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Interactive comment on "Improving streamflow predictions at ungauged locations with real-time updating: application of an EnKF-based state-parameter estimation strategy" *by* X. Xie et al.

Anonymous Referee #2

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In this paper the authors discuss a data assimilation method for parameter and state estimation with application to ungauged watersheds. The methodology uses streamflow observations of a neighboring catchment to resolve states and parameters of another (ungauged) basin. The methodology is illustrated using data from a nested watershed with immediate upstream and downstream subbasins.

The paper is well written and discusses an important and difficult subject in hydrologic modeling and prediction. I am not convinced whether the methodology is useful in realworld situations, particularly when the assimilated catchment and ungauged catchment have different geology, climate conditions, topography, slopes, and soils (among others). I believe that the methodology will only work well if a strong correlation exists between the gauged and donor catchment – thus significant correlation between the assimilated discharge and streamflow of the ungauged basin. And this is the case in the present situation with immediate upstream and downstream basins. Otherwise, the methodology serves no purpose and goal. But if the streamflow is so highly correlated why not use another methodology to transfer the states and parameters? Would the EnKF and presented methodology really provide so much advantage? I doubt that this is the case.

Technical comments

1. Joint parameter and state estimation. Do the parameters converge to their appropriate values? This is a technical question that requires simulation with synthetic data to demonstrate that the methodology converges adequately, both for the gauged and ungauged basin. I believe a synthetic case study with known states, and parameters would help to elucidate the theoretical foundation of the applied methodology. This is often not so important in practical application but I think the impact of the paper would be enhanced significantly if the authors can underpin their method with convincing convergence results.

2. Page 13449: The authors provide a recipe of their assimilation methodology, where one parameter is considered at a time. I cannot believe that this approach would converge adequately. It might be applicable in practice but ignoring parameter correlation will not lead to the "best" possible model performance. Indeed, one can rapidly calibrate a distributed model by estimating one parameter at a time (based on order of sensitivity), but the parameters estimated with this strategy cannot give the best possible model performance, nor will it lead to reasonable parameter values that can be used in regionalization. A joint updating scheme would seem more appropriate but is computationally much more demanding. A synthetic study would demonstrate the

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limitations of this approach.

3. Page 13447: The algorithmic parameters used in the kernel smoothing will strongly determine the spread of the parameter ensemble, and hence the convergence properties of the EnKF. How are these settings determined on a case by case basis? The final parameter distribution, at the end of assimilation, will be strongly dependent on the properties of the kernel, which in my view is not desirable. A synthetic study will evidently demonstrate this problem.

4. Figure 2 (and others). Why not include the discharge observations in the same figure (left panel)? This would give a better understanding of the behavior of the model rather than a separate plot of the residuals (right panel).

5. The authors use the word "prediction", but use measured rainfall (with some perturbations). The word prediction would be appropriate if rainfall was assumed unknown and derived from other sources/models.

6. The data assimilation results are evaluated using measures of central tendency such as RMSE, MAE, etc. What about the ensemble spread? And how realistic are these intervals? Are they statistically significant? In other words, do the 95% simulation intervals contain 95% of the discharge data? I think that the authors should include explicit measures of ensemble width.

7. Figure 4: I think the histograms of the parameters in each subplot should have a common x-axis – makes it easier to compare and graphically diagnose convergence. Also the y-axis used in the three big panels – are they consistent with the prior distribution? Or are they chosen so that the histograms fit within the figure? What I miss again is a synthetic study. There is no way to verify whether the parameter estimates at the end of simulation are reasonable or not.

8. Figure 4: The parameters have nicely converged to a limiting distribution, with relatively little uncertainty. I question whether these distributions are realistic and if

the system properties suddenly abruptly changed the filter would be able to cope with this. The parameters should be able to continue to travel – this ability all depends on the chosen kernel smoother, and so does the final shape of the histogram of the parameters. The Gaussian perturbation in Eq. (3) favors normality of the parameters. If another kernel smoother was used, the parameter distributions would be different, and so will their distribution.

9. The authors present the results of a single filter run. Are the results similar if another run was done? My experience suggests, that with sufficient state and parameter dimensionality, the filter results are somewhat run dependent, unless an extremely large ensemble is used. For practical application it is desirable that the filter results are stable and convergent, and for instance not smoother dependent.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 13441, 2013.