significant effect on flood regimes, such as elevation, aridity index, land use, ... **The use weather patterns based on geopotential has not been considered, as it is event characteristic, not catchment characteristic. Some kind of "preferential" weather pattern could be defined on a catchment base, but this would require for this large database, on the authors perspective, a much greater amount of work (define weather patterns for each event) than working with other proxies e.g. mean flood seasonality.**

References

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 6321, 2013.