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Comment

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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In this manuscript, Bonan et al. present improvements to the stomatal conductance routines in the CLM land surface model, in particular with respect to drought stress. The authors present a detailed account of the changes, and demonstrate that they were able to improve the current implementation, as compared to data from several flux sites. 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). However, I do

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have a number of issues with the current version of the manuscript. Many of these relate to the presentation and explanation of the model and results, but also relate to justification of a complex model in CLM, and details of the implementation. Overall, the improvements to CLM are perhaps not entirely novel, but the authors admit this clearly in the manuscript. The work does represent a substantial contribution to improving a much-used land surface model, and its impact will likely be significant.

In the following, page numbers are -3000, so 87 refers to 3087.

General comments

- The authors present an optimization of dA/dE as an alternative to the dA/dgs optimization currently employed in SPA, but do not admit that this method is identical to the Cowan-Farquhar optimization framework. Instead, the method is presented as an alternative without explanation (e.g. 89-21). That is, no explanation is given as to why we would follow that optimization algorithm. This can be resolved by explaining clearly throughout that the authors have implemented a numerical solution to the Cowan-Farquhar optimization hypothesis, rather than ‘an alternative algorithm’. In fact, the authors do seem to be aware of this in the Discussion (107-24 and onwards), when they discuss the correspondence between λ (the marginal cost of water in the Cowan-Farquhar framework) and the stomatal efficiency (the ‘iota’ parameter in SPA and this manuscript). This fact should be made much more clear throughout the manuscript.
- This leads to my second point. Why is it necessary to implement a full numerical solution to the optimization hypothesis? Medlyn et al (2011) showed that their simplified (Ball-Berry like) equation fits very well to a full numerical solution (their Figure 2). The closed-form equation would save on computing time (as one fewer iterative loop is necessary as shown in Fig. 2). It does raise the question of why the (simplified version of the) Medlyn equation did not compare very well to the dA/dE optimization method (Fig. 6), but this may be due to effects of soil

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water stress? To resolve this, the authors should explain their motivation for a full numerical solution, also in the context of the added simulation time (which is relevant for a journal like GMD).

- The stomatal conductance model includes a full implementation of the energy balance, which is necessary as it affects the leaf-air temperature difference ($T_{\text{leaf}}-T_{\text{air}}$), which affects leaf-to-air VPD and thus stomatal conductance (g_s) itself. However, it would be very helpful to understand the results if the authors would discuss how well coupled the canopy is (that is, values of the boundary layer conductance both at leaf and canopy scales). For example, in Fig. 5, a curvilinear response of A to g_s is demonstrated when V_{cmax} (and I assume J_{max} , though this is not stated) was varied across a wide range. If $T_{\text{leaf}}=T_{\text{air}}$, this relationship should yield a set of straight lines, not curves (as at a given set of environmental drivers, the ratio A/g_s will indicate the marginal cost of water. Therefore it is not affected by V_{cmax} or J_{max}). These curves can be explained by the boundary layer : as V_{cmax} increases, g_s increases, which decreases T_{leaf} , decreases VPD, and allows a higher g_s at given A (because lower VPD means lower E at a given g_s , which would open the stomata slightly if dA/dE is constant). It would be very helpful if the authors explained this, and other, results in terms of $T_{\text{leaf}}-T_{\text{air}}$, as currently it is not possible to separate those effects from the assumptions embedded in the optimization hypotheses. At the moment, this pattern is not explained whatsoever, just stated (102-14).
- Related to the above, the degree of coupling can be expected to be very different for the canopy simulations (canopy+leaf boundary layers) compared to the leaf simulations (leaf boundary layers only). It seems from the main results of the canopy-scale simulations that the different model implementations actually did not have a very large effect on simulations of the latent heat flux (Figs. 9, 11, 14 and possibly the Taylor diagrams but those confuse me I have to confess). This could be the case if canopy latent heat flux is mostly energy limited (poorly cou-



pled) , but the authors have not presented the information necessary to evaluate this. Also, net radiation simulations are only relevant in the context of modelling Tleaf-Tair, but this is not discussed nor presented. I find a thorough discussion of the boundary layer conductances and their effects on gas exchange simulations crucial to understand the results in this manuscript.

