HESSD

Interactive
comment

Interactive comment on "On the uncertainty of initial condition and initialization approaches in variably saturated flow modeling" *by* D. Yu et al.

Anonymous Referee #1

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The manuscript investigates the effect of the uncertainty of the initial conditions in the context of soil water movement described by the Richards equation. First the necessary warm-up times for different soils and climates are determined and then the effects of different methods to describe the initial condition on a subsequent data assimilation are compared. The comparison is additionally shown on a real-world case.

I think the manuscript is interesting and and shows the effects of the uncertain initial conditions nicely. I have few comments that may require some additional investigation or discussion. However, the manuscript is sometimes difficult to read and could be clearer. Therefore, many comments ask for some clarification.

Comparison of the different initial conditions:

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General comment:

The required computational power varies between the different initial conditions. The most expensive ones (with warm-up period) seem to give the best results in the subsequent data assimilation. I would find it very interesting whether this finding holds if for each method a similar total computation time (computation time for intitial condition + computation time for data assimilation) is available. This means that e.g. IC-ObsInt or IC-Flux could afford more ensemble members than IC-WUP. The question is then, if e.g a higher number of ensemble members (in combination with a larger uncertainty in the representation of the initial condition) of IC-ObsInt or IC-Flux could lead to similar, or even better results.

Specific comments:

Line 222-226: When the initial condition ensembles are generated for IC-HfSatu, IC-ObsInt and IC-Flux, is uncertainty added? How exactly? The uncertainty in the initial water content must be represented in the initial ensemble. If no uncertainty is added, this could explain partly the inferior result compared to IC-WUP. Please clarify and discuss.

Line 247: The spatial resolution of 5cm is rather low for a 1 dimensional case. Is this a computational limitation? Otherwise, I would suggest to reduce the grid size to e.g. 2cm. This is especially relevant for sandy soils where sharp infiltration fronts can develop and require such high resolutions. Could this impact the results?

Lines 379-383 and Figure 7: The biases of Ks, α, and n as well as their uncertainties differ. Therefore, their RMSEs should not be compared directly. I think it is not a meaningful result that α, which has the largest initial bias and uncertainty, also has the larger RMSE and that n, which has the smallest initial bias and uncertainty, has the

smaller RMSE. Their relative improvement might be a better measure.

Warm-up time:

Specific comments:

Line 286: You find that t_{wu} is less than one day for sand. I think that this result might be due to the chosen initial condition for the warm-up. It is true, that for the chosen high water contents sand will drain very fast and rapidly approach a similar water content state. However, in case of an initial condition in a very dry state (which should be relevant for the arid climate), the hydraulic conductivity of sand drops to very low values and the initial spread can extend for a very long time, or until a sufficiently large rain event increases the water content and then leads again to the rapid approach of the similar water content.

I think it would be interesting to investigate this by choosing a different (dry) initial condition. At least this should be discussed in the manuscript.

Line 273-275 and 477-478: Since you recommend the choice 0.5% as a threshold: Please explain why. What is the advantage? Why should I not choose the other mentioned thresholds (e.g. 1% or 0.1%)?

Lines 306-313 and Figure 5: If I understand correctly, you investigate when the uncertainty for the full profile drops below the 0.5% threshold. In addition (or possibly as replacement) I would find it interesting to see the spatially resolved times for each depth for the deepest profile (20m).

Line 152 (Equation 8): If the monthly average of the previous year is required, wouldn't that imply that PC is not defined for the first year? However Fig. 3 shows PC starting

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from time 0. Please clarify.

Figure 3: Why does the water content state after 24 months differ between panel (a) and (b)? Since both are initialised with the same parameter values and the UIC has already decayed, they should show the same soil moisture. Please clarify.

Details of the used data assimilation:

Specific comments:

Lines 370-377: Based on Figure 6, I disagree with the statement that filter inbreeding is not a significant issue for the EnKF case. In Figure 6, it seems that the final parameter value does not change any more over time and is over 5 standard deviations away from the truth. This means that the uncertainty is too small. Part of the reason could be that the initial uncertainty is chosen way too small. It is over 9 standard deviations away from the true value. This makes it very difficult for the EnKF to find the true value. I would suggest to repeat the simulations with a larger parameter uncertainty.

Line 177-178: "..., uk are state variables (i.e., pressure head and soil moisture) ...". Do you update water content and matric potential of the same node simultaneously in the augmented state? Due to their nonlinear relation, the analysis would lead to inconsistencies between water content and matric potential for the analysis. How is this handled in the forecast? Please clarify.

Technical comments:

Lines 228-238: This part describes IC-WUP and IC-WUE. However, this is not a general description. In Section 3.2, when the spin up periods are investigated, a different

procedure is used. This confused me when reading the paper the first time. Please, only mention the general settings in 3.1 (i.e. climates, soils and model representation), and not specifics that only apply to 3.2 or 3.3. Therefore I would suggest to move this part to Section 3.3.

Additionally, here it is not clear how the parameter and initial condition ensembles are exactly generated. Please clarify.

Line 222-226: I think this part should be moved to Section 3.3 as well.

Line 243: "Fig. 1" should be "Fig. 2".

Line 254 and Fig. 3: The text mentions a simulation length of 10 years, the figure shows only 2 years. I would suggest to mention that you only show the first 2 years.

Lines 338-342: How many observations are there? In what depths are the observations? What is the assimilation frequency? Or is only a single observation in the depth of 10 cm assimilated every 10 days? If that is the case this has to be clarified.

Lines 343-350. I think this part should be moved to methods in Section 2.

Line 352-353 and Figure 6: I would mention that this is case 1 and case 2.

Line 399: "Field" instead of "Filed".

Figure 4: Since essentially the times for sand for all climates as well as silt and clay

loam for the M-Ac and the M-SC climate can not be properly displayed: Maybe the logarithm of the time could be more meaningful (like in Fig. 5).

Figure 6: The line for IC-WUE is essentially not visible. Is it below IC-WUP? At least mention this in the caption.

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