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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 #1

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This paper compares various stomatal schemes within the CABLE land surface model and undertakes an evaluation at site and global scale. The goal of this paper is to compare a well known empirical model of stomatal conductance with a slightly modified version. The original model is the Ball-Berry-Leuning model, and has global parameters from unknown derivation. The novel model is from Medlyn et al, which is presented with some different parameterisations. In both models conductance is a function of the same soil moisture stress function, VPD, CO2 concentration and gross assimilation rate. The key change is that one of the model parameters, g1, is now proportional to a marginal carbon cost of water, and varies with climate. The gs models also have different sensitivity to VPD.

The conclusion of this paper is "This work paves the way for broader implementations

of optimisation theory in LSMs and other large-scale vegetation models ". I remain unconvinced for a number of reasons –

1. Model testing needs more effort. Testing at flux sites is limited to comparison with LE data. Comparison with GPP and H should also be included, to show that C-water interactions are effectively coupled and build trust in the model(s). Detailed statistics are required, and discussion about model validity. I have more detailed comments below in the section on single site results.

2. Optimal is arguable. I am unclear why a gs model that is described as "optimal" needs to be calibrated with empirical gs data. My understanding of an optimisation model is that it predicts optimal behaviour (gs), without direct calibration, and is then verified against independent data (of gs). If a model has to be calibrated, it can't really be revealing some fundamental biological property. Can the authors clarify what they mean by 'optimal', and perhaps be more cautious in their claims in this regard?

3. Global evaluation lacks conviction. The biome differences between the models are regarded as significant – for instance a 30% reduction in evapotranspiration for evergreen needle-leaf forests and tundra PFTs. These differences are highlighted in figures 6 and 7. But the latitudinal outputs of all the models are similar in figure 8. These results seem contradictory. By their own admission the impacts on the model outputs of the new scheme are negligible. The outstanding mismatches in the GPP signal (Fig. 8) are not solved by the new scheme. So I am left to conclude – why not just stick with the BB model, and its basic parameters? The new approach has not moved the modelling forward, even after a lot of extra effort.

So, I am not led towards the authors' conclusions – which seem to lack foundations in the content of the paper. The structure of the paper is also problematical, with results and methods inter-mixed, key details left out, and long paragraphs that lack clear topics. The paper needs to build on the model outputs, model-data comparisons, towards clear and universal conclusions.

More detailed comments:

Single site results We are pointed towards figures and tables recording the results of this model experiment, but the text is not helpful in guiding the reader towards the critical outputs. My inspection of table 5 shows a mixed set of results for each model and no clear patterns. What has this exercise shown us?

Figure 3: Why are observed GPP and H not added to the panels? It would help to have a clear evaluation of the model-data mismatch across all sites and variables.

Fig. 4. I am confused why MED-P shows a pronounced dip in mid-day E, but not in LE for DJF. Is this an evaporation/transpiration issue? This figure is only referenced once in the paper, with little detail provided in section 3.1. Surely more focus in required, and this figure should be referenced from the discussion.

p. 6858 l.20. Is there any confirmation that this boundary layer hypothesis is correct? Can the values of boundary layer conductance be provided in support? Are these values defensible?

Global results I would suggest a restructuring of this section. We are presented with many tables and figures, but without topic sentences to highlight the critical results. It would help the reader to have the salient points of this comparison presented step by step, with reference to specific figures and tables to provide direct support. Currently the reader is referred to 3 figures and 2 tables in first few lines, without guidance as to the key points.

I notice that MED-L differs by seemingly similar magnitudes to MED-P and MED-C from the LEU model – it would be useful to have a statistical analysis presented in the text (i.e. Mean % differences in each case). It is helpful that errors are provided for the mean PFT analyses with each model (although exactly what these errors are is not explained in the table captions). I would like more discussion of what these errors mean and how they affect the interpretation. For instance, if the errors are larger than

the differences, I suspect we assume there is no significant difference. If this approach were used, it would be possible to highlight in the tables which differences we should take notice of as important.

I also wonder why the error on deciduous needleleaf is the smallest in table 6, and yet this PFT lacked calibration data, so one would expect a large error.

Despite the authors' assertion in section 4.1, GPP and ET are not principally controlled by gs (and climate) – soil moisture and LAI are other (and often more) important variables in reality (and in LSMs). The short term response to a change in climate (e.g. more drought) may be an adjustment in gs, but the long term response is an adjustment in LAI. Do these variables (LAI, soil moisture) differ among any of the simulations with the various calibrations? i.e. We need to know whether it is just gs variation that is generating the model differences.... [you do confirm prescribed LAI later in the discussion I see, but this really needs to be set out in the Methods].

It is also important to register that model-data mismatches for GPP/ET may be significantly affected by LAI and soil moisture uncertainties, i.e. better gs predictions may not be the answer to a perceived problem. I would like the authors to discuss this issue.

The paragraphs in the discussion are long and hard to follow. Paragraphs have multiple topics, switching from ET to GPP without warning. Please restructure, improve the topic sentences, and refer back to figures and tables consistently. Also, I get lost among the model calibrations – when the test reports a parameter value was "used in CABLE", which model run is being referred to?

p. 6861. The discussion on boreal forests lead to a warning about ET modelling, but I don't understand it. The recalibrated g1 parameter in the MED model reduced boreal ET. I suspect this tells us something specific about the CABLE model, rather than some general result about gs in boreal regions. ET in boreal latitudes is a complex outcome of moss, understorey and forest canopy interactions with snow and permafrost. Are these processes included in CABLE?

Section 4.2 on the g1 parameter seems to reprise the results of previous papers. There are no references to figures or tables produced in this paper. I would suggest this entire section be removed, or significantly rewritten to link to the current work.

Section 4.3 What is the relevance of the first paragraph? Some potential values of g0 are mentioned, but there is no conclusion.

Section 4.4. It is good to read here about other components of the land-atmosphere exchange pathway in CABLE, and issues with boundary layer conductance. I would expect that comparison with eddy flux data, including LE, H and Rnet would allow testing of these problems. In fact I expected this would be the role of figures 3 and 4, although the paper pays little attention to these figures and the model-data mismatch. It is good also to read here about compensatory effects that can minimise the role of gs on ET. Can the authors

Bonan et al (2014) found improved simulation with their optimal scheme during drought periods – that should be noted here.

p. 6846, l. 26: vegetated surface evapotranspire – not all losses are through stomata.

p. 6847, l. 19. There are examples of more mechanistic stomatal models that have been widely tested, e.g. SPA model of Williams et al. Please make this clear.

p. 6848, l. 5. I can't find any reference in the Bonan et al. paper that their calculations are "highly computationally expensive".

p. 6849, l.. 17. "excessive evaporation". This would suggest the problem with CABLE relates to soil or wet leaf evaporation, and not the stomatal modelling of transpiration.

p. 6866, l.5. It is not correct that numerical solutions behave incorrectly, compared to analytical solutions, for simulating optimal stomatal responses to increase CO2.

p. 6853, l. 11. Do you mean you fitted eq 7?

p. 6853. I am confused as to why results are presented in the Methods section.

References Please check the reference list, there are several citations that are missing.

Table 6 and 7 captions. Please explain what the +/- means.

Interactive comment on Geosci. Model Dev. Discuss., 7, 6845, 2014.