

Interactive comment on “Identifying uncertainties in simulated streamflow from hydrologic model components for climate change impact assessments” by Dongmei Feng and Edward Beighley

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This paper presents some limited results of evaluating the impact of different formulations in runoff generation schemes when simulating streamflow. My major objection with the paper is that it really is not assessing the uncertainty but rather the variability of the simulated streamflow and how each of the forcings, model parameters or formulations contribute to it. Although that is valuable in itself, the authors claim that the objective is to identify the uncertainties in the context of climate change simulations. However, that is not what was done here. The calibration of the parameters was done

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using an observation-based forcing dataset and although I can understand the rationale, I believe that any calibration of parameters should have been done in a way that would emulate the intended application (i.e. using GCM output to drive the hydrology model). I believe historical simulation are available from CMIP5 and if so they should be used to evaluate the actual uncertainty of simulated streamflow within the framework that the authors have developed. The end of 21st century simulations should be a final experiment (if included at all) given the objective of the paper. Consequently, I recommend major revisions before publication that will include new simulations that test the different model parameter sets, runoff generation schemes and downscaled GCM output for the period when streamflow measurements are available, so that the actual uncertainty can be quantified. In addition, I believe the study area is rather limited and an opportunity is being missed by not including additional basins with different physiography and climate. Some additional comments are outlined below:

* How does the uncertainties in the prescribed ET affect the results? Why weren't they accounted for? * Abstract needs some attention, especially after l. 21 in terms of cohesiveness. Right now, it reads as bullet points stitched together. * Some proofreading needed for redundant articles and grammatical errors. * l. 53: what is the need for naming the "land-atmosphere interactions" as "runoff generation process" when the latter is clearly one of the processes that manifest from those interactions? * l. 175- : Not sure whether this much detail is needed for the description of the runoff generation models, since they are well established. * l. 354: does that mean that there is bias in the validation data (i.e. streamflow)? * l. 362-363: this highlights another problem that has not been addressed in this study: the downscaling of GCM outputs to drive the hydrology model.

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