Reply to Referee's comments for article hess-2013-463

Dear Editor and Referees

We greatly thank you for providing very helpful comments to improve this paper. Based on your comments and suggestions, we made substantial revisions. Please see the point-by-point reply. A list of relevant changes is shown at the end of this reply.

Note: The text in *italic type* is the original comments copied from the referees, and the text in normal style with 1.5 line spacing, headed with "Reply", is the reply from the authors. When we mention specific sites (e.g., Line 20-24 of Page 4) in Reply to indicate revisions, these sites are all with respect to the revised manuscript instead of the printed version of HESSD.

Please let me know if you any other comments.

Best regards,

The Authors

Reply to Referee #1

Dear Authors, I enjoyed reading this manuscript, which I consider to be very interesting. The study addresses very important issues associated with the assessment of the streamflow prediction in ungauged locations transferring information (in terms of covariance matrix) from gauged locations. An Ensemble Kalman Filter, partitioned forecastupdate scheme, was used to update the states and parameters of a distributed hydrological model, SWAT. The previous methodology was applied to the Zhanghe basin, in China, assuming different scenarios of gauged and ungauged locations. The results of this study showed how the assimilation of streamflow observations at gauged locations can improve the prediction of discharge at ungauged positions. The paper is generally well written and easily understandable by the readers. However, the introduction has to be better organized to focus on the main innovation of this study. The study is, in my opinion, of broad international interest and it can be considered worthy for publication after a minor revision. I list below some main comments which I sincerely hope can become useful.

Reply Summary：

We would like to thank the reviewer for giving positive and constructive comments. We revised the introduction and provided explanations to the comments. Please see the reply below to each comment.

(1) As mentioned before, the novelty of this study it is not well presented in the paper. The section "Introduction" of this paper can be schematized in two different parts. In the first one, a description of the PUB initiative and a brief review of the regional methods used to propagate information from gauged to ungauged basins are proposed. Then, methods for data assimilation (Ensemble Kalman filter) with respect to the states-parameters estimation are reported. Honestly, I cannot see a connection between these two parts. Is this study the first one which deals with implementation of a data assimilation method in estimation of streamflow in ungauged sub-basins? My suggestion is to better explain if the proposed approach is actually new, by providing a better review of related publications about this issue (regional methods based on data assimilation techniques).

Reply：

Both of the regionalization and the data assimilation techniques can be used to address the issues associated with PUB. The regionalization technique is intentionally developed for PUB, and it is usually based on either a similarity approach or a statistical approach (Sellami et al., 2013). The data assimilation technique transferring information from gauged to ungauged basins is based on physical correlations between the neighbouring basins. So in the introduction we provide a brief review of the two techniques: regionalization and the data assimilation.

The data assimilation method used in this study (i.e., the PU_EnKF) was proposed by Xie and Zhang (2013) who have presented extensive documentation based on synthetic studies. To our knowledge, this study is the first one which explicitly employs a data assimilation method (i.e., PU_EnKF) with state-parameter estimation to improve streamflow prediction in ungauged locations. We do not find any references discussing regionalization methods based on data assimilation techniques. In the revised version of this paper, but we present more explanations of related publications about hydrological predictions with data assimilation to make the introduction more informative.

The main points of the reply are included in the manuscript, please see Line 20-24 of Page 4, Line 23-27 of Page 5, and Line 1-2 of Page 6.

(2) Another issue is related to the concept of gauged and ungauged locations. Sivapalan (2003) mentioned that ungauged case is the case in which observations of the variables we are trying to predict are short, of too poorly quality, or even nonexistent. My concern is that, in the framework of ungauged basin and streamflow estimation, the authors applied distributed hydrological model which usually require a significant amount of data. May the Authors explain this choice (in addition to the reasons described at line 13 of page 13451 of the manuscript)?

