Reviewer 1 (H Zheng)

This paper is representing several improvements to the coupled soil-canopy processes in the CABLE land surface model, especially a single-parameter drought response function to solve the decoupling of transpiration and photosynthesis fluxes under drying
soil conditions. These improvements are important, and the estimations of the terrestrial
carbon budgets and simulations of the ecosystem's response to drought events would greatly benefit from this work.
The paper is well-written and clearly aligned with the goals of the Geoscientific Model
Development Journal. I recommend its publication subject to some questions on the technical details.

Comment 1.1.

Eq. (7) differs from original root shut-down function of Lai and Katul (2000). In the original function, $\alpha(\theta)$ is a product of two items and its value will be 1 if the soil is saturated. However, Eq. (7) does not adhere to this feature, that does not seem reasonable. Why?

Response 1.1.

We have clarified the difference between our formulation and Lai and Katulas follows (p6 L14-19):

"Note that while the functional form of Equation (7) is taken from Lai and Katul (2000), there is not a direct equivalence of parameter values because of its different implementation here.. In particular, we use the root "shut-down" function to determine stomatal drought response via Equation (9),whereas Lai and Katul (2000) multiply it by a "maximum efficiency" function, which is in turn scaled by local root density and potential evaporation to obtain actual root water extraction."

The reviewer is right that Equation (7) doesn't equal one at saturation , but it is very close (typical values 0.95-0.97). One could rescale the function to equal one at saturation but ,after retuning gamma, this would have negligible impact on results.

Comment 1.2.

Would Eq. (8) experience a "division by zero" error? How to avoid this error?

Response 1.2

We have modified Equation (8) to account for the condition when the denominator is zero.

Comment 1.3. Eq. (4): Is the coefficient 1.1 necessary, while $\left(\theta_j - \theta_w\right)\Delta z_j$ represents the water available in the jth soil layer?

Response 1.3
Yes, the reviewer is correct. However this equation is part of the standard model configuration, and is therefore required as it is to describe the formulation of the model prior to our impriovements.

Comment 1.4.
Eq. (14): Please describe the variable csw? Also in Eq. (17) and Eq. (33).

Response 1.4
We have included this information (P9, L3):
"c_{sw} is a constant determining the rate of decrease of σ_w with depth in the canopy, with value set to 1.0."

Comment 1.5.
Eq. (28): Please describe the variable Δx1?

Response 1.5: This should be Δz_1. We have corrected the Equation.

Some specific comments:

Comment 1.6.
Page 1, Line 19: global Eddy covariance flux network ! global eddy-covariance FLUX NETwork.

Response 1.6
Done

Comment 1.7
Page 3, line 10: (2011)(CABLE1.4b) (2011) (CABLE1.4b). Missing space.

Response 1.7
Done

Comment 1.8
Page 9, line 13: (21) (21). Italic fonts.

Response 1.8
Done

Comment 1.9.
Page 9, line 17: 2:38 m s-1 or 2:38 m-1 s? Please check the unit. Also please correct the minus signs on this line.

Response 1.9

2:38 m^{-1} s: we have corrected this.

Comment 1.10
Page 10, line 22: hmin = 10-6 or _min = 10-6? Please check it.

Response 1.10
h_{min} is intended.

Comment 1.11
Page 13, line 14: 0.01-0.12 0.01–0.12. hyphen en dash.

Response 1.12
Done

Reviewer 2 (E Blyth)

Comment 2.1
This paper presents a thorough examination of the impact of various improvements to the CABLE land surface model. Covering several really interesting aspects of land surface modelling: the drought response of the vegetation through their roots, the aerodynamics of the canopy and its effect on the energy and water balance and the addition of a leaf litter layer to inhibit the evaporation from the bare soil. All of these aspects need improving - probably in many of the current land surface models - and it is really interesting to see a paper lay all these out and then check the performance against some data. I guess the only thing missing is to see the performance checked when it is run in coupled mode - but I suppose that is the task of a different paper. This one is really setting the scene and explaining the changes to the model. I think it suits the journal well and it will be of interest to many readers - both users of the CABLE model and to other modellers. It is also good to see the data used in an intelligent way.

Response 2.1
Thank-you for the positive comments.

