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Hydrology and Earth System Sciences Discussions

Interactive comment on "The importance of parameter resampling for soil moisture data assimilation into hydrologic models using the particle filter" *by* D. A. Plaza et al.

Anonymous Referee #1

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This study presents an application of the EnKF, SIR and its variant in assimilating soil moisture (SM) data into the CLM model for updated soil moisture and basedflow simulations using a set of synthetic experiments. The paper is fairly well written, with consistent (but minor) grammatical issues throughout the context.

My major concern lies in the synthetic nature of the study. SIR with parameter resampling is not new. Therefore, "proof of concept" via synthetic tests does not hold true in this case. Furthermore, this special issue focuses on real-world operations rather than pure research. Having said that, the paper as is might not interest the potential audi-

ence of this special issue. To fit into the scope of this special issue, it is recommended that the authors add real-world experiments where real soil moisture observations are assimilated and the resulting discharge can be evaluated against real discharge observations (e.g., at the station in Pfaffenthal). In that way, the "importance of parameter resampling" (as stated in the title) can be justified in a meaningful way (the current justification based on synthetic experiments is quite weak). Consequently, the impact of this paper will be considerably elevated.

I am also concerned about the experimental design of this study. First, the inclusion of EnKF needs further justification. Apparently, EnKF and PF differ from each other in concept. Comparing the performance of two approaches is hardly fair. I would be cautious about this kind of comparison. As an example, the authors state that the SIR+PR serves as a solution to the shortcoming of EnKF performance (Page 5871, Lines 14-15). Note that EnKF has the capability to simultaneously update model states and parameters (and thus has the potential to overcome "the shortcoming" and even outperform the SIR+PR). However, if we have to compare both approaches, I would state that the EnKF is limited by its Gaussian assumption which likely has "negative effect" on baseflow. Second, I would like to see more detailed explanation on the DA-CLM setting up. I believe that the model is configured with 4 grid cells and produces total discharge at the basin outlet, yet results from only one cell are shown. I am wondering whether four SM timeseries (for each of the four cells) or only one SM timeseries (for the cell of which results are illustrated) is assimilated. I am also curious how assimilating SM in the top layer impact the SM in the remaining nine layers and subsequently the surface runoff, interflow, and baseflow. I am suspicious that this impact would be more in the sense of statistics instead of physical dynamics, given the weak physical correlation between top layer SM and SM in bottom layers (e.g., the 100 cm depth).

I have some further concerns on the results presented. First, Fig.5 and Figs. 7 – 10 show the SM comparisons. Given the fact that there are only SM (synthetic) observa-

tions in the top layer, I guess the SM shown in these figures is exclusively for the top layer (other than the total SM from 10 layers). Looking at Figs. 9 and 10, after Feb.10, 2007, the SIR+PR derived baseflow is almost identical to the synthetic truth, which is mathematically astonishing. However, physically, it is hardly possible that assimilating only top layer SM would lead to "perfect" baseflow simulations, since top layer SM is more correlated to surface runoff or interflow rather than baseflow which relates more to SM in bottom layers. As an example, in Figs. 7-10 when no SM is assimilated (i.e., from Jan. 1 to Feb. 8), model-simulated SM is fairly close to the synthetic true SM. However, the discrepancy between model-simulated baseflow and synthetic true baseflow is significant, indicating that the baseflow is not dominated by top layer SM. These observations raise the skepticism that the SIR+PR might violate the physical relationship between SM and baseflow through mathematical intervention (i.e., adjusting model parameters in a disconnected way), which is more evident when comparing to Figs. 7 and 8 (which show that assimilating top layer SM via EnKF and SIR actually can't improve the baseflow simulation much, while EnKF and SIR preserve the model physics but not introducing inconsistent parameters in different measurement intervals). Yet it might be arguable that assimilating top layer SM would improve estimates on SM in other layers, thus resulting in improved baseflow estimates. However, whether (and how much) assimilation of top layer SM would improve SM in other layers needs to be justified, which goes back to my second point in my concerns on the experimental design. Second, Table 4 shows that SIR+PR derived SM deteriorates in accuracy with decreasing observation frequency (e.g., from every week to every four weeks). In contrast, Table 5 shows that SIR+PR derived baseflow improves in accuracy with decreasing observation frequency. Does this imply that the less SM is assimilated, the more accurate the baseflow estimates? And in Figs. 9 and 10, if only 4 DA events (rather than 16) are considered, the baseflow simulation will further better mimic the synthetic truth?

Lastly, I have some minor comments: a) In Abstract, Results, and Conclusions sections, the usage of "discharge" is confusing. I guess the authors mean baseflow specif-

ically. Note in Line 6 (Page 5854), "discharge" represents total outflow at the gauge. b) The paragraph in Lines 11-22 in Page 5856 seems to be unrelated to the work presented in the study and thus redundant. c) The sentence in Lines 16-18 in Page 5857. The reason is invalid in that I) direct relationship is not the equivalent of linear relationship; II) there are other states like temperature which does not "correspond directly" to soil moisture in an explicit way. d) "Negative (positive) effect" or "negatively (positively) affect" appear numerous times in the context. Please be specified (e.g., overestimate, underestimate) since "negative" (or "positive") is more a relative concept and thus not clear in meaning sometimes. e) When calculating RMSE for baseflow (in Figs. 7 – 10), an alternative period should be used (i.e., starting from Feb. 8 rather than Jan.1), since the model is not warmed up enough in the beginning and errors in initial conditions largely impact baseflow simulations. Additionally, DA is not applied prior to Feb. 8, that period (Jan.1-Feb.8) contains no information regarding the performance of DA. Inclusion of that period in calculating the metrics (e.g.,RMSE) dilutes the actual performance of DA. As can be told from Figs. 9 and 10, the RMSE of baseflow is largely from this period (while the DA is only applied in the remaining period).

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