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Interactive comment on "A test of an optimal stomatal conductance scheme within the CABLE Land Surface Model" *by* M. G. De Kauwe et al.

Anonymous Referee #2

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The analysis by De Kauwe et al. highlights an important shortcoming, the parameterization of g1, in the stomatal conductance model that is often used in land surface models (LSMs). Particularly, the g1 parameter in stomatal conductance models typically only varies by photosynthetic pathway (C3 or C4), though it is likely to change based plant-specific parameters. De Kauwe et al. implement a new stomatal conductance model based on work by Medlyn et al. (2011), and then adjust the g1 parameter, which in this new model is defined as the marginal carbon cost of water use. However, it is not clear that this new conductance model makes any improvement to predictions of carbon and water fluxes compared to the current parameterization. Though the lack of improvement does not preclude the manuscript from publication, the authors need to be cautious in framing the conclusions so as not to claim that this new model is "better".

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De Kauwe et al. insist that this new stomatal conductance scheme improves the representation of stomatal conductance in models because it is an optimized analytical, rather than an empirical, solution. Yet it is based on empirical data, so it does not seem that this model is entirely different from the empirical solution of the commonly used Ball-Berry-Leuning (BBL) model. The authors do not test adjusting the g1 parameter in the BBL that they replace, and would perhaps argue that the g1 parameter in the BBL model, described as the slope of the conductance-photosynthesis relationship of the plant, cannot be estimated from empirical data. However, it is not clear to me that these two g1 parameters have entirely different functions, and the new g1 parameter is also not directly measured, but estimated, from empirical data. What would the results be if the authors simply adjusted the g1 parameter in the BBL model based on the data? Perhaps adjusting g1 for different plant types in the BBL model would be more broadly applicable to other LSMs than entirely changing the stomatal conductance parameterization.

The paper includes important points and deserves to be published after some revisions. Many paragraphs throughout lack focus (each paragraph should have a topic sentence and the remaining sentences should support that topic), and need to be revised to remove extraneous information. The methods section in particular needs clarification. Specifically, this section should include a subsection to pull out the g1 dataset development for each MED parameterization. Within this sub-section, the authors need to explicitly state how g1 was derived from the Lin et al. (2014) data. Which data did they use (and how?) to estimate g1? Additionally, the results & discussion should be removed from the methods. Last, it is not clear why the authors did not use the same g0 values for both the LEU and the MED simulations to make comparisons easier. As it stands, the authors must highlight this difference to explain some of their results. While they state that its best to develop g1 without changing g0, that is precisely what they did when they set g0 to 0.

These comments, in addition to other specific comments and technical corrections,

are included as notes throughout the text in the attached document.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/7/C2545/2014/gmdd-7-C2545-2014-supplement.pdf

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