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Interactive Comment

Interactive comment on "Estimation of high return period flood quantiles using additional non-systematic information with upper bounded statistical models" *by* B. A. Botero and F. Francés

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GENERAL COMMENTS

The uncertainty is worked in a specific section in the paper, but as suggested by referee#2 and also by John England, we will extend the discussions in sections 6 (uncertainty) and 7 (robustness).

SPECIFIC COMMENTS

Page 5419, line 22; the threshold level of perception

The threshold level of perception is defined always as a discharge. However, in order to generalize the results, it is used in the paper the corresponding return period.

Concerning the variable threshold, my answer to a similar comment of John England was:

The censoring threshold can be different each year: the year in equations 6, 7 and 8 is a subindex for the upper (U) and lower (L) censoring. On the other hand, it is true in figure 1 the censoring threshold is fixed. We fixed it for the sack of simplicity. We can add in the figure caption "for a fixed threshold" or change the figure as John England suggests.

Page 5421, line 24; Uncertainty on G

John England has a similar comment. My answer was:

All flood information has errors and we agree it seems G (the a priori deterministic computation of PMF) has more uncertainty than the rest of the information. John England comment of including explicitly the G uncertainty is reasonable. In fact, we use this way (in our paper G is a normal random variable with some bias, instead being the lower limit of a LN3 as suggested by John England) not in the model, but in the Monte Carlo uncertainty analysis in our Section 6 for the ML-PG estimation method.

What I should add to complete the answer to referee#2 is there are few works which permit to parameterize the distribution function of flood observations error and none about the G error. It was our "flood frequency analysis" experience who determined the used bias and coefficient of variance. See also below my reply to John England concerning this topic.

Page 5424, line 20; beginning of the historical period

Yes, we must clarify our selection. John England has a similar comment. My answer to John was:

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We don't begin the historical period with the first flood date precisely because the results of the Hirsh and Stedinger (1989) described in their appendix A. Their conclusion was clear for us:

”Clearly, the estimation of n based on the date the first extraordinary flood occurred exacerbates the severe imprecision of any of the plotting position formulas and the severe upward bias that exists in the W formula and less so in the E and B formulas.”

And they recommended:

“Every effort should be made to establish n accurately on the basis of the quality of historical evidence and not on the basis of the occurrence of the first extraordinary flood.”

However, their recommendation cannot be followed in most cases, not in our case study. What we are doing to avoid (better, to reduce) this problem is eliminate the first historical flood and consider the beginning of the historical period in the middle year between the first and second historical floods. However, we cannot assure all bias is eliminated with this method. So, in the final paper, we must cite Hirsh and Stedinger's paper (because we used their conclusion) and add a proper discussion. Also, it must be mentioned in this case study the sensitivity of the results to this decision is small (maximum EV4 estimated quantile change of 5%).

Pager 5425, line 4; Authors state that the lower bound for EV4 and LN4 distributions has been fixed to zero.

We fixed them to zero because they are non influential parameters for the extreme quantiles. For example, in the case study with the EV4/ML-PG any value between 0 and 20 (minimum systematic flood was 28 m3/s) doesn't produce significant differences for any quantile bigger than 5 years return period. For the EV4/ML-C there are no significant differences for quantiles bigger than 200 years return period.

Page 5425, line23; the proposed distributions show very different behaviors

In general, referee#2 is right: any extrapolation depends on the shape of the used function. Our preference for upper bounded distributions, when dealing with very high return period quantiles, is because the physical meaning of the upper limit. Between them, for me it is clear more information is needed to have a reliable extrapolation.

But I don't agree in our case study "In the upper tail no data are available to support the choice of one specific distribution". If you see our figure 2b (with ML-PG method), only with the EV4 there is a coincidence between the fitted function, the systematic plotting positions and the historical threshold plotting position (triangle). So, the selection is clear.

Concerning the model averaging approach (I couldn't access to the book referenced by referee#2), my answer to a similar comment of John England was:

"Combining results" in a Bayesian framework (O'Connell et al., 2002) is a good way to avoid the inherent error in the parametric function selection, but to apply the O'Connell et al. (2002) methodology was not the objective of our paper. We were aware of this problem and, in fact, this was the aim of our Robustness Analysis section.

Page 5426, lines 9-16; reliable estimate of g.

The general procedure to discard a g estimate is to estimate its reliability by the uncertainty analysis. For the EV4/ML-GE, it is stated in our conclusions (page 5430 lines 4-5) the g estimate uncertainty is a 20%, similar to the 10 000 years quantile uncertainty. We will stress this point in the final version.

Page 5427, line 8; values assumed in G uncertainty

John England has a similar comment. My answer was:

John England finds more frequently PMF underestimation of about 44%. We are assuming a 10% positive bias (overestimation) plus a coefficient of variation of 30% with a normal error distribution. It means a theoretical error in terms our eq. (13) of 32%, which is in the same magnitude (i.e., not too small), but with slight more frequent overestimation than underestimation (i.e., not in accordance with John England experience). Of course, John has much more experience than us dealing with PMF estimation, but what I can say from our results is the values for bias and coefficient of variation don't change the main conclusions (at least for the EV4):

i) The uncertainty for high return period quantiles using ML-PG method is controlled by the uncertainty in G (the a priori deterministic estimation of the PMF), as it is shown in figure 3 in our paper.

ii) And, unfortunately, introducing a prior estimation of G with relative small errors (as we are using in our paper) is worst than don't use it (figure 3 of our paper).

We will better state these conclusions in the final paper.

TECHNICAL COMMENTS

Page 5423, line 11; missing 's' in 'expresion'

Ok

REFERENCES

Hirsch, R. M. and Stedinger, J. R.: Plotting Positions for Historical Floods and Their Precision, Water Resour. Res., 23 (4), 715–727, 1987.

O'Connell, D. R. H., Ostenaa, D. A., Levish, D. R., and Klinger, R. E.: Bayesian Flood Frequency Analysis with Paleohydrologic Bound Data, Water Resour. Res., 38 (5), 16.1–16.13, 2002.

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