**Biogeosciences
Discussions**

Interactive
Comment

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

E. Simon et al.

Received and published: 27 June 2005

First of all, we would like to thank both referees for their constructive comments, which will help us much to improve the paper.

Response to interactive comment by Referee #1

Model sensitivity analysis (Specific comments 1 and 2)

A major criticism of Referee #1 is our sensitivity analysis (section 3.7 in the paper). Summarizing, we repeat his three major points:

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Methodology: Instead of comparing the model output for a modified parameterization with a "reference" parameterization, he proposes to calculate the sensitivity and elasticity, as defined by dy/dx and xdy/ydx, respectively, for each model parameter.

Estimation of parameter uncertainty: Furthermore, he states, that the applied parameter uncertainties are *"statistically poorly defined as the range of possible values guessed by the authors, based on fitness between predictions and observations"* and that *"these uncertainties should better be constrained using the range of value published in the literature"* .

Choice of parameters for the sensitivity analysis: In his second specific comment, Referee #1 asks *"Why the authors didn't test the model sensitivity for other parameters?"*

From a strictly mathematical point of view (i.e. the mathematical definition of parameter sensitivity) his argumentation is probably right. However, by this definition it is required to calculate a matrix of partial differential equations, consisting of the elements $\delta y_j/\delta x_i$. y_j represents the i -th model output parameter, with $j = 1..n$, and x_i the j -th input parameter, with $i = 1..m$, respectively. This exercise would result in a large number of model calculations. In the end, the outcome of this analysis has to be summarized to describe the sensitivity of the most important model output parameters (the canopy net fluxes) in relation to the model key parameters.

Actually many different simulations have been performed with the model. The most important informations are provided by Table 6. and Fig. 14. From Table 6, a certain partial differential quotient $\Delta y_j/\Delta x_i$ can be derived easily. For example, the sensitivity of CO_2 net ecosystem exchange (NEE) to the photosynthetic capacity at the canopy top (v_{cmaxhc}) for a mean flux $y_{NEE} = 20 \ \mu$mol m^{-2} is $(0.05 * 20)/(70 - 50) = 0.05$.

Referee #1 criticizes the choice of parameters and the choice of parameter uncer-

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tainties applied in the sensitivity analysis. We to admit to some extent that this is an arbitrary procedure. However, as stated above, it is practically impossible to infer the whole model parameter space. Therefore, the expected parameter range has to be chosen "à priori". We think, we did it very carefully. Our choice is based upon a combination of "recommended by the literature" (if available) and our own results (sections 3.1-3.6) which are indeed based on the *"fitness between predictions and observations"*. However, this is also the case for the recommended literature values.

Parameter modifications (3 and 4)

See our response to a similar comment made by Peter Harley.

Data used for calibration and evaluation (5)

We used many different data sets for the parameterization of the model and for the evaluation of this parameterization by comparing the output of the sub-model components with observations. This might be confusing at some points in the paper. For clarification we will adopt the Referees' idea of a synthesis Table by extending Table 2 in a revised paper.

"Why the authors didn't use measured Vcmax?" (6)

Measurements of v_{cmax0} have not been available for the investigated set of gas exchange measurements. Instead we scaled the value observed at the canopy top $v_{cmax0hc}$ with respect to canopy position (see interactive comment by P. Harley, p.S196, second paragraph).

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Respiration fluxes (7)

We are aware of the importance of respiration fluxes in the context of carbon sequestration. However, the dynamic processes that affect the soil and wood carbon pools are very complex and they are related to time scales that are beyond the scope of the present study. The addition of soil processes to the presented CANVEG scheme would unnecessarily increase the complexity of the model. The applied temperature relationship to calculate respiration is well known, transparent and accepted. We would propose, to investigate the role of soil processes with an existing model of nutrient dynamics in different ecosystem pools (roots, litter, stems,..).

We also see a weakness in the procedure of leaf respiration calculation (although R_{d0} rather than R_d is a fixed proportion of v_{cmax0} and not v_{cmax}, as stated by Referee #1). However, in contrast to gross photosynthesis, a well-accepted operational "model" to describe the regulation of leaf respiration in the dark and in the light is still not available (Note that this point is also addressed in the interactive discussion on the companion paper).

Technical comments by Referee #1

1. Accepted.

2. Accepted.

3. Accepted.

4. Accepted, will be clarified.

5. Accepted (8 layers from 0 to 40 m with $\Delta z = 5$ m)

6. Accepted.

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7. Accepted.

8. Accepted.

9. Schulze et al. 1994, see line 6.

10. Accepted.

11. Accepted, see Response to specific comment (5).

12. No, not only Q_{v0}, the relationship is applied at the leaf scale to obtain the quantum flux from the amount of absorbed radiation in the visible waveband.

13. Accepted.

14. *"Why you didn't use measurements made at the site?"* We used measurements made at the site. However, instead of using only two single point profiles from the two towers, we used additional data from similar sites in the Amazon basin. Due to measurement errors, the measured accumulated leaf area index profiles may decrease in the lower canopy where the measurement uncertainty of optical devices is relatively high. Therefore we derived a smooth parameterization, which represents an estimate of mean canopy structure for terra firme rain forest (which might be improved or modified by using additional measurements). This point is also addressed in a recent study on vertical dispersion, first cited at page 338 (S2005b, footnote 3 on page 337, meanwhile accepted for publication in "Agricultural and Forest Meteorology")

15. The canopy height estimate $h_c = 40$ m represents the upper limit of a wide range of values that are reported in the literature (cited at page 345). Actually, this parameter is not well defined for the Amazon rain forest. While the mean canopy height is some meters below 40 m, single trees may even excess this height, which has an important impact on the flow field above and within the canopy (see

Kruijt et al. 2000) This point is discussed in more detail in the manuscript S2005b mentioned in footnote 3 on page 337 (see above).

