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Geoscientific Model Development Discussions

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

Anonymous Referee #1

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The manuscript discusses different formulations of the soil moisture plant physiological stress within the model JULES. Multiple formulations are presenting, based on either soil moisture or water potential. Beyond those, the importance of the vertical discretization of the Richards' equation is explored, along with the assumed soil depth and root distribution. For a limited number of sites, where soil moisture and LAI data existed, the authors also used them as prescribed values to the model in order to disentangle the importance of the soil water stress from the remaining model errors related to plant phenology and hydrology. Overall, the manuscript is within the scope of GMD. Even though focused on JULES, almost all state-of-the-art ecosystem models adopt similar formulations, and thus the results are likely important for many similar models. It is also

clearly written. However, I have a few concerns that need to be clarified:

Specific comments:

1) While it is perfectly reasonable to seek a unified equation to model plant water stress, the same is not true for model parameters, such as the threshold where plants start experience stress, the soil depth, and the root distribution (something also pointed out by the authors in the manuscript's introduction). These would be site-specific parameters, and seeking a model set-up that fits all, would be unrealistic in my opinion. The authors need to further explain their rationale regarding the choice of the numerical experiments, and what is the information they wish to extract from each of them. Possibly adding some detailed hypotheses linked to each scenario would help the reader. As a suggestion, in order to fully evaluate the performance of soil water stress formulations (e.g. moisture-based vs potential based formulations) a small number of sites where the root distribution, the soil depth and the soil hydraulic parameters are known would be very useful.

2) Increasing the soil layers from 4 to 14 will lead to a more accurate solution of the Richards equation (in terms of numerical accuracy). Solving a highly nonlinear PDE, with a very coarse spatial resolution (e.g. just 4 layers) is expected to lead to biases. Because of that I would suggest the authors to present this choice, not as model improvement, but likely as a warning against using coarse vertical resolutions to gain computational speed.

3) Tables 3 and 4 would be much better if they included the actual numbers (correlation coefficient, RMSE, absolute error) for all sites and simulations (and possibly a rank for each simulation, from best to worst), rather than the classification per biome. I also suggest the authors retain the qualitative classification for the results section itself rather than presenting the numbers there, as this section is currently a bit convoluted and difficult to follow.

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