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Interactive comment on "Revisiting the First ISLSCP Field Experiment to evaluate water stress in JULESv5.0" *by* Karina E. Williams et al.

Anonymous Referee #2

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This manuscript describes an evaluation of the JULES land model using field data from the FIFE dataset. Three simulations are presented: a replication of a simulation from an earlier model version, a standard simulation using the current version, and a tuned simulation using site specific parameters rather than global parameters.

Numerous model intercomparison projects (MIPs) have been published in recent years, and one of the criticisms of these studies is the lack of adequate control in the experimental design. For example, models participating in a study may differ in forcing data, structure, and parameters, which makes attribution of differences in the results to these model characteristics difficult if not impossible. This study can be thought of as a three model MIP, and it features the same difficulty. Although the three models are all versions of JULES, they differ in multiple ways, making the interpretation of the results

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Discussion paper

unclear.

For example, simulations 2 and 3 differ due to the tuning of simulation 3 of multiple parameters for stomatal conductance and C4 photosynthesis, such as: SLA, leaf nitrogen, light response parameters, Vcmax temperature dependence, A-ci response at low ci, ci-ca relationship, dark leaf respiration - Vcmax ratio, p0 (water stress onset parameter), and canopy structure (uniformity). Figures 8-10 show that the site specific tuning exercise leads to large differences between simulations 2 and 3; in fact, these differences are larger under *unstressed* conditions than under stressed conditions. Moreover, while the tuned parameters should by design lead to better agreement with the observations against which they were calibrated (figures 3-7), they do not in general improve the simulation of GPP or An under unstressed conditions. I recommend to the authors to give the reader a better understanding of the individual impacts of this tuning exercise in the context of their effect on GPP and An. Which parameter changes improved the comparison, and which degraded the comparison? This is important to understand, more so because the agreement differs for the different days presented in the analysis. While it is subjective, I did not agree with the authors' statement that the model was "...able to successfully reproduce the net canopy assimilation and latent heat energy flux reasonably well through the season". Given this perhaps unsatisfactory starting point for unstressed conditions, I recommend to the authors to focus first on obtaining more credible results under unstressed conditions before addressing the model's response to drought.

The title indicates that the goal of the manuscript is the evaluation of water stress in the current version of JULES, thus my expectation was that the experimental design would isolate the behavior of this parameterisation; however, this is not really the case. Only figures 11-13 show single factor analyses, and of those, only figure 11 directly examines the water stress parameterisation used in the model.

Figure 11 shows the results of varying the p0 parameter that determines the initial soil moisture value at which vegetation experiences stress. I recommend that the authors show a few actual lines rather than the spread of p0 = [0,0.4] to enable the reader to easily see whether increasing p0 increases or decreases GPP. One might expect that water stress based on soil moisture would not exhibit large diurnal variation, and this is confirmed by these plots. Figures 8-10 show that the diurnal variation in GPP and An can be simulated (in this case by simulation 1) if a predictor having stronger diurnal variation (such as temperature) is used. However, the authors note that this type of parameterisation is not well supported ("The repro-cox-1998 simulation is more successful, but this response is mediated by a temperature dependence in leaf carbon assimilation which is not supported by observations") as shown by figure 4.

At this point the analysis is basically complete, with no improvement in the diurnal cycle of carbon flux. This seems not to support the authors' conclusions regarding outcome of the study (e.g. "FIFE provides an ideal case study for improving the model representation of water stress on carbon and water fluxes on a tallgrass prairie site") as no significant improvement was made aside from tuning the p0 parameter based on unstressed soil moisture conditions. The authors note that leaf water potential was used by authors of previous studies to simulate the diurnal cycle of GPP under dry conditions, leading them to conclude "JULES is not currently able to capture the diurnal cycle of net canopy photosynthesis at this C4 grass site, due to the lack of a strong dependence on the canopy vapour pressure deficit (indirectly or directly)", but this is largely conjecture and not actually tested by the authors of this paper.

In summary, this manuscripts describes many of the issues that one encounters when attempting to constrain a model to field observations, such as uncertainty in measurements and spatial heterogenaeity. It highlights the fact that greater model complexity does not guarantee greater model fidelity. It also shows that site-specific model parameters may give significantly different results relative to global parameters. This is valuable information and worth presenting. However, that is not the stated focus of the manuscript, which is water stress and its improvement in JULES. To that end, I recommend to the authors to revise their title and to shift the focus of their discussion towards the actual content of the paper.

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