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Discussions

Interactive comment on "Climate change and non-stationary flood risk for the Upper Truckee River Basin" *by* L. E. Condon et al.

Anonymous Referee #1

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In this paper, a non-stationary approach is applied for the estimation of the probability of failure of infrastructures in two locations in the Upper Truckee River Basin (US). The approach uses climate scenarios as input to determine the expected (and range of) changes in precipitation and temperature. The results show that, based on the assumptions made, the probability of failure of infrastructures increases considerably with time, from now to the end of the century.

The paper is well written and interesting. I like the fact that the evolution of probability of failure is investigated, instead of the change in return period. However I have the following concerns, which I believe should be addressed/discussed before publication in HESS:

1) What is the novelty of the paper? Non-stationary models for flood hazard are not

new and nor is the use of the probability of failure in climate change studies (i.e., the “design life level” of Rootz and Katz, 2013).

2) The results are conditioned to strong assumptions and there is no explicit uncertainty analysis in the paper (e.g., Steinschneider et al., 2012, provide a framework for that). Prediction bounds are plotted in the figures, but they just show the range of variability of climate model inputs once propagated through the hydrologic models (VIC + non-stationary GEV). In my opinion, it would have been more interesting to analyse if, based on the observations in the last decades, the use of non stationary flood-frequency models gives results significantly different from those obtained with stationary models. To do so, the uncertainty associated to the use of both approaches should be quantified: Fig. 6 could contain the stationary models results with confidence bounds + the non-stationary model results for the observation period with confidence bounds that account not only for the variability of the climate models, but also for the uncertainty in the estimation of (VIC+ non-stationary GEV) model parameters. It would be very interesting to see how the two ranges of estimates differ.

3) The Authors use the wording “flood risk” to refer to the probability of failure. Even though in engineering books “risk” and “probability of failure” are used interchangeably, “flood risk” is widely accepted in the literature as product of hazard (probability of flooding) and consequences (see e.g. Plate, 2002, among many). Since this paper looks at hazard only, I would strongly suggest to change the wording in it (including the title).

4) The references in the paper are biased toward US, while relevant literature exists abroad. As a suggestion, since I am European, the Authors could refer to some of the many studies cited in Hall et al. (2013) about flood changes in Europe (and scenario approaches).

Specific comments:

Page 5079, line 11: i do not agree with the wording “additional non-stationarity”. It does not make sense unless stationarity is defined (see e.g., Koutsoyiannis, 2006; Montanari and Koutsoyiannis 2012). Under my understanding, stationary models can cope with long-term climate oscillations (see e.g., Koutsoyiannis, 2011).

Page 5083, line 7: being HESS international (and European) international unit system (e.g., km instead of miles) should be used. This comment applies to the all paper.

Page 5088, Eqs. (2) and (3): one line could be added to motivate why the shape parameter xi is considered stationary.

Page 5088, line 18: are the GEV distributions fitted to simulated streamflows only? The Authors should add a line here to motivate why the observed streamflows are not used here. I see that Fig. 2 and 3 include observed flows and provide a kind of validation of the procedure.

Page 5089, Section 3.2: the section discusses “flood hazard”, not “flood risk”. The same applies to the rest of the paper, specially to Section 4.3 and related figures (e.g., y-axis in Figs. 5-8 should not be “risk” but “probability”)

Additional References:

Hall, J., et al. (2013) Understanding flood regime changes in Europe: a state of the art assessment, Hydrol. Earth Syst. Sci. Discuss., 10, 15525-15624, doi:10.5194/hessd-10-15525-2013.

Koutsoyiannis, D. (2006), Nonstationarity versus scaling in hydrology, J. Hydrol., 324, 239-254.

Koutsoyiannis, D. (2011), Hurst-Kolmogorov dynamics and uncertainty, J. Am. Water Resour. Assoc., 47, 481-495.

Montanari, A., and D. Koutsoyiannis (2012), A blueprint for process-based modeling of uncertain hydrological systems, Water Resour. Res., 48, W09555, doi:10.1029/2011WR011412.

Plate, E.J. (2002) "Flood risk and flood management." Journal of Hydrology 267.1: 2-11.

Steinschneider, S., A. Polebitski, C. Brown, and B. H. Letcher (2012), Toward a statistical framework to quantify the uncertainties of hydrologic response under climate change, Water Resour. Res., 48, W11525, doi:10.1029/2011WR011318.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 5077, 2014.