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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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Reply to Anonymous Referee 2

We thank the Referee for their interest in our work and for helpful comments that will greatly help to improve our manuscript. The Referee has brought up some very good points and we appreciate the opportunity to clarify our research objectives and results. As indicated below, we have checked all the general and specific comments provided by the Referee and will make necessary changes to the manuscript according to their

indications.

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.

We thank the Referee for highlighting this aspect. Indeed, the purpose of the model is to make predictions also without observations: as mentioned in the end of the Referee's note, a long period of the whole simulation has been devoted to the validation of the model (no assimilation) as we wanted to verify the model in a situation where no measurements are available. The overall purpose of the data assimilation framework will be made clearer in the Introduction of the revised manuscript.

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.*

We thank the Referee for raising this point. The non-linearities we mention several times in the article refer to the inherent nature of the Richards equation, which is solved by the model. These non-linearities are enhanced by the fact that our hillslope, as opposed to many other previous applications of data assimilation with physics-based models, is characterized by dominant unsaturated conditions. We will clarify this aspect in Section 4 of the revised manuscript.

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?

We thank the Referee for spotting this: the evaporation rate has been measured throughout the experiment by an atmometer. The average measured rate is quite small (< 1 mm/day), consistent with weather and period of the year (November). We will include this information in Section 2 of the revised manuscript.

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?

In the CATHY model, seepage face boundary conditions are realized by imposing atmospheric pressure (ψ = 0 m) at the nodes below the "exit point" (i.e., the intersection of the water table with the interface) and zero flux at the nodes above. Additional details can be found in Camporese et al. (2010), already cited in the paper.

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

Equation (1) will be amended.

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.

We thank the Referee for raising this point, which is important because it represents one of the peculiarities of our test case compared to similar studies. At the beginning of the experiment, the entire hillslope was in a highly unsaturated condition, as derived from the tensiometer data. This means that, even without an initially hydrostatic (i.e., equilibrium) profile, water is basically prevented to flow in or out of the hillslope due to the very small values of relative hydraulic conductivity. That is why we opted for a uniform value of initial pressure head, based on the average provided by the measurements. Preliminary analyses showed that the model is not sensitive to this value, because in any case the initial hydraulic conductivity is so small that the hillslope responds only when the first rainfall event occurs and starts wetting the soil. That is why, in this specific case, no warm up or spin up was necessary. We will include this discussion in Section 4 of the revised manuscript.

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.

Only one single transformation of the three available was used for each of the van Genuchten parameters, according to Carsel and Parrish (1988), where the nominal values and the proper transformation applied to each parameter depend both on the type of soil and the parameter considered. For clarity, we will indicate the specific transformation for each parameter in Table 2 where the reference values had already been reported.

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.

We thank the Referee for this comment, which allows us to further clarify this aspect. We agree that true parameters are unknown, as in all real-world systems such as our experimental hillslope; assimilation of real data is indeed one of the strengths of our study. Therefore, the Referee is right in that the purpose of Section 5.3 is to discuss the benefits of parameter estimation in our data assimilation framework. However, we do have some information that can help us assess whether the estimated parameters are consistent with the soil type or not. This was already discussed in the manuscript, but, to address the raised issue, we will add a figure in the revised version showing the evolution in time of the updated parameters. The figure, reported below, shows the time evolution of the saturated hydraulic conductivity and van Genuchten parameters (mean values, solid line, together with minimum and maximum values, in dashed lines, to indicate the ensemble spread) for the two types of soil, sand and clay, for scenario S17 (one of the scenarios reported in figure 9). The results show that convergence towards stable values (in other words, indentifiability) in the data assimilation phase (the first 5 days) is only achieved for the parameters K_s and n of the sand layer, while the dispersion is generally higher for θ_r (to which the model is typically not very sensitive) and α and especially for all the parameters of the clay layer. This can be explained by the fact that all the sensors are located in the sand layer, while no experimental data from the clay layer are applied in the filter.

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.

