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Interactive comment on "Do land surface models need to include differential plant species responses to drought? Examining model predictions across a latitudinal gradient in Europe" *by* M. G. De Kauwe et al.

Anonymous Referee #1

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The focus of the paper in on modelling drought impacts on ecosystem gas exchanges, with the hypothesis that species respond differently to drought. While interesting for the LSM community, the hypothesis is somewhat trivial to ecologists, who know species behave differently in respect of drought.

The approach is to try three different parameterisations, and also three different root uptake models, and evaluate model outputs against 5 flux sites over a European drought. The core output of the paper is table 4, where a range of statistics are applied to the comparison of observation and fluxes.

I remain to be convinced of a main conclusion – that there is high drought sensitivity at northern sites. The hypothesis testing is not robust. I worry that there are a range of alternative model tweaks that could get similar improvements in the flux comparison. We really need further independent checks on model outputs using other data streams, for example local LAI data, biomass increments, soil moisture time series etc.

We know that the PFT approach is a weakness due to its one-size-fits-all approach, and therefore finer scales of parameterisation will help. The problem is to figure out how to make that happen in a tractable and robust manner, and this paper is not written in a way to tackle that problem.

The existence of a model bug is another major concern that undermines confidence.

Abstract The text is not clear about what drought response is analysed – is it C cycle, water cycle, energy balance?

Introduction:

The focus of the final paragraph is on improving CABLE too much. The text should develop knowledge of broader interest than for a single model user group.

Methods:

p. 12355 l. 10. “Optimally” needs to be defined carefully – what is optimised, over what time scale? This stomatal model is a modification of a well used empirical model (Ball Berry) and this should be stated.

p. 12356. Sensitivity of Vcmax and Jmax to predawn water potential There is not consistent evidence that these parameters are related to soil conditions as specified here. For instance, Wright et al. (2013) show that these parameters are higher or unchanged in a temperate forest growing in droughted conditions compared to well watered conditions. It is premature to construct global parameterisations on this assumption when it does not hold across all species.

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p. 12357 l. 5. Constant Jmax/Vcmax ratio is assumed Misson et al. (2006) hypothesized that Jmax is more sensitive to low water availability than Vcmax, so drought conditions may also lead to a decrease in the Jmax/Vcmax ratio. Data from Wright et al. (2013) support this hypothesis.

l. 8. Refers to eqn 6, but is this an error?

Model Simulations:

The models are run with MODIS LAI as a driver – but there is a problem in that MODIS LAI contains significant biases when used at site (flux tower) scale. Thus the LAI drivers used are unlikely to be correct, and this will lead to model biases. This issue needs to be addressed.

Why are these species (Quercus, Cedrus) chosen? Why not use the species that are found at the flux sites (Table 2)? A consistent approach would be more valuable.

Water use efficiency bug I appreciate the openness of the authors on this issue. But I remain unclear on the implications of the bug and to what degree it invalidates the conclusions of the paper. Is photosynthesis over-estimated during drought? – this would seem to indicate that the paper must only focus on water and energy responses to drought. The authors seem to suggest that root water supply is always sufficient to meet demand, and so transpiration is never down-regulated – but I am confused as I would suggest that water limitation is a definition of drought, and that water limitation must have occurred in Europe in 2003 due to high demand by plants and low rainfall.

This issue needs much more clarity if the paper is to be useful.

Results:

The opening of the results should be targeted towards key knowledge, rather than a bland listing of tables and figures.

This section needs sub-headings to provide structure. It is hard to see what has been learned. We need clear statements.

When referring to GPP, be clear whether this is flux-derived or model-derived.

P 12361. "CTRL simulation" repeated

The discussion here on the WUE bug just confused me further. I don't know what we can learn from these simulations when a bug is complicating matters so much.

There are simulations for sand and clay soils. Why not use an appropriate soil parameterisation for the site in question? This would target the analysis more effectively. At present the comparison across soil texture is confusing.

We are given three statistical outputs (RMSE, NSE, R), but the text focuses on RMSE alone, and the main conclusion re trait changes N-S is derived from RMSE. What is the point of the other stats? It seems to me they do not support the conclusions about N-S trait changes derived from RMSE.

Discussion:

4.1 This section is well written and interesting. The modelling is used to advance understanding of root zone effects on drought. But this issue needs to be better introduced in the results section.

The rest of the discussion drifts away from the experiment and the detail of the research – the focus is lost and an array of topics related to model application are raised. These do not seem pertinent to the paper. There is no final concluding paragraph to emphasise the key learnings.

Figures and Tables Table 3. Adjust column headers to add a delta term to each for clarity. Some statistics describing the variation in the deltas should be added.

Figure 1. Why not also show the 2002 data?

Figure 2. Legend not clear – explain panels a b and c.

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Fig 3-7. There is a lot of information in the figures, but it is hard to extract, so their value is not clear. Presenting a large number of time-series output of models in this unstructured manner is not really helpful. Pick which panels are important and discuss them properly.

Refs:

Misson, L., K. P. Tu, R. A. Boniello, and A. H. Goldstein. 2006. Seasonality of photosynthetic parameters in a multi-specific and vertically complex forest ecosystem in the Sierra Nevada of California. Tree Physiology 26:729-741. Wright, J., M. Williams, G. Starr, J. McGee, and R. Mitchell. 2013. Measured and modelled leaf and standâĂŘs-cale productivity across a soil moisture gradient and a severe drought. Plant, Cell & Environment 36:467-483.

Interactive comment on Biogeosciences Discuss., 12, 12349, 2015.