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Interactive comment on "Improving Soil Moisture Prediction of a High–Resolution Land Surface Model by Parameterising Pedotransfer Functions through Assimilation of SMAP Satellite Data" *by* Ewan Pinnington et al.

Anonymous Referee #3

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The paper explores the use of SMAP soil moisture products with the JULES land surface model with a data assimilation framework. The framework is applied in a region of the UK where soil properties from pedotransfer functions are constrained with data assimilation. The topic has potential and the paper started very well with its Introduction and Methods sections. However, I found the Results and Discussion very weakly presented, without in-depth analyses and implications. It is not clear what is the lessons learned and how it can benefit the wider community. In addition, these two sections read much more like a technical report. There are many additional tests that can be

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made to improve this study (I've made some suggestions). For that reason, I believe this paper manuscript requires considerable changes, hence I recommend major revisions before making my decision on its acceptance.

List of comments:

L63-64: Notice there are several approaches that constrain model parameters that do account for uncertainties, please refer to works by Keith Beven, Jim Freer, Jasper Vrugt, Grey Nearing, Hamid Moradkhani, Martyn Clark; to name a few.

L64-65: First, can the authors please point out the references for the 'Previous studies' mentioned in the sentence?

L65-66: Note that usually, the term data assimilation has been used in different ways by the atmospheric sciences and land surface modeling community in relation to the hydrological modeling community. 'Data assimilation' in general refers to using/fusing observed quantities to better constrain model components (i.e., parameter, states, etc...). Typically, the use of 'parameter estimation', 'state estimation', or 'dual parameter-state estimation' would be more clear. The reason I am mentioning this is because, although not technically a classic data assimilation application, the group by Luis Samaniego in UFZ Germany has explored similar approaches to this one using their mHM with their MPR framework. Additional work 'assimilating' both state and parameters include groups from Harrie-Jan Hendricks-Franssen, for example.

L79-81: This seems to be related to Results, not sure why it is included at the end of the Introducion section.

L94-95: The direct information obtained from SMAP is typically for the first few centimeters of soil; yet your JULES model is configured with a relatively thick initial soil layer and only 4 layers in general. Have the authors considered revising their soil layers in JULES? Have they done any simple sensitivity study to check how influential the choice of soil layer discretization is when assimilating SMAP data. If I recall correctly, CLM (which is similar to JULES) is run with a much finer soil layer discretization.

L113-115 and Table 1: It is unclear to me how the prior is used. Don't you need an emsemble (i.e., range) for each prior factor shown in this Table? How is a single prior applied in this case?

L138-140: Can the authors be more specific about this? There are many studies that have used the COSMIC operator which is available (refer to works by Jim Shuttleworth, Rafael Rosolem, Harrie-Jan Hendricks-Franssen, as examples). Have the authors consider implementing this operator?

Section 2.6: Needs to be expanded as it is very vague and general.

Figure 3: Typically, DA are justified as an operational tool for models (in the case of state estimation). This figure here shows the Bayesian optimization approach (prior –> likelihood –> posterior) which is fine. However, I'd be interested to see the time-series of the final soil parameters (produced with the updated pedotransfer function) to check for any inconsistencies in the way a particular parameter change from time to time. I'd expect soil properties to be fairly constant (relatively to the fluxes and states in the JULES model). Also, the authors should consider checking which of the PDFs shown in the figure are expected to be significantly different. One way to do this is for example by checking whether two samples come (or not) from the same probability distribution. This can be easily done with a two-sample Kolmogorov-Smirnov test.

Figure 6: It is important to show how the prior and posterior spread compare with the actual RMSE calculated against the actual observation to check for consistencies with the DA setup. Without this analysis shown (for some points and maybe regionally), it is hard to diagnose the DA results. The goal is for the spread to have the same magnitude of the RMSE (not too large, nor too small)

Figure 4: It is not clear to me how RMSE is calculated in percentage. Maybe I missed something. Can the authors made this clear in the captions.

Results section: I found the results section to be presented in a very weak way. It seems to be rushed with the same regional map shown only for different metrics. The section is written almost like a technical report just going from figure to figure with very little in-depth analysis. How does the soil moisture in the region change from time to time (the metrics are only aggregated for the period)? Are the soil properties and consequently soil moisture profiles realistic? What are the impacts on other components of the model? Does 'improving' soil moisture improves other fluxes in JULES? My understanding is that COSMOS-UK also has flux data that can be used (H, LE, G???). The simple exercise of assimilating soil moisture to constrain parameters and/or states and evaluate the impact on soil moisture only does not seem to be particularly novel in my opinion (the DA frameowrk and the use of COSMOS-UK do, but should be explored further). This item is a major issue I have with the current manuscript.

Figure 10: There seems to be some systematic biases in the model that suggests non-optimal DA setup (DA requires errors to be around a zero mean). How much that impacts the results? Are there other sites with similar issues (can you expand the discussion)? Have you tried some initial pre-calibration prior to runninng the DA to reduce/remove the biases?

Discussion section: I also found the discussion a bit weak. Very little is further discussed and explored. Sometimes the discussion is mainly focused on aspects that can be done in the future. I'd suggest the authors to define 2-5 clear objectives –> questions –> hypotheses that can be presented in more detail in the Results section, and discussed more in-depth in this current section.

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