Authors reply to: Interactive comment on "Modeling stomatal conductance in the Earth system: linking leaf water-use efficiency and water transport along the soil-plant-atmosphere continuum" posted by M. De Kauwe

We thank Martin for his informative and constructive comments, which have helped improve the manuscript.

1. The authors don't appear to acknowledge that detlaAn/deltaEl is essentially Cowan and Farquhar. Given this, I am struggling a little with figure 6 and the lack of text afforded this key figure. The Medlyn model is derived from Cowan and Farquhar and so my expectation is that detlaAn/deltaEl would be more similar to this model, however there is clear ordering to the scatter. Do the authors have any thoughts as to why this might be? One suggestion I would have is to ask them what they fit the g1 parameter to, i.e. what range of VPD? I see that the figure caption says 0-2.6 kPa, but is this actually what the model parameter was fit against? Alternatively how is moisture stress accounted for in this plot, is it excluded, apologies if this was made clear but I have missed it.

Response: (a) We do not mean to imply that our dA_n/dE_l optimization is anything but the Cowan-Farquhar optimization. We discuss the Cowan-Farquhar water-use efficiency optimization in the introduction and assumed that the details are commonly known. To clarify that we have indeed implemented a numerical version of this optimization we re-write this text (new text in italics): "This theory assumes that the physiology of stomata has evolved to constrain the rate of water loss (E_l) for a given unit of carbon gain (A_n) (Cowan, 1977; Cowan and Farquhar, 1977). This optimization can be achieved by assuming that g_s varies to maintain water-use efficiency constant over some time period (*formally this means that $\partial A_n/\partial E_l = constant$; note that Cowan (1977) and Cowan and Farquhar (1977) discussed optimization in the context of the marginal water cost of carbon gain so that $\partial E_l/\partial A_n = constant$).*" Additionally, where we contrast the SPA dA_n/dg_s optimization with the dA_n/dE_l optimization we re-write the text to read: "*We additionally tested the Cowan (1977) and Cowan and Farquhar (1977) water-use efficiency optimization (WUE; $\Delta A_n/\Delta E_l$, the marginal carbon gain of water loss)*"

(b) With regard to Figure 6: Our intent with this figure was merely to show that the numerical stomatal optimization can reproduce the Ball-Berry and Medlyn stomatal models. We do not believe that the figure should be interpreted to say that the Ball-Berry model (with h_s) is better than the Medlyn model (with $1/sqrt(D_s)$). Many environmental factors (air temperature, relative humidity, solar radiation, wind speed) varied simultaneously in the data presented in the figure. This does not provide the best basis for comparison (a controlled experimental protocol would likely be more informative). We delete this figure to avoid confusion and to reduce the manuscript length. Instead, we present in the text the correlation coefficient (r), the slope of the regression, and statistical significance.

2. Following on from the above, a question that I feel should be explored in the discussion is "how much of an improvement in model skill makes such an implementation justified"? This is somewhat provocative, but I think it might be worth tackling. I feel figures such as 11 are a little bit of a straw man, though I understand why they exist and don't have a major issue with the point being made. But the authors are advocating an iterative optimisation framework should be

inserted into a detailed land surface model. How computational expensive is this likely to be? Is the improvement in model skill justified by the expense? Given that models derived from Cowan and Farquhar exist and are by their nature similar to this approach, what is the trade off in not using them? The authors make the valid point that understanding how these simpler models operate with moisture stress and VPD are difficult, but is this alternative approach really a step forward? Certainly work exists to show how such relationships could be derived (see Zhou et al. 2013, AFM).

Response: Referee Duursma and referee De Kauwe both make a similar point with reference to analytical stomatal conductance models derived from water-use efficiency optimization theory, particularly the Medlyn et al stomatal model (Duursma was second author on that manuscript and De Kauwe has authored subsequent studies with Medlyn using the model). In these comments they ask us to justify why we do not use their stomatal conductance model. Our intent was (and still is) to compare the CLM approach with the SPA approach, not to evaluate the merits of the Medlyn stomatal conductance model. A key feature of the SPA stomatal conductance parameterization is the soil moisture control of stomatal conductance. This, not the details of numerical (SPA) or analytical (Medlyn et al.) water-use efficiency optimization, is important for the CLM and SPA comparisons.

