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Natural Hazards and Earth System Sciences
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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.

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Response to Reviewer 1, Ben Livneh

Review comments are in double quotation marks. Line numbers (e.g. lines xxx-yyy) refer to the manuscript attached as the supplement.

General:

Comment: "Overall the authors address an important question related to streamflow predictability at monthly leads. A notable feature in their analysis is the simplicity of the input data, which has both advantages and disadvantages. The authors do a reasonable job highlighting these issues and therefore provide an informative and useful analysis. The writing style is clear and cogent and the results are generally adequately described. Therefore, it is my recommendation that the manuscript be published after minor revisions." Response: Thanks for the positive comments, they're much appreciated. And thank you for the detailed review.

Major:

1) Comment: "Given that floods are largely governed by dynamic meteorological inputs, namely precipitation, it should not be surprising that using static 'indicators' in the analysis adds little skill. The authors should consider using, or at minimum mentioning the role that seasonal precipitation forecasts could provide, via either numerical weather prediction models, or using an ensemble approach. Since the catchment wetness represents antecedent conditions, the authors could easily make use of precipitation observations (e.g. gauge measurements). This too, at minimum, deserves mention, since it has the potential (together with temperature) to characterize frozen precipitation storage (relevant to other analyses making use of this methodology)."

Response: We agree that generating these forecasts with NWP/GCM forecast precipitation ensembles, which are then run through a hydrological model is an attractive (and perhaps preferable) alternative to our forecasting method. We have now altered the following: 1. Added a more detailed discussion of possible alternative approaches to this work, particularly the use of GCMs in forecasting rainfall (lines 553-571). 2. Changed title to include 'lagged climate indices' to give more focus to the paper 3. Noted the potential use of soil moisture accounting models to simulate catchment wetness, including the use of precipitation as inputs: "Catchment wetness can be modelled more effectively for forecasting with so-called 'dynamical' approaches (Rosenberg et al., 2011; Robertson et al., 2013) that use soil-moisture accounting models (e.g. conceptual rainfall-runoff models forced by observed rainfall and evaporation) to improve estimates of catchment wetness and thereby improve forecasts." (Lines 498-502.) 4. Noted the possibility of including predictors of seasonal snowmelt to adapt our model to other regions (lines 572-578).

2) Comment: “The general application of ‘leave-one-out’ analysis, i.e. jack-knifing, is sound. However, the inclusion of information from years that occur later than the year being forecast (i.e. future information), would necessarily not be available to any true implementation of this method. This caveat is worth mentioning in the manuscript.”

Response: We have now added this qualification: “Leave-one-out cross validation ensures that a forecast model is not validated against data used to build that model. We note that in this approach we use data after the forecast date to condition the forecast model, data which would not be available to build operational real-time forecast models. The purpose of cross validation is to get an indication of model performance for future events. For future events, one would use all historical events to establish the model. The length of record used in model establishment in cross validation is similar to (more precisely just short of) the full record length. In this sense, cross validation gives a good indication of the skill of a true implementation for the future events.” (Lines 270-277.)

3) Comment: “A clearer explanation of the utility of the different skill scores is warranted, i.e. justification of these metrics in terms of the diagnostic information they provide (Equation 4 and 5).”

Response: Thanks for pointing out that this was not clear. We have now expanded the description of these metrics as follows: “RMSEP (eq. 4) demonstrates the ability of the model to forecast the rank of a given event, ranked in relation to historical events (i.e., the ability to forecast an event’s place on a cumulative distribution function generated from historical data). While this does not necessarily give an indication of how well the model is able to forecast the magnitude of an event, the ability to forecast an event’s rank is likely to be very useful to users of the forecast, who could, for example categorise a forecast as ‘likely to exceed the 50 percentile of high flows’ (or similar). SSRMSEP (eq. 5) measures the ability of the forecasts to outperform a naive climatology forecast." (Lines 304-309.)

4) Comment: "Given the relative hydroclimatological and geographical similarity among basins, a comment is warranted on the applicability of this approach to other regions and climates."

Response: While we would argue that there is some diversity in the catchments (the most distant catchments are separated by more than 1000 km, with climates ranging from temperate to subtropical), we agree that more discussion of the applicability of this method is warranted. We have addressed this issue as follows:

- We have added more emphasis on the location of the study by adding 'in southeast Australia' to the title of the paper. - We have added a paragraph discussing how the method may be adapted to other regions (lines 572-578).

Minor:

1) Comment: "The distinctions of 'high' flows, 'small-medium' sized catchments. Perhaps more succinct language could be used, e.g. monthly flood forecasting, etc."

Response: Thanks for pointing this out. We now use the term 'mesoscale' throughout the paper to define catchment size, following a number of other studies. The use of the term 'high flows' rather than 'floods' was a deliberate choice, because for highly seasonal catchments 'high flows' defined by rank (i.e., by percentile) often do not constitute floods for months where flows are low. We have included additional justification of the term 'high flows', as follows: "While we have pursued forecasts of large streamflows in a bid to improve information available for the management of floods, we employ the term 'high flows' rather than 'floods' in this paper. This is because we sought to build monthly statistical models in catchments that often have highly seasonal flow regimes. We sample high flows from each month defined by exceedance probability, and in months where mean flows are low these 'high flows' often do not constitute what would be considered flood flows in other months." (Lines 141-146.)

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2) Comment: "L24 Pg 3130: "flood stage" should be used instead of "flood heights'."

Response: Changed. Thanks.

3) Comment: "L17 Pg 3132: The following references are most relevant for the impact of soil wetness on forecast skill: Mahanama, S.P., B. Livneh, R.D. Koster, D.P. Lettenmaier, and R.H. Reichle, 2012: Soil Moisture, Snow, and Seasonal Streamflow Forecasts in the United States, Journal of Hydrometeorology, 13, 189-203, 10.1175/JHM-D-11-046.1. Koster, R.D., S.P. Mahanama, B. Livneh, D.P. Lettenmaier, and R.H. Reichle, 2010: Skill in Streamflow Forecasts Derived from Large-Scale Estimates of Soil Moisture and Snow, Nature Geoscience doi.10.1038/ngeo944."

Response: We have included these references – thanks for pointing them out.

4) Comment: "Clarification is needed as to whether Max 5D represents an accumulated volume, versus a mean flow rate."

Response: It is the average across the 5 days. We now state this explicitly (lines 149-150)

5) Comment: "For ease of interpretation, a column of 'runoff ratio' should be added to Table 1."

Response: Added. Thanks.

References

Robertson, D. E., Pokhrel, P., and Wang, Q. J.: Improving statistical forecasts of seasonal streamflows using hydrological model output, Hydrology and Earth System Sciences, 17, 579-593, 10.5194/hess-17-579-2013, 2013.

Rosenberg, E. A., Wood, A. W., and Steinemann, A. C.: Statistical applications of physically based hydrologic models to seasonal streamflow forecasts, Water Resources Research, 47, W00H14, 10.1029/2010WR010101, 2011.

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Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/1/C2012/2013/nhessd-1-C2012-2013-supplement.pdf

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