Referee comment on "Impact of parameter updates on soil moisture assimilation in a 3D heterogeneous hillslope model" by Natascha Brandhorst and Insa Neuweiler, Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-311-RC2, 2022

This is a very clearly written, well-constructed article that documents a numerical study of data assimilation using an integrated numerical model. I found this article scientifically interesting, as I also think the readership of HESS would. I recommend publication pending minor revisions. I have some general and specific comments listed below.

We thank the reviewer for the effort and time to revise our manuscript. We are pleased that the manuscript aroused the reviewer's interest and are thankful for the very positive rating and the constructive comments to which we will respond in the following.

General comments:

The integrated model used here incorporates overland flow. However, it's unclear what the role of overland flow was in the study as I don't believe it was used in the DA. Did the presence of overland runoff modify the soil moisture in some way (beyond acting as a boundary condition at the bottom of the hill slope)? Would the results be essentially the same with a Richards' only hill slope model?

The reviewer is right that overland flow was not used in the DA. In this model, overland flow only acts as a boundary condition, although there may be minor influences on soil moisture. Yet, we are convinced that the conclusions drawn in this work are not affected by the usage of overland flow and would have been the same when using a Richards' only model. Overland flow was included in the model as it allows for an investigation of the effect of streamflow data assimilation on soil moisture estimates. This was left for future work, though. Another reason to include overland flow was to avoid implementing an infiltration condition as a flux boundary at the soil surface, where the total infiltration flux is imposed. This can lead to numerical problems, in particular under dry conditions. We will add a sentence where we briefly discuss the influence of overland flow in the model.

The authors found that porosity was a particularly sensitive parameter in the DA. This makes sense, as they state (e.g. 615), as this limits the total amount of water available in the soil. However, this sensitivity is likely larger on the wet side of the soil moisture curve, on the dry side other parameters (processes) may play a larger role. The aridity of the simulations is driven by the meteorological forcing used in the experiment, did the authors consider the impact a different forcing dataset might have on these findings?

We agree with the reviewer that the wetness (or aridity) has an impact on the sensitivity of the individual parameters, especially porosity, and thus on their relevance in data assimilation. In previous one-dimensional experiments (Brandhorst et al., 2017), we had investigated the role of the different parameters in data assimilation under varying moisture conditions, although not generated by using different forcing data, but different reference soils. There, we saw a decreasing influence of porosity with increasing aridity. In the present model, we would expect a similar behavior albeit less pronounced since the heterogeneity of the soil always covers a larger range of the soil moisture curve. This is one reason while the concluding suggestion based on the experiments is to include all four parameters in the updates which was shown to be a good option for all test cases (including the 1D experiments conducted in

Brandhorst et al., 2017). In this case one might include a less-sensitive parameter (as porosity in dry soils) but this would not have a negative effect on the assimilation. We will add a part to the manuscript where we discuss this.

Specific comments:

Section 3.1.1: Are the random fields for ln(K), ln(alpha), phi and n spatially correlated? I realize they are correlated with each other, but are the fields disordered in space or correlated with some spatial correlation structure? There is a lot of evidence in the literature demonstrating the spatial correlation of random fields and this should be discussed in the manuscript. If the fields are correlated, I recommend including some discussion of the correlation model and associated parameters.

Yes, the fields are also spatially correlated. The horizontal correlation length is L_H=2m and the vertical correlation length L_V=4m, such that the vertical variability is negligible inside a soil layer. We agree that this is important information, which is missing in the manuscript, as the spatial correlation influences the data assimilation via the covariance matrix. We will add it in the revised version.

Section 3.1/Table 2: For the numerical simulations were the random fields constrained in some way to prevent non-physical parameter values? Or even parameter values that would be outside the range of solution (VG parameters such as n that result in eqs 2, 3 being nondifferentiable).

This is a good remark. Yes, in addition to the requirements regarding mean, variance and correlations, we constrained the parameters to avoid unphysical or numerically difficult values. In detail, the limits were: 0.14 < porosity < 0.76; 1 < VG-n < 5; 0.1 < VG-alpha < 57.5. For the saturated hydraulic conductivity, we did not use hard limits, but regulated the range by the applied variance and mean and then checked the values of the resulting fields on their physical plausibility. We will mention this in the revised manuscript.