Hydrol. Earth Syst. Sci. Discuss., 7, C2508–C2515, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C2508/2010/

Hydrology and Earth System Sciences Discussions

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

J. England

jengland@usbr.gov

Received and published: 28 September 2010

General comments

The authors present a very interesting, and practically-relevant paper. There are very few recent papers on integrating deterministic, design floods (PMFs) and flood frequency curves. This paper is a welcome addition to this topic. I commend the authors for working on the problem. They present some useful results that I consider to important for advancing hydrologic risk analysis methods for dam safety.

In fact, I have had some very brief email discussions with the second author of this paper, when Pepe Salas sent me Blanca Botero's thesis in the summer of 2006. It is good to see her results get submitted and published.

I have several comments on this paper. I submit them so that the authors might make some improvements in several areas. These include: adding some generalizations to Section 2 Upper bounded distributions; some clarifications and corrections to Section 3 Data Classification; and various specific comments on other portions of the paper.

Basically, the approach offered is mathematical curve fitting to the best, combined data sets available. There are a couple of unanswered questions that the authors could address as part of a "Discussion" section. These are:

What should this upper tail distribution function look like, and why?

What is the physical basis (if any) of the upper bound location parameter?

What physical arguments can you use to complement or justify the LN4 choice? Is there anything on rainfall or flood volumes (e.g. Naghettini et al. 1996), those from recent SCHADEX research, or from Sivapalan and coauthors that might guide you?

My simple view is this. Without strong physical justification for a particular distribution, the best approach would be to utilize several distributions and combine the results. O'Connell et al. (2002) do this in a Bayesian context, but importantly without the PMF and with a different class of distributions. In this way, important model uncertainty is attempted to be quantified. The authors should add some comments about the shape of the upper tail and its physical significance (or lack therof) for the EV4 and LN4 distributions.

If the authors could continue their investigations to address some of my questions, as part of subsequent studies and papers, I would certainly welcome some collaboration, and can be contacted at:
John F. England, Jr., Ph.D., P.E., P.H.
Flood Hydrology Technical Specialist

HESSD

7, C2508–C2515, 2010

Interactive Comment

Bureau of Reclamation
Flood Hydrology & Emergency Management Group, 86-68250
Bldg. 67, Denver Federal Center
Denver, CO USA 80225-0007
jengland@usbr.gov

Specific comments

Here are some specific comments by line number and page. I was one of the SERRA reviewers for this related paper:

Fernandes, Wilson and Naghettini, Mauro and Loschi, Rosângela (2010) A Bayesian approach for estimating extreme flood probabilities with upper-bounded distribution functions, Stochastic Environmental Research and Risk Assessment, 1436-3240, 1-17, http://dx.doi.org/10.1007/s00477-010-0365-4

Some of my comments on that paper are relevant for this paper. As I suggested Fernandez et al. cite your work; it is appropriate for you to cite Fernandez et al. (2010). That paper has some similar elements to your paper; readers need to be aware of the relevant literature.

page 5415, last line onto next page: " The use of parametric distribution functions allows the increment of the return period of the requested quantile as much..." You need to clarify this statement. What you mean is: it is easy to extrapolate a parametric distribution.

page 5416, near line 10. Here a citation to Enzel et al. 1993 is appropriate, reinforcing the idea of a physical upper limit.

You may wish to provide an alternate view - that of no upper bound. See, for example, Papalexiou and Koutsoyiannis (2006) or Koutsoyiannis (2004). However, those arguments conflict with your central approach.

page 5417, Section 2, Upper bounded distribution functions.

HESSD

7, C2508–C2515, 2010

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

page 5417, line 13. You really should add figure 2.21 from Botero 2006:

Figura 2.21. Aproximación al límite de las distribuciónes EV4 (con v=20.64, k=1.3), TDF (con a=1163, b=-2.49, k=-0.46) y LN4 (con μy =,-1.18 y =1.19). g=12887 para todas ellas.

You could consider adding some more detail from Botero (2006) on EV4 and LN4, such as figures 2.7 - 2.8.

Similar to my comments to Fernandez et al., there is no physical justification of any of the distributions presented here. You need to consider adding some key missing distributions, thereby pointing out to readers there are different choices here. This is relevant because your results rely on a "best-fitting" single distribution.

Some key missing pieces in this section are other distributions with upper (and lower) bounds. These include:

1. four parameter kappa distribution (Hosking, 1994);
2. 4-parameter beta;
3. any generic mixture of other 2-parameter and 3-parameter distributions - here you could expand TCEV to function as TC-GEV or TC-GPA;
4. 4-parameter beta (with upper and lower bounds);
5. wakeby (Houghton, 1978).

What matters most in all this is the shape of the distribution function in the range of CDF=0.995 to CDF=0.99999. By limiting the class of distributions considered, you should add some stronger caveats in sections 5 through 7. For example, you could have sampled from a Wakeby as part of robustness. Would your results have changed?

The point here is the shape of the distribution function dictates your answer. This is clearly an unexplored area. Perhaps you can consider this as future research. This has been partially explored in a rainfall context with the PMP and a parabolic distribution. See Nathan and Weinmann (1999) and the differences in results for AEP of PMP and

HESSD

7, C2508–C2515, 2010

Interactive Comment

the distribution shape, sections 3.6.2-3.6.3. Siriwaneda and Weinmann (1998) provide the study used in Australian Rainfall and Runoff.

page 5420, Section 3, data classification.

