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Interactive comment on "Modeling stomatal conductance in the Earth system: linking leaf water-use efficiency and water transport along the soil-plant-atmosphere continuum" *by* G. B. Bonan et al.

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This paper documents an alternative stomatal conductance implementation in an attempt to move away from empirical Ball-Berry type models. Overall I applaud the authors for the detailed level in which they have attempted to document these model adjustments and I think this journal is an appropriate place for such a manuscript to be published. 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. I think the framework of an interesting paper is here and one that will no doubt have wide reaching

impact in the community, however it should not be published in its current format.

I will outline a series of key revisions below:

* 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.

* 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).

* 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?

* 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.

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.

* 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?

* Coupling. The authors make the point in section 2.4.1 that the 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.

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