Answer to interactive comment by Anonymous Referee #3

We would like to thank the Referee for carefully reviewing the manuscript. We thank the Referee for his comments on the structure of our manuscript, specification and quantifications, and the English language. We will address these issues in the revised version of the manuscript. Below, we focus on the comments that concern with methods chosen and the conclusions drawn.

GENERAL COMMENTS

COMMENT:

The manuscript is quite well written and clear, even though some parts should be reduced 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.

ANSWER:
We thank the referee for his positive comments regarding the relevance of our work to the HESS community.

COMMENT:
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.

ANSWER:
We thank the Referee for this valuable comment. As also suggested by Referee #2, we will include a discussion on temporal stability/soil moisture scale issues in the revised version of the manuscript. We will also discuss the impact of quality of observations on the soil moisture signal in the revised version of the manuscript.

COMMENT:
(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.

ANSWER:
We thank the Referee for this comment. This is a point that was also raised by the other two referees. We agree that our use of the word coupling was not appropriate as also pointed out by the other reviewers. Our interpretation is, in fact, in line with Reviewer #2 comments which should emphasize that the results obtained in our study are a consequence of calibrating soil parameters, rather than the soil moisture – evapotranspiration coupling in JULES. This will be appropriately revised in the next version of the manuscript.

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

ANSWER:
We thank the referee for his/her comments. Note, this issue was also raised by Referee #1. We will re-assess this section of the manuscript for the revised version of the manuscript.

COMMENT:
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.

ANSWER:
This is an important point raised by the reviewer and it is in line with Referee #2's comments. Our interpretation is, in fact, that the results obtained in our study are a consequence of calibrating soil parameters, rather than the soil moisture – evapotranspiration coupling in JULES. We will improve the discussion and analysis for the revised version of the manuscript as also recommended by the reviewer.

Specific comments

COMMENT:
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.

ANSWER:
We thank the reviewer for raising this point. This was also mentioned by reviewer #1. We provide a table below summarising the average (average of the seven forcing variables) percent gap for each site. Precipitation was not gap-filled; missing points were set to zero instead. Overall, average gaps vary among sites between near zero to 15%. As pointed out by the reviewer, this can introduce

some uncertainty in the analysis and we will highlight data when describing the dataset used in this study. We will also mentioned in the text the original gap period in the data.

Site	Percentage missing hours filled: mean (range)	Size of time series in years
UM	7 (2-12)	1.5
DC	1 (0-5)	3.7
SO	7 (0-15)	3.8
KE	1 (0-3)	4.6
ME	0 (0-0.1)	1.6
SR	2 (0-14)	3.6
CS	2 (0-5)	3.7
MM	1 (0-2)	2.7
TR	0.1 (0-0.2)	3.6
AR	10 (8-14)	2.5
WR	2 (0-4)	2.6
MO	3 (1-7)	2.7

COMMENT:
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.

ANSWER:
We will remove this text.

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

ANSWER:
With this analysis we wanted to see if surface energy partitioning estimation did improve during periods of soil moisture stress, when soil moisture was below the critical point. During these periods soil moisture provides a first order control on evapotranspiration. We will improve this discussion accordingly in the revised manuscript, as also suggested by Referee #1.

COMMENT:
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.

ANSWER:
We thank the reviewer once again for making this excellent point. As we previously mentioned, our use of the word coupling was not appropriate in the manuscript. Our interpretation is, in fact, in line with Reviewer #2 comments which should emphasize that the results obtained in our study are a consequence of calibrating soil parameters, rather than the soil moisture – evapotranspiration coupling in JULES. This will be appropriately revised in the next version of the manuscript.