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Hydrology and Earth System Sciences
Discussions
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Interactive comment on "State Updating and Calibration Period Selection to Improve Dynamic Monthly Streamflow Forecasts for a Wetland Management Application" *by* Matthew S. Gibbs et al.

Anonymous Referee #2

Received and published: 28 August 2017

General:

The manuscript of Gibbs et al. evaluates the effect of calibration setup on the GR4J model performance within 2 Australian basins on 1-month lead-time hydrologic forecast. Authors draw mainly following conclusions based on 2-basin analysis using 5 indicators: (I) the length of calibration period does not necessarily should be as long as possible, in particular when changes in flow regimes are observed. Additionally, (II) the authors state that a simple model state updating improves hydrological forecast at 1-month lead time.

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Based on my review, I consider the overall topic to be relevant for HESS, however, some parts of the manuscript need to improved and clarified, as further suggested below.

Major comments:

1. In general, it is not surprising that updating model initial conditions has benefits on hydrologic forecast. Additionally, it is not surprising that changes of physiographic conditions may change the catchment's response, indicating different information content/validity of observed discharge data on model parameters. This only confirms observations of previous studies, which some of them are cited. Would be nice to more clearly demonstrate benefits over existing/operational approaches (in terms of costs etc). In particular, when the title includes words like "Management Application".

2. Unfortunately, the analysis is limited to two basins, which really can't be used to draw any conclusions (as authors also recognise in the end). I would strongly encourage authors to enlarge the number of basins and events. Another two basins may yield completely different results; therefore, generality should be avoided.

3. Would be nice to relate your results with another study, which details CRR state updating for 1-month forecast. However, I wonder, whether it is really the effect of state updating here. It is well recognized that the effect of initial conditions (based on discharge observations), in such small basins, diminishes after a couple of days. Please, comment on this.

4. The description of basin and data is way too long and detailed (3 pages), in particular when the discussion and conclusion do not come to those details at all.

5. Please, place error bars into figure 4, in the same way as the uncertainty is presented in following figures and provide discussion.

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6. Discussion about alternative types of observations (besides Q) may be provided in the manuscript.

7. What is the main applicability of your findings? Are they going to be used operational, if yes, what are the benefits over existing forecast method? Please, clarify.

Minor:

- Third sentence from the Introduction regarding Drain M catchment should be moved somewhere towards the end of Introduction.
- P 6, L.5: evapoconcentration => evapotranspiration?
- Section name 2.2 “Streamflow and streamflow data”: sounds a bit repetitive
- P 9, L.21 “burn-in” into quotes
- Eq. 12 is wrong, sum in the denominator is missing
- Caption of figure 2: “POAMA” => “POAMA-2”

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