Review of "Improved representations of coupled soil-canopy processes in the CABLE land surface mode" by Haverd et al.

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:

Major comments:

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.
2. The work of Li et al. (2012) and De Kauwe et al. (2015) needs to be better explained 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?
3. Provide the reference for this "data on maximum vegetation rooting depth" at page 2, line 33.
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.
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.

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.
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.
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.
9. A map showing the locations of the 18 sites would be good.
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.
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?
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.
13. Table 3 – The bias (model – obs) should be added.
14. Page 16, line 4, by "contrasts", you mean contradictory? If yes, then some more in-depth discussion of why would seem appropriate.
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.
16. This study make 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.

Decker, M. (2015), Development and evaluation of a new soil moisture and runoff parameterization for the CABLE LSM including subgrid-scale processes, J. Adv. Model. Earth Syst., 7, 1788–1809, doi:10.1002/2015MS000507.

De Kauwe, M. G., Kala, J., Lin, Y.-S., Pitman, A. J., Medlyn, B. E., Duursma, R. A., Abramowitz, G., Wang Y.-P., and Miralles D. G. (2015) A test of an optimal stomatal conductance scheme within the CABLE Land Surface Model. *Geoscientific Model Development*, 8, 431-452, doi: 10.5194/gmd-8-431-2015

Kala, J., M. G. De Kauwe, A. J. Pitman, R. Lorenz, B. E. Medlyn, Y.-P Wang, Y.-S Lin, and G. Abramowitz (2015) Implementation of an optimal stomatal conductance scheme in the Australian Community Climate Earth Systems Simulator (ACCESS1.3b). *Geoscientific Model Development.*, 8, 3877-3889, doi:10.5194/gmd-8-3877-2015

Kala, J., M. G. De Kauwe, A. J. Pitman, R. Lorenz, B. E. Medlyn, Y. P. Wang, and S. E. Perkins-Kirkpatrick (2016) Impact of the representation of stomatal conductance on model projections of heatwave intensity. *Scientific Reports,* 6, 23418, doi:10.1038/srep23418