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Discussions**

Interactive comment on "Coupled carbon-water exchange of the Amazon rain forest, I. Model description, parameterization and sensitivity analysis" *by* E. Simon et al.

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For some reason, I've agonized long and hard over this review, and I don't know exactly why. It is a comprehensive description of a CANVEG-type canopy exchange model developed and parameterized for an "average" Amazonian terre firme forest site. The manuscript pulls together and summarizes an impressive amount of data utilized both for parameterization and validation. Considering the difficulty of obtaining these sorts of data for the field sites in Amazônia, this is an impressive achievement, and this paper and its companion clearly deserve to be published. So why have I spent the better part of three days worrying about it? I attribute it to the tyranny of the on-line review process, in which one feels compelled to say something profound or provocative in order to stimulate some discussion. Be that as it may, my review follows.

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Though I have worked on developing and parameterizing gas-exchange models at the leaf scale (assimilation, conductance, isoprene) I am not a modeler of canopy-scale processes. I will offer only general comments on the overall model structure and behavior, confining any specific comments to areas in which I have some expertise. In a very general sense, I felt the Introduction failed to provide adequate justification for the overall effort. Why was this model developed and applied to the Amazon? Is it just an intellectual exercise, an attempt to simulate accurately measured data, or is there a more significant justification? Will the modeling effort help us to better understand these exchanges of matter and energy at a mechanistic level? Will the model be used to fill in the inevitable gaps in the long-term data record from these sites? Or will the model be used to predict responses of the Amazonian forest to future changes in climate, land use or CO2, for example? I know the authors have thought about these issues; a brief paragraph in the Introduction summarizing their thoughts might be helpful.

In terms of model structure, this manuscript breaks no new ground, relying heavily on previously models developed largely for temperate systems. This is not a criticism; applying such models to tropical forest systems remains a significant accomplishment. Model description of radiation interception by the canopy and of sensible and latent heat fluxes seem quite robust, and where physical processes such as canopy albedo are not well simulated, suggested modifications to the model seem reasonable. The soil respiration model [$F_{csoil} = f(T)$] seems pretty simplistic, though I have no expertise in this area. In other systems, soil moisture and organic content obviously play a role; do soils in these systems never dry to the point of affecting microbial or root respiration?

With respect to leaf surface exchanges of CO2 and H2O, the model employs well-tested sub-models of the biochemistry/biophysics of CO2 fixation and of stomatal conductance. I understand that a lot of short cuts in model description are necessary in describing a complex model such as this, but one issue concerns me. Although the CO2 fixation and stomatal models are frequently described as "coupled", the mecha-

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nism by which they are coupled is never made explicit. It is unclear to me, for example, how the model arrives at a value for Ci, the key variable that links the two sub-models. In both Figs. 12 and 13, Ci appears to be prescribed, either by using observed values or by assuming a constant value. When the model is run to predict canopy scale fluxes, how is Ci determined? When the Farquhar model is run in conjunction with the Ball-Berry model, Ci is generally calculated iteratively, and the value of Ci is found that simultaneously satisfies both the photosynthesis and conductance equations. I assume that a similar procedure is performed here, but nowhere is that made explicit. If the two sub-models are not truly coupled, but run independently of one another, predicted values of An and gs would be internally inconsistent with one another.

Clearly, issues of scaling-up leaf/branch level measurements to the canopy scale are of great importance here as in all forest canopies. I think the authors do a nice job of justifying their decisions with respect to initial parameter values, and of justifying their scaling of parameters with N as it decreases through the canopy. Clearly a leap of faith is required to assign "average" values to a system as biodiverse as terra firme forest based on measurements from 8 species, but I was actually quite impressed that the "default" parameterization did as well as it did–a 20% overestimate ain't bad! I do have some questions regarding the authors' decisions about modifying the model parameters to achieve more realistic fits to measurements however. Figure 12 (panels d, e, and f) clearly indicate that the model, using the default parameterization, overestimates An. But there a great many model parameters which could be "tweaked" to bring the observations more in line with measurements. How did the authors arrive at the decision to alter only alpha and the temperature dependency of Jmax? In theory at least, alpha is considered to be relatively invariant, barring significant stress, dependent only on the quantum requirement for electron transport. Any decision to alter it by a significant amount (25% in this case) should be justified by data. Otherwise, it just becomes a "fitting factor." Furthermore, the overestimation evident in Fig. 12 (d-f) is driven more by model overestimates at high values of An where alpha is not the primary limitation. What other model parameters were modified to test their effect on predicted An? One

