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Hydrology and Earth System Sciences Discussions

Interactive comment on "Variational assimilation of streamflow into operational distributed hydrologic models: effect of spatiotemporal adjustment scale" *by* H. Lee et al.

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The authors present a variational assimilation of discharges in a distributed model. This study is interesting and fits well with the topics of interest of HESS. I suggest publication after some minor revisions.

Main comments:

Section 2.1:

- The presentations of the models is very poor. For example, the choice of the models used for this study has to be justified. Especially because the grid scale is 16kmˆ2 and

this seems to me very coarse for basins ranging from 16 to 2258 kmˆ2.

- which input meteorological data do you use? At which resolution?

- I see later in the paper that predictions are performed. But no meteorological prediction is introduced, so I guess that observations are used in a predictive way (as it seems to be explained briefly at the beginning of section 4.2.2). Then the authors should carefully define that and maybe rename the so-called "predictions" as "pseudo-predictions". In any case, please explain carefully what is done and how, in the models description, not in the middle of the text.

Section 4.1: if I'm not wrong, the optimal streamflow observational error variance is determined when assimilating outlet flow, and then it will be applied to every other cases. What about the two other scenarios? What is the impact of the nine spatiotemporal adjustment scales on this optimal observational error variance? You have to justify the assumptions you made here.

Reduce the number of figures, 14 are too many. For example: fig, 11 and 12 are barely described. In fact, only the percentages of improvement are described in the text. Fig, 3 could be also removed.

Specific comments:

Title: I would remove "operational" from the title, since the model is not used in a real-time mode here.

Page 96, lines 1-2: this is not entirely true. More and more, satellite sensors are developed to be able to monitor variables of interest for hydrology, such as soil moisture and snow. These data being spatially distributed, the problem of under-determination should not be a limitation. This has to be discussed in the introduction.

Page 96, second paragraph: here the authors discuss only the issues on state and/or parameter identifiability. I think the authors should not restrict to what is not working well, and should also introduce studies that showed improvements and positive results.

Page 97, line 8 and following: the authors state that “To address the aforementioned issues…”. The previous literature part has to be substantially improved in order to justify the aim of this article that is described in these lines. What I see in the literature given by the authors is a succession of references, that the authors only describe by writing what was assimilated (streamflow, soil moisture – real or synthetic - , SWI) and what was not improved (independent stations discharges, groundwater depth, groundwater flow and percolation). The reasons of these issues are only briefly described. Please re-write this part.

Section 2.2: I am missing a justification of the data assimilation data method chosen for this study. The EnKF is a commonly used method in hydrology for example. Why did you prefer the variational data assimilation? The particle filter is a method that has the advantage of not modifying the model states, which can be interesting for some applications. Please discuss that at the beginning of this section instead of at the end of section 2.2.2.

Section 2.2.1: please detail the state vectors, the input of the model, and the observations, by linking them to your actual case, as you did for M and H.

Section 2.2.2:

- explain why U_k (input data) does not appear anymore in Eq. (4).

- Seeing Eq. (5) and (6) it appears that multiplicative adjustment factors for biases and PE exist in SAC. Introduce them in the description of SAC and explain how they intervene.

- Please justify why you consider W_s,k null in Eq. (9)

- Please discuss briefly the impact of having X_p,k and X_e,k hourly, 6-houly or time-invariant on the computational time. Same thing for lambda.

Section 2.3:

- please explain what the aim of the correlation matrix r2 is. What information do you expect to obtain from this matrix?

- Could it be useful to weight the RMSE?

- Consider making the TE score description easier to understand for persons not used to this kind of scores. Especially what is the unit of the TE value that is obtained with Eq. (14). Discuss what is considered as a good performance or not (regarding the basin size or other feature).

Section 3:

- A justification of the choices of the basins is needed. Why did not you apply this work to larger basins, since the variational is, according to the authors, cheaper than the other assimilation methods?

- Fig. 3: is the soil type useful to present in the paper? The authors did not use this information to explain the performance of the model or of the DA. Which criteria are used for determining the sub-basins for each basin? Please discuss. Generally, this figure seems not very needed in the paper.

Section 4.2.1:

- line 23-24: “In Fig. 4, streamflow observations…”: please explain how it reflects on the matrices.

- You have to comment in the text the r1 matrices for simulation streamflows or remove them from Fig. 4

Section 4,2,3: Fig, 10: why all curves do not converge towards the no-assimilation run? Why the no-assimilation run of HNTT2 is not constant?

Fig, 11-12: please consider choosing plotting signs that make these figures easier to read. For most of them it is too difficult to understand if there is an improvement or not of the performance due to DA.

Fig, 13: typo mistake in the title

p112, line 14: a point is missing: “process. Especially”

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 93, 2012.