Reply：

We agree with the reviewer that the application of a distributed hydrological model (DHM) is limited by its extensive requirements of data sets, including system input and response data (e.g., runoff, evapotranspiration, soil moisture). With the development of observation technology (e.g. remote sensing), most of model input data (e.g., forcing data, land cover, soil properties, topography) are becoming available in certain precision. So we think the dominant factor restricting the application of DHMs is the system response data, especially the water discharge data which are generally used to calibrate the DHM.

If system response data are not available for a basin of interest (or the data quality is too poor), one may resort to credible input data and a capable DHM, but the model effectiveness is not guaranteed due to various unknown uncertainties. The evapotranspiration, soil moisture data and others from remote sensing retrieval would be useful for model calibration, but they are not so widely used in calibration due to notable uncertainties in these data. Much of the success for PUB decade so far has been in gauged basins instead of in ungauged (Hrachowitz et al., 2013).

In this study, the gauged data are also required for some sites (at least one site) in a basin of interest, and those data information is transferred to ungauged locations in the same basin by the data assimilation method. But the issue of extensive data requirement for DHM can be eased to some degree, because data from a few critical locations (e.g., the data from the basin outlet) can favor acceptable predictions as illustrated in this study and the study by (Xie and Zhang, 2010). The points are included at Line 13-18 of Page 12.

(3) As described by the Authors, the correct estimation of the number of ensemble used in the EnKF is a delicate problem since the EnKF performances are directly connected with the model spread. The Authors provide a clear description of the method used to estimate the number of ensemble members but I could not find this last information in the paper. I think that an indication about the number of ensemble members (e.g. 10, 50 or 100) might be interesting for other researchers.

Reply：

The ensemble size in this study is 80. We included this information in the revised manuscript (see Line 19, Page 15). Sure, the larger ensemble size the better assimilation performance, but it will render higher computational cost.

(4) In the section "Assimilation setup and scenario design", the Authors proposed to assimilate observations in interior points of the basins (ASS_BD and ASS_AB) in order to improve the streamflow prediction in pseudo-ungauged location (location C). Assimilation of discharge data in interior points of the basin was already analyzed in other studies (Clark et al., 2008; Rakovec et al., 2012; Chen et al., 2012; Lee et al., 2012; McMillan et al., 2013;). My suggestion is to include these papers in the references of this manuscript.

Reply：

We included these papers as references (Line 27, Page 5). They are valuable for authors and for readers. Thanks.

(5) In the section "Prediction in ungauged locations" the Authors state that "Adding an observed gauge (Gauge B) at the upstream in the basin, i.e. the ASS_BD scenario, provides better streamflow predictions in the pseudo-ungauged sub basins than the ASS_D scenario; the RMSE drops to 1.741m3 s^{-1}*" (around line 15, page 13456). On the other hand, in the section "Conclusions" it is reported by the Authors that "the downstream data have more important roles in the data assimilation than those from upstream" (line 5, page 13459). In my opinion the interior location B provides an improvement in the model performance in C and this can be related to the spatial correlation between the streamflow in B and C. The sentence in the conclusion should be rephrased and it should include the reason why, using a particular location of interior point, there is an improvement in the model performances.*

Reply：

We modified the sentence in the conclusion. The improvement of streamflow prediction using data assimilation depends on the correlation of physical processes between gauged and ungauged locations. If the two locations are very close (which means the correlation of flow processes will be strong), quit encouraging data assimilation performance will be shown. Generally, the downstream data (especially the data from outlet) have important roles to get a big picture of streamflow for the entire basin, since they contain accumulative flow information from all subbasins. Please see Line 13-15 of Page 20 for including of the point in this reply.

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Reply to Referee #2

Anonymous Referee #2
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 real-world 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.

Reply Summary:

We thank the reviewer for providing very useful comments that help us to improve the paper. Based on these comments, we revised the paper and gave a detailed response to each comment.

