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Biogeosciences Discussions



# *Interactive comment on* "Coupled carbon-water exchange of the Amazon rain forest, II. Comparison of predicted and observed seasonal exchange of energy, $CO_2$, isoprene and ozone at a remote site in Rondônia" *by* E. Simon et al.

**E. Simon et al.**

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## Responses to comments from both referees

### Ozone

Ozone deposition to vegetation, being the dominant sink of ozone in tropical regions, is calculated according to the resistance analog including leaf and soil uptake processes. Our model calculations imply two important assumptions:

1. Chemical reactions with ozone within the canopy are regarded negligible

2. At the leaf surfaces, deposition concurrently occurs via a stomatal and a non-stomatal pathway. We defined the non-stomatal pathway as cuticular uptake.

We admitted that this approach may be not straight forward (p.408,l.20). However, both referees are concerned about the methodology (p.407/408) and the interpretation of our results (p.422/423). We identified three major points of criticism and want to address their concern in a general response:

1. Referee #1 states, that we can not exclude chemical reactions of ozone within the canopy, because *"... these rates have not been inferred from observations at [our] site, so that the presence of chemical loss can not be totally excluded."* Then he resumes that *".. such process could actually explain some discrepancies.."*

2. Peter Harley suggests to discuss destruction of ozone by highly reactive biogenic volatile organic compounds (BVOC) and refers to important results of Goldstein et al. (GRL, 2004).

3. As a third point, Peter Harley critically evaluates our interpretation of the disagreement between observed and predicted ozone fluxes:

   *"I don't really see any justification for doubling modeled fluxes by reducing the cuticular resistance for O3 by a factor of 5."* (p.S218).

First we would like to respond Referee #1, that we ignored the role of $NO_x$ chemistry based on back-on-the envelop calculations and reaction rates of ozone with NO and $NO_2$. The latter have been inferred from very extended observations at our site (Meixner et al. 2002, Gut et al. 2002b, Rummel, 2005, all cited at p.408/409). The chemical term is only relevant for the NO-$NO_2$ budget, not for ozone, because NO-$NO_2$ concentrations are too low to produce a significant loss of ozone (p.409,l.5-8). This will be stressed a little bit more in a revised paper.

On the other hand, destruction of ozone by gaseous organic compounds of sufficient reactivity (i.e. BVOC) have to be discussed. We agree with Peter Harley, that this point should be discussed in a revised paper. However, the large chemical loss of ozone in a ponderosa pine plantation in the Sierra Nevada Mountains, California (Goldstein, GRL 2004) should be regarded as a special case. Within that experiment there was a tremendous increase of VOC concentrations due to forest thinning which caused the release of numerous compounds due to wounding of wood and needles. This special situation was a great opportunity to demonstrate a chemical loss of ozone. However, we are not completely convinced that such a result can be transferred to our measuring site. At our site, lower emissions and concentrations of the highly reactive BVOC's have been observed (see Kesselmeier et al. 2002, Greenberg et al. 2004) which might give the ozone a good chance to reach the plant surface. Despite all discussions, within a revised version we will consider the chemical sink as given by highly reactive VOCs (and $NO_x$) in a short additional discussion chapter.

Within this context, we altered the cuticular conductance to ozone deposition by a factor of five (paragraph starting at p.422, l.18) to demonstrate that non-stomatal processes might become very important for ozone deposition. Regarding our parameter modifications, Peter Harley admits, that *".. in any case, one obtains better fits during one season only at the expense of poorer fits for the other season."* In other words, we can not explain the observed ozone fluxes consistently for both, wet and dry season conditions, with our current knowledge on ozone deposition. We think, that this is actually a good demonstration of the diagnostic capabilities of the model and may answer the question of Peter Harley, asking *"So what exactly does this model simulation teach us?"*: It shows us an important lack in our understanding of ozone deposition. Since we used an explicit multi-layer approach, the potential role of different processes (chemistry, turbulence, stomatal uptake) can be investigated (and discussed) at the relevant scale, i.e. the leaf scale. We will also add a reference, which supports our discussion of the model results: In a recent field study on shoots of Scots pine, Altimir et al. (Atmosph. Env. 38, 2004) investigated the important role of non-stomatal uptake processes. Consistently with our model calculations, they observed, for high relative humidity conditions, non-stomatal ozone deposition rates on the order of 50% of the total flux.

## Respiration fluxes

Referee #1 criticizes our treatment of respiration fluxes:

*"Obviously there is a problem in computing respiration from soil, stem, leaf at the ecosystem level and this need to be discussed."*

and Peter Harley adds *"is there any data demonstrating that a change from 25% to 15% soil water content has no affect on respiration?"*.