Reviewer 3

The paper documents a solution to a known issue with the CABLE LSM, namely that CABLE simulates un-realistically high WUE (GPP/ET), even under drought conditions. This is fixed by 3 different changes to the code. The paper fits very well within the scope of GMD. The paper should be considered for publication in GMD after the following comments have been addressed:

Comment 3.1

The introduction needs a bit more clarity, especially for the non-CABLE expert. At page 2, line 25, it is stated that Haverd et al. (2013) implemented an alternative formulation for coupled drought response and root water extraction in CABLE (no version is mentioned). At line 32, it is mentioned that one of the aims of the paper is to implement the new scheme from Haverd et al. (2013) in CABLE2.0. This reads like you are repeating work already done, as you do not explain that the version of CABLE used by Haverd et al. (2013) is for BIOS2, and this is Not the version of CABLE current used in ACCESS as we speak. A non-CABLE expert will be left confused if you don't explain this a bit more.

Response 3.1

We have clarified the transfer of paramterisations from the Austtralian regional application to the global context as follows (p3 L7-10):

"In this work, we take lessons learnt from the Australian regional application (Haverd et al., 2013) and apply them globally. In particularl, we seek to resolve in CABLE 2.0 the problems of over-sensitivity of ET to drought and decoupling of transpiration and photosynthesis fluxes under drying soil conditions."

Comment 3.2

The work of Li et al. (2012) and De Kauwe et al. (2015) needs to be betterexplained and put into context of this paper. Namely, how does this current paper differ from the previous two, since these also addressed broadly the same issue in CABLE. My understanding of the work of Li et al. (2012) is that it was at a single site, and this work cannot be generalized when running CABLE as a global model, whereas your can be. I think you should make this clearer. Also, how are you building/improving on De Kauwe et al. (2015), it is not clear. The latter addressed broadly the same issue. So, how is your paper different?

Response 3.2:

We have clarified (p2 L17-18):

"The responses of gross primary production (GPP) and evapotranspiration (ET) to soil water availability in CABLE have featured in recent studies by Li et al. (2012) and De Kauwe et al. (2015a), who both considered a limited number of locations (3 and 5 respectively)."

Comment 3.3.

Provide the reference for this "data on maximum vegetation rooting depth" at page 2, line 33.

Response 3.3.

Done

Comment 3.4.

Page 3, lines 1 to 5, the second and the third aim both relate to the implementation of the SLI model in CABLE. How are these two distinct aims? This paper is documenting not one, but two major code changes to CABLE, the new drought response, as well as the SLI model. Hence, a lot more information/context should be provided about SLI in the introduction. You leave the reader with many questions and clarification is needed on all 3 stages on development, why each one is needed on its own, and why the combination of all 3 is necessary to fix this issue in CABLE.

Response 3.4.

We have clarified the rationale for including SLI in the series of model configurations as follows (P3 L14-18):
"By default, SLI includes the alternative drought response and litter effects. In contrast to the standard model configuration, it also represents coupled heat and moisture fluxes within the soil column and at the soil-air interface, and newly accounts for local stability effects on the resistance of transfer from the ground to the canopy air-space."

Comment 3.5

In the description of Canopy photosynthesis, it should be noted that CABLEv2.0 has a new Stomatal Conductance Scheme, which is an improvement on the default scheme, as documented by De Kauwe et al. (2015), Kala et al. (2015), Kala et al. (2016). Almost all future simulations within ACCESS are likely to use the new scheme, rather than the default, hence this is worth noting.

Response 3.5

This work has now been referenced in the introduction (P2 L26-28):
"Modification to the vapour-pressure deficit response of stomatal conductance in CABLE (De Kauwe et al., 2015b, Kala et et al. 2015, Kala et al. 2016) has also featured in recent studies, but it is evident that deficiencies in the predictions of seasonal cycles of evaporation are not resolved by this modification (De Kauwe et al., 2015b; Fig 3) "

Comment 3.6

Equations 16 and 17, it is simply stated that a different integration of Eq. 13 is used, as compared to the default, without any explanation(s) and leaves the reader wondering.