Yes, this methodology, as any other data assimilation methods, depends highly on the correlation between the gauged and donor (ungauged) catchment. We believe "correlation" is a general assumption within most methodologies (including the regionalization methods (Hrachowitz et al., 2013)) for predictions in ungauged basins. This assumption is valid at basin scale, even not for all. For a particular situation when the correlation is quite week, the EnKF-based methodology is not so effective. However, it still has the advantage that the ensemble simulations/predictions are expected to reduce the streamflow uncertainties. That is to say, the ensemble prediction usually provides better results than a single-run simulation.

The reviewer suggested several times the authors employ synthetic cases to demonstrate the convergence of the methodology used in this study. Actually, synthetic cases associated with this methodology have been done in another study by Xie and Zhang (2013). So here we only show real-world applications for streamflow predictions at ungauged locations.

We have coupled most of the replies in this repose into the manuscript.

Technical comments - Reply

(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.

Reply:

We agree with the reviewer that synthetic experiments are useful to demonstrate the methodology. We have done such experiments with known states and parameters (named as true values), and then examined the performance of the EnKF-based portioned update scheme, i.e., PU_EnKF (Xie and Zhang, 2013). In this case, the parameter estimates successfully converge to their true values after 500-step data assimilation. Please see the left panel of Figure 1. This scheme has also been diagnosed extensively with different iteration update schemes, parameter evolution algorithms and ordering effects (Xie and Zhang, 2013).

In this study, we intend to demonstrate it in real-world case. Although it is hard to detect the parameter estimates with their true values (because the true values are always unknown in a real-world case), we resort to validation of the parameter estimates using conventional hydrological simulations in which the model is fed with the estimates and then compare the simulated streamflow with the observed discharge. That is the case shown in section 3.5 in this paper. So we deduce that the parameters also converge to their appropriate values because the simulation gives acceptable streamflow estimations.

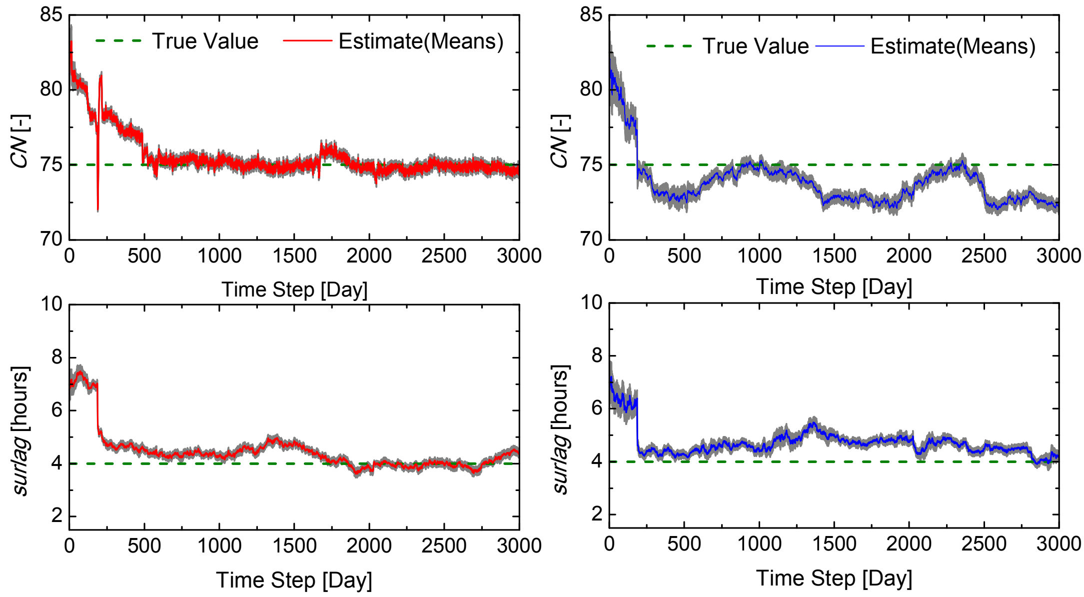

Figure 1 Parameter estimation for a synthetic case using the PU_EnKF (left column) and the conventional EnKF-based joint update scheme. The gray shaded areas is the 95 percentile confidence intervals. This figure is adapted from Figure 3 in (Xie and Zhang, 2013)

(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 limitations of this approach.