Since the referees raise the issue, it is worth pointing out that the Medlyn model is derived from the Farquhar photosynthesis model based on water-use efficiency optimization, but only for RuBP-limited (light-limited) assimilation. They argue that this is appropriate, because much of the canopy is light-limited (see also Medlyn et al. 2013; Agric. Forest Meteorol., 182/183, 200-203). More complex stomatal conductance models are obtained for Rubisco-limited assimilation (Katul et al. 2010; Annals of Botany, 105, 431-442) or co-limited assimilation (Vico et al. 2013; Agric. Forest Meteorol., 182/183, 191-199), and there is some sharp disagreement on the merits of these various stomatal conductance models. Our intent is not to enter this debate, but we clarify our statements with respect to closed-form variants of the Ball-Berry model. We add the text: "*Variants of the model can be derived from the Farquhar et al. (1980) photosynthesis model based on water-use efficiency optimization, after some simplifying assumptions, but the form and complexity of the model varies among Rubisco-limited (Katul et al., 2010), light-limited (Medlyn et al., 2011b), or co-limited (Vico et al., 2013) rates.*"

A second key point is that the numerical solution optimizes photosynthetic carbon gain per unit water loss while also preventing leaf water potential dropping below a critical minimum level. The model is therefore an optimality model with two distinct criteria (water-use efficiency and hydraulic safety) as opposed to implementations of the Cowan & Farquhar concept, which only consider water-use efficiency. It is this latter control of stomatal conductance by leaf water potential that is critical to the numerical optimization. Other approaches use a Ball-Berry style conductance, empirically modified for soil water. Our approach allows us to calculate the soil moisture stress directly from physiological principles.

As for the added computational cost, that is very subjective and varies with computational platform and software engineering. At NCAR, the run time for CLM is less than 5% that of the atmosphere model. Part of the reviewer's concerns over computational costs may have arisen from our description of the stomatal optimization routine, which was described as numerically

incrementing stomatal conductance until the optimization is achieved. This gives the impression that the algorithm increments g_s by a fixed amount at each iteration. In fact, numerical techniques (Brent's method) can efficiently find the optimal g_s. We re-write the text and change Fig 2 to clarify this.

We add text to the discussion to address the computational cost:

"*Our approach, as in SPA, numerically optimizes photosynthetic carbon gain per unit water loss while also avoiding desiccation by preventing low leaf water potential. Alternatively, Ball–Berry style stomatal conductance models provide an analytical equation for stomatal functioning and can be combined with an empirical dependence on soil moisture or leaf water potential (Tuzet et al., 2003; Duursma and Medlyn, 2012; Zhou et al., 2013). However, the greater computational cost (and also the benefit) of the model presented here, relative to CLM4.5, is not the numerics of the stomatal optimization but rather resolving gradients within the canopy. Bonan et al. (2012) showed that inexactness in the absorption of diffuse radiation by shaded leaves leads to errors in GPP for a sunlit/shaded big-leaf canopy model relative to a multi-layer canopy model. This error can be decreased with high values for the nitrogen decay coefficient (K_n), but such values are inconsistent with field estimates (Lloyd et al., 2010). A similar inexactness arises due to gradients of leaf water potential within the canopy. One of the outcomes of the SPA stomatal optimization is that leaves in the upper canopy, with high solar radiation and high transpiration rates, close their stomata to avoid desiccation. Non-linear gradients of light, nitrogen, and leaf water potential must be accounted for when formulating theories of canopy optimization (Peltoniemi et al., 2012). Just as multi-layer profiles of soil carbon are being recognized as important for carbon cycle–climate feedbacks (Koven et al., 2013), profiles in the plant canopy may similarly be important for vegetation–atmosphere coupling. Here, we resolve the canopy leaf area profile at high resolution (increments of 0.1 m^2 m^{-2}). Other SPA simulations successfully divide the canopy into fewer layers (e.g., 10 layers with a leaf area index of 3.5 m^2 m^{-2}, Williams et al., 1996).*"

3. Furthermore, if you look at figure 13/15, I could envisage it might be 'cheaper' to implement a alternative moisture stress scalar on the Ball-Berry model, or additionally adjusting the slope of stomatal conductance model and I would suggest this would arrive at a better model-data match from the Ball Berry model, perhaps questioning the necessity for an iterative optimisation scheme?

