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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Empirical streamflow simulation for water resource management in data-scarce seasonal watersheds" *by* J. E. Shortridge et al.

Anonymous Referee #1

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I found it quite difficult to review this paper, largely because the empirical models presented are all OK, but a great deal of the presentation and discussion is focussed on the statistical/mathematical structure of the models and not very much on what I would consider to be hydrology. For example, the good performance of the climatological model is almost completely a result of the low interannual variability in the flow regime as evidenced by Figure 3, but this issue is never mentioned. Such a result would never occur in more variable flow regimes. Some of the other comments in the paper about how the emprical models can be used to assess physical realism are also, in my opinion, rather tenuous. 'runoff increasing with higher precipitation levels and decresing with higher temperatures' (page 19) is hardly a measure of physical realism.

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Similarly, differences in runoff generating mechanisms in seasonal rivers and differences in relationships between precipitation and runoff in different seasons is hardly surprising in strongly seasonal regimes with very little dry season precip. but with low flows. I therefore cannot agree with the authors that their models can be used to characterise 'watershed behaviour in a manner that could shed light on underlying physical processes' (page 18). If that is the case, what are the processes? Are the dry season processes groundwater driven or drainage from wetlands? These are reall hydrological questions that might be important for water resources and environmental management in the basins that would be difficult to answer with this type of model and this issue is not given any attention. Some of the difficulties in capturing high flow responses by models may also be due to poor rainfall data inputs in topographically diverse terrain, again an issue that is not noted.

The argument that these types of models are good for places where there are good climate data but poor physical data may be valid, but the real question is how often do such situations occur and if you have good flow data, why do you need a model to make water resources decisions. We are more frequently faced with situations where we have poor to no flow data and have to generate estimates based on limited data of any type.

My recommendation is that the authors should be more circumspect about their conclusions with respect to the physical hydrology interpretations of their model results. I am not sure about the value of the climate change scenarios as they appear to me to be very simplistic and add very little to the study.

Some other specific comments:

On page 6 the authors suggest that empirical models can provide more comphrehensive uncertainty analysis results. Why when there are many recent examples of rainfall-runoff models being used for uncertainty analysis and the assessment of model results from a behavioural and non-behavioural standpoint.

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There are many places in the text where the word 'data' is treated as singular, while it should always be treated a plural (i.e. 'these data', 'date were', 'data area', etc.).

The reference to the estimates of rainfall intensity on page 7 should be removed as this method will never give a proper estimate of intensity.

Page 8 refers to a log transformation of monthly streamflow to get a better match to normal, however, the distribution properties of the monthly flow data are not assessed.

If NSE is considered such a bad statistic, why not use something else. Even NSE based on log transformed values can remove some of the bias to high wet season flows.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 11083, 2015.