You should modify figure 1 to indicate x_h changes over time, and is not fixed. See for example, figures 14 and 15 in Naulet (2002). The word “Proposed” for your classification scheme needs to be removed. This classification scheme was best described first by Stedinger et al. (1988a,b) and graphically shown by Baker (1989). You should reword the sentence on page 5420 line 9 to reflect this, rather than Naulet.

page 5421, last line before section 4.1.:

Delete ’s’ in ’parameters; should read “parameter space.”

page 5422, lines 2-3. Here you should point out that PMF estimates are nonstationary - they tend to increase over time, and are highly uncertain. From U.S. experience, G is nonstationary and can increase by $1.5G$ over time.

page 5422, equation (9):

Could this be modified to be a conditional pdf for the second part? Could g in the integral be some other value?

page 5423, equn (12):

Future research should consider using a pdf for g, such as a 3-parameter lognormal with a minimum estimate of the PMF as the lower bound.

page 5424, line 21:

The statement “To avoid a bias estimation in the number....” is incorrect. See Hirsch and Stedinger (1987) Appendix A, pages 724-725. The first flood in your example was 1778. The historic period should be started before 1778. If the magnitude of this flood is in question, then you could either use a varying threshold, or simply raise the discharge threshold to address this perceived “bias” issue.

page 5425, line 22: Sentence starting with “A very interesting first conclusion...” This is a very important sentence! You should consider emphasizing this result with a figure. You say the “different behavior can be generalized...”, however I missed seeing this generalization later in the paper. Could you clarify?

page 5426, line 1: delete “d” in “appropriated”.

page 5427, about line 10.

The PMF uncertainty estimates chosen by the authors are too small, as compared to what I have seen in nearly 20 years of practice in flood estimation for dam safety. The Bureau of Reclamation has found PMF increases (nonstationarity) at many sites by a factor of 1.5. This is at the upper limit of your implementation for your G and $\hat{g}$.

Laurenson and Kuczera (1998) suggest an order of magnitude on PMP probabilities is an appropriate estimate of one standard error, and two orders of magnitude might represent notional 95% confidence limits.

page 5430, line 14. You should add a caution statement here, that the results are dependent on your EV4 distribution assumption.

References

Baker, V.R. (1989) Magnitude and frequency of palaeofloods. In Beven, K. and Carling, P. (eds.) Floods: hydrological, sedimentological, and geomorphological implications, Wiley and Sons, Chichester, pp. 171-183.

Enzel, Y., Ely, L.L., House, P.K., Baker, V.R., and Webb, R.H. (1993) Paleoflood Evidence for Natural Upper Bound to Flood Magnitudes in the Colorado River Basin. Water Resour. Res. 29(7), pp. 2287-2297.

Hosking, J. R. M. (1994). The four-parameter kappa distribution. IBM Journal of Research and Development, 38, 251Â■258.

HESSD

7, C2508–C2515, 2010

Interactive Comment

Houghton, J. (1978), Birth of a Parent: The Wakeby Distribution for Modeling Flood Flows, Water Resour. Res., 14(6), 1105-1109.

Koutsoyiannis, D. (1999) A probabilistic view of Hershfield's method for estimating probable maximum precipitation. Water Resour. Res., 35(4), pp. 1313-1322.

Landwehr, J., N. Matalas, and J. Wallis (1979), Estimation of Parameters and Quantiles of Wakeby Distributions 1. Known Lower Bounds, Water Resour. Res., 15(6), 1361-1372.

Laurenson, E.M. and Kuczera, G. A. (1998): An nual exceedance probability of probable maximum precipitation - report on a review and recommenda tions for practice. Report prepared for the NSW De partment of Land and Water Conservation and th Snowy Mountains Hydro-Electric Authority.

Naghettini, M.D.C., K.W. Potter, and T. Illangasekare (1996) Estimating the upper tail of flood-peak frequency distributions using hydrometeorological information, Water Resources Research, vol. 32, No. 6., pp. 1729-1740.

Papalexiou, S.M. and Koutsoyiannis, D. (2006) A probabilistic approach to the concept of Probable Maximum Precipitation. Advances in Geosciences (EGU), 4, pp. 51-56.

Siriwardena, L. and Weinmann, P.E. (1998): A technique to interpolate frequency curves between frequent events and probable maximum events. CRCCH Research Report 98/9, Cooperative Research Centre for Catchment Hydrology, December 1998.

Stedinger, J.R., Surani, R., and Therivel, R. (1988a) Max user's guide: A program for flood frequency analysis using systematic-record, historical, botanical, physical paleohydrologic and regional hydrologic information using maximum-likelihood techniques, Dept. Environ. Engin., Cornell Univ., Ithaca, New York, 51 pp.

Stedinger, J.R., Therivel, R., and Baker, V.R. (1988b) Flood frequency analysis with historical and paleoflood information, Salt and Verde Rivers, Arizona. Proceedings of the Eighth Annual Meeting of the U.S. Committee on Large Dams, Salt River Project,

HESSD

7, C2508–C2515, 2010

Phoenix, Arizona, pp. 3.1-3.35.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 5413, 2010.