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Hydrology and Earth System Sciences Discussions

Interactive comment on "Long-range forecasting of intermittent streamflow" *by* F. F. van Ogtrop et al.

Anonymous Referee #1

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p682l4 - I wonder if it is correct to use "probabilistic statistical" together p686l14 - authors need to appreciate the additive part in the model is an assumption they are making - in many cases the additive assumption is not going to be valid. This is something they should ideally check and present a short statement for.

p688l3 - To include a time component as a predictor variable assumes a trend in the data. I am unsure about the merit in doing this. I would have attempted a different approach of using a derived covariate that is effected by warming too (such as globally averaged SST or mayb its rainfall/flow equivalent for Australia). Assuming time as covariate is messy - I can foresee problems in model specification because of this - would be great if authors could justify this or add references showing how/where this has been done before. p705tbl4 - I dont see much merit in using multiple indices of

ENSO in formulating the predictive model - surely these are being picked up in the stepwise procedure due to randomness - the joint dependence of the various measure is remarkably high.

Also, the authors should acknowledge the very limited variability explained by ENSO in the rainfall context in Australia and globally (suggest they have a look at Westra S., Sharma A. (2010) An upper limit to seasonal rainfall predictibility? Journal of Climate 23:3332-3351. DOI: 10.1175/2010JCLI3212.1. which quantifies this and ends up with only 2% if variance being attributable to ENSO), and also the fact that many indices are interrelated and often smoothed representations of each other (atleast this is something that is often mentioned in the context of PDO/IPO).

p701Table 1 - the authors have forgotten to mention that there are a class of models that use fully distributed sea surface temperature anomalies to formulate the forecasts. While one example of such models are the dynamical models used to infer rainfall (such as the ECWMF forecasting system), the other are statistical models that identify predictor variables consisting of sea surface temperature anomalies anywhere in the world. The following paper [Sharma A. (2000) Seasonal to interannual rainfall probabilistic forecasts for improved water supply management: Part 3 - A nonparametric probabilistic forecast model. Journal of Hydrology 239:249-258.] is an example of a nonparametric model that selects SSTAs from any part of the world and demonstrates skill greater than the GAM based counterparts. Such a model has obvious problems - it can exhibit artificial skill as predictors are chosen from a large set, and as a result is cast as an ensemble average with each ensemble representing a unique predictor set, thereby reducing model uncertainty. Ann example of such a model averaged forecasting system is presented in [Sharma A., Chowdhury S. (2010) Coping with model structural uncertainty in medium term hydro-climatic forecasting. Hydrology Research. DOI: doi: 10.2166/nh.2010.104.]. I guess the interesting thing about these models is their departure from the use of simplistic indices - which makes them rather different to the list given here.

p707Table 6 - Given the close priximity of the catchments in question, I am confused by the considerable differences in the different locations (this concern holds for table 4 too). I think the authors need to argue why these models are stable - they may need to make arguments based on the cross-correlations of the responses being modelled, and show that low cross-correlations are often responsible for the different model structures. I am especially worried by the Paroo occurrence model which apparently requires no predictor. Is that because of the instability in the stepwise procedure used? Maybe a sensitivity analysis that shows the changes from a forward to a backward stepwise could be included to help allay worries about the stability of the models. Authors should appreciate that the models have not been fitted in cross-validation - hence showing that they are working better than null models in cross-validation is not exactly OK. While I am not arguing for the models to be refitted in a cross-validatory framework, some testing of the stability of the models using different fitting rationales would definitely help. If models are not found to be stable, an ensemble averaging approach such as the one mentioned above could be considered.

p712fig4 - I would like to see the actual flow occurrence (0,1) superimposed on this figure. I would like the authors to comment on potential edge effects of nonparametric aproaches such as splines - would one non-zero in 2006 change the rlationship completely? p713fig5 - FD curves are often very deceptive because of long tails. While often not a pretty plot, it helps showing example years with observed flows and those forecast by the model along with associated confidence limits. I encourage the authors to include atleast one such figure to give confidence to the reader on the efficacy of the model. With the FD plots, I encourage them to include confidence limits and comment on reasons for the biases that are visible - and suggest fixes that they think can help in such cases.

Overall a pretty interesting paper - very well written and presented. The comments above will help make it more useful to the journal audience.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 681, 2011.