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Discussions

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

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Responses to RC2

Major Concerns

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 assimilation as opposed to variational approaches and smoothing methods. The objec-

tive 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.

Author response: The concern raised by the reviewer points to two sentences of relatively minor significance to the paper. These two sentences will be removed:

The following sentence in section 1 (p2, lines 19 – 21) will be removed. "One drawback of traditional DA (of states only) and of the aforementioned parameter and state DA methods, however, is that the resulting parameters and states are not necessarily compatible with one-another."

The following sentence in section 4.4 (p18, lines 6 and 7) will be removed. "Another advantage is that the parameters and state variables are always consistent with one-another. This cannot be said for other approaches such as the dual Particle Filter or dual Ensemble Kalman Filter."

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.

Author Response: Our response here is the same as for the first reviewer, and is repeated below:

This is an excellent point. The relatively small size of the basin (1,324 km^2) provides some justification for smaller time periods to be used as filters. The 3-day filter would likely be completely unsuitable for basins that are much larger. In addition, the results of the parameter and preceding streamflow filter illustrate that obtaining reasonable results during rainfall events require more than the 3-day filter of streamflow alone. The reviewer's point that parameters that produce good baseflow are rarely the same parameters that produce good flood peaks provides justification for an application of the P-SEDA method that includes parameter sets that produce good baseflow as well as parameter sets that produce good flood peaks, or more likely parameter sets that produce simulations that capture the transition between low and high flows.

The final sentence of the third paragraph in section 4.2 will be changed to include "... rain during dry conditions, baseflow conditions, flood peaks, transitions between low and high flows, or whatever else..."

In addition, the following paragraph after the third paragraph in section 4.2 will also be added: "The relatively small size of the basin (1,324 km^2) provides some justification for the relatively small time period of three days to be used as a filter. The 3-day filter would likely be completely unsuitable for basins that are much larger. However, the results of the parameter and preceding streamflow filter illustrate that obtaining reasonable results during rainfall events requires more than the 3-day filter of preceding streamflow alone. One possible solution to this problem is to have a longer filter period, but other options also exist, as described in the following paragraphs."

Finally, a short paragraph will be added at the end of section 4.2. "All such possible methods to improve the P-SEDA approach require further examination that is beyond the scope of this paper."

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.

Author response: This limitation will be addressed in future work.

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

Author response: This sentence is relatively insignificant for the paper and will be removed. Please see response above.

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

Author response: This sentence is relatively insignificant for the paper and will be removed.

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

Author response: An earlier version of the paper included more details about the simple study conducted to select the parameters. A small amount of detail will be reinstated by adding the following sentence at the end of the paragraph on page 6, line 2. “The parameters that were perturbed were based on the lead author’s experience with the model. Parameter intervals were set based on the ranges found in sources identified under the source column of Table 3. In the case of User specified parameters, these were set by the lead author.”

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

Author response: Thank you for catching this. H-EPS on Page 6, line 11 will be replaced with “Hydrological-Ensemble Prediction System (H-EPS)”

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

Author response: The choice of the 10 best parameters was arbitrary and is deemed to illustrate the P-SEDA methodology The following sentence will be added on page 6, line 20. "(The number 10 was arbitrarily chosen because it illustrates the P-SEDA methodology.)"

6. Page 7, line 9. Abbreviation "CaPA" not defined.

Author response: CaPA is defined on page 5, line 25. No changes to the text will be made.

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

Author response: We will rephrase the entire paragraph as follows:

"Note that these periods do not necessarily correspond to the rising-limb and recession periods of the hydrograph since the river does not always respond strongly to the precipitation for the time period of study in this basin. As a result, for lack of better terminology, these periods are hereafter referred to as "rain-influenced" and "rain-free". We ask for your indulgence in the potentially confusing use of the terms "rain-influenced" and "rain-free." It would be more correct to say "periods during and immediately after the rainfall within the 3-day period" and "otherwise rain-free," but this terminology would be cumbersome throughout the remainder of the paper."

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

Author response: The existing (short) paragraph will be altered as follows:

"Recall that MESH is run in a continuous simulation mode for the period of June 2002 to November 2014, with a more detailed analysis of the ensemble selection methodologies from June 1 to October 31, 2014. Within this time period, there are five significant precipitation events. The beginning and ending of the precipitation events are considered as follows:"

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).

Author response: The following sentences will be removed from Page 10, line 26-29

"The skill is calculated with an unskilled reference forecast, which in this study is taken to be the measured streamflow at 00 UTC and 12 UTC each day as the forecast for the next 72 hours. This reference forecast is a persistence forecast, which assumes the streamflow is persistent for the forecast period."

and inserted in the first paragraph of page 9 as follows:

"First, a qualitative analysis is undertaken to take advantage of the human brain's ability to synthesize information. The results are then quantitatively verified using the Ensemble Verification System (EVS, Brown et al., 2010). To examine the quality of the ensemble mean when compared with the corresponding observation, the mean error (ME) is calculated. Then the quality of the ensemble distribution is calculated using rank histograms. Finally, the skill relative to using the current streamflow as the forecast is calculated using the mean Continuous Ranked Probability Skill Score (CRPSS). The unskilled reference forecast in this study is taken to be the measured streamflow at 00 UTC and 12 UTC each day as the forecast for the next 72 hours. This reference forecast is a persistence forecast, which assumes the streamflow is persistent for the forecast period."

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?

Author response: This storage is a state variable in the model. The text on Page 11, line 28 will be changed by replacing the words ". . . water storage values for each. . . " with "water storage state variables for each. . ."

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?

Author response: The 91 parameter sets are chosen by confining the values of the normalized parameters based on the author's interpretation of Figure 6. The text will be adjusted as follows:

"Based on a subjective visual analysis of these box-plots, the 10,000 parameter sets are reduced to 91 parameter sets by confining the values of the normalized parameters as follows. . ."

This approach cannot be applied in an operational setting and simply provides some assurance that the method has the possibility of being useful, as discussed in section 4.2.

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

Author response: "reference forecast" will be used 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).

Author response: Thank you for this reference. It will be added to the list of references in the paper.

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)?

Author response: Please see response to the General comment above.

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

Author response: Please see response to the General comment above.

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

Author response: H-LSS is defined on page 2, line 3.

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.

Author response: There is a very brief description of the model in section 2.3. Tables 2-3 can be moved to supplementary material if required.

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

Author response: “low-skill” will be deleted in the caption of Table 5.

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