Response 3.6

We have expanded the text and equations as follows (p8 ; L15 forward)
"The default model uses an approximation to the integral in Equation 13, which assumes a fixed value of σ_w with height over the range of interest:

$$r_{soil} \simeq \frac{1}{\overline{\sigma_w^2}} \int_{z0s}^{d} \frac{1}{\tau_L}$$

$$= \ln\left\{\frac{d}{z_{0S}}\right\} \frac{\exp\{2c_{sw}L\} - \exp\left\{2c_{sw}L\left(1-\frac{d}{h}\right)\right\}}{a_3^{\,2} c_{TL} 2c_{sw} L} \quad (1)$$

where

$$\overline{\sigma_w^2} = \frac{1}{d}\int_0^d \sigma_w^2 dz \quad (2)$$

as used by Raupach et al. (1997) and subsequently propagated to CABLE (Wang et al., 2011, Eq A.14). However the analytic form of the integral is (Haverd et al., 2013):

$$r_{soil} = \frac{1}{u_*} \ln\left\{\frac{d}{z_{0s}}\right\} \frac{\exp\{2c_{s,w}L\}\left(\frac{d}{h}\right)}{a_3^2 c_{TL}} \quad (3)$$

and results in higher values of r_{soil}."

Comment 3.7

Page 9, lines 5 to 10 – Clitt parameter values are obtained by separate offline spin-up using GSWP2-3 forcing. Firstly, which one did you use? GSWP2 or GSWP3? Or both? If both, then did you take the average from the two? Did you run CABLE offline globally with GSWP2/3, then take the average over all PFTs? Or did you extract single site forcing from GSWP and run single-site offline simulations? Secondly, these parameters are therefore model dependant, namely CASA-CNP, rather than have any direct link to observations. This is not discussed at all. This is parameter tuning, and you need to make this explicit and flag the implications.

Response 3.7

We have clarified this (P10, L6-12):

"These were obtained by running the model for 18 FLUXNET sites (Table 2) with biogeochemistry enabled (carbon-cycle only: nitrogen- and phosphorous-cycles were disabled) using repeated GSWP-2 three-hourly meteorology for the 1986-1995 period (Dirmeyer et al., 2006) until carbon pool convergence was achieved. Values of C_{litt} used here are internally consistent with the carbon-cycle enabled version of CABLE. They don't reflect observation directly and were extrapolated to PFT-specific parameter values for the purpose of simulations (such as those presented here) which don't include the carbon-cycle. However, for simulations with carbon-cycle enabled, we recommend the use of internal litter carbon pools instead."

Comment 3.8

This paper has 35 equations in total within the main text, and the reader feels rather dazzled after going through all 35! I strongly recommend moving some of these to an Appendix, and focus only on the relevant equations.

Resposne 3.8

We consider all the equations presented here to be relevant and feel it appropriate to retain them in the body of the text, particularly since this is a model development paper.

Comment 3.9

A map showing the locations of the 18 sites would be good.

Response 3.9

We feel that the location co-ordinates in Table 2 are sufficient.

Comment 3.10

Page 13, lines 12-15, the tuning of the parameter, gamma, is suddenly introduced. This parameter is used in Eq. 7, which is from Lai and Katul (2000). There is no discussion if this parameter value of 0.03 obtained from tuning is different to the value used by Lai and Katul (2000) or any other study? This is just presented without any context and leaves the reader wondering.

Response 3.10

Where we present the drought-response function, we now include the following qualification (p6 L14-19):

"Note that while the functional form of Equation 7 is taken from Lai and Katul (2000), there is not a direct equivalence of parameter values because of its different implementation here. In particular, we use the root "shut-down" function to determine stomatal drought response via Equation (9),whereas Lai and Katul (2000) multiply it by a "maximum efficiency" function, which is in turn scaled by local root density and potential evaporation to obtain actual root water extraction."

We now highlight that this is the only tunable parameter in the new drought response formulation, and compare with the value derived for Australian vegetation (p17 L3-8):

"We explored a range of values (0.01–0.12) for the parameter γ, which determines the steepness of the root shut-down function of Lai and Katul (2000)(Equation 7), and is the single tunable parameter in the new drought response function (Equations 7-9).

Across the 18 FLUXNET sites, a value of γ= 0.03 gave the best results for the SLI model configuration., slightly higher than the low value of γ= 0.01 (reflecting high drought-tolerance) for Australian vegetation (Haverd et al., 2013)"

Comment 3.11

Additionally, there needs to be a discussion about the parameter tuning carried out in this study (offline only) and what the implications would be for coupled (ACCESS) simulations. Would one simply used the same parameter values for coupled simulations?

