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Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Reducing soil moisture measurement scale mismatch to improve surface energy flux estimation" *by* Joost Iwema et al.

Anonymous Referee #3

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OVERVIEW

The manuscript investigates the use of different in situ soil moisture datasets for improving surface energy flux estimation from land surface modelling. Specifically, the JULES land surface model is calibrated against point scale (PS) and cosmic-ray neutron sensor (CRNS) soil moisture data. The rationale is that CRNS provide measurements at larger scale than PS observations and, hence, they are more appropriate for surface energy fluxes estimation.

GENERAL COMMENTS

The manuscript is quite well written and clear, even though some parts should be re-

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duced and summarized. The topic is surely of interest for the HESS readership as cosmic-ray probes represent a new technology for ground measuring soil moisture over large areas. Therefore, we need to assess the impact of this new technology for improving land surface modelling. The paper describes the calibration of JULES land surface model with PS and CRNS at different sites in US. The manuscript is well conceived and applied over a large number of sites thus obtaining reliable and robust results. However, I mostly agree with the comments of previous reviewers, and particularly I believe that several aspects should be improved/changed before the publication. I reported below a list of the general comments to be addressed with also the specification of their relevance.

1) **MAJOR**: I found some of the explanations/justification of the results given in the paper quite weak. They appear to me as speculations, not supported by the performed analyses and results. For instance, I refer to:

(A) The comparison between PS and CRNS soil moisture data (section 3.1.3) shows that soil moisture timeseries are quite similar. The authors expected better performances at homogeneous sites but it was not the case. As shown in "temporal stability" papers (see also Teuling report), PS measurements are usually very well correlated with large scale measurements. Therefore, I expect good correlations. In my opinion, the good (bad) performances are mostly related to the good (bad) quality of soil moisture observations that may be affected by a number of factors (e.g., soil texture, sensor malfunctioning, ...). Therefore, theoretically I could expect that CRNS are better than PS measurements, but due to measurement uncertainties and errors, the larger support scale of CRNS is masked out by the (likely) lower quality of their measurements. This important aspect, i.e., the quality of soil moisture observations, should be carefully addressed in the paper.

(B) The authors attributed the low differences in estimating surface energy fluxes when PS and CRNS measurements are considered to the weak coupling in JULES between soil moisture and evapotranspiration. Actually, I do not believe it is the case, but it should not be a speculation. It should be tested. If the authors want to give this message, they should demonstrate that with a different model or land surface scheme the differences are higher, and likely CRNS is better that PS data (as expected at the beginning). Therefore, I suggest changing the conclusions or, better, implementing an additional LSM and demonstrate the results through a scientifically sound approach.

(C) The range of reasons reported at page 13, lines 13-34 are only speculations. I suggest removing.

2) **MAJOR**: I found quite strange that by using the default parameter values performs the same than using the parameter values calibrated on soil moisture data in terms of evaporation fraction (EF) estimation. Even though soil moisture data were of low quality, or the coupling between soil moisture and EF is weak, soil moisture observations represent local data that should give some information to the model. Therefore, I expected better results with respect to the default parameterization. What happens if JULES is calibrated on EF data? How the corresponding modelled soil moisture data compare with PS and CRNS observations? By looking at the results reported in the paper, it seems that using soil moisture observations is needless if we have the purpose of improving land surface modelling. I suggest the authors to improve the discussion and the analysis of the results.

3) **MODERATE**: I found the description of the results with too many details in several parts of the text(e.g., section 3.1.1, page 9 lines 14-28, page 12, lines 3-18). I suggest not discussing the results for each site, but trying to summarize the most important findings and to focus the discussion on these results.

In the specific comments, I added some corrections and suggestions that should be implemented.

On this basis, I believe the paper deserves to be published only after a major revision.

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

P2, L26: The sentence "past research indicates ...wetting and drying periods" is too vague. At least, references should be included. However, I note that it is still an open issue to fully understand in which conditions soil moisture variability is higher. For instance, it is not the same if absolute or anomaly soil moisture values are analysed (see e.g., Mittelbach and Seneviratne, 2012; doi:10.5194/hess-16-2169-2012).

P3, L8-10: The sentence is incomplete (only a single sensor is needed for?), please check.

P4, L32: The gap-filling of 30 days seems to me a very large window. Does it affect the results? Some tests should be made.

P5, L6-10: It should be better to insert an equation here.

P9, L20: The larger differences between PS and CRNS at wetter sites are expected. Higher is soil moisture, higher will be the differences.

P10, L5: Figure A1 should be Table A1?

P10, L18-22: The difference in sensing depth might be the cause of some of the differences between PS and CRNS. However, it could be checked with specific analysis. Otherwise, I suggest removing.

P10, L29: It is obvious that after the calibration on soil moisture data the RMSE values will reduce. The model is tested with the data used for calibration.

P11, L1-2: The use of RMSE for calibration reduces the mean error between modelled and observed data. Therefore, the effect of having less extreme peaks and valleys is due to the selected objective function.

P11, L24-28: From here it is not clear the number of sites over which an improvement in EF estimation is obtained. 11 sites (P11, 22) or 12 sites (P11, L28). Check also later in the text (e.g., P13, L36).

P12, L3-6: It is not clear to me what the authors want to demonstrate with this analysis. Please clarify.

P12, L32: Change “on the edges” with “within the edges”.

P15, L9-12: Not clear to me how the “multiplier” values are used. Please clarify.

P16, L4-11: As mentioned above, I found not scientifically sound to attribute the low performances in term of EF improving to the weak coupling of JULES. Moreover, it’s not clear to me the discussion of the value of absolute soil moisture with respect to anomalies.

Figure 8: Labels a) and b) are missing.

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