Reply:

The PU_EnKF scheme employs an iterative manner to update each parameter estimates **at each time step, not only is one parameter considered at a time**. At time t, the new estimated parameter values from previous loops are used for the model forecasting (Eq. (2)) in the current loop in which a target parameter is estimated. This iterative update is expected to push the estimates towards their optimal values. Please note the parameters are updated through the computed correlation (i.e., the covariance matrix K_t) between the parameter and observable state variable, rather than the correlation between parameters.

Sure, the joint update scheme is alternative for parameter update, but it is vulnerable to corruption due to spurious covariance computation (since the approximation of with a limited ensemble size) and parameter interference especially for high-dimensional state spaces containing various parameters (Moradkhani et al., 2005). To relieve this issue, Xie and Zhang (2013) proposed the portioned update scheme (i.e., PU_EnKF). PU_EnKF has been examined with synthetic cases by comparing with the joint update scheme. PU_EnKF provides better estimations for states and parameters than the joint update scheme, particularly for distributed hydrological models with high-dimensional state and parameter spaces. For low-dimensional problems (such as the lumped hydrological model), both of them may have similar performance (Xie and Zhang, 2013). We coupled the main points in this reply into the manuscript; see Line 7-12 of Page 10.

(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.

Reply:

The Kernel smoothing method was proposed by West (1993) and extended by Liu (2000) for parameter evolution. There is only one parameter to be determined, i.e., the shrinkage factor α. Sure, its setting will determine the spread of the parameter ensemble, but it is typically constrained within [0.95, 0.99] (Liu, 2000). Moradkhani et al. (2005) demonstrated the effectiveness of this kernel smoothing **using synthetic study**. Xie and Zhang (2013) presented extensive discussions on this method also **using synthetic studies**, and the result indicated that it has better behavior for parameter estimations than a random perturbation scheme. When removing the kernel smoothing, the ensemble spreads quickly shrink and their estimates hardly approach to the synthetic true value. So the kernel smoothing is a very favorable scheme for parameter estimaiton. Given such synthetic studies on the kernel smoothing, we do not provide any more experiments to demonstrate the properties of the kernel smoothing. The shrinkage factor α is specified with 0.98 in this study according to the suggestions by (Moradkhani et al. (2005); Xie and Zhang (2013)). The points in this reply are included at Line 3-7 of Page 8.

(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).

Reply:

Please note the eight plots in Figure 2 (and others) are all streamflow prediction errors/residuals (streamflow estimates minus streamflow observations). To make a comparison between the two cases – the control-run simulation and the data assimilation scenario ASS_D, we just present the errors rather than the streamflow observations. Some of the streamflow observations are so large that the difference between the cases is not observable if we include the streamflow observations in the same figure. Please see the indication at Line 1-2 of Page 17.

(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.*

Reply:

The PU_EnKF scheme used in this study is also applicable to hydrological prediction based on rainfall data from weather forecasting and other sources unknown. We present a real-world case with measured rainfall to demonstrate the capability of the PU_EnKF scheme. The rainfall is perturbed to represent the uncertainty probably from weather forecasting and other sources. We think the word "prediction" has an extended meaning, i.e., simulation with measured or forecasted rainfall from other sources/models, which is included in the initiative on Predictions in Ungauged Basins (PUB) by the International Association of Hydrological Sciences (IAHS) (Sivapalan, 2003; Sivapalan et al., 2003). So we use "prediction" as a general term in this paper. These points are included at Line 13-17, Page 14.

(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.

Reply:

It's a very useful suggestion. To measure the ensemble spread of streamflow in data assimilation, we design a measure, i.e., Ensemble Coverage Index (EnCI) that is a percent of discharge data contained in the 95% simulation intervals. The result is shown in Figure 2 and Figure 3. The EnCI for Gauge D is up to 94.8% (see Figure 2). This means that 94.8% discharge data are contained in the 95% ensemble intervals, except that some discharge data with considerable magnitudes of flood are outside of the intervals. The lowest EnCI for Gauge A (73.89%) is partly due to the fact that Gauge A is the farthest gauge to the outlet (Gauge D, its data are assimilated). Nevertheless, all ensemble spreads for the four gauges are reasonable to trace and to contain the discharge data. Please go to Line 13-18, Page 17 for including of this reply.

