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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Is inversion based high resolution characterization of spatially heterogeneous river bed hydraulic conductivity needed and possible?" *by* W. Kurtz et al.

Anonymous Referee #1

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General comments:

The paper addresses how to characterize the river bed heterogeneity in the context of river-aquifer interaction problem through ensemble Kalman filter by assimilating hydraulic head monitoring data. The analysis is conducted with the aid of real field setup of the Limmat aquifer in Zurich, but the river bed hydraulic conductivity is synthetically generated as the reference. Full heterogeneous representation and different zonation representations of the spatial distribution of the river bed hydraulic conductivity are experimented with different reference field generation scenarios in terms of how strong the heterogeneity is and absence or presence of prior information on zonation. The

paper concludes that full heterogeneous representation is possible and necessary to obtain better river-aquifer exchange flux estimations, correct prior zonation information will improve the predictability of river-aquifer exchange flux, and more interestingly, full heterogeneous representation of river bed hydraulic conductivity is still possible when less hydraulic head monitoring information is available but with higher estimation uncertainty. Overall, this paper widens the application of ensemble Kalman filter and makes a valuable contribution to science hence it is recommended to publish this paper.

Specific comments:

The main concern for me about this paper is the following. When parameters and state variables are updated simultaneously in each assimilation step with the updated state variable values at t-1 as the initial state variable values at t, the updated parameters and state variables are only consistent with each other for linear model. The simulation model used here, which predicts the head values from t-1 to t, is a non-linear model. That means updated parameters and updated state variables are NOT consistent, especially when heterogeneity is strong, which is reflected in the fact that the updated state variables at t is different from these predicted state variables at t based on the updated parameters at t. See Thulin et al. (2007) and Gu and Oliver (2006) for references. Therefore, only use $RMSE_h$ as the performance assessment criterion is not appropriate in that it may not fully represent the estimation performance of hydraulic conductivity or leakage coefficient in this case. $RMSE_L$ or $RMSE_{LnL}$ is also necessary to be used directly when hydraulic conductivity estimation is the main task through EnKF. In Particular, the paper points out that when less measurements are available (with only 10 measurement points), the estimation of leakage coefficients can be comparative to the case when more measurements are available (with 100 measurement points), and eventually results in that high resolution representation of river bed hydraulic conductivity is still beneficial. That is an interesting point, but it may be more appropriately justified by $RMSE_{LnL}$ instead of $RMSE_h$. Considering that the author shows $\log_{10}$L values on Fig. 10 and 11, I don't see why not use $RMSE_{LnL}$ to justify

the conclusion. What's more, I think the author should list at least some representative RMSE values (if listing all of them for different reference fields and scenarios is tedious) when making conclusions based on them, such as Line 5 on Page 5848, Line 2-5 and Line 21-22 on Page 5849. It would give more quantitative sense for the readers in that way.

References:

Thulin, K., G. Li, S.I. Aanonsen, and A.C. Reynolds (2007), Estimation of Initial Fluid Contacts by Assimilation of Production Data With EnKF, Paper SPE 109975 presented at the SPE Annual Technical Conference and Exhibition, Anaheim, California, USA, 11–14 November. doi:10.2118/109975-MS.

Gu, Y., and D. S. Oliver (2006), The ensemble Kalman filter for continuous updating of reservoir simulation models, Journal of Energy Resources Technology, 128(1), 79–87. doi:10.1115/1.2134735.

Technical corrections:

1. Line 5 on page 5833: It might be better to make it clear that "classical approaches" is actually "classical zonation approaches", because sometimes "classical approach" reminds readers of non-sequential inverse method.

2. Line 8-10 on P5838: How EnKF can improve the prediction stated by the author is confusing for me. Based on my understanding, the prediction is improved by adding an optimal weighted "innovation term" which is the difference of predicted and observation data. It may not be that "measurement errors and the uncertainty of model predictions are optimally weighted", but use "measurement errors and the uncertainty of model predictions" to weight the innovation term.

3. Line 7 on P5839: It may be better to explicitly characterize the normal distribution of perturbations, $\varepsilon_i \sim N(0, R)$.

4. Line 19 on 5839: Damping factor has similar effect with adding an extra model

error in addition to the observation error. It is usually used as a strategy to avoid the ensemble inbreeding. The selection of its value affects the estimated results. Is there any basis to choose this value as 0.1 in this case?

5. Line 6 on Page 5847: The analysis about Z5 is not so accurate because we can see the net flux can be overestimated for reference I, VII and others. A more complete and accurate result analysis may be needed here.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 5831, 2013.