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Interactive Comment

Interactive comment on "Applying sequential Monte Carlo methods into a distributed hydrologic model: lagged particle filtering approach with regularization" *by* S. J. Noh et al.

Anonymous Referee #2

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The authors assess the applicability of three particle filtering approaches (with increasing complexities) in streamflow simulation at one test basin by assimilating observed streamflow into a widely-used hydrologic model. I have several major comments:

1. The paper, as it stands, does not specify the generation of gridded (250 m resolution) model forcing and parameters as well as the connectivity between neighboring grids (flow direction in individual grids, the conflux of flow from different grids). Additionally, soil moisture states at different layers and different grids are perturbed using a same error definition (Equations 22 and 23) (so is the overland flow state). Those

observations elicit a set of probing questions which require comprehensive answers:

a) Is this study really a distributed case or more like a lumped case? Are the paper subject and the context overstating the work and thus confusing the readers? Note that a distributed model can be applied at a lumped scale.

b) What is the rationale of picking up only soil moisture and overland flow as model states to be updated? In such a humid basin, interflow and baseflow could significantly contribute to the flood volume.

c) What if r_s in equation (22) is greater than 1 and thus produces unrealistic soil moisture (e.g., equation (23) provides soil moisture values greater than 1)? Applying a same multiplier to all layers and all girds is problematic (same for the overland flow).

d) Erroneous uncertainty definition (for soil moisture and overland flow states) very likely creates pseudo correlations between measured variables (to be assimilated) and model states (to be updated). As such, the update could be unfaithful. Should this be investigated and justified (e.g., at a small basin in a lumped fashion)?

e) The big size (1100/0.25ˆ2 = 17600 grids) of the test basin makes the problem more intractable. How the lag time associated with each grid can be reasonably defined (all grids sharing a single lag time is no better than treating the whole basin as one lumped grid)? Are the correlations between the outlet streamflow and the soil moisture states at three layers (and/or overland flow) of each grid physically sound or just purely statistical? Would assimilating streamflow indeed improve estimates on current soil moisture/ overland flow and subsequently improve streamflow predictions in the future time?

As a matter of fact, many of above questions can be addressed by adding a simple study (e.g., via synthetic experiments at a smaller scale (1 grid), as conducted in the study of Weerts and El Serafy (2006) in the reference list).

2. The experiment design of the study could be improved.

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As an example, selecting a calibration period and a validation period needs further clarification. Is the calibration period used to determine filter parameters (e.g., error configuration) and/or distributed model parameters (e.g., saturated hydraulic conductivity)? Either way, how the calibration is conducted (e.g., procedures, objective functions, algorithms etc.)? Why Fig. 10 and Fig. 11 present results from two different sub-periods of the calibration period (for the validation period as well)? What alternative periods could be selected as calibration period and validation period (the current periods representative?)?

Another example is that the study focuses only on 2-h-lead predictions. The potential added value of DA to any operational forecasting system is that the technique could provide improved estimates on model states at the current time (forecast time) by assimilating the most recent observation(s). These "optimized" model states are expected to produce meaningful streamflow predictions with long lead time. In operations, the lead time for (typically called) "short-term forecasting" is up to 36 hours or even longer. If a DA technique only shows benefits for 2-h-lead predictions, for one, the technique would not be applicable in any operational environment; for two, most likely, the DA technique has significant flaws in science (refer to comment # 1). To be more specific about the second point, due to the fact that flow observations are assimilated so frequently (every hour) and the forecasting window is so short (2 hours), it is not surprising at all that the flow predicted highly resembles the observed. The flow prediction is actually dominated by the observed flow and not produced from the updated states at the forecast time. What if the update frequently is different (e.g., every 6 hours) and forecasting window is larger (e.g., 36 hours)? This study will make a high impact if it could show that these approaches used really improve estimates on model states which lead to improved flow predictions with a large lead time. The authors could refer to the study of Seo et al. (2003) (in the reference list) regarding flow predictions at various lead times.

A further example is that the analysis only focuses on limited periods and events (e.g.,

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Fig. 8). As such, the results are not generic (so are the conclusions). In operations, a reliable technique performs well in all events during any periods. Again, the authors are referred to Seo et al. (2003) paper on the analysis of multiple events.

3. The manuscript, as is, is not self-explanatory in many ways. Examples, among others, include a) Equation (16) is incomplete. What if the denominator equals to zero? b) /alpha is used in equations (16), (18), and 21) c) S_k is not used but explained in equation (15). It appears in equation (22), though. Not all terms in equation (15) are defined. d) The authors should be cautious by stating "real-time streamflow forecasting" (L10, Page 3387). It requires forecasted model forcing produced from numerical weather models, which is not the case of the current study. The current study is more doing the "hindcasting" with all the forcing data and model output (streamflow) observed. e) A set of sentences/statements could be improved. Examples include, but not limited to: L1-2, Page 3384: "Applications. . ...used to. . ." L13-15, Page 3386 L24, Page 3386 and L18, Page 3403: "forecasting capability of streamflow" L3-6, Page 3390 L22, Page 3392 L24-25, Page 3393 L19, Page 3396 L6-7, Page 3404: any references? f) Table 1, Page 3409: Actually, the differences in NSE values of three methods are negligible. The flow predicted by these methods is tuned according to the observed flow assimilated into the model every hour, and thus resemble the observed flow more than the deterministic flow simulation (also refer to the 2nd example of comment # 2). g) Rainfall is represented by bars (Fig. 7) and in lines (Figs. 8 and 9). Fig. 11, the validation period is not consistent with which defined in the context.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 3383, 2011.