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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Multivariate hydrological data assimilation of soil moisture and groundwater head" *by* D. Zhang et al.

Anonymous Referee #3

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General comments

This paper presents a study of hydrological data assimilation for integrated catchment modeling, using a combination of ensemble Kalman filter and the MIKE SHE model. By assimilating groundwater head and soil moisture data, the Authors investigate the filter performance in various scenarios characterized by different observations assimilated, distance- and variable-based localization, and ensemble size. Two test cases are considered, one with purely synthetic data and one with real (albeit "processed", see specific comments below) observations. The paper is of interest for the readers of HESS and, although the methods are not new, there are some original features, such as the use of distance and variable localization for data assimilation in an integrated hydrological model. However, a number of issues should be addressed before the paper can be considered for publication.

1) Many details are missing and therefore the numerical experiments are not reproducible. In particular, there is no mention whatsoever of the model parameters (e.g., soil properties) and how they were perturbed to generate the initial ensemble of realizations. Precipitation and potential evapotranspiration were also perturbed but no description on how this was done is currently available in the paper. This is especially relevant for the Ahlergaarde catchment, while perhaps some information on the Karup catchment might be available in previous publications by the same research group.

2) The "deterministic" model has been calibrated against observation data for both catchments, yet for the Karup catchment this is not showed neither in figures nor tables, while for the Ahlergaarde catchment only a comparison between observed and simulated discharge at the outlet is included, which is a bit limited, compared to the capabilities of the model. I think it is important to show and briefly discuss the model calibration performance in both cases, maybe also in terms of water table, to build some confidence that the subsequent analyses are realistic.

3) One of the recurrent results of this study is that assimilation of groundwater head does not improve (or even worsen) soil moisture and vice versa, whereas Camporese et al. (Vadose Zone Journal, 2009), in a similar study, showed that EnKF-assimilation of surface soil moisture can improve the saturated zone and assimilation of groundwater head can improve surface soil moisture. This is probably due to the fact that the model used by Camporese et al. (VZJ, 2009) seamlessly solves the entire subsurface domain with the full 3D Richards equation, while here there might be some "disconnection" between the unsaturated domain and the saturated one. This may lead to the risk of physically inconsistent updates and hence the need for variable localization. I believe that a more insightful discussion is required about the relationship between the DA results and how the saturated zone and the unsaturated one are coupled in MIKE SHE, especially for the readers that, like me, are not entirely familiar with how process coupling is done in such model.

Specific comments

Page 1, lines 18-19: improvements obtained in discharge and ET with data assimilation were only marginal, as acknowledged by the Authors later on in the manuscript. I suggest this statement be relaxed.

Page 2, lines 4-6: this is not a general definition of hydrological data assimilation. Please rephrase.

Page 4, lines 22-23: irrigation is not included, yet it is said that the catchment is located in one of the most irrigated areas of Denmark. I think a much stronger justification is warranted than simply “for computational efficiency”.

Page 7, line 8: the true state in reality is never known, therefore it is not sure it can be always represented. I suggest this sentence be reformulated.

Page 9, line 15: “appropriate model error” is not sufficient, please give more details.

Page 10, line 23 and Figure 3: I assume RMSE is averaged not only in space but also in time. Please clarify.

Page 12, line 18: does it mean that previous experiments were run with an ensemble size of 60? If so, please clarify. Also, please provide justification for using 60 (e.g., previous sensitivity analysis?).

Page 13, lines 22-26: more details are needed regarding the perturbation of precipitation, evapotranspiration, and parameter values. For instance, how were the variables perturbed? With additional Gaussian noise? And using what variance? Also the model parameters (e.g., soil properties) must be specified.

Page 13, lines 29-30: from the figure it is clear that the model consistently underestimates low flows and overestimates peak flows. Therefore, I do not think that “the model is good at predicting low flow” and suggest this sentence be reformulated. What about performance of the model against other data (e.g., water table, soil moisture)?

Page 14, lines 7-10: this procedure for “adjusting” the observations and make them

closer to the model predictions is a strong assumption and represents a significant limitation of this study. By doing this, what would be a real test case becomes basically another synthetic experiment. If the Authors really think the observations are biased (and it seems they justify this hypothesis based only on the fact that the calibrated model is not able to match the data, which is a weak explanation, in my opinion), why don’t they use the framework proposed by themselves in Rasmussen et al., “Data assimilation in integrated hydrological modelling in the presence of observation bias”, HESS, 2016? Such an analysis would make the paper more robust and interesting.

Page 14, lines 16-19: this explanation is needed earlier in the manuscript. See previous point concerning Page 4, lines 22-23.

Page 14, lines 19-20: more details are needed here. Which criteria were used to remove unreliable observations from the dataset?

Page 16, line15: data used in this study are not exactly “real” (see previous comment). Please relax this statement.

Page 17, line 4: improvements in discharge and ET are very small, I would say “marginal”, instead of “relatively small”.

Page 20, Table 2: please remove the column with cumulative ET and replace it with measures of performance of the various DA scenarios, as done with discharge at the outlet.

Page 22, Table 4: first column (cumulative ET) can be removed.

Page 31, Figure 9: not clear what this figure should represent. Here RMSE is computed with respect to the “deterministic” model, which is clearly not the truth, is surely affected by strong uncertainty, and may well be erroneous (as shown by model results in Figure 6). If the Authors want to quantify the spatial distribution of the system corrections made by DA, I suggest replacing RMSE with, e.g., AAD (average absolute deviation) between the DA runs and the deterministic model. Finally, please add labels to x- and y- coordinate axes.

Technical corrections

All over the manuscript: correct all citations where the Authors are erroneously in parentheses, e.g., (De Lannoy et al., 2007) applied EnKF . . .

Page 4, line 4: replace “relative” with “relatively”.

Page 4, line 27: correct “Metrological”. In general, small edits of English are required throughout.

Figure 5: please correct labels in the top three panels.

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