CABLE is the land surface scheme in the ACCESS earth system model. This is stated by the authors themselves in the Introduction. By definition, it is critical that advancements to the CABLE model be applicable at the global scale, both offline and coupled. The logical step in model development is to go from single site offline, to global offline, to fully coupled. In response to comment 3.11, the authors state that: "it is out of scope to explore the transferability of parameters to coupled simulations". I am Not asking you to carry out coupled simulations, I am saying that you need to discuss this explicitly in the paper, rather than just in the reply. Additionally, the title should also be changed to "Improved representations of …….. CABLE land surface model in offline single site simulations".

I also would like to remind the authors that GMD policy requires model version numbers in the title:
"The main paper must give the model name and version number (or other unique identifier) in the title". For more details, please see:
http://www.geoscientific-model-development.net/about/manuscript_types.html

The response to comment 3.2 has not really answered my question. The only difference between this work and that of Li et al. (2015) and De Kauwe et al. (2015) is surely not just that the latter used 3 and 5 sites respectively. I was referring to how the approach taken by these papers is different, not just that they used fewer sites.

The response to comment 3.4 is also only partial. I asked for clarification on the 3 stages of development, but all you have done is provide some more background information on SLI.

Response 3.11 – I am NOT arguing with you that it is out of scope of this paper to include coupled simulations. I am saying that you need to make it explicitly clear in the manuscript that the introduction of this tuneable parameter(s) poses a limitation in using your modified version of CABLE in coupled simulations within ACCESS.

Response 3.15:

1. I completely disagree that according to the authors that benchmarking against global observationally derived products isn't justification for the advancement or not of an LSM. Global ET products such as LandFlux EVAL (http://www.iac.ethz.ch/group/land-climate-dynamics/research/landflux-eval.html) are an invaluable dataset which one can use to determine how the LSM compares to best available Remote Sensing estimates, as well as other LSMs. For example, Decker et al. (2015) used LandFlux data, amongst others, to benchmark CABLE ET, and showed significant improvements in CABLE ET compared to best available estimates, taking into consideration the uncertainties within these best available estimates. This is just one example of how several papers have made correct use of such products in quantifying the advancement of LSMs.

    a. I never asked the authors to fine-tune CABLE to match global products. This would be pointless. Benchmarking does not mean fine-tuning a model to observations.
    b. The authors claim that "Biases and errors in response in the global products gets transferred to the LSM". Sorry, but this makes no sense to me.
2. Yes, of course any comparison between model-model and model-observation should be expressed within the error bounds of the two datasets. That's Exactly why products such as LandFlux provide error bounds. Not having observationally based data on extremes, does not preclude you from using observationally based products all together. Your paper examines monthly means anyway as shown in figures 1 and 2. You paper does not focus on drought periods, so what point is being made here?
3. Yes, of course it would, and there is plenty of space in this paper to cover this analysis. You paper currently only includes 3 figures.

The reasons given as to why global offline simulations would produce little advancement are not valid in my opinion, as explained above, and I simply don't see how this would confuse the reader.