Interactive comment on "Identification of hydrological model parameters variation using ensemble Kalman filter" *by* C. Deng et al.

Anonymous Referee #1

The authors present an approach to estimate time-variable model parameters within an Ensemble Kalman Filter based framework. Therefore, a two-parameter hydrological model is applied, which estimates runoff based on precipitation and evapotranspiration data. In contrast to other EnKF-applications with time-invariant model parameters, the state prediction is separated into a two-step process. First, the model parameters are predicted (with some well defined uncertainty bounds). The state is then predicted using these new parameters. By that, the presented method is able to describe and estimate temporal variations (e.g. trends) in model parameters.

First of all, I totally agree with the authors that the time-variability of model parameters require attention and are worth to analyze. This holds especially true in the context of a changing climate and anthropogenic interventions in the water budgets,

where relationships between variables and parameters might change significantly over time.

However, I was rather disappointed when reading through the paper. My main point of criticism is the overall immature state of the manuscript. While the title and the abstract sounded promising, the presented analyses, together with a significant lack of motivation, justification, and information left many open questions. The inconsistencies in the formulas as well as a confusing structure of the manuscript, bad language, and quite strange word choices further make it very difficult to understand what the authors really want to show and how they obtain their results. Overall, I have the feeling that the manuscript requires a thorough proof reading by e.g. an experienced senior-scientist.

Many parameters and variables seem to be defined completely arbitrarily or taken from other studies without motivating and discussing the reasons for these choices. Furthermore, there is no justification about the different methods applied (EnKF, the dual-state parameter estimation approach from Moradkhani, 2005, . . .). The authors simply take these methods as a given without discussing the advantages and disadvantages with respect to their study.

The results section left many open questions and lack of significant analyses and findings of the approach presented. The authors further draw some confusing conclusions from their data (e.g. the trend line in Fig. 10; increased water storage capacity in the basin due to "land use changes", but no trend in the estimated SC). It is further left open if the abrupt changes (Fig 10, top) and trends (Fig. 11) in the estimated parameters make sense and how they might influence the runoff estimates. In fact, I don't think that the comparison against the SCE-UA-method is reasonable for analyzing the strengths of the proposed method. The authors should rather compare the performance of the EnKF with and without time-variable model parameters. But in

the current version, it is completely unclear if the proposed method is able to improve the runoff estimates compared to a model run with time-invariant parameters.

Overall, this suggests that there is a lack of understanding and motivation behind the presented study and the methods applied. However, I think that the general idea of the development of a data driven (EnKF) and simple hydrological model (the two-parameter model) which considers time-variables parameters (the dual-state parameter estimation approach) sounds very interesting and is worth to investigate.

Therefore, I would be happy if the authors could improve the paper significantly, as their basic idea sounds really promising. But I am not sure if all the issues and inconsistencies in the paper can be addressed within major revisions. The authors might have to re-write large parts of the manuscript, add a lot more details and information, and perform new analyses and calculations. **Therefore, I have to reject a publication of the manuscript in its current form in HESS**.

Here are some further points which should be addressed in a potential revision:

1. You should add some more motivation of your work and a better overview over similar studies to the introduction.

2. Explain in more detail which methods you're using and why you're using them. Why do you use an EnKF? You don't have a lot of data so there is actually no need to approximate the propagated covariance matrices with the empirical sample covariances.

3. You refer multiple times to the term "data assimilation", which (in my opinion) does not make sense here. You are using only single time-series for precipitation, evapotranspiration, and runoff which are perturbed with some predefined noise. Thus, there is no real "assimilation" of e.g. a large ensemble of data into their

two-parameter model.

4. Go through your equations! You're using some parameters twice (e.g. S). Please explain how the "forward- and observation operator" look like. Furthermore, the definition of the variables seems quite confusing (e.g. below equation 9).

5. The set-up of the synthetic experiment needs to be discussed in more detail (Why did you chose these 4 scenarios? What do you want to find out with these? . . .).

6. Combine all performance metrics (NSE, VE, RMSE, . . .) in one section.

7. Explain in more detail how you generate the synthetic data and which observations you're using. Furthermore, it would make much more sense if you could use more data (e.g. different modeled and observed precipitation and evapotranspiration data). Then, you could derive some reasonable uncertainty bounds, which you could use within your EnKF.

8. What is the SCE-UA-method and why did you chose it for comparison?

9. You have to analyze your results more carefully. Please try to give an explanation for some of your findings (e.g. why are the results for the SC- better than the C-parameter and why is there a time lag in the "assimilated" C-parameter?).

10. The results from the case studies sound more like catchment description rather than a thorough analysis of the method, the estimated parameters, and the estimated runoff time-series.

11. You could combine Figures 1-4 and Figures 5-8 in two plots.

12. The distinction between the two trend lines in Fig. 10 (top) does not make a lot of sense. That being said, can you give an explanation for this sudden change in 1972?

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