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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "The challenge of forecasting high streamflows in medium sized catchments 1–3 months in advance" *by* J. C. Bennett et al.

Anonymous Referee #3

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Overall recommendation: Return to author for some revisions

Overall this is a good paper on a very important topic. The paper is well written, well organised and the aims of the study are outlined well in the introduction. The conclusions and discussion are also sound and backed up by the results that were obtained.

The result that antecedent conditions are an important predictor is good...and consistent with previous studies (e.g. Chiew, Verdon etc)...

However, I have some concern that the results and main conclusion (i.e. little skill associated with inclusion of large-scale climate drivers as predictors) are more to do

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with methodology rather than there actually being no skill associated with inclusion of climate indices as predictors.

Specifically:

1) more information is needed on the indices and how exactly they were used? Are you basing your predictions on correlations between climate indices and flow 1mth or 3mths later? Or are you using stratification/phase type approaches as per some of the studies you mention (e.g. Verdon, Chiew etc)?

2) what about SAM, STR-intensity, STR-position, Interdecadal Pacific Oscillation (or similar PDO)?

3) what about potential interaction between different climate drivers (see for example Gallant et al Understanding hydroclimate processes in the Murray-Darling Basin for natural resources management. Hydrology and Earth System Sciences, 16, 2049-2068, doi:10.5194/hess-16-2049-2012)....such that two or more climate drivers acting at the same time result in different conditions than if they were acting individually?

4) for the SST related indices, basing your predictor on only 1 month may not give a proper indication of the true climate state (e.g. for the climate state to be considered a "true" La Niña the SSTs needed to be persistently warmer than average to the north of Australia for several months).....because your method only considers the one mth prior to the period you want to forecast for there is the possibility that whatever happened in that one month prior may not be indicative of the overlying "climate state".....

5) as you state in the paper for atmospheric related indices it is the opposite to the point above.....1mth prior is too long to pick up things like cut-off lows, east coast lows etc (i.e. the main weather events associated with high flows).....

I think it is too easy to conclude that "including climate indices as predictors adds little skill to the forecasts" when in reality what you have actually shown is that "including climate indices (as chosen and utilised by you in the method chosen and developed by

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you) as predictors adds little skill to the forecasts (based on skill assessments chosen by you)". There are several assumptions and sensitivities in there and I think caveats should be made in the paper along those lines rather than just concluding catchment wetness is pretty much all there is too it (intuitively and anecdotally this doesn't make sense - as alluded to page 3148, lines ∼15-25.

For example, other work (e.g. SEACI, Verdon, Kiem, Pook, Timbal etc) has shown that the frequency and intensity of synoptic events typically associated with high flows (e.g. east coast lows, cut-off lows etc) is dependent on the overarching climate state. So it isn't so much that climate indices do not add skill as it is that climate indices will only add skill if they are used in such a way as to capture the variable frequency of sub-monthly weather events that are associated with high flow events. "Normalising and stabilising variance" in predictands and predictors (as the BJP does, as described section 2.2.4) I don't think is a good way to do this given the inherent variability, non-linearity, and non-stationarity associated with flow (especially high flows) in Australia. On top of this, it seems there is another "smoothing" step introduced via the BMA.....given all the "normalising" and "averaging" is it really a surprise that abnormal, infrequent, and highly variable timeseries such as high flows are not well predicted?

Minor comments:

-Table 2. Anomalies are used for some indices. What periods were the anomalies calculated based on? Why were anomalies used for some indices but not all?

-Table 2. For the indices, which SST and SLP data sets were the indices calculate from?

-Table 1. Different analysis period was used for different stations. Are the results sensitive to this? Why not use a consistent analysis period?

-Where was the daily flow data obtained from? Was it complete over the periods indicated in Table 1? Was any infilling conducted? How were losses/gains due to non-

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natural influences accounted for (e.g. irrigation, farm-dams, reservoir spills etc)??

-I don't think enough (or any) comment is made about the role of multi-decadal climate variability. The paper by Kiem et al 2003 is cited which showed that the analysis period used in this study is split into two epochs (predominance of high flows pre ∼1978; lack of high flow post-1978). This is associated with the IPO which Scott Power in his 1999 paper also showed influences predictability (i.e. when IPO is negative (i.e. ∼1948-78) predictive skill, associated with SOI in the Power paper, is enhanced and when IPO is positive (as it has been since late-1970s until ∼2010) predictive skill is reduced. Given the majority of your study period is dominated by IPO +ve conditions (i.e. low predictability) I think some discussion and caveats along these lines is warranted.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 3129, 2013.

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