

Interactive comment on “Comparison of predictions of rainfall-runoff models for changes in rainfall in the Murray-Darling Basin” by J. M. Whyte et al.

Anonymous Referee #1

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This paper examines the performance of a commonly used rainfall-runoff model, SIMHYD, and numerous other model structures proposed by the authors on 4 small catchments of the upper Murray in the Murray-Darling Basin in Australia. The authors also examine the impacts of changes in rainfall on runoff as predicted by the models. The results for SIMHYD are much as expected and the performance of the additional model structures proposed by the authors do not warrant their inclusion in the paper.

In general, this paper does not provide any new insights into rainfall-runoff model behaviour, nor does it provide any additional insights into the response of catchments to a change in rainfall. For that reason and the reasons detailed below, I suggest that the

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paper be rejected.

P919. Line 20. Suggests that most studies focus on annual or monthly runoff. However, most studies in the MDB have run models at a daily time step and then aggregate results to seasonal or annual for reporting. Eg CSIRO MDBSY Project <http://www.csiro.au/partnerships/MDBSY.html> as reported in Chiew et al 2008. <http://www.csiro.au/files/files/plol.pdf> and Chiew et al. 2009. Estimating climate change impact on runoff across southeast Australia: Method, results, and implications of the modeling method. *Water Resour. Res.*, 45(10), W10414.

P923. Proposed models. There is an array of model structures proposed in this paper, some of which may be of use, and some of which will not. Why do the authors take this route rather than simply evaluate some of the many models which are in the literature, eg IHACRES, Sacramento, SMARG, AWBM, GR4J? It appears that the authors are testing a variety of model structures, and this is certainly OK, but it is not worthy of reporting in the literature.

P926. Line 10. Using pre-existing robust models avoids the need for only calibrating models over periods with no missing values unlike the structures proposed by the authors. Such periods are rare in hydrological records.

P926. Line 20. In calibration, why minimise the residual sum of squares and not the NSE? It will undoubtedly lead to the same model parameters being chosen, but it seems strange. P926. Line 24. Model validation can use rainfall right up to the day of prediction. I do not see why it needs to stop the day before the runoff prediction is made (assuming that rainfall can influence runoff on the same day it falls). In fact the authors say this on p930 Line 5.

P927. Line 21. I have no idea what the `lm` function in R does. This is not a sufficient description of the model calibration procedure. Ditto the TVAR function (line 23).

P928. Line 24. The BoM do not record streamflow. The 401 station data probably

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came from NSW DNR and the 403 station data from Vic DSE.

P934. Line 7. Increases in NSE may be statistically significant, but it does not necessarily follow that inclusion of the additional parameters is warranted. The authors need to look more closely at the hydrologic literature dealing with parameter identification and the perils of parameter equifinality.

P934. Line 9. The performance of Model 3 is indistinguishable from Model 1 and 2. It does not justify the inclusion of 5 additional parameters. Model 4 is much poorer than Model 3. The authors state it is much better. Similarly, model 5 is better than Model 4, not poorer as stated by the authors. In fact, the authors dismiss Model 4 on p935 Line 1.

Minor comments: P920. Line 3. 'Basin' should be capitalised.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 917, 2011.

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