16. *"How did you estimate WB, WT, and zB? How did you 'optimize' ai1 and ai2?"* The parameters can not be inferred straight forward because the vertical distribution of leaf area is non-linear. We found a "good solution" for the weight coefficients and optimized a_{i1} and a_{i2} by applying a general gradient method.

17. Actually, we had two graphs included in an earlier version of the manuscript. Since the parameterization performs quite well and is also widely accepted and documented in the literature, we removed that figure from the manuscript.

18. Accepted.

19. Accepted.

20. Accepted.

21. Accepted.

22. The range depends on the averaging intervals shown in Fig. 10 c. On a first glance, it is large. Considering the narrow range of observed soil temperatures, the resulting range in calculated soil respiration is very small (< 10%, see the dashed and solid lines in Fig. 10c).

23. Accepted (see Table 7).

24. Accepted, see response "Parameter modifications" to comments by Peter Harley.

25. See response "Parameter modifications" to comments by Peter Harley.

26. Accepted.

27. Parameter have not been optimized locally because no significant and consistent improvements have been found.

28. See response "Parameter modifications" to comments by Peter Harley. We do not exclude that soil moisture may effect stomatal behavior. However there is no evidence in our data. On the other hand, the data shown in Fig. 13 suggest a higher value of a_A for the wet season data (dashed line compared to the dotted line).

29. Accepted, NEE.

30. Accepted.

Tables and Figures

Table 1,2 Accepted.

Table 2 Accepted.

Table 3 Accepted.

Table 6 Accepted.

Fig. 1 Accepted, list of symbols and legend have to be updated.

Fig. 6 Accepted.

Fig. 8b Accepted.

Fig. 10 Accepted.

Fig. 13 Accepted.

Response to interactive comment by Peter Harley

Comment on the introduction (p. S195, first paragraph)

The *"justification for the overall effort"* is given in more detail in the companion paper (this issue, p.399-449, see also the interactive comment on that paper by P. Harley). We will mention this in a revised manuscript.

Soil respiration

"The soil respiration model [..] seems pretty simplistic, [..]; do soils in these systems never dry to the point of affecting microbial or root respiration?" (p. S195, 2nd paragraph). See our response to a similar comment made by Referee #1.

Leaf model coupling mechanism

At p.S197, first paragraph, Peter Harley adds: *" 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)"*.

In fact, the mechanism how stomatal conductance and leaf photosynthesis are coupled in the model is not explicitly described in the paper. Instead two references at p.340. l.25-28 are given, which describe in detail the combined stomatal-photosynthesis model (Leuning 1995) and the numeral procedure to solve the coupled equations (Wang and Leuning, 1998). We explicitly state here (and will do this in a revised paper), that c_i is not fixed, but the linking variable of the leaf model (as already assumed

by Peter Harley). The caption in Fig. 12 will be clarified to avoid misunderstandings. "Standard conditions" with "$c_i = 320 \mu$mol m^{-2} m^{-1} are only applied for the ideal light response curve, shown as the solid and dashed lines in Fig. 12a-c. By using a high value, it is assured the Rubisco is CO_2 saturated (i.e. A_n light limited).

Parameter modifications

At page S196, second paragraph, Peter Harley arises some questions concerning the modification of leaf model parameters:

"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 [..] should be justified by data. [..] What other model parameters were modified to test their effect on predicted An?."

As shown in Fig. 12, the leaf photosynthesis model overestimates the observed assimilation rates, using the recommended parameter values, but what parameters are wrong? Although it is not shown in Fig. 12, the measurements indeed justify our parameter modifications (we will modify Fig. 12 for a revised paper and show also the observed light response):

α : The initial slope of the observed light response is up to 30% lower compared to model predictions, suggesting a lower electron transport rate, i.e. a lower value of α.

θ : The transition from linear increase to saturated A_n in the observed light response is sharper compared to model predictions. This is considered by reducing θ.

J_{max} : As recognized by Peter Harley, the shift of the temperature optimum of J_{max} is the result of modifying the activation energy and entropy terms. This will be clarified in a revised paper. The observed light response shows a decline in A_n at very high light intensities, which might be explained by temperature stress. Thus we decreased the temperature optimum of J_{max} compared to the initial parameterization.

We admit that there may be more explanations for the observed disagreement between observed photosynthesis and the predictions of the leaf model using recommended parameter values. However, this type of questions has to be addressed by experimental work.

Specific comments by Peter Harley

p.338,l.16 Accepted.

p.340,l.25 See above

p.339,l.25 Accepted.

p.341,l.10 Well, maybe "run" or "forcing" is better.

p.34[5 ,l.2] Accepted.

p.350,l.11 See response to comment (4) by Referee #1

p.350,l.22 See response "Parameter modifications"

p.350,l.27 Accepted.

p.394 Accepted.

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p.395-'96 See section "Leaf model coupling mechanism". a) shows an "idealized" g_s response (c_s constant) b+c): dotted line: linear fit for dry season data, dashed line: linear fit for wet season data, solid line: linear fit for all data (c_s observed).

Interactive comment on Biogeosciences Discussions, 2, 333, 2005.

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