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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 #1

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Williams et al. explored parameterising three different configurations of JULES to capture the diurnal cycle of GPP, net canopy assimilation, and latent heat flux during the dry spell at FIFE site 4439, Kansas, US, in 1987. They chose this site because it is of historical importance to the JULES community, having been used to develop the parameterisation of water stress in the model, but also because of the wealth of data collected there during 1987-1989. Out of the three configurations they tested, the authors found that the repro-cox-1998 was most successful at capturing the site fluxes, i.e. the closest approximation to the original Cox et al. 1998 model. Despite incorporating physical processes which are not supported by observations at the site (e.g. Vcmax declining at leaf temperatures above 32°C), this configuration is heavily tuned to the site data and mimics the model historically used to derive the JULES' parameterisation

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of water stress (i.e. Cox et al. 1998). The two less successful configurations both embed more sophisticated and mechanistic representations of the canopy, soil, and radiation modules; however, they are not run with the same PFT-specific parameters that were used in repro-cox-1998.

Overall, the authors' results seem to highlight the need for: (i) more coherent / less error-ridden site forcing data, (ii) more thorough evaluation at different stages of model development with regard to the assumptions in calibrating vegetation parameters. As presented, it is unclear what novel advances to the literature these broad conclusions brings. Nevertheless, there are interesting elements within the study, such as the author's effort to test three different configurations of a single model, representing different levels of complexity, with a variety of data for a specific PFT. For the value of those elements to clearly appear to the reader and for this manuscript to be ready for publication, I believe major revisions are necessary.

It is especially important in revision that the authors reorganise their manuscript to more clearly demonstrate their findings. It is likely that separating the result and discussion sections will help the authors to more clearly present the paper's findings. In particular, thinking beyond the JULES community may help them articulate their findings - why would a developer of another LSM care about what is in this manuscript? Could more process-level interpretation arise from the simulations? But also, what are the advances for the JULES model community? If this is meant as a benchmarking type of effort, where is the performance evaluation? The latest more sophisticated configurations appear to perform "worse" than repro-cox-1998, so should JULES swap back to repro-cox-1998 for C4 grasses?

Main comments

This paper focuses on how well a model can simulate a C4 tallgrass prairie's response to water stress. So, generally, what are the valuable lessons? Why does the model fail to capture the dry-down response (what mechanism)? What have the authors tested to capture the missing mechanism? Even if simply empirically? Why not also run the global-C4-grass and the tune-leaf simulations with the JULES parameters used in repro-cox-1998 to highlight where the process based differences play a role? Unless I have missed this analysis in the paper, I think the respective parameterisations are different enough to hinder a mechanistic understanding of why differences occur. Where they simply assert: "inherent uncertainties in key observables, such as leaf area index, soil moisture and soil properties", could the authors attempt to constrain one of these, e.g. LAI? Otherwise, I fail to see the point if we simply end up concluding these data are too uncertain to evaluate against. Or, given one of the paper's aims to "demonstrate how the wealth of data collected at FIFE and its subsequent in-depth analysis in the literature continues to be a valuable resource for the current generation of land-surface models", what are the immediate next steps the authors intend to make to exploit these data to improve the JULES model, without data related uncertainty hampering model development?

Far more evidence is required to substantiate some of the points/arguments made to explain why the model is failing. As currently presented, they are purely speculative. For example, the authors speculate that an empirical link between leaf water potential and Vcmax/Jmax could improve models simulations. It would be an advance to the literature to actually show such a model (given they are relatively trivial to implement, e.g. Zhou et al. 2013, AFM; Kim and Verma 1991a, AFM), or at the very least, link more explicitly to literature that has done this (e.g. Tuzet et al. 2003, PCE; Zhou et al. 2013, AFM). Further, it would be useful to discuss the mechanism behind a direct limitation of leaf water potential on Vcmax and/or Jmax. It is unclear why the authors feel like the influence of VPD is negligible in the existing approach in JULES (lines 6-8, p.11), given that increasing VPD would drive a reduction in Ci? Indeed, in the last paragraph of Section 3.2, they show that for tune-leaf VPD influences GPP via this mechanism on both the 30th July and the 11th August. They also show that this mechanism alone doesn't have the flexibility to reproduce the observations. For all the days presented in Figure 13, it would be interesting to also plot Ac / E (the transpiration can simply be derived from the latent heat) relative to the declining soil moisture (or/and time), to see where the relative constraint is greater (on A more than E or vice-versa?) which might help further understand why the model is failing for the more extreme dqcrit parameterisation.

