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Interactive comment on "Parameter-state ensemble data assimilation using Approximate Bayesian Computing for short-term hydrological prediction" *by* Bruce Davison et al.

Anonymous Referee #2

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General comments

This paper introduces a data assimilation approach that uses a filtering method to select an ensemble of best parameter sets over a certain time window from a total population of generated parameter sets. The simplicity of the method is appealing. However, several methodological choices are questionable and not properly justified.

1. The authors state that a drawback of traditional filtering methods that update the model state only or update state and parameters simultaneously provide state and parameter values that are not consistent (described in the Introduction, and re-iterated in the Discussion). But this "inconsistency" is the exact nature of filtering-based data

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assimilation as opposed to variational approaches and smoothing methods. The objective of filtering is to provide the best estimate of the state (or state and parameters) at a certain time, taking the underlying model predictive and observational uncertainties into account. In the approach presented in the paper, only model parameter uncertainty is accounted for, and the results clearly show that this is not sufficient to produce skilful predictions from the filtering. These results are not surprising, since the filter does not explicitly account for one of the major error sources in hydrological modelling, the error in the precipitation forcing. Data assimilation methods that update the model state directly account for this error.

2. The filtering approach developed uses a 3-day window to select the top 10 best parameter sets. The use of a 3-day window is not justified, and it seems questionable whether such a short window is sufficient considering the different time scales of runoff responses, ranging from slowly varying baseflow to fast responding overland flow contribution. The optimal window size will depend on flow regime.

3. A latin hypercube sampling approach is applied for generating the population of parameter sets from which the top 10 parameter sets are selected in the filtering approach. The authors discuss the limitation of the LHS approach. I wonder why this limitation has not been addressed in the work. The results of the bulk calibration filter that corresponds to a classical calibration-validation approach clearly show the limitation of the LHS approach.

Detailed comments

1. Page 2, line 19-21. Not clear exactly what you mean by this statement (see General comments above).

2. Page 5, line 17-18. Explain “CLASS tile” and “GRU”.

3. Page 6, line 1-2. How were the parameters and parameter intervals chosen for the LHS sampling? Based on a preliminary sensitivity analysis?

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4. Page 6, line 11. Abbreviation “H-EPS” not defined.

5. Page 6, line 19-20. How did you justify that the choice of the 10 best parameter sets is optimal?

6. Page 7, line 9. Abbreviation “CaPA” not defined.

7. Page 10, line 8-13. A long explanation. Rephrase.

8. Page 10, line 20-25. Include a paragraph where you introduce the test period and test events.

9. Page 10, line 26-29. Description of the reference forecast is out of place here. Move to the previous section where it is already introduced (page 9, line 5-6).

10. Page 11, line 27-29. Not clear how the water storage value is calculated. Is it a state variable in the model? Or is it assessed using the water balance calculations described in the discussion?

11. Page 13, line 4-6. Not clear how the 91 parameter sets are chosen. And how can this approach be applied in an operational setting?

12. Page 13, line 11 and line 21. Use “reference forecast” instead of “unskilled forecast”.

13. Page 15, line 28-29. An example of using SMOS for DA in a hydrological model can be found in Ridler et al. (2014).

14. Page 16, line 8-10. The use of LHS is identified as one of the key limitations of the approach developed. So why wasn’t this issue further investigated (see General comments above)?

15. Page 18, line 6-7. Why is it an advantage that parameters and state variables are consistent (see General comments above)?

16. Page 18, line 12. Abbreviation “H-LSS” not defined.

17. Tables 2-3. Very detailed information, and difficult to understand without knowledge of the model applied. I suggest to move this to Supplementary material together with a brief description of the model applied.

18. Table 5, caption. Delete “low-skill”.

References

Ridler, M.E., Madsen, H., Stisen, S., Bircher, S., Fensholt, R., 2014, Assimilation of SMOS derived soil moisture in a fully integrated hydrological and soil-vegetation-atmosphere transfer model in Western Denmark, Water Resources Research, 50(11), DOI: 10.1002/2014WR015392.

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