Response 3.11

As state above (Response 3.10), only one new tunable parameter was introduced, and optimized. The focus of this paper is offline simulations and it is out of scope to explore the transferability of parameters to coupled simulations.

Comment 3.12.

Figure 1 – This is no explanation of how the black circles differ from the grey ones? It seems to me that the main improvement is in latent heat, very little in GPP, so the main improvement in WUE is due to latent. The simulation of latent heat is largely improved. This is a great achievement.

Response 3.12

The following text has been added to the caption of Figure 1: "Darker shading indicates higher density of points "

Comment 3.13

Table 3 – The bias (model – obs) should be added.

Response 3.13

Bias Error is now included in Table 3.

Comment 3.14

Page 16, line 4, by "contrasts", you mean contradictory? If yes, then some more in-depth discussion of why would seem appropriate.

Response 3.14

We have clarified the different findings as follows (p17 L9-13):

"Further, the same was true when the data-set was reduced to the drought-affected European sites (Tharandt, Hesse, Castelporziano, Roccarespampani, Espirra) during 2003, as selected by De Kauwe et al. (2015a). In this respect, our results do not confirm the finding of De Kauwe et al. (2015a) that parameters representing high drought sensitivity at the most mesic sites, and low drought sensitivity at the most xeric sites, are necessary to accurately model responses during drought. "

Comment 3.15

I was rather surprised that the authors did not conduct or show any results from Global offline simulations using GSWP2 or GSWP3, especially, given that they used GSWP2/3 to tune some parameters. To better inform the use of these modifications in CABLE when coupled to ACCESS, global offline simulations are extremely valuable, and would make a very useful addition to this paper (rather short with only 3 figures). Additionally, other studies which have tested new developments to CABLE have used both single site and global offline GSWP simulations (De Kauwe et al. (2015) and evaluated CABLE's ET against gridded observational products such as LandFlux data. This study should present some global offline results using GSWP2 or GSWP3.

Response 3.15

This would be a significant extension (details below) , which we will consider in due course, but consider to be out of scope for the current paper . Global simulations on their own would produce little advance in understanding and risk confusing the reader.

Reasons for global benchmarking being a significant extension:

1. Benchmarking/comparing the global offline simulations against global products isn't justification for advance (or not) of an LSM. Two reasons – both stem from the fact that global products are exactly that "products" of another model.

    (a): using the global products to fine tune an LSM assumes that the underpinning models are in some way congruous (i.e. outputs are exactly equivalent)

    (b) biases and errors in response in the global products get transferred into the LSM. These can come from inherent weaknesses in the global products (i.e. missing processes), the parameters used within the model deriving the global products and, importantly, the structure of the model itself.

    Consequently while comparison against global products is useful such studies have to be done with a full and careful analysis of how (if) the LSM and global product can be compared.

2. Any comparison between model-model or model-observation should be expressed within the uncertainties (error bounds) of the two sets of data. While we (may) have a handle on these issues for the means we do not have the equivalent knowledge for the extremes. For example, on the basis of using a global product could we actually definitively show that the new drought response parameterisation improves CABLE's performance?

3. As the reviewer him/herself points out this work involves 3 quasi-independent advances – each of which leads to impacts on least 3 time scales (diurnal, seasonal, interannual). Consequently any extension of the work to global offline simulations would need to cover this breakdown.

Comment 3.16

This study makes no mention of the fact that CABLEv2.0 now has a new, improved and more physically realistic hydrology parameterization, as described in detail by Decker et al. (2015). The new hydrology makes significant improvements to CABLE excessive ET. Whilst it is well outside the scope of this paper to test the current modifications with the new hydrology by Decker et al. (2015), this must be explicitly discussed as critical future work which needs to be carried out.

Response 3.16

We now reference Decker (2015) in the introduction (P2, L28-31):
”Recently Decker (2015) introduced to CABLE new conceptual parameterizations of subgrid-scale soil moisture, runoff generation, and groundwater, and showed improved performance against observation-based estimates of global ET, without modifying CABLE’s vegetation response to soil moisture.“

and in the conclusion (P18 L26-27):

“Future work will entail merging the improvements demonstrated here with the new hydrological parameterisations in CABLE (Decker 2015), and testing against global estimates of ET and runoff.”