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

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Received and published: 25 July 2014

Comments by John England, Bureau of Reclamation

## Major Comments

This paper is an interesting case study using non-stationary concepts to illustrate a changing flood risk profile. The authors suggest that the paper "provides the first end-to-end analysis using non-stationary GEV methods coupled with contemporary downscaled climate projections to demonstrate how the risk profile of existing infrastructure evolves with time over its design life."

In my opinion, this topic is very interesting, and certainly timely and relevant to HESS.

Given ongoing activities within the United States to revise flood frequency guidelines (England and Cohn, 2008; HFAWG, 2013; England et al., 2013) this paper could potentially serve as a reference or example methodology to estimate flood frequency and flood risk in a non-stationary environment dominated by future trends in climate (precipitation and temperature), at least within the U.S. It is a very nice example of illustrating time-varying distribution parameters, relevant for flood frequency applications.

However, in my opinion, there are some technical issues with the methodology and application, as presented in this paper, that need further investigation and/or simply explanation, prior to publication in HESS.

1. Functional form of flood non-stationarity (equations 2 and 3), covariates and non-stationary parameter selection.

One key issue in non-stationary flood frequency analysis is the identification of the functional form of non-stationary behavior. For example, Salas and Obeysekera (2013) highlight three situations: increasing floods; decreasing sea levels; and shifting floods. Other examples are in Olsen et al. (1998) and Stedinger and Griffis (2011).

With all the information behind Table 2 on covariates (page 5105), and presumed functional form (equations 2 and 3), the authors need to clearly demonstrate the evolution of aggregated winter monthly precipitation and monthly average temperature trends with time. Are trends in location and scale needed? Are there any shifts in these time series? Why are these linear models adequate? Describe model performance between nonstationary location, and nonstationary location and scale. Do model diagnostics clearly show that the scale parameter needs to be included (adding 3 more coefficients), or would a simpler model be sufficient just based on location (e.g., Towler et al., 2010)? Figure 2 isn't compelling to distinguish between competing models, or discriminate between tail behavior at the 2 sites.

Supplemental material could be used here to build your case for the functional form and the choice to use two time-varying parameters. You could also show the simple plots

of precipitation and temperature evolution over time, for historical and future periods. This would help partly explain the non-overlapping risks, depending on period (Figure 5). You could also show cdfs from the 2 sites, enhancing and further explaining the discrepancies on the shape parameters between these two nested watershed sites.

2. Discussion on quantiles of interest, record length used in analysis, and risk

The authors need to clearly articulate what they mean by infrastructure (levees? flood protection?), quantiles of interest (Q50? Q100?), and how their key measure is risk (of failure, in the binomial sense). How does the record length chosen (50 year windows) affect the quantile estimates, particularly the precision of the shape parameters at these two sites, for the flows of interest (37600 cfs)? Could you better explain (page 5088), that using block maxima (6 per year), your main output is risk (and changes) for a particular flow level (Fig 5), rather than a "traditional" annual flood frequency curve? Some minor adjustments would be needed to estimate annual one-day maximum flood probabilities. You could cite (for example) Towler et al. (2010, p. 3).

3. Uncertainty estimates for quantiles

Uncertainty estimates are required (NRC, 2000; USACE, 2011) to evaluate flood risk and alternatives in a stationary environment. In a non-stationarity framework, there are existing tools (Gilleland and Katz, 2011; Obeysekera and Salas, 2014) to estimate uncertainty. The authors need to address or add a discussion of uncertainty of the estimates provided. Would confidence intervals for the future period(s) overlap the stationary model estimates?

4. Use of the term "Data", consideration and use of additional data relevant for floods and flood frequency

The authors need to add some caveats in Sections 2.2. and 2.3 on the use of the term "data" that include VIC unregulated flow simulations and CMIP-5 downscaled projections. In both situations these are model results, not data. I suggest a simple

C2707

title change of each section to "Streamflow data and models" and "Climate data and models".

Why were not other streamflow data sources considered, particularly for calibration of VIC soil moisture parameters? There are monthly unregulated streamflow estimates available in the Truckee River Basin (Rieker and Cowan, 2005) that may be useful. How would one extend the historical period, with evidence of several very large floods, for the period prior to 1950? There are at least 3 large floods prior to 1950 that are equal to or exceed those used in the analysis.

On streamflow, please explain how VIC was calibrated (or not) and how parameter estimates were made for the historic period (Section 2.2). Why are these stationary streamflow model parameters representative of future periods? Validation is mentioned (page 5084, line 17), but only in reference to six one-day flows (Figure 3). Were these six events used in any way for model calibration?

**Some Specific Comments**

page 5078, line 14: Clarify that existing infrastructure – might be appropriate for flood-plain management activities.

page 5079, lines 28-29. Notably missing in the citations to US flood trends is Hirsch (2011).

page 5082, lines 21-22. May want to expand this crucial definition of risk.

page 5083, lines 20-22. This statement is erroneous. Snowmelt flood peaks from April to July are are nearly always within the channel; peaks are not substantially reduced by upstream storage as reservoirs are usually full. Forecasting plays little to no role in reducing flood peaks using reservoir storage. Peaks and max daily flows in these months are much, much smaller than winter floods.

page 5084, line 17. You mention validation; can you describe calibration here?

C2708

page 5085, lines 15-16. Can you demonstrate that cumulative monthly precipitation in winter months is adequate, rather than pairing individual monthly precipitation and flow estimates?

page 5089, line 22-23. Highlight a bit more here what the "risk of flood" means. Can you also clarify here (Table 1) how you obtain an annual "p" (in year x) from the GEV for use in these binomial risk models, or that annual probabilities are not needed?

page 5090, discussion on Table 2. Here is where more explanation and justification is needed on the use of time-varying location and scale.

page 5090, line 24. Recommend using supplemental material to show cdfs of these distributions. The pdfs do not adequately show the right-hand tail to demonstrate what you are saying here.

page 5091, line 1. Did you fix the shape parameter in any way across future months? Why would it vary in time?

page 5091, Section 4.2. The term "ungaged" is in error throughout this paragraph. These flows are "unregulated". Gages exist at both locations. You could have compared the USACE unregulated flows with those from the gage, and examined the differences.

page 5092, lines 10-15. Citations are needed to support these numbers. Can you explain the precipitaiton gradient from upstream to downstream, and where the snow resides in this basin? It may help explain the differences in shape parameters at Farad (relatively fatter tail) and Reno, and why these distributions are bounded.

page 5093, lines 15-16. Clarify here the uncertainty of the GEV model parameters and quantile estimates for the stationary case (as well as other cases) is not included. How might its inclusion change the perception shown in Figure 5?

page 5094, line 10. Clarify that you use MLEs to estimate the LP3 parameters (same as GEV), so that the differences you mention truly are the models, and not mixed

C2709

with parameter estimation. Moments and MLEs of the LP3 can sometimes give very different results for the shape parameter.

Fig 3. Correct caption to read "unregulated" not "ungaged". If you had used the shape parameter from Farad, or some weighted combination of Farad and Reno, how much would the tails of these GEV distributions widen?

**References Cited and Other Relevant Information** (with hyperlinks to pdf copies, as available)

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C2710

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