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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.

J. L. Salinas et al.

salinas@hydro.tuwien.ac.at

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Response to V. Iacobellis (Referee)

The authors are thankful for the well review the referee did, and in particular for pointing out to literature with very similar findings that the authors missed. Corrected manuscript will be uploaded in 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. My first concern was already raised by reviewer Francesco Laio and is entangled with the question: "Do we really need a pan-European flood frequency distribution ?". Following the EU Flood and Water Framework Directives this may appear necessary, or "at least" useful, at the district level. Nevertheless the various typologies of districts that the different EU member states have created since 2000 is by itself the evidence of the heterogeneity observed in Europe.

The authors fully agree with the reviewer. The title and introduction could be at some point misleading, as the main outcome of the first part of the manuscript is to reject the GEV as a single pan-European frequency distribution. Also, some examples of the flood process in Europe from the literature are already addressed in the conclusions, making clear that we do not necessarily need one single pan-European frequency distribution; this fact is now also stated explic-

itly in the introduction. The title of the Part 1 manuscript is also less ambiguous in this sense ("Regional parent flood frequency distributions in Europe – Part 1: Is the GEV distribution a suitable pan-European parent?")

2. Should we find a common pan-European parent distribution, this would be a consistent result from the scientific point of view but still we should have to investigate and assess the spatial variability of the involved parameters.

It is now stated clearer that we do not need one single pan-European frequency distribution, and that the existence of it is rejected, so there is no need to discuss the spatial variability of the parameters.

3. Despite some not-conclusive considerations reported in the first part of the discussion in section 3.4, the general conclusion of the paper seems to be the rejection of the GEV as a parent for a pan-European flood frequency distribution. This conclusion seems reasonable to me, nevertheless, being this basically the result of the Monte Carlo simulations described in section 3.3, I believe that more space could be devoted to the description of the choice of the distributions used to represent L-Cs and record length. Moreover, considering the extension of the available database I wonder if somehow different results could be found if using the empirical distributions of L-Cs and record length.

In line with the comment nr.3 of referee F. Laio, the Monte Carlo simulation strategy has slightly changed, in order to address the effect of sample length. The authors hope that the new description of the procedure, and in particular the choice of the distributions is more clearly discussed now.

4. An important topic which is not addressed in the paper and I believe that should be at least commented according to the authors' feeling, regards the evaluation of the error in prediction that could descend from using the pan-European GEV obtained from the WMA obtained by averaging the 200 neighbouring L-Cs values which is not rejected for some ranges of L-Ck and L-Cs values (see table 2). Also, I believe that it is important

to underline that such GEV parent fails to pass the test for the higher quantiles of L-Ck, those that in principle, at the at-site level could be significantly affected by the presence of outliers, thus leading to a possible underestimation of the predicted discharge for high return period.

A couple of sentences are added, with respect to the implications in design value, underestimation and overestimation of flood quantiles and the effect of model choice derived from the results of the analyses.

5. In section 2.1, the description of the database, reported also in table 1, highlights the presence of a consistent number of sites with only daily flows, compared to others with instantaneous flows. This presence is not addressed in the paper when discussing results. Considering that the statistics of floods could be significantly affected by daily averaging, I wonder if the authors tried to make separate evaluations for daily and instantaneous data.

Unfortunately this division has not been formally done in the analysis, but one sentence is added on the possible effects of daily averaging.

6. I do not agree with the other reviewers that raised the point of separate and different topics in the first and second part of the paper. I think that addressing the existence of the pan-European flood frequency distribution and, then, providing a deeper investigation by means of two main descriptors (MAP and basin area) is a straight choice. May be that the two parts just have to be better assembled. For example by merging the two discussion sections 3.4 and 4.4. On the other hand, I agree that the choice of the different dataset has to be better explained. Why, for example, not studying one of the European cross-boundary river basins?

As pointed out at the beginning of the response, the authors have, for a series of reasons (for more details see response to Referee F. Laio), to split the manuscript in a 2 parts paper. One of the reasons was the difference between the two datasets used in the different parts. 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. With regard to the second part of the paper I only wish to raise a couple of points. While one may agree on the general authors' observation of decreasing L-moments with increasing Area and MAP, according to results shown in figure 4, I would like to better focus on Figure 4a (L-Cv vs Area). Looking at grey dots I see that they seem to show not a general decrease but an ascending-descending behavior with a maximum located around basins of 100 kmËĘ2. This behavior is basically masked by the WMA but has been already observed in other, less extended, databases and also somehow explained (see for example Blöschl and Sivapalan, 1997; Iacobellis et al. 2002). Finding it here is important. Also the increase of L-Cv (or more in general of the distribution dispersion) with aridity has been observed by many researchers, I would mention at least Farquharson et al. 1992.

Data-based exploratory analysis are always subject to some degree of subjectivity, but the authors agree with the referee, that on the envelope of the points this ascending-descending behavior is observed (maybe not at WMA stratified by precipitation classes, which is also a significant fact), and it is relevant that this fact was previously found by other authors. It is acknowledged in the corrected manuscript. Farquharson et al. (1992), is also cited in the corrected manuscript, with the assumption of a correlation between MAP and aridity (which is not necessarily true).

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