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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "A data-based comparison of flood frequency analysis methods used in France" *by* K. Kochanek et al.

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A data-based comparison of flood frequency analysis methods used in France

by

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The answer to reviewer Francesco Serinaldi.

The authors would like to thank Professor Francesco Serinaldi for his valuable hints and critics. We hope that the alterations made to the text with the answers stated in this document will satisfy the Reviewer.

Comment: In this study, the Authors assess the reliability of eight different approaches that are commonly used in France to perform ïňĆood frequency analysis (FFA). In particular, they compare the classical FFA based on parametric distributions directly ïňĄtted to discharge data (annual maxima) and a continuous simulation approach involving a rainfall generator and a rainfall-runoff model. Moreover, the impact of using local, regional, and a combination of local and regional information is also taken into account. The comparison is performed by a set of statistical procedures proposed by the Authors in previous papers and comprising a reliability index and a stability index. The analysis relies on a large data set covering the whole France. The paper is well written and technical details are properly described and/or referenced. In my opinion, the results are valuable and of interest for a wide audience. Even though the ïňĄnal message (the usefulness of combining local and regional information) in not fully new, this study conveys it effectively with a clear and concise language, using well devised diagnostics and summary statistics easy to be understood and interpreted by researchers and practitioners. In this respect, the paper will surely contribute to the claimed homogenization of FFA procedures in France. I suggest the publication after some minor (mainly editing) changes, which the Authors are free to incorporate or not.

SpeciïňĄc comments

P4447L5: maybe "plants" instead of "plant".

P4449L5: "results of a nation-wide".

P4449L13: "Conclusions".

P4450L10: maybe "for estimating extreme values" or "for estimation of extreme values".

P4450L20: maybe "not suitable" instead of "not adapted".

P4453L9-12: Please deïňĄne CDF, FF and each acronym at its ïňĄrst citation in the text. According to ISO standards, multiple-letter labels such as FF should be upright.

Answer: Thank you very much for the uplifting introduction of your review. All the

suggestions concerning the spelling and editorial mistakes will be introduced to the new version of the manuscript.

Comment: P4453L17: To the best of my knowledge, the Kumaraswamy distribution was introduced to approximate the Beta distribution by a family with an explicit (and analytically tractable and invertible) CDF. Nevertheless, please note that the CDF of a generic order statistic is a Beta distribution. Of course, this holds also for the largest order statistic Xn:n $\equiv$ FF(i). In this case, Xn:n $\sim$ Beta(n;1) $\equiv$ K(n;1), i.e. Beta and Kumaraswamy coincide. However, even though the approximation is generally pretty good, order statistics are not Kumaraswamy but Beta distributed.

Answer: Thank you very much for this comment. The Reviewer is of course right: although these two definitions are equivalent since Beta(n;1) $\equiv$ K(n;1), it is much more logical to use the Beta distribution. This will be modified in the manuscript.

Comment: P4454L15: Please deïňĄne “SPAN” if this is an acronym. Answer: It is actually not an acronym – it rather measures the span of two quantiles derived with distinct calibration datasets. We will reword the sentence to make this clearer.

Comment: P4457L16: maybe “decomposing all dataset” or “decomposition of all dataset”. P4458L21: “local implementation”. Answer: This will be corrected as suggested.

Comment: P4459L9-15: maybe I missed something, but I cannot see the discussion about the stability index for the regional models (i.e., SPAN10, SPAN100, and SPAN1000 for REG_GUM and REG_GEV summarized in Fig. 8).

Answer: Indeed, we decided not to comment on the stability results for the regional implementations REG_GUM and REG_GEV. This is because, as explained in section 2.3.2, stability is only a secondary consideration compared with reliability, and is only used to discriminate implementations that would be comparably reliable. In this particular case, reliability is poor for all implementations (see figure 4), so we decided

that stability was not worth considering (a stable but non-reliable implementation being worthless). We will add one or two sentences in the revised paper to explain this point.

Comment: P4460L24-26: N10 (10-yr) and N100 (100-yr) should be switched. Answer: This will be done.

Comment: P4461L1-4: I wonder if the 1:1 lines can be complemented by conïňĄdence bands. This can help visualize the different uncertainty affecting 10-yr and 100-yr ïňĆoods and further support the discussion.

Answer: Such bands around the 1:1 line have actually been described in the ensemble forecasting literature, based on a Kolmogorov-Smirnov test (see e.g. Laio, F., Tamea, S., 2007. Verification tools for probabilistic forecasts of continuous hydrological variables. Hydrol. Earth Syst. Sci., 11(4): 1267-1277.). Unfortunately, this cannot be applied here because the test assumes independent data, but the values taken by the reliability indices are not fully independent from site to site (due to the spatial dependence existing between series from nearby sites). This limitation will be added in the discussion section 4.3 of the revised paper.

Comment: P4462L8-10: Maybe it could be fair to mention here an interesting series of papers by Merz and Blöschl (2008a,b) and Viglione et al. (2013). Answer: Indeed, we have missed these interesting articles. The mistake will be corrected.

Comment: P4463L9-13: This statement is coherent with the claims of e.g. Klemeš (2000) and Serinaldi (2013).

Answer: We will add these references.

Comment: P4466L12: Recently, Papalexiou and Koutsoyiannis (2013) showed interesting results concerning the sign of the shape parameter of the GEV distribution ïňĄtted on a worldwide data set of daily rainfall AM. Even though the present paper deals with discharge data, I wonder if the local GEV results could be improved by using a prior with negative mean for the GEV shape parameter.

Answer: It may indeed be the case if such a prior is precise enough. But we are reluctant to specify a too narrow prior for this parameter based only on expertise. Instead, we believe that regional analysis is a sensible way to exert an additional constraint on this parameter. This is nicely illustrated by the superiority of local-regional implementations: the “regional part” yields a quite narrow prior for the shape parameter (which is assumed constant in a given region), while the “local part” allows adapting the other parameters to the local data. However, we take the reviewer’s point that a parallel with the work of Papalexiou and Koutsoyiannis (2013) is of interest. We will therefore add a comment on this paper in the discussion section 4.2 “comparison with literature results”.

Comment: Figures 1, 5, 6 and 9: Please, consider to arrange the panels in a 2x2 matrix or in one column. This can improve the readability in the ïňĄnal two-column typesetting.

Answer: This could be done, however we find that a horizontal figure spanning two columns is more readable than a vertical figure. But we are open-minded on this, so we’ll follow the advise of NHESS editorial staff.

Comment: Figure 3: Please, consider enlarging the panels and using a more effective colour scale. For instance, use red and blue for extreme values and pink and light blue for intermediate values.

Answer: This will be done as suggested.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 4445, 2013.