It is clear that FIFE site 4439 has historical value for the JULES community and that a lot of data is available. I am uncertain, though, as to how representative of the C4 grass PFT or the tallgrass prairie vegetation in general it is? Could the authors elaborate on this point, perhaps in the discussion? Why is this site a good proxy to calibrate the model for this PFT, in particular considering the variability of the site data? And is there any indication that it behaves like any other C4 grass site would during a dry spell? If this cannot be shown, I would encourage the authors to reword statements like "... FIFE observations were used to derive the original soil moisture stress parameterisation that was incorporated into JULES. This therefore makes FIFE an ideal test case for evaluating and improving this process.".

The authors could also more clearly demonstrate the impact of key assumptions. The following is an important point concerning the physical representation of the response of Vcmax to leaf temperatures above 32°C in Cox et al. (1998): "However, as discussed in Section 2, this temperature response is not supported by observations in Knapp (1985) or Polley et al. (1992). Therefore, it appears that, while the model is successfully capturing the shape of the diurnal cycle during the dry period, it is not achieving this with the correct physical process." It would be easy for the authors to test this, simply by swapping the temperature response function and determining if this statement is true or false.

Finally, I would like to thank the authors for making all of the code and data available. Their careful description of the steps taken to set up the simulations is also appreciated.

Minor comments

Lines 28-30, p.2: the precision that "changing p0 can be considered a proxy for changing the critical soil moisture" which appears lines 8-9, p.16, could probably appear here, or at least in Figure 1's caption

Lines 1-2, p.5: even though the differences in evaporative schemes aren't the focus here, can the authors estimate how those might influence their conclusions?

Section 2.3: how is the tune-leaf configuration calibrated exactly? What is matched for in the calibration process? What does "approximately representative of the dominant species" mean? How so?

Lines 9-10, p.7: it is unclear to me why the burned plot was not water-limited. Could the authors please elaborate?

Lines 31, p.7: why do the authors assume that the best parameter set is a composite of two species' parameter sets, given the non-linear response of photosynthesis to plant traits?

Line 10, p.8: I don't understand which mean the Al-Ci curves were normalised against

Lines 11-13, p.8: can the authors demonstrate this claim or refer to studies that do?

Lines 30-34, p.8: does this mean the dark respiration at Tleaf different to 30°C is then still scaled according to the temperature dependency in JULES (if so, can the authors justify this approach)? Or does the scaling follow Polley et al. (1992)?

Lines 4-14, p.9: that whole paragraph could be moved to the beginning of section 2.3.1, thus the text that follows might be less confusing for the reader

Line 22, p.9: missing the work "water" after "vegetation. Leaf"

Lines 22-23, p.9: maybe specify what other factors can affect leaf water potential

Lines 1-2, p.15: is this observed during the dry period as well? Does this mean that it is constantly proportional through time?

Lines 6-8, p.16: please add the missing words in the sentence

Line 3, p.17: missing "of" before "the humidity response"

Lines 4-6, p.18: leaf rolling/folding implementation feasibility in a global model should at least be discussed in view of the existing literature and considering the author's

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previous statement that “this behaviour cannot be modelled in the current version of JULES” (line 28, p.11); the same goes for including leaf nitrogen

Line 9, p.25: the authors should specify “for C4 grasses” or something equivalent

Line 14, p.26: I believe this is the first time senescence is mentioned. Do the authors propose to do this via the leaf water potential parameterisation? Or do they envision it might somehow relate to a LAI phenology?

Line 20, p.32: the approximation that soil evaporation can be neglected for days without rainfall seems rather big to me; have the authors considered including soil evaporation (though it isn’t the focus of the study) to reduce the uncertainty?

Figure A4: the depth unit should appear somewhere in the plot

Suggested references

Tuzet, A., Perrier, A., Leuning, R. (2003). A coupled model of stomatal conductance, photosynthesis and transpiration. Plant, Cell Environment, 26(7), 1097-1116.

Zhou, S., Duursma, R. A., Medlyn, B. E., Kelly, J. W., Prentice, I. C. (2013). How should we model plant responses to drought? An analysis of stomatal and non-stomatal responses to water stress. Agricultural and Forest Meteorology, 182, 204-214.

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