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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Multi-source data assimilation for physically-based hydrological modeling of an experimental hillslope" *by* Anna Botto et al.

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In this manuscript, the Ensemble Kalman Filter (EnKF) with an unsaturated flow model is used for data assimilation. Observations are taken from an artificial hillslope with artificially generated rainfall (a physical experiment in a laboratory), where water content and pressure head at several locations as well as outflow at the bottom of the hill are monitored over time. The augmented state approach is applied in the EnKF, where states and model parameters are updated jointly. Different combinations of observations are used for data assimilation and different sets of parameters are included in the augmented state vector. By comparing the performance of the predictions with the data assimilation model, the benefit of multivariate data assimilation for prediction of different variables as well as the benefit of parameter updates is analyzed. It is con-

cluded that multivariate data assimilation leads to tradeoffs between prediction quality of the different variables. Also, parameter updates can lead to improved predictions for some variables, but might deteriorate the predictions of others. This depends on the parameters that are updated. This paper is a very interesting study on the assimilation of different observations in a hill slope. The use of real data from the artificial (but physically real) hillslope experiment is a strength of the paper. The study highlights the potential for data assimilation in hillslope hydrology (or rather more general: In coupled hydrosystems, here subsurface and surface), but also reveals that multivariate data assimilation setups have to be carried out carefully as more observations and parameter updates do not necessarily lead to better predictions. I think that the paper should be published in HESS. It is well written and well organized. Nevertheless, I have a few comments.

1. In general I think it could be made clearer what the purpose of the data assimilation framework is, so that it is easier to follow how the performance is evaluated. It makes a difference if the purpose is to make predictions based on continuously measured observations or if predictions should be made also without observations. In the first case the question of parameter identification is less important than in the second case. If observations are available all the time, parameter updates might improve state updates and state predictions, but it is not important that parameter updates yield reasonable parameter values. This is different for long prediction periods without observations. I assume that the purpose is here to make predictions also without observations, as a long validation period is chosen.

2. Page 3, lines 3-4: Here and also at other places it is stressed that the flow processes considered in the experiments are dominated by strong non-linearities. It would be good if this could be explained a bit more in one or two sentences or if the statement could be taken out. I do not see where non-linearities are dominant in the experiments. In Figure 2 it seems as if all variables follow well the rainfall signals. I would expect strong influence of non-linearities in the extreme cases when ponding of water occurs

because the flow rate exceeds the conductivity of the soil or when pressure heads go to extremely high negative values due to upwards flow.

3. Section 2: I think it would be good to write already at this place about the evaporation. In Section 2 evaporation is not mentioned, so one assumes that it is neglected. Only in Section 4.1 does one read about the rates. It would also be interesting to learn how they were measured. Or were they estimated? In this case: How were they estimated?

4. Section 4.1: How were seepage face boundary conditions realized? By imposing a water pressure of zero or by imposing zero gradients across the interface?

5. Eq. (1): The left bracket on the right hand side should open after the hydraulic conductivity.

6. Section 4.1: It did not become clear to me how the initial condition was set in the simulations. From Table 1 and eq. (1) and from lines 23-24 on page 6 I would think that a constant pressure head was set in the whole domain. If this is so, I do not understand why this initial condition was chosen instead of a hydrostatic pressure distribution. With a constant pressure head, the system is far away from any equilibrium and I would assume that a long spinup time is needed before a good assimilation run is possible. Either water has to flow out or it has to flow into the domain to achieve equilibrium. If I understand the model setup right, it could only flow in and out from the river of 50 cm water depth on the bottom of the hillslope. This will be a slow process, or not? Maybe this is a misunderstanding, but in this case the initial condition should be explained more clearly.

7. Section 4.2, parameter transformations (16)-(18): Maybe I missed it but it did not become clear to me what transformation was used for what parameter. Or were all three used for all cases? In this case it would be interesting to learn if any of them works better than the other ones.

8. Section 5.3: I am not sure that I agree that parameter estimation capabilities are discussed here or could be discussed with the observations at hand, as the true parameters are not really known. I think the important question addressed in this section is rather if parameter updates are useful for data assimilation. The parameters might be optimal for a given situation, but it might be that with more observations, the optimal parameters would be different. It would be interesting to see the parameter updates over time. To my understanding, it is an indicator for reasonable model parameters if the parameter updates converge to a value and do not change with boundary conditions.

9. Figure 6 and end of Section 5.3: I have problems seeing a real improvement by parameter updates in Figure 6 in case of the pressure head. Although the uncertainty is reduced with the parameter updates, considering the large discrepancy between measured and simulated values, this reduction is not necessarily an advantage, as the observations are no longer inside of the uncertainty interval. 10. The pressure plots in Figure 6 and also in Figure 9 seem a bit odd to me. From estimating roughly from Figure 1, I would expect that the water table in the hillslope should be about 50 cm above ground. The sensor for P2 should be about 40 cm above the water table (only guessing, this is not so clear from the sketch). Without rainfall, the pressure head should in this case be -40 cm. It is clear that it increases during infiltration, but the hydrostatic condition would be in this range. The observations show a lower value, but the simulations show a value of -20 cm. This should be much too high. In the validation period it seems that the pressure head in the model is falling after the rainfall has stopped and it seems not to have reached an equilibrium at the end (in contrast to the observations). Could this be an effect of the initial condition and the spinup is not finished?

11. Section 5.4 first paragraph: Is it so surprising that updating the van Genuchten parameters has a strong impact on water content predictions but updating only Ks not? The water content is related to the primary variables of the model (pressure head) via

these parameters, so I do not find the result so surprising.

12. Page 10, line 31-32: Why is this point shown once more? I think it is an important point to make, but I do not think that it has been made before. Or do you mean that it has been made before in other papers? There remains an open question: How would one proceed in this situation in the best way? In reality it is very unlikely that all soil zones could be probed sufficiently. So how does one deal with heterogeneous structures that are not covered by observations? I do not think that this question should (or could) be answered, but it is an interesting point.

13. Last paragraph of Section 5.4 and results and discussion: In general the tradeoffs are described, but not really discussed much. Can one understand this behavior so that one could draw general conclusions? Otherwise it is not so clear if the results are specific for the case that is here studied. I find it also remarkable that including pressure head observations leads to reasonable pressure predictions, while water content is poor, and vice versa. This behavior should be linked to the van Genuchten Parameters, which must be poorly matched in these cases. If all observations are assimilated, both predictions are reasonable. Can one see that in the van Genuchten parameters? Are they improved if all observations are used? Again, it would be interesting to see parameter updates.

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