- In the Abstract and elsewhere, ‘the Ball-Berry model’ is confused with the actual implementation in CLM, especially in reference to drought stress. In CLM, drought stress effect is only on g_0 and A , not on g_1 (as many others have done, which is admitted in the manuscript). So, a statement like “The SPA-WUE optimization performs significantly better than the Ball–Berry model when there is soil moisture stress” (106-19) cannot refer to the Ball-Berry model (since that does not include any soil water stress effect), but only to the particularly strange implementation in CLM. This should be carefully worded throughout the manuscript. It also raises the question why the authors did not try a simple soil moisture stress effect on g_1 in the Ball-Berry model as an alternative to the full complex numerical simulation?
- The authors have implemented a hydraulic maximum on transpiration rate, which is a useful way to include soil water stress effects on g_s . However, it should be made more clear that we really have two models here : an optimization algorithm (either dA/dg_s or dA/dE), coupled with a maximum transpiration rate framework. When $E = E_{max}$, the optimization framework is not relevant, since transpiration will simply follow the maximum allowable E , given soil water stress and components of the soil-plant water transport pathway. A lot of the results could be better understood if it is shown whether the g_s predictions are from optimization or the hydraulic constraint. For example, in Figures 8 and 14, in which of the timesteps was $LWP = \min LWP$? This could be shown with two different colours of the symbols. At the moment it is unclear whether the improvements are due to the optimization criterion, or due to the inclusion of a hydraulic maximum transpi-

ration rate.

Specific comments and technical corrections

87-16 : ‘vapor pressure deficit term’, this is confusing because VPD has not been mentioned yet. To resolve, write the Ball-Berry model in the paragraph above with an $f(\text{VPD})$ term instead of h_s .

87-24 : Oren et al. did not demonstrate a $D^{*-0.5}$ dependence of g_s (though the $\ln(D)$ response is similar).

88-4 : Other methods have used both, and this seems to be quite successful (see Zhou et al. 2013 10.1016/j.agrformet.2013.05.009).

88-6 : ‘alternative approach’. Alternative to what? The previous paragraph already mentioned optimization, and the approach described in this paragraph does not account for soil water stress either.

89-5 : What about Hickler et al. 2006 (10.1111/j.1466-8238.2006.00254.x)? They included a hydraulic framework into LPJ, this should be cited and discussed here.

89-21 : The SPA algorithm has no inherent stomatal response to VPD, but this does emerge as a result of the minimum leaf water potential (minLWP) assumption. Once this minLWP is reached, a further increase in D would decrease g_s at a rate $1/D$ (so as to keep LWP constant). This is not described well in this manuscript (and Fig. 3 implies there is no VPD response, but this result is only for when $LWP > \text{minLWP}$). It would also be relevant to cite Misson et al. 2004 in this context (TreePhys 24, 529-541).

91-25 : “The Ball–Berry model requires an iterative calculation of g_s and A_n ”. Actually C_i may be calculated without iteration, as the intersection between supply curve (diffusion constraint) and demand (the Farquhar $A-C_i$ model) can be written as a quadratic and solved (as is done in Maestra/Maespa, see <https://bitbucket.org/remkoduursma/maespa/src>, file `physiol.f90`, subroutine PHOTO-

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SYN). This may be worth considering for the implementation of CLM, as again it may yield a decrease in simulation time.

91-28 : Stomatal conductance

92-3 : decrease

93-3 : 'can be observed directly', What does this mean? Actually the minimum leaf water potential is very difficult to measure; when do we know it really is the minimum value? Even Choat et al. 2012 (there cited) uses the lowest value ever recorded for that species, hardly a direct measurement.

