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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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We would like to thank Guillaume Thirel for very helpful comments. In the following, our response to each comment is given.

Comment 1) 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.

Response) We are a little puzzled by this comment given that, in practice, lumped

models are very often used over this range. As an extension of the lumped SAC-SMA model currently used operationally in most River Forecast Centers in the US, the distributed SAC-SMA model aims at providing operational streamflow forecasts at a smaller spatial scale that is commensurate with the operationally available forcing data. Since the NEXRAD-based multi-sensor precipitation data is available on the HRAP grid, using this grid as the analysis unit of the distributed SAC-SMA model is a natural choice. This aspect has been extensively studied through the Distriubuted Model Intercomparison Project (DMIP) I (Reed et al., 2004; Smith et al., 2004), DMIP II (Smith et al., 2012a & 2012b) and others. Unless there is a compelling need, we do not see simulation (and data assimilation) at a sub-HRAP scale justifiable given the increased dimensionality of model states and parameters and the associated increased underdeterminedness. The sensitivity of the model performance to grid resolution (see, e.g., Smith et al., 1999) is a separate research topic which is beyond the scope of this work. These points are now briefly described in Subsection 2.1 of the revised version.

Comment 2) which input meteorological data do you use? At which resolution?

Response) We used the NEXRAD-based multi-sensor precipitation and monthly climatology of potential evaporation at the HRAP grid scale. This is now stated in Subsection 2.1 in the revised version.

Comment 3) 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 "pseudopredictions". In any case, please explain carefully what is done and how, in the models description, not in the middle of the text.

Response) In Subsection 2.1 in the revised version, we explain the forcing data used to generate streamflow prediction and the choice. Regarding terminology, such model output is commonly referred to in the literature as predictions, i.e., predictions under perfectly known future precipitation. That we assume perfectly known future precipitation is already clearly stated in the manuscript.

Comment 4) 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.

Response) We apologize for lack of clarity that led to the above misunderstanding . The experiment was carried out for all basins for all combinations of assimilation scenarios, spatiotemporal adjustment scales and different streamflow observational error variances. However, the exhaustive results were purposefully spared from the text to focus on the spatiotemporal adjustment scale. In doing so, the corresponding author mistakenly misstated the description on how the optimal streamflow observational error variance for each basin was specified. This is now corrected in the revised version. Regarding the reviewer's first comment above, the optimal streamflow observational error variance turned out to be largely insensitive to the assimilation scenario. The similar properties associated with flow processes and channel geometry at upstream and downstream locations in the same basin may result in the similar error characteristics of streamflow observations at interior as well as outlet locations in the same basin. This is now briefly discussed in Subsection 4.1 in the revised version. As to the reviewer's second question, the streamflow observational error variance has little to do with the spatiotemporal adjustment scale. Note that the streamflow observational error reflects the accuracy of the measurement device used and error in the rating curve.

Comment 5) 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.

Response) Figures 11 and 12 have been combined. In our opinion, the single figure is

worth keeping as it visually summarizes RMSE reduction for individual peak flows for different events at different gauge locations for each assimilation scenario. The subbasin maps in Fig. 3 are necessary for the reader to see the scales at which the state variables are updated in the semi-distributed way. Fig. 3 is also used to discuss in Subsection 4.2.1 rainfall spatial variability, the soil type information and the complexity of the assimilation problem.

Comment 6) Title: I would remove "operational" from the title, since the model is not used in a real-time mode here.

Response) We disagree. While the work reported in the manuscript is not carried out in a real-time mode, the model in question is used in real-time operations at various RFCs in the US.

Comment 7) 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 underdetermination should not be a limitation. This has to be discussed in the introduction.

Response) We disagree. There are at least two issues with the reviewer's statements. That they are spatially distributed does not mean that they actually are skillful at the resolution of the data. This is particularly important for catchment-scale, as opposed to continental-scale, hydrology. The second is that the sampling frequency of nongeostationary satellites is generally too low to be useful for operational river and flash flood prediction at the scale of headwater basins. This is now briefly discussed in Introduction.

Comment 8) 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.

Response) The benefits of data assimilation and the relevant literature are already described in the second paragraph in the Introduction Section in the original version. The purpose of the paragraph in question, on the other hand, is to identify the issue.

Comment 9) 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.

Response) Additional descriptions are now given in the revised version as suggested.

Comment 10) 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.

Response) We have added a discussion in the revised version as suggested.

Comment 11) 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.

Response) Done.

Comment 12) Section 2.2.2: explain why U_k (input data) does not appear anymore in Eq. (4).

