We thank the reviewer for their positive and constructive comments and we address the various concerns below. Referee comments are highlighted in red, with our response below in each case.

Following the helpful reviewer suggestions we have made a significant effort at reshaping the manuscript to aid its interpretability along the suggested lines. In particular we have:

I. Clarified what we mean by “optimal” behaviour and clarified what the MED-C simulations add.
II. Restructured the entire results and discussion sections, adding new sub-sections.
III. We have added two additional figures: figure 5 to deal with the issue of boundary layer conductance and figure S2 to deal with the issue of our g_0 assumption.
IV. Removed results information previously presented in the method section (which involved reordering figures).
V. More clearly addressed the question of why one would want to move away from an empirical Ball-Berry approach in the discussion.
VI. In figure 9 we have now masked missing data area in the data products, for example over the Sahara desert. These areas are not masked in CABLE by default, but have very small fluxes and as such, the previous figure 9 showed an erroneous bias between model and data which related to missing data.

In combination we hope this redrafting will help more clearly define how we have advanced modelling with this manuscript.

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".

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 all-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 reviewer is correct that an alternative approach would be to use the stomatal database to derive slope parameters for the BBL model. However we will leave such a direction for other researchers. The rationale for our approach is that the new stomatal conductance scheme has parameters that can be interpreted as the "cost of water to the plant", rather than just an empirical value. In this paper we demonstrate the "added value" of our approach, by not only calibrating the g_1 parameter, but also predicting it based on two bioclimatic indices (temperature and aridity) (the MED-C simulations). We suggest in the discussion ways in which the role of the g_1 parameter could be extended within models (lines 506-536), for example it may be possible to predict g_1 from modelled wood density. There is also the potential to hypothesise how the g_1 parameter would behave under water stress (lines 526-536) and this potentially opens up new avenues in land surface modelling (see discussion and Zhou et al. 2013;

2014). These new approaches would not be possible if we had not used the new stomatal conductance scheme. Finally, the new scheme is functionally similar to the empirical one, is trivial to implement, adds no additional parameters or computational costs and therefore is particularly suited to global models.

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 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.

We have significantly revised the structuring of both the results and discussion text to aid the reader. We have added sub-headings and broken the text up as suggested by the reviewer. We have also amended the methods sections adding the requested details and removing the results text 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.

To clarify, we did use the same g_0 value in the MED-L simulations as was used in the original CABLE LEU simulations. To more clearly tackle the issues relating to g0 we have added a new section to the discussion (4.4) and a new supplementary figure S2, which supports the choices made in this paper.

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

We have addressed the various comments highlighted in the annotated document in our revised text. In particular, we draw the reviewer's attention to our new text about the use of night-time g_0 values in models. This text addresses the reviewer's suggestion about the use of night-time data for example from the study by Caird et al.