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Comment on hess-2021-460

Anonymous Referee #1

Referee comment on "Soil moisture estimation in South Asia via SMAP retrieval assimilation" by Jawairia A. Ahmad et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-460-RC1, 2021

OVERVIEW

The study investigates the assimilation of SMAP soil moisture retrieval in land surface modelling (Noah-MP) for improving soil moisture and other fluxes estimation in northern India, southern China region.

GENERAL COMMENTS

The paper is fairly well written and clear. The topic, i.e., the use of satellite soil moisture measurements for improving the prediction of fluxes and states in land surface modelling, is surely of interest for the readership of Hydrology and Earth System Sciences journal. It is particularly relevant in regions where the human impact on the water and energy cycle is large, as in the investigated study area. Therefore, I believe the paper might deserve to be published but, in my opinion, after the clarification of some important points.

I listed here the main comments also including their relevance:

- MAJOR: The main problem with the paper is that the better results are obtained when SMAP soil moisture data are assimilated without the correction of the BIAS. Of course, the correction of the BIAS in regions in which we have irrigation, that is not modelled by the Noah-MP version used in this study, will provide wrong results. However, without the correction of the BIAS, i.e., no CDF-matching (or other methods), the assimilation method is not correct as well. Indeed, the assimilation should correct the random error, not the BIAS. The bias between modelled and satellite data is related to multiple causes, differences in spatial scale, differences in parameterization (e.g., wilting point and saturation), differences in land cover specification. A BIAS between modelled and satellite data is expected. For instance, a positive BIAS is observed also in the summer period (see Figure 4 and 7) or in areas not irrigated (Figure 4), and it cannot be easily explained. I am sure that in Pakistan and northern India the satellite

soil moisture data can contribute to see the unmodelled irrigation and hence to contribute to improve land surface modelling. However, the approach used in the study should be corrected. E.g., the Noah-MP model with the irrigation module can be run. BIAS correction only during summer (i.e., in which irrigation is negligible) can be implemented (not month by month). These are just two suggestion to the authors.

- MODERATE: The title and the text are misleading. The study is carried out in a limited region between northern India and southern China and Pakistan, not South Asia. Moreover, from the results it is not shown that soil moisture is improved over irrigated areas, as the comparison with in situ data is carried out over non-irrigated sites. Please correct the title and the corresponding text.

- MAJOR: The results of the comparison with in situ data are not robust. I believe that direct comparison of SMAP soil moisture against in situ observations provides a much better agreement with in situ data. Therefore, the data assimilation configuration is not optimal, and very likely the model error has been underestimated (or overestimated the SMAP observations error). Also, I believe that the sample size for which the analysis has been carried is quite low, sample size should be added to assess the significance of the results. On this basis, I believe such analysis should be improved and that meaningful inferences cannot be done due to, e.g., the very low correlation values or very high relative RMSE (>1.5 and should be lower than 0.7). Note that SMAP vs in situ provides R values greater than 0.8 (see e.g., https://www.mdpi.com/2072-4292/10/4/535/htm, but there are several other papers). In my opinion, in the paper it is not shown that the assimilation improves soil moisture estimates, at least not a robust assessment, and not in irrigated areas (as in the title).

- MODERETE: As expected, the impact of the assimilation on the evapotranspiration fluxes and on GPP is limited. It depends on the variable that is assimilated, i.e., surface soil moisture, but also on the coupling of such variable with root zone soil moisture and fluxes. It happened frequently that models are not able to transfer surface soil moisture information to deeper layer due to model and data assimilation technique limitations. There's a lot of scientific literature on the topic. Please consider this important aspect in the assessment of the impact on fluxes, and possibly to improve the data assimilation framework employed in the study.

I listed in the specific comments a number of corrections and changes that are needed.

SPECIFIC COMMENTS (P: page, L: line or lines)

P1, Title: Please change by considering the general comment above.

P3, L77: Typo "populace"

P6, L128: Show in the map the location of in situ soil moisture stations with the name of the networks.

P7, L142: Please check the acronyms definition throughout the text, e.g., MODIS defined below, not the first time used.

P7, L143: Please specify the version of the datasets used, and the link where the data are available.

P7, L169-170: It is likely that 5-year of spinup is not enough for a correct initialization of the model. In our simulations we typically consider 30-year spinup.

P9, L185: Where is Table 2? It is the table in Kwon et al. Please check.

P10, L230: It should be clarified the soil layer that is considered for the assimilation of surface soil moisture data, and how the information is propagated with depth.

P11, L269-270: In Maqu results are different, i.e., RMSE is lower for MERRA2. Please reformulate.

P15, L323: Why overestimation also for Savannas? It should be clarified.

Figure 6: Due to uncertainties in soil map, I believe this figure is not really useful for the paper and it can be moved to the appendix.

P16, L349: Note that Noah-MP includes an irrigation module that I believe can be very useful for this paper (see also General Comments).

P24, 479-480: It should be added more details on how irrigation can be quantified using "an inverse method" otherwise I suggest to remove the sentence.

RECOMMENDATION

On this basis, I found the topic of the paper relevant, and I suggest a major revision before the paper can be published on Hydrology and Earth System Sciences.