Response) We dropped Subsection 2.2.1 to avoid confusion with Subsection 2.2.2. The latter is sufficient to describe the variational technique used in this study.

Comment 13) 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.

Response) The reviewer is mistaken. The multiplicative adjustment factors for precipitation and PE in Eqs. (5) and (6), i.e., Xp,k and Xe,k respectively, are not the same adjustment factors used in the original SAC-SMA model. The former, i.e., Xp,k and Xe,k, varies with time and works in smaller time scale, i.e., the temporal scale of the assimilation window, or several hours to a few days. The latter, on the other hand, represents long-term bias in the forcing data and hence time-invariant. In the Introduction Section in the revised version, we now briefly state the motivation for Xp,k and Xe,k used in this study.

Comment 14) Please justify why you consider W_s,k null in Eq. (9)

Response) The spatiotemporal structure of the model error (W_s,k) in distributed rainfall-runoff modeling is not well understood. Therefore, realistically, accurate modeling of W_s,k is a very tall order. Accordingly, weak-constraint formulation may only increase the complexity of the assimilation problem rather than improve performance. For this reason, we dropped the model error term in the objective function used in this study. This is now explained in the revised version.

Comment 15) Please discuss briefly the impact of having X_p,k and X_e,k hourly, 6-houly or timeinvariant on the computational time. Same thing for lambda.

Response) The difference in the computation time among different adjustment scales is marginal. This is now stated in the revised version.

Comment 16) Section 2.3: please explain what the aim of the correlation matrix r2 is. What information do you expect to obtain from this matrix?

Response) This is already explained in lines 1-3 in page 104 of the original version.

Comment 17) Could it be useful to weight the RMSE?

Response) We do not understand this comment.

Comment 18) 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).

Response) We provide additional descriptions on TE in Subsection 2.3 in the revised version.

Comment 19) 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?

Response) Unfortunately, such basins are few. Flows in most large basins around the world are regulated, which is very difficult to model. In addition, stream gauges are needed also at interior locations. The basins used in this study represent a large pool of all basins in Oklahoma, Texas and vicinity in the US that meet the above requirements for this study.

Comment 20) 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.

Response) For the first question, please see our response to Comment 5. As for the second question, sub-basins are delineated with the COTAT algorithm (Reed, 2003) that determines flow direction, flow accumulation, channel identification and sub-basin delineation as described in Subsection 2.1. In theory, one could use many different scales for basin delineation. For our study, we used a single intermediate scale at which delineated channel network best matches with the actual channel network of the basin. This is described in Section 3 in the revised version.

Comment 21) Section 4.2.1: line 23-24: “In Fig. 4, streamflow observations...”: please explain how it reflects on the matrices.

Response) This is already explained in lines 21 to 23 in page 108 of the original version. It is now editorially improved for clarity in the revised version.

Comment 22) You have to comment in the text the r1 matrices for simulation streamflows or remove them from Fig. 4

Response) The r1 matrices are now briefly explained in the revised version.

Comment 23) 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?

Response) All curves will asymptotically converge to the no-assimilation results at sufficiently long lead times. The length of the forecast window is purposely shortened as small gain at longer lead times is not our interest. The RMSE of streamflow for the base model simulation for HNTT2 shows an odd pattern due to different sample sizes at different lead times. This can happen when a large event occurs at the end of the simulation period.

Comment 24) 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.

Response) Figs. 11 and 12 in the original version have been combined into Fig. 11 in the revised version, which uses different statistics on the y-axis, i.e., reduction in the RMSE of streamflow analysis and prediction due to DA. The new figure shows clearly the amount of improvement in streamflow analysis and prediction due to DA.

Comment 25) Fig, 13: typo mistake in the title

Response) Corrected.

Comment 26) p112, line 14: a point is missing: “process. Especially”

Response) Corrected.

References

Reed, S.M., Deriving flow directions for coarse-resolution (1-4 km) gridded hydrologic modeling, Water Resour. Res., 39, 1238, doi:10.1029/2003WR001989, 2003.

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Smith, M., Koren, V., Finnerty, B., Johnson, D., Distributed Modeling: Phase 1 Results, NOAA Technical Report NWS 44, 1999.

Smith, M.B., Seo, D.-J., Koren, V.I., Reed, S.M., Zhang, Z., Duan, Q., Moreda, F., Cong, S., The distributed model intercomparison project (DMIP): motivation and experiment design, Journal of Hydrology, 298, 4-26, 2004.

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