We want to thank Nicolas Rodriguez for his supporting and motivating words as well as for the valuable and positive criticism. First, we accepted all the minor suggestions, all of them will be implemented.

Below are our responses to each of the main comments.

1. **In this work it is not immediately clear why one should bother with a hydrological model which largely simplifies transport processes in the soils and comes with many sources of uncertainties (P, Q, ET, parameters) instead of using simple topographically-derived or precipitation-derived proxies of soil moisture. Why use a model if simpler metrics are satisfactory (especially when keeping table 10 in mind)?**
2. **Given the relatively small size of the catchment (6 ha), the assumption of spatial homogeneity of inputs / land use / soil water parameters, wouldn't a physically-based distributed model (e.g. CATFLOW, Zehe et al., 2001) be more appropriate to simulate soil moisture? This question arises specifically after such models are mentioned in the introduction (L30-36) but not discussed further. In addition, physically-based distributed models would allow a direct and meaningful comparison of simulated vs observed soil moisture.**

The use of the hydrological model and an index derived from it, SWI, opens up possibilities for its future application in different areas. If the SWI correlation is proven as a representative index of soil moisture, it can be used in future works that use soil moisture as a parameter for decision-making in alert for floods, drought, etc., using only data easily available (P, Q, ET) than physical soil parameters, which are difficult to obtain.

3. **The equations could be re-written in order to avoid current ambiguities: • Using subscripts and superscripts for the indexes (0, 1, 2…) • Writing any explicit dependence on time t with (t), and avoiding using a subscript for this • Replacing t-1 by t-Δt • Correctly placing the bound variables (i, j, c) below the sums and giving the lower and upper bounds • Avoiding using a dot symbol between bound variables and elements in the sum (this will be confused with a product) • Correcting equations 7 and 8; Δt is missing to go from fluxes (L/T or L3 /T) to water amounts (L or L3)**

Ok, the equations will be re-written for better understanding.

4. Would it not make much more sense to calibrate the DTM rather than transferring the parameters of the calibrated TM to DTM? In the end, the procedure would be the same, but the DTM would be used in the DREAM algorithm instead of TM. Seeing TM working better than DTM in both calibration and validation (tables 5 & 6) suggests that it should have been done this way. As a result, the derived uncertainties are all coming from the calibration of TM, not from DTM which is the model actually used to derive SWI. Thus, the true uncertainties which will affect SWI estimations are not shown.

We agree that it would be interesting to calibrate the DTM, instead of transferring the calibrated parameters, and this is one of our goals for future works. This procedure requires a computational effort for which we are not ready yet. This transfer of parameters was made in other previous studies that used different versions of the distributed Tank Model. We agree that the real uncertainties affecting the SWI are not being represented.

5. The models are calibrated to hydrographs and not soil moisture which is the target. Very good model fits to hydrographs may not guarantee very good fits to soil moisture (e.g. in a catchment dominated by groundwater responses, Loritz et al., 2017; Rodriguez et al., 2019). Is it not possible to similarly calibrate and validate DTM, but to soil moisture observations instead?

We do not believe it is possible to calibrate the Tank Model or DTM using soil moisture as a parameter without completely modifying the models' equations, proposing a new, physically based model. Lara and Kobiyama (2009) proposed the PM-Tank Model, a physically based version of the Tank Model, which cannot be used in this work, as it requires other physical parameters that we do not have. In this work, the model guaranteed a good fit to the hydrographs, and when we compared the SWI calculated from these with the soil moisture values, there was a good correlation, which for us is an indication of good performance.

6. The visual comparisons for soil moisture are misleading. Why show only 2 locations out of 9? Is it really meaningful to compare SWI and soil moisture directly? Figure 8 suggests that their relative variations differ by orders of magnitude (especially in Fig8b), and that they may be poorly related (especially in Fig11, the step-wise behavior). The use of a correlation coefficient does not take additive and multiplicative biases into account (Legates and McCabe, 1999), so I suggest to use another performance measure. Also, the correlation coefficient does not allow a spatially-distributed comparison of the results with the observations; while it seems important for the aims of this study (see useful suggestion from first reviewer)

We agree with this change, we will do the analysis taking into account the bias, using the suggestion of the first reviewer.

7. How were parameter sets chosen (L192)? I think that the use of an average parameter from all calibration events is not a standard method. In addition, no verification was done in order to check that the mean parameter set actually works well for the calibration events. In my opinion, the standard method would be to calibrate the DTM to all 5 events simultaneously, and validate to the 2 last events simultaneously. Shouldn't DTM work well for all events, to be representative of transport processes in the soils?

The last 7500 samples are used to represent parameter uncertainty for all the 5 events of calibration. After, an average parameter for all calibration events were calculated. Yes, using the average of the parameters is not a usual method. The average of parameters presented in the work was tested for the calibration and validation periods, and represented all of them well. We can add this information in the work to make readers more secure.

8. Was the Global Likelihood of Schoups and Vrugt really used? Why choose a constant homoscedastic gaussian likelihood error model, which is already available

in DREAM, then? 9. How was "total uncertainty" derived? In what way does it differ from parameter uncertainty? How does it affect SWI uncertainties (seemed to be one aim of the study)?

As we said to the first reviewer, we agree that the uncertainty analysis can be better detailed in terms of methods and results. In this work we used a generalized likelihood function proposed by Schoups and Vrugt (2010), which relaxes the commonly assumed premises on residual errors. We chose this function precisely because it is already easily available in the DREAMzs algorithm. What happened was that we did not detail the results regarding the parameters uncertainty, nor the analysis of residual errors. This happened because initially we did not want to take the focus away from the main analysis, which is the presentation of the SWI index as a parameter of the variability of soil moisture over time.

Even, given the difficulty in representing the real uncertainty inherent in the calculation of the SWI, or the "total uncertainty", we consider removing this step of uncertainty analysis from the work, and simply using a standard calibration method as a genetic algorithm; we can even do this if the reviewers find it more consistent. But we can include one more item in the results section showing the uncertainty of the model parameters in the flow generation, which is directly linked to the SWI calculation, since both are highly correlated.

References

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