Editor

Your manuscript "Improving Soil Moisture Prediction of a High-Resolution Land Surface Model by Parameterising Pedotransfer Functions through Assimilation of SMAP Satellite Data" has been subjected now to re-review by the original three reviewers. They recommend now moderate revision, minor revision and acceptance of the paper. Apart from handling the minor comments, the major point raised by reviewer #3 is important. It implies that additional verification of the estimates is needed, for example evaluating the impact of soil moisture assimilation on evaporative fluxes. If the authors cannot give such a verification, a motivation should be given why this is not done. Clearly, further independent validation would be of interest. For example, in several studies it was found that soil moisture assimilation hardly improves the characterization of ET. It would be important to document such a finding which allows for further research actions in the future. I recommend therefore moderate revision.

Looking forward to the revised version of your manuscript.

Dear Professor Hendricks-Franssen,

We thank you for providing a decision on our manuscript and the clarification of final points which need addressing. We have managed to acquire access to observations from 3 flux tower sites with which to assess the performance of both latent and sensible heat flux for the experiment, 2 new sections have been added to the paper comprising approximately 40 new lines of text, 3 new figures and 2 new tables. We have also added two additional authors to the manuscript who were responsible for providing the flux tower observations. From the flux tower sites we see an improvement in the modelled heat fluxes after the data assimilation procedure is conducted, giving us further confidence in the results. We thank you again for your handling of this manuscript and hope that this will satisfy the final comments.

Kind Regards,
Ewan Pinnington

Reviewer 1 (R#1)

The authors have significantly improved the manuscript to the first submission. The method and results have been well described and meet high scientific standards. Great to see the well-developed section 2.3 SMAP Observations and the Section 2.5 Data Assimilation Framework. Both sections are clarify the questions about methodology. Same holds for the discussion section.

We thank the reviewer for their comments that have helped to polish the final few things in the manuscript.

Only few minor issues /suggestions:

Abstract: Split the last sentence into two.

Line 87: Replace JULES model with 'domain'. Reason: The number of grid points does not depend on the model but on the resolution and the domain.

Corrected.

Line 87: Replace the two points with a single paragraph and three sentences. An enumeration of two points is not worth an enumeration. e.g. 'We defined two objectives. At first...'

Corrected.

Line 119: replace ‘van Genuchten soil model’ with van Genuchten soil parameters, ‘parameterizations scheme’, PTF, or something alike. Look for further appearances (e.g. line

120/121). In Line 130, 'soil parameters' was used, according to this suggestion. This is more specific and much clearer to 'soil model'.
Corrected.
Line 134: capital T for 'Table 1'.
Corrected.
Figure 3:Please, clarify the meaning of 5 blue lines. The reader expects 50 lines or an indication of spread. Consider using shading, dashed lines, dotted lines, etc. The messages of the figure are clear, though and the figure is well placed along the methodology section.
We have updated the captions in this figure to represent *N* ensemble members and have indicated that, although 5 are shown in the Figure, the experiments use 50 ensemble members.
Line 345/346: Two grammar mistakes in one sentence. Although these are the only ones I spotted, consider running the script through a grammar check. 'pedotranfer', 'the the'
Corrected.
Line 426: 'have have'
Corrected.
Congratulations to this manuscript.
Thank you again!

Reviewer 3 (R#3)

I have read the revised version of the MS entitled "Improving Soil Moisture Prediction of a High–Resolution Land Surface Model by Parameterising Pedotransfer Functions through Assimilation of SMAP Satellite Data" by Pinnington and others. I can say that the authors have made a significant effort to improve the MS and to address all reviewers questions. This has resulted in a significantly better manuscript. However, I do still have concerns about the study, especially its lack of validation and overall impact on JULES predictability of other hydrometeorological variables (explained below). For that reason, I recommend moderate revisions for the paper either by introducing additional experiments/analysis or by carefully and clearly explaining why the decision for not to do so with an explanation for the possible consequences of such changes in the model performance.

We thank the reviewer for their additional comments which have helped us further improve the manuscript. We agree that including some additional validation will help to improve the strength of the manuscript. We have therefore managed to gain access to observations from three flux tower sites located within the experiment domain for comparison to the modelled estimates. We find improvements vs these flux tower observations for sensible and latent heat fluxes. We have added these results to Section 3.3 of the manuscript along with three additional figures (Figure 15-17) and 2 additional tables (Table 4 & 5).

My main point relates to the fact that SMAP data is ingested into the JULES-DA system to improve the estimate of static soil properties as well as then soil moisture dynamics in a region. The authors have shown for some few selected grid-points, a comparison against independent soil moisture from the COSMOS-UK network. However, it is unclear with that information, how one should expect the region to be improved or not. For example:

1. How do we know the updated regional maps of soil properties are realistic? Could the authors compare with independent maps (e.g., Cranfield soil database in the UK, or using the global SoilsGrid database) at least as a baseline?

Whilst we agree that further validation of the retrieved soil properties would be beneficial, we do not believe that comparison to the other datasets mentioned would achieve this. The

technique outlined here updates the parameters of a pedotransfer function. This optimised pedotransfer function is the applied to the Harmonized World Soil Database (HWSD) for the experiment domain. As the soil properties themselves are not updated within the experiment comparing results to the Cranfield or SoilGrids database will only tell us about the differences between these soil property datasets and not the performance of the outlined technique.

2. How do we know the claimed improvements in the soil moisture will ultimately result in improvements to other hydrologically-relevant variables in JULES? My understanding is that a good number of COSMOS-UK stations provide at least sensible heat flux estimates and evapotranspiration can be estimated as a residual given the other surface energy balance components are also provided. Yes, there are some uncertainties in such approach but for, say, daily estimates this should give the authors more/less confidence in their experiment. Additionally, the authors could have also run their experiments trying to estimate streamflow at particular catchments and compare their results against UK's NRFA streamflow data, although this can be a bit more complicated if the region were characterized by groundwater dominated catchments and I believe the operational JULES version doesn't yet include such parameterizations.

As stated above we have now included comparison to the observations from three flux tower sites within the experiment domain. We believe that the observations from these tower sites will be more accurate than those estimated using the residual method at the COSMOS probe locations. At all three sites we find some improvement in the modelled sensible and latent heat fluxes, with the largest improvements corresponding to the site where the data assimilation is having the greatest impact on the model soil moisture trajectory. The reviewer is correct that the current JULES version does not include a ground water parameterisation, the water ways of East Anglia are also quite heavily managed so may impede such a comparison to streamflow data. We therefore leave evaluation of the effect on streamflow for further work, which we are beginning to pursue at present.

Minor comments:

With regards to my question about the thickness of JULES layers against shallower SMAP estimates, I appreciate that the results from the 5-cm thick experiments were similar to the original 10-cm thick simulations. I am fine to accept that. However, the additional explanation that 10-cm thickness was kept to be consistent for the wider community is very weak in my opinion. The increase in horizontal resolution should be thought simultaneously with its vertical resolution, and old parameterizations or model structures need to be frequently "challenged" otherwise understanding of model structures and their limitations are hindered.

We appreciate the reviewer's comments here and will keep it in mind in the future to challenge the status-quo! Thank you again.