94-6 : I don't understand the relevance of the detailed discussion of the components of the plant hydraulic conductance, as the only value really needed is the whole-plant leaf-specific hydraulic conductance (94-25 onwards)? The latter is normally estimated from the nearly straight relationship between E and LWP-LWPreddawn.

94-12 : It is not possible to convert hydraulic conductivity (measured on shoots by Tyree) to whole trees when only considering the path length. As vessels taper (and to a differing degree), whole-stem conductance does not simply follow from shoot hydraulic conductivity.

95-15 : Explain here in detail that stomatal efficiency is the inverse of lambda (for the dA/dE algorithm)

95-19 : This is very tentative, as Glopnet likely includes a wide range of VPD (not just very low; many of the measurements are taken around midday). How different are the estimates of λ when you assume, say, a VPD of 1.5 or 2kPa?

96-11 : It is not clearly described why an estimate of the root hydraulic resistivity is needed here. It affects the total plant k_L , but does it matter otherwise? Please explain.

97-7 : For which year were the simulations performed (CO₂ = 367ppm is quite low).

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98-9 : This section is not clear to me. What do the ‘random errors’ represent and why is this approach useful?

98-20 : Why was g_0 varied to include even very large values ($0.1 \text{ mol m}^{-2} \text{ s}^{-1}$)?

98-21 : It should be mentioned that the SPA routines implicitly assume that $g_0 = 0$ (unless this was added as a minimum conductance, but this is not mentioned).

98-25 : Why vary the four components of the belowground conductance, not just the total hydraulic conductance? The four components are varied by different orders of magnitude (MT 2.5 fold, rr 5 fold, rd 2.5 fold and Rr 50 fold). It is then no surprise that “Root resistivity [Rr] explains 85

100-12 : Was J_{max} co-varied with V_{cmax} ?

100-22 : This is the further simplified form of the Medlyn 2011 model (mention that here). It is useful because it can be used as an index for illustration (unlike the $1+$ form of the equation).

101-16 : As mentioned above, this misses the point that if $LWP = \text{minLWP}$, g_s responds steeply to VPD in the dA/dg_s algorithm.

101-20 : Why is this analysis presented here, how is it relevant to the manuscript? As the number of figures is far too large anyway, perhaps this is not necessary. This issue was already discussed by Katul et al. (2009), to which nothing new is added here.

102-2 : Please admit here that Glopnet is not very well suited at all to testing stomatal conductance models, in fact I would advocate removing that entire comparison as Glopnet does not even include fundamental drivers (VPD, Tair, CO₂). It is a useful dataset to study broad plant trait patterns, not detailed gas exchange models.

102-20 : This should be no surprise, given that Medlyn et al. 2011 already demonstrated that these stomatal indices fit well to the full numerical solution,

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because they are based on the same principles. However it is unclear (as mentioned above) why panel 6d shows such a poor correlation. Can you explain the reason for this? Was it inadvertently switched with 6c (the Ball-Berry model surely should give a worse comparison, since it uses relative humidity).

105-13 : 'to some extent' is vague.

105-21 : This is not unexpected, but it is useful to discuss Barnard Bauerle 2013 (10.1002/jgrg.20112) in this context, who did not take into account this strong correlation in a sensitivity analysis (and thus wrongly concluded g_0 is very important). 106-2 and Abstract : as commented above, the only reason R_r seems important is because it was varied 50-fold. Also, the Abstract focuses exclusively on this parameter but it is not demonstrated that the model fit could only be improved by varying R_r (and not other parameters related to belowground hydraulics).

107-15 : This is correct, but only when T is constant. In field conditions D and T are strongly correlated, which actually gives a different effective relationship between g_s and D (see Duursma et al. 2014 (10.1016/j.agrformet.2013.12.007). This point is often overlooked .

Conclusion – though well written, I find the conclusions to be much too general compared to the current study. Perhaps this can be focussed to be more relevant to the current study.

Fig. 9 – the modelled diurnals are obscured by the measurements. Use of transparent colors would resolve this.

Fig. 10 and 12 – what does REF mean?

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