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of the advantages of using a more or less mechanistic model of photosynthesis is that it provides a tool to elucidate what is limiting photosynthesis at a given moment. For example, when the model overestimates An, is Vc or Jc limiting, or is there an overestimate in both cases? As mentioned above, I am particularly concerned with how Ci is determined in the model, which is unclear to me from the text. The data in Fig. 12 (a-c) appears to use measured values of Ci whereas the lines in the figure assume Ci = 320 umol mol-1. Presumably the same is true for Fig. 12 (d-f). Does the value of 320 represent the average measured Ci? If so, it seems high (for temperature species at least; I have little experience with tropical trees). If the average observed Ci is less than 320, this would also lead to model overpredictions.

Although the English is quite good and nowhere is it difficult to discern the authors' meaning, the text could use a thorough editing by a native English speaker. I offer the following questions/comments/suggestions on the text. There is some redundancy with issues discussed above, for which I apologize.

p. 338 l. 16 INPA = Instituto Nacional de Pesquisas da Amazônia

p. 340 l. 25 text refers to a "combined stomatal-photosynthesis model for C3 plants"; these models are generally integrated by solving iteratively for the value of internal CO2 concentration (Ci) which simultaneously satisfies the stomatal model (which is dependent on net assimilation) and the assimilation model (which relies on Ci). It is not clear whether this was the approach used here; the Appendix (p. 358) describes both the Leuning stomatal model and the Farquhar assimilation model, but doesn't mention how they are coupled. I.e., how was Ci determined to drive the assimilation model, or was it prescribed?

p. 339 l. 25 Sentence beginning with "Mean ratios . . ." needs revision.

p. 341 l. 10 I'm not sure that "constrained" is the appropriate word here, since these are driving variables

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p. 345 l. 2 please give units for gRad, presumably W m-2

p. 350 l. 11 I'm not clear what "calibrated" means in this context

p. 350 l. 22 "The observations show a lower light use efficiency (alpha). . ." I am confused as to what "observations" are being referred to here. Does it refer to measured light response data which is not shown here, or does it refer to Fig. 12? If the former, a reference should be provided. If the latter, I am unclear what the "observations" are; all symbols and lines in panels a, b, and c appear to refer to model results rather than observations. On the other hand, panels d, e, and f clearly compare model predictions with observations, but the data offer no clues as to what model parameter ought to be changed in order to better simulate observations.

p. 350 l. 27 326C should presumably be 32.6C; but how was the change in the temperature optimum of Jmax achieved? One doesn't simply "change the temperature optimum" in the Farquhar model, the temperature optimum changes as other parameters are modified. Apparently in this case (based on Table 7) the activation energy was changed from 79.5 to 108 kJ mol-1 while the entropy term was changed from 0.65 to 0.66. Presumably, in order to maintain Jmax (29C) at 2.1*Vcmax (29C), the scaling factor ('c' in Harley et al. 1992) was also adjusted upwards. None of this is transparent to the reader, nor is the end result, which is to drop Jmax by approx. 20% at temperatures above the optimum.

p. 394 Theobroma misspelled; also, units for LambaZ should be m2 m-2

p. 395 & 396 Captions indicate that Ci (Fig. 12) and Cs (Fig. 13) were both fixed at 320 umol mol-1. The value of 320 seems high for Ci and low for Cs, and they certainly shouldn't be equal. In Fig. 13, to which linear fits do the equations refer?

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