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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Operational river discharge forecasting in poorly gauged basins: the Kavango River Basin case study" *by* P. Bauer-Gottwein et al.

Anonymous Referee #3

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This manuscript addressed operational forecasting in the catchment located in Africa. The topic and contents of this study will be of interest to broad ranges of the scientific and engineering community especially because they achieved improved forecasting via combination of a well-known rainfall-runoff model, SWAT, and a basic data assimilation technique, Kalman filtering on a linear routing scheme. However, no innovation is found in their methodology on data assimilation and rainfall-runoff modeling compared to their previous publications in HESS (Michailovsky and Bauer-Gottwein, 2014) and WRR (Michailovsky et al., 2013). Although their focus seems to be on "operational" applications in "poorly" gauged basins, new methodology or finding is limited for these

two targets. Therefore, major revision should be required for possible publication in HESS considering following comments:

1. Methodology for operational forecasting

Even though values of this manuscript could be found in terms of "engineering", they didn't present any advanced method required for operational setting. Without innovation for these main keywords, this study could be mere applications using modeling techniques, developed by themselves in the previous publications, and operational input forcing. Therefore, I strongly suggest authors would provide additional methodology or analysis on operational applications for readers to have more confidence and understanding on their approach. For example, rainfall forecasts could be analyzed for varying lead times. If significant bias exists in operational forecasts, authors should re-think additional treatment such as bias correction or pre-processing, which shouldn't be remained as future study in such a case.

2. Data assimilation

2.1 It is interesting that the study utilizes the AR1-type model and runoff correlation matrix in the model noise specification. However, the impacts of the noise specification were not clearly verified in the manuscript. Please clarify how the error specification would affect the performance of discharge forecasts with additional evidence and analysis.

2.2 Although authors used several probabilistic measures such as coverage, sharpness, ISS, and CRPS, it was hard to find analysis on appropriateness of probabilistic forecasts. As DA is expected to reduce uncertainty range, only comparison between DA and open-loop could not justify appropriateness of probabilistic forecasts. It is recommended that authors should add evaluation and analysis of probabilistic forecasts based on their uncertainty assumption on observation and simulation (10% of standard deviation of discharge uncertainty seems to be underestimation especially in poorly gauged basins as well as in high-flow seasons). An additional measure such

as predictive Q-Q plot could be useful to assess appropriateness of the probabilistic forecast.

2.3 Description on DA procedure is not enough for readers to understand and reproduce this study. Authors should revise the manuscript with additional description and equations about how noise specification would be applied and updated in Kalman filtering equations.

2.4 Please clarify how the error model is applied in DA if observation is missing. It is not clear how confidence interval is estimated in open-loop simulations, either.

3. Methodology for poorly gauged basins There is no doubt that the study area is a poorly gauged basin. But, there is no technical treatment or analysis for poorly gauged basins in the manuscript, although authors might think public-domain input forcing and models are solutions. Therefore, the current title could confuse readers who are finding new methodology for data-sparse regions. I wonder if it is proper to use the term "poorly gauged basins" in the title with the present content.

4. Revision of abstract

4.1 As an anonymous reviewer addressed, description on funding body in the abstract is not desirable especially because this manuscript covers limited parts of this project. Referencing the website in the middle of the manuscript might be enough to show relationships of this study with the entire project.

4.2 I couldn't find any evidence supporting the sentence "the value of the forecasts is greatest for intermediate lead times between 4 and 7 days". In the contrary, the accuracy of forecasts seem to degenerate gradually according to increasing lead times.

5. Terminology

The authors used the term "hydrodynamic model" to indicate the Muskingum routing scheme, which might be different from general usage. As my limited knowledge, the hydrodynamic models usually refer to simulation models to represent the motion of

the flow by momentum and continuity equations. I don't think the Muskingum routing scheme belongs to a range of hydrodynamic models. Instead, river routing scheme, as the authors used in their previous publications, would be a better term to indicate the Muskingum scheme through the manuscript.

6. Formulation of objective function

The authors included NSE and ME (Eq. (2)) in the objective function. However, ME varies in wider ranges compared to NSE. Please justify how two measures could be used having similar influence.

7. Figure 7

It would help readers to understand discharge forecast more clearly to add hyetographs of (catchment-averaged) forecast input forcing for each lead time in Fig. 7.

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