Response: Yes, this is exactly the point of these figures. With less soil water stress, the CLM Ball-Berry model works fine. Note that the original submission states: "In our simulations, higher β_t improves the Ball-Berry model" in the presentation of the US-Me2 simulations. We reiterate this point in the discussion: "*In our simulations, higher β_t (less soil moisture stress) improves the CLM-BB model (Figure 12), suggesting that the parameterization of soil moisture stress, not the stomatal model per se, is erroneous.*" However, we also note that our approach calculates the soil moisture stress directly from physiological theory whereas soil moisture stress scalars applied to the Ball-Berry model are by nature empirical.

4. 20 figures feels excessive and in my opinion makes the story of the manuscript hard to follow. Often very little text ends up being dedicated to figure discussion. For example, what is the (fig

10) Taylor diagram actually meant to show? The figure caption offers little detail on how to interpret such a diagram. Do we really need net radiation on the figures when assessing model 'improvement'? Does it add anything? Is Figure 20 necessary? Figure 13 and 15 seems excessively detailed (number of panels). I could go on, I think many of these figures could comprise a supplementary section as they currently detract from the message the authors wish to express.

Response: We delete two figures from the original submission (Figs. 6 and 9). We add text to the discussion of the Taylor diagrams to help readers understand the plots. Net radiation is important to include when comparing model simulations with flux tower observations, because net radiation constrains sensible and latent heat flux. It is necessary to show that the model can reasonably simulate observed net radiation.

We agree that 20 figures (reduced to 18) is large. However, we note that the reviewer stated that: "I particularly like the ways the authors have separated estimates at the leaf and canopy scale and the way they have tackled explaining the differences." Referee Remko Duursma similarly endorsed the leaf- and canopy-scale evaluation with the comment: "The simulations are separated by leaf-scale and canopy-scale, which I find very useful to help understand the differences between models (rather than just showing the full model behaviour as a black box, which is still too common)." In revisions, we have: 3 figures (Figs. 1-3) describing the model formulation; 3 figures documenting leaf-scale simulations (Figs. 4-6); 8 figures (Figs. 7-14) for canopy simulations; and 4 figures (Figs. 15-18) documenting model sensitivity. These last 4 figures are necessary to address parameter sensitivity and why the model gets the correct results.

5. Minor - The literature review appears to miss a key text when discussing the debate over what stomata respond to: Mott '88. And generally the text seemingly skates over many other important works in this area.

Response: We purposely do not provide a thorough review of stomatal physiology and stomatal models. Our intent with this manuscript is merely to compare the Ball-Berry model (which is commonly used in land surface models) with the SPA numerical optimization model (which has not been widely used). We re-write the introduction to make this intent more obvious.

6. The authors should dedicate more text to what they mean by optimisation, they describe in terms of a 'model time step'. How long is the model time step they are referring to (presumably 30 minutes)? Are the stomata always behaving optimally? Or do they generally behave optimally over the course of day?

Response: We re-write the text (new text in italics): "Stomatal conductance is *numerically solved* at each *model* time step (*30–60 minutes depending on frequency of flux tower data*) such that further opening does not yield a sufficient carbon gain per unit water loss (defined by a stomatal efficiency parameter ι, μmol CO_2 mol^{-1} H_2O) or further opening causes leaf water potential (ψ_l) to decrease below a minimum value (ψ_{lmin})."

7. Coupling - The authors make the point in section 2.4.1 that they have used a parameterisation that would suggest strong coupling to the atmosphere (roughness length). Is this the only

resistance in the model? Is there also a boundary layer at the leaf surface? This is not clear from the text.

Response: Equations in the appendix show that leaf sensible heat flux, latent heat flux, temperature, and photosynthesis depend on leaf boundary layer conductance. In the original submission we do not give the equations and state simply that: "Leaf boundary layer conductances vary with leaf dimension and wind speed." In the revision, we provide these equations.