Hydrology and
Earth System
Sciences
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Discussions

# Interactive comment on "Attribution of hydrologic forecast uncertainty within scalable forecast windows" *by* L. Yang et al.

Anonymous Referee #1

I would like to thank the authors for the interesting paper. I think the use of ratio of initial storage over total rainfall to analyse the impact (dynamic in time) of Initial Conditions and Future Conditions (e.g. rainfall) on the forecast errors is a valuable contributions of this paper. This approach is simple, but I think original and very effective, and can be clearly explained.

The methodology that has been developed is a contribution to finding out which uncertainties weigh most in which conditions or time of the year, and therefore can provide a way to prioritise efforts for improvement of the forecasts.

The paper is well written with a clear structure.

General comments:

The analysis part for the model uncertainty, by recalibrating the model for the dry period, is limited, as is the analysis and discussion of the results. The authors should consider expanding this part of the research or at least enhance the discussion of the outcome of the experiment and proposing more enhanced and standard ways of Model Uncertainty analysis.

I am not sure the term Forecast Window is completely new. I think forecast window, and moving window, are being used regularly, but it may be that Forecast Window has not yet been clearly defined in a peer-reviewed paper. (A first scan only reveals mentioning in conference abstract). The authors could consider putting less emphasis on it in the Conclusion section.

Detailed comments:

The conclusions in the Abstract are formulated more clearly than in the Conclusion section, so the authors could consider updating the Conclusion text with some of the formulations used in the Abstract text.

A figure with an example ESP hindcast is missing (forecast traces and measured streamflow). Please consider to add.

p. 11798 hypothesis 1) has been tested extensively before. E.g. seasonality effects in hydro-meteorological forecasting. Consider leaving it out here, and keep hypothesis 2 as the only and main hypothesis. p. 11798 hypothesis 3) is not a hypothesis, but a statement. Consider to leave out. p.11802 l.21 Suggest to add here that these initial states are taken mid-season. p. 11802 l.21-23. This is repetition of the previous lines. Please re-write, putting in that the 2 forecasts are made for a period of 30 years from 1970 to 2000. p. 11803 l.7-8 This is stating the obvious. Consider reformulating or leaving out. I think the value of this research is more in providing a method of analysing for any case study what the function is and why. p.11800 l.7-9 Taking the mean to issue a forecast is only correct for the cumulative variables over the forecast window. Eg. Total streamflow over a month. If the sequence of daily flows matters,

then taking the mean each time step is not correct. p11807 l. 20 Suggest a white line after "..flood period." Starting with "In this study.." as a new paragraph. P11807 l.24-25 A literature review on calibration metrics will already table ideas. P11808 l. 3 This is not a clear sentence. Perhaps an editorial. Please revise. P11808 l. 5 "...and has been proven to be.." P11808 l. 21-22 "..are difficult simulated by.." ".. are difficult to simulate with.." P11809 l. 4 ".. the ESP approach has skill." P11809 l. 19-21 This is already a result of the last step in the analysis. Suggest moving it down in the next section where the results are summarised on p11810. P11810 l.4. This is too general. There are also many drought studies, or reservoir optimisation studies, that focused their calibration on the dry season or continuous simulation. Consider revising. P11811 l 4-20. This paragraph reads as a literature review. Suggest leaving it out, or fitting it in the Introduction when explaining that this research is not focussing on the aspects described.

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