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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 #2

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De Kauwe and others explore drought parameterization in the CABLE model. An alternate drought formulation is found to improve modeled GPP and LE across five European flux sites in response to the 2003 drought.

The paper as written is interesting and complete but in many cases must be revised for clarity. The choice of sites is poorly described, as is the justification for the drought schemes chosen. The tendency to describe the gradient of sites as north/south rather than xeric/mesic is distracting. That being said, the results are logical with a simple and clear message that will benefit global model development. I recommend publication following (many) minor revisions.

The introduction is well-written and well-cited but could use improvement. The passage 'Our ability to model drought effect on vegetation function is currently limited' is vague. Some drought responses are simulated very well, others poorly, and the challenge remains to model drought response well, all the time. The following paragraph discusses the Galbraith results, then the Powell results, then the Galbraith results again. A good argument that PFTs are insufficient to capture the range in drought responses. It would be even better to give examples within PFTs that differ with respect to their isohydric or anisohydric behavior. In this case, might the behavior of species in a PFT average out or would all different species (or groups thereof) emerge to become important? The need to test drought parameterizations across sites is described nicely. What was not described well is the justification for the hypothesis that drought sensitivity would increase as a function of latitude. First and foremost, latitude is only ever a correlate of something else like temperature or daylength. If this justification is improved, the manuscript would be more compelling.

It may be argued that the optimal stomatal function framework falls victim to the simultaneous need for plants to not succumb to hydraulic stress (e.g. Sperry 2004). That being said, optimization theory is important to consider in models although for the case of drought it might be superseded by hydraulic considerations, which are described nicely in equations 3-5. In other words, the model as written incorporates optimal stomatal behavior and conductance, but it is able to simulate tree death?

It would be good to cite the work of Katul, Leuning, and Oren (2003) with respect to the coupling of hydraulic and photosynthetic parameters; I believe this is the original reference for this notion. (http://onlinelibrary.wiley.com/doi/10.1046/j.1365-3040.2003.00965.x/full)

Why were the three approaches on page 9 tested? Are they meant to simulate a gradient of complexity from simple to complex?

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I like the honesty of section 2.3.1. That being said, is the problem simply and conveniently avoided in this case? How is a reader to know that it does not factor into the results?

Section 2.4 could use expansion to justify the choice of the 5 sites. Why were they chosen?

The results section is succinct. Note that RMSE has units. Also, back to the question about why the three different drought parameterizations were chosen, were the first two straw men or are these common in LSMs for simulating drought?

Per the comments above regarding latitude, the first sentence of the discussion sounds more robust with mesic species exhibiting higher drought sensitivity than xeric ones for which one can assume that plants have adapted. That being said, there must be some good references for this basic concept. In the first paragraph of the discussion the authors move back to this north/south framework rather than the wet/dry framework, which is perhaps additionally surprising from a group from Australia.

In section 4.1 I wouldn't say that pot moisture is necessarily uniform but rather the relationship between active root area and the moisture profile does not match what is commonly observed in the field.

Interestingly, section 4.1 provides much of the justification for choosing the different weighting schemes that was lacking above.

Regarding the comment about plant traits and drought sensitivity at the bottom of page 18, not the TRY database?

On page 20 line 19, the 'drought-deciduous' concept could be introduced more clearly.

The following sentence could use re-working: Overall however, there remains a tendency to trade mechanistic realism is often traded for present day accuracy,

From Table 1 the sites go at least as far maritime/continental as they do 'north/south'.

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A relatively far northern site wasn't chosen. Just another reason to couch things in terms of water availability rather than latitude.

In figure 1 (and figures 3-7), how was transpiration measured?

References Sperry J.S. (2004). Coordinating stomatal and xylem functioning: an evolutionary perspective. New Phytologist, 162, 568-570.

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