Review of "*Improvement of modelling plant responses to low soil moisture in JULESvn4.9 and evaluation against flux tower measurements***" by Harper et al.**

The authors explore multiple mechanisms for improving the simulation of vegetation soil moisture stress in the JULES land surface model. This is a very topical study, with biases in the representation of soil moisture stress identified as a key weakness amongst land surface models. These biases not only have important implications for future projections of water cycling and drought but also the carbon cycle. As such, improving mechanisms controlling soil moisture stress has relevance to the wider land surface modelling community. I believe this paper can make a valuable contribution to the literature but requires major revisions before it is suitable for publication.

1) The modelling choices are not well motivated and lack observational basis, coming across as *ad hoc* choices. For example, what was the basis for setting soil depth to 10.8m? Similarly the rooting depth changes or doubling the d_r parameter. I agree that there is evidence for too shallow rooting depths in LSMs but I wonder how true this is for grassland/crop ecosystems? The authors also provide observational evidence for rooting depths in Figure 2 which does not support the chosen 14-layer rooting depths. I acknowledge observations are very uncertain but the authors should nevertheless justify their choice. The paper also lacks in-depth discussion on the pros and cons of the new alternative methods and their merits in improving the representation of soil moisture stress in LSMs.

2) Section 3.3: this section is the most important one of the paper but is very difficult to follow. It should be re-arranged to a more logical order, either stepping through the experiments or the categories defined in the previous section. It was also a shame more emphasis wasn't placed on this section, with the bulk of the results concentrating on evaluating the default model at seasonal- to annual scales despite the paper's focus on soil moisture stress. I am also wondering why only a small subset of sites were used in section 3.3?

3) The paper needs cleaning up. Multiple figures are not referred to and I got rather confused reading some of the results sections. I have provided specific suggestions below.

Minor comments:

L60: Would add *water* stress

L67: Not clear what you mean with "when onset of stress was delayed"

L80: "unusually" dry soils is not accurate here as the study doesn't differentiate between sites experiencing droughts (anomalously dry conditions) or those experiencing soil moisture stress due to regular dry seasons.

L81: This sentence is quite vague

L89: happened -> happen

L92, L106: Here and multiple other instances, need to correct brackets and spacing around references

L99-100: And also further desiccation of soils

L117:119: Clumsy sentence

L144: Could also cite Mueller and Seneviratne 2014 (GRL, 41, 128-134)

L164: *A* defined on L170, not needed here.

L180: "in various places" is too vague

L214: systematic biases have been found in both grass and woody ecosystems, is there evidence grassland rooting depths are also too low?

L216: What is the basis for 10.8m?

L218: Provide justification for doubling d_r

L220: Why was a value of 0.4 chosen?

Eq. 8: Ψ open and close not defined

L246: Not clear what you mean here (root fraction equal to layer thickness)

L250-251: Need a reference here for this being "observed" and "more realistic"

L268: Ideally should include a map of sites in the main paper so the reader can see the spatial distribution of sites. Also no information here on how the sites were chosen

L275-276: This reads as if the authors did filtering and partitioning, was this the case or were the data derived directly from FLUXNET2015? Also should mention what NEE and GPP variables were used since FLUXNET2015 provides multiple options

L278-279: repeats what's on L275?

L283: Would be useful if values for the obtained site properties were provided in Table SM1

L290: tile fractions: not clear what you mean here

L299: Do you mean RMSE? I don't see RME used anywhere

L300: Please explain how NAE values are calculated and how to interpret the values

L305: RMSE not defined. Also why were annual means used? Water stress is often experienced seasonally (e.g. dry seasons in the tropics) and using annual means could lead to compensating errors (underestimation during water stress, overestimation during well-water conditions as noted in previous studies).

L314: would be better to report the range separately for sites that are over- vs. underestimated, rather than a 0.5-1.5 range.

L316: report the range for cold grassland and cropland. Similarly tropical forest and grassland on following line

L317: What does "in this case" refer to?

L318: Fig. 5 mentioned here before any reference to Figs 3-4

L319-21: Sentence should be rewritten for clarity

L321-22: need some metric to back this up

L326: Are the biases larger in the tropics simply because the fluxes are larger?

L332: how many sites were considered here?

L334: Fig SM5 mentioned before any reference to earlier SM figures

Figure SM5: remove duplicate legends

Figure 5 and SM5: would be useful to show rainfall bars on these plots

L336, 339: what does standard approach refer to?

L344: any reason why this was?

L346: But would one expect plants to access frozen soil moisture? Or is the implication here that JULES overestimates the extent of frozen soil?

L333: This sentence needs unpacking

L387: How many sites and how were the sites chosen? Also where prescribed data used where possible?

L396: SM figure numbers should be re-ordered so they appear sequentially

L398: this doesn't logically follow from the previous paragraph

L417: "correlation was high for these four experiments" doesn't match the numbers provided in brackets

L418-429: need to refer too figures in this section

L430-435: The values for all metrics should be provided in a Table or in the text

L442: do you mean the annual absolute error?

L460: space missing in "aminimum"
L476: I still wonder if there is evidence for this in grassland ecosystems?

L505: Observed -> Observation

L509: please give more information than "replace Eqs 4-5 with Eq 8". Also should not cite work in prep

L536: Should acknowledge FLUXNET2015 as per their data use requirements

Discussion: missing discussion on how the results here can help other modelling groups.

Figure2: The righthand panel is not discussed anywhere