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Hydrology and Earth System Sciences
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 #2

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In summary I found this an interesting paper and a useful contribution to the incorporation of non-stationarity in water infrastructure planning. As noted in the comments below, I think a restructuring of the paper is needed to improve the contribution and make it more useful to water planners and managers. Most importantly, the thrust of the paper should be the demonstration of a method – with essentially no calibration or quantitative validation of results for the Truckee river, this case study is not useful in itself.

1) The title "Climate change and non-stationary flood risk for the Upper Truckee River Basin" does not reflect the contribution of the paper, which is really a demonstration of a methodology. This is noted in p. 5082, line 18: "This paper provides an end-to-end demonstration of nonstationary GEV analysis coupled with contemporary down-

scaled climate projections (specifically, downscaled climate projections from the Coupled Model Intercomparison Project Phase-5 (CMIP-5)), to quantify how the risk profile of existing infrastructure, designed on the basis of a specified flood event, evolves with time over its design life." As noted in my comments below, the Truckee seems to be more of a demonstration data set. This should not be interpreted as a paper providing significant planning information for managers of the Truckee system.

2) p. 5084, line 15, it states "we simulate unregulated flows from 1950 to 1999 using the Variable Infiltration Capacity (VIC) model and validate results using the available unregulated flow estimates." There does not appear to have been any calibration done as part of this effort. Is the validation done on an uncalibrated model? Some basic hydrology validation statistics would be helpful (NS, RMSE, ...) is assessing the streamflow simulation. The qualitative interpretation like "in close agreement..." (p. 5092, l 9) and "in good agreement" (p. 5092, l 14 and l 19) needs to be quantitative. That would provide support for the claim "This demonstrates that the model behavior is a reasonable match to the natural system."

3) p. 5085, line 20, 234 projections are analyzed, which lumps together extremely aggressive mitigation futures (like RCP 2.6) and more business as usual scenarios (RCP 8.5). It would seem that, for planning purposes, these should be separated. Only one pathway into the future will actually be experienced, and the variability among GCM projections should reflect that. It would make more sense to present each RCP separately, as this allows a consideration of the variation due to following different pathways from the variation in how the atmosphere might respond to the changed atmospheric conditions. These are very different sources of variability. A second point is that this includes multiple contributions from some GCMs and single contributions from other GCMs. Plenty of research demonstrates that different runs of a single GCM are less independent than runs of different GCMs, and lumping them all together inappropriately weights models that happen to have submitted many runs as part of CMIP5. To demonstrate the method for this paper, there is no need to use 234 projections – a

more carefully selected set of a dozen or two would seem to suffice, and also provide a better demonstration of appropriate use of climate model output.

4) Section 2.3, last paragraph, it is mentioned that the Bureau of Reclamation has developed an archive of downscaled data, but then the downscaling is described as if it were done again for this effort. Were the projections obtained from a published archive? If so, state that, provide an appropriate citation and acknowledgement (I see on the BoR site there is a standard citation and acknowledgement).

5) p. 5092, the lack of qualitative model validation appears again here, such as "the VIC simulated and observed floods are in close agreement and the discrepancy with the GEV model is explained by the flood timing described above." What constitutes 'close agreement' and at what point would they be considered not in agreement? And the discrepancy is not explained by the timing, but is apparently consistent with it, which is much weaker. Later instances in this section show things like "the GEV model is in good agreement with the VIC simulated flow", and ultimately "This demonstrates that the model behavior is a reasonable match to the natural system." These general observations are not helpful in determining significant correspondence of modeled and simulated values.

6) p. 5094, Figure 6 is presented, which is interesting. another way to cast this would be in a manner similar to that of Mailhot and Duchesne (J. Wat. Res. Plann. Mgmt., 2010, doi 10.1061/_ASCE_WR.1943-5452.0000023) Figure 3, which aims to provide planners with a design return period for today that would be needed to provide protection at the level of a historic return period (in a stationary climate).

Minor comments: - SI units should be used throughout, not square miles, feet , etc. - p. 5088, line 16, is the 0.05 alpha? And what significance test is being referred to here?

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