We agree that visualizing the improvement is not so easy. However, by looking very carefully, it can be seen that, in the validation phase, the magenta line (ensemble mean with data assimilation) departs from the black line (ensemble mean in the open loop) and gets closer to the green line (measurements). Also, the plots in figures 6 and 9 only report the results in one single location (P2). In order to make the analysis more objective and comprehensive, we used metrics such as the NRMSE, reported in Table 4, by which we can appreciate the real improvement, calculated over the whole simulation and all the measurement locations, of the data assimilation compared to the open loop.

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 spin-up is not finished?

This point is related to previous points 2 and 6. As previously mentioned in our replies to these points, due to the highly unsaturated initial conditions and associated non-linearities of the flow processes, it is difficult to make predictions on the response of the hillslope based on assumptions of linearity. Overall, the pressure plots in Figures 6 and 9 are consistent with the pressure measurements and, after all, the goal of data assimilation is to include observation information (including uncertainties) into model predictions. Finally, please note that the channel at the toe of the hillslope is never full with water, as it drains out from the opening visible in the plan view of Figure 1. As a consequence, the perched water table that forms as a result of infiltration never exceeds a value of about 20 cm above the sand-clay interface.

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.

We agree that these results are not surprising. Nevertheless, they are discussed as part of the physical rationale behind the main point of this Section, which is to describe the trade-offs in the results stemming from different data assimilation and updating strategies.

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.

We thank the Referee for giving us the opportunity to better clarify our position on this point, which was already mentioned at lines 23-25 of the same page 10 (that is why we write "once more" in line 31). Indeed, we agree that it is unlikely, especially when dealing with large heterogeneous structures, to have every soil zone properly probed. Within this context, it is even more important to assess whether (or not, as in this case) multivariate data assimilation approaches are capable to compensate for the lack of distributed observations with alternative sources of information, such as, in this case, subsurface outflow.

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.

As mentioned in our reply to point 8, the parameter updates will be shown in a new figure, from which we can see that the only van Genuchten parameter that can be clearly identified is n for the sand layer. However, we cannot match it to any real value, as this is not a synthetic test case. Therefore, we have to accept a relatively large residual uncertainty on the retention curve parameters. By comparing the final ensembles of the sand parameters in S17 with those in S15 (Figure 8), it is also interesting to note that the main difference is given by the estimated values of the saturated hydraulic conductivity, which is strongly affected by the assimilation of pressure head measurements. We expect our conclusions to remain valid on most applications of soil hydrology with similar characteristics.

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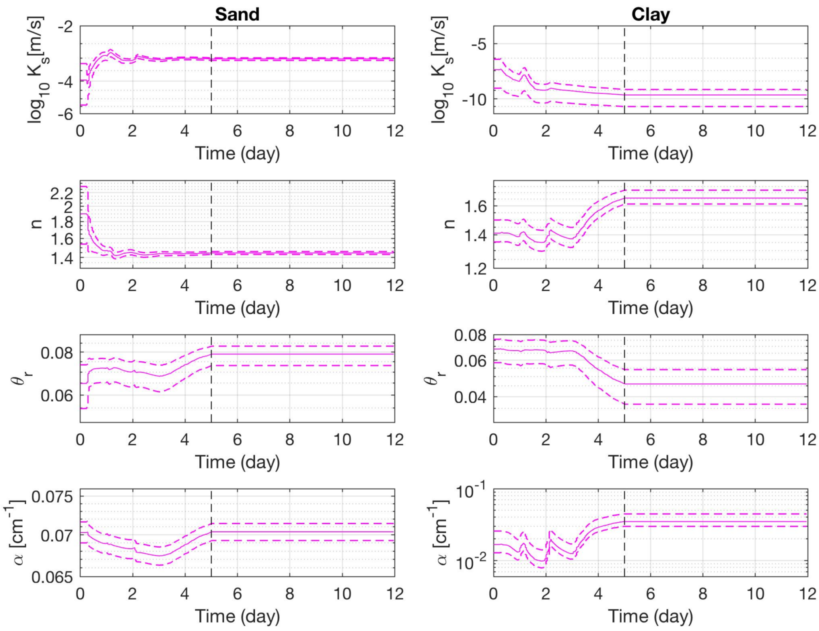

Fig. 1. Time evolution of hydraulic parameters for scenario S17. The solid line represents the mean, while the dashed lines are the maximum and minimum values.