(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.

Reply:

We modified the three histograms in Figure 4 to have common x-axis. The estimations of parameters are obviously convergent. The samples of parameter are within the prior ranges (Min – Max, see Table 1). The chosen histograms are intent to indicate that the samples are close to Gaussian distribution which is favorable for Kalman filter-based data assimilation schemes. Yes, we cannot verify whether the parameter estimates approach to their true values due to this real-world case, but we have a validation simulation by prescribing the parameters (which are used in the simulation) with values derived from the estimation of data assimilation, please see Section 3.5. The simulated streamflow matches the observations very well (Figure 6). Therefore, the estimates from the data assimilation are reasonable and may approach to their optimal values. Please note the validation simulation is a generally used strategy to verify model parameters in hydrology.

(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.

Reply:

The reviewer raised an interesting question. The kernel smoother is important to determine the parameter evolution within data assimilation. For a successful estimation, the parameter estimation based on the PU_EnKF scheme is expected to trace the changes of the system properties (which drive the model parameters). Although the parameter estimations converge to a limiting distribution, after a few time steps, they still keep at stable levels (see Figure 5) due to the Gaussian perturbation in Eq. (3). With such stable levels and by tuning the two factors, i.e., α and h, the parameter estimations are able to travel with the system changes. Moreover, if the intervals of samples at stable levels are too small, the factor h can be inflated (h = 1.0 in this study) to create a broad range of parameter samples (see Line 1-3 of Page 8). In this study, we exclusively present the results of improving the streamflow prediction in ungauged basins using the PU_EnKF scheme. We are doing another synthetic study with extensive topics: tracing model parameter changes due to the system evolution. Thanks.

(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.

Reply:

We agree that the filter results are run dependent to some degree on the ensemble size, modeling and observation error estimations, smoother factor setting, etc. Some of them are still challenges in hydrological prediction. Xie and Zhang (2010) provided a few general suggestions: the ensemble size is favorably prescribed with 200 for distributed hydrological modeling to balance the approximation of the state distribution and the computational cost; the fractional perturbation (used in this study, see section 3.2) is effective to quantify the modeling and observation errors. The issue associated with the parameter evolution scheme (e.g. the smoothing kernel) was discussed in several studies (Liu, 2000; Moradkhani et al., 2005; Xie and Zhang, 2013) as stated in the reply to question 3. Based on those suggestions, we present the results of streamflow prediction in ungauged basins and exclusively investigate the influence of assimilating data from different locations in a basin.

Although the data assimilation methodology shows limitations in hydrological modeling, it has attractive features to estimate the hydrological variables (such as streamflow) and system properties (e.g. model parameters) with real-time updating.

References

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List of all relevant changes made in the manuscript

No.	To Referee #1's comments: Site for relevant changes in the manuscipt
(1)	Line 20-24, Page 4, Line 23-27, Page 5, and Line 1-2, Page 6.
(2)	Line 13-18, Page 12.
(3)	Line 19, Page 15
(4)	Line 27, Page 5
(5)	Line 13-16, Page 20
No.	**To Referee #2's comments: Site for relevant changes in the manuscipt**
(1)	Line 23-27, Page 18; Line 1-6, Page 19
(2)	Line 7-12, Page 10
(3)	Line 3-7, Page 8
(4)	Line 1-2, Page 17
(5)	Line 13-17, Page 14
(6)	Line 23-25, Page 16; Line 13-18, Page 17; Figure 2 and Figure 3
(7)	Figure 4; Line 23-27, Page 18; Line 1-6, Page 19
(8)	Line 1-7, Page 8
(9)	Line 7-16, Page 10