We would like to respond Peter Harley, that soil data as obtained at one of our measuring sites (soil chamber measurements made by Gut et al. 2002a,b) showed no significant effect of soil moisture on soil respiration.

The "simple" parameterization has been used to avoid "modeling overload". We fully agree that night time $CO_2$ fluxes are generally underestimated. This can be regarded as a result of the large uncertainties of EC measurements. However, the present study focuses on canopy processes that usually act on short time scales. The dynamic processes that affect the soil and wood carbon pools act on much longer time scales and are beyond the scope of the present study. To address carbon sequestration, we propose to integrate the CANVEG scheme into one of the very powerful process-based models on nutrient dynamics in different ecosystems, e.g. the CENTURY model (current version 5 at http://www.nrel.colostate.edu/projects/century5/). See also our response to the interactive comment on the companion paper and section "Doubled atmospheric $CO_2$" below.



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## Doubled atmospheric $CO_2$

Since both referees recommended removing of chapter 3.5, we will do this in a revised paper. Nevertheless, the results *"suggest a number of potentially important impacts"*, as Peter Harley admits in his comment. The issue should therefore be addressed more exclusively in a future study, which also analyses potential interactions with soil processes. The latter might be addressed by coupling the CANVEG scheme to existing models of the relevant scales (see section "Respiration fluxes").

# Response to comments by Referee #1

## Clarity of text, figures, and tables (1)

Accepted.

## Response to specific comments 2,3 and 4

See section response to both referees above.

## Technical questions and corrections

1. We state "substitution", not replacement.
2. Accepted, structural changes are not addressed here, this is right. However, we showed in the companion paper, that model predictions are not very sensitive to realistic structural changes of the canopy.



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3. Accepted, $CO_2$ concentration at the canopy top is an input parameter.
4. We follow the common procedure of calculating the canopy storage flux. A detailed discussion of this topic is given by Rummel (2005)
5. Table 1 shows integrated daily values for radiation as MJm$^{-2}$ per day, which is equivalent to W m$^{-2}$ s d$^{-1}$.
6. Accepted.
7. The “optimization” of the optimum temperature for $J_{max}$ reported by Simon et al. (2005a) is the result of the optimization of the activation energy and entropy for $J_{max}$ (see our response to the interactive comment on that paper).
8. See section “Parameter modifications” in our response to the interactive comment on the companion paper.
9. This point is discussed at p.408, l.20.
10. Accepted.
11. See section “Ozone”.
12. Accepted, will be clarified. The surface is the leaf surface, except for the ground layer, where it is the leaf and soil surface.
13. Accepted, will be clarified (actually convergence is defined as the iteration step, when the sum of squared changes in the ambient air temperature profile is below a particular limit, i.e. 0.01 K) , this point is also addressed by Peter Harley in his comment on the companion paper.
14. The statement is not “all physiological processes follow a $Q_{10}$ value of 2”, but 2 is “a typical $Q_{10}$ value”

15. Accepted (the diel pattern of the canopy thermal stratification)
16. *".. goes from 1 to 0.2, and is about 0.5 at noon"*, which means that the latent heat flux represents about 65% of the total turbulent energy flux at noon time, and 80% in the afternoon.
17. Fig. 1 (p.412, l.8) and Fig. 5 (p.411, l.17)
18. Accepted, this should be mentioned.
19. Accepted, we should say "NEE in absolute numbers"
20. Accepted, should be clarified. Actually, the fraction of sunlit leaves is very small ($< 5$%, see p.418,l.3).
21. See specific comment 3.
22. Accepted.
23. The sensitivity analysis (see p.414,l.23) shows, that *"a problem with the calculations of respiration"* is obviously not the problem in this case.
24. Accepted, must be clarified.
25. Accepted ("close to the ground", i.e. lowest canopy layer from 0 to 5 m)
26. Accepted.
27. Accepted (see also response to comment by Peter Harley).
28. Accepted.
29. See response "Ozone".
30. See response "Ozone".



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**Table 1** see response to Technical Comment 5.

**Table 2** Accepted.

**Table 2** Accepted.

**Fig. 2** Accepted.

**Fig. 4 and 5** Accepted, Fig. 4 must be enlarged. However Fig. 4 and 5 contain many different informations, and maybe the most important evaluation results of the modeling study. Consistently in Fig. 4,5 and 7, we used open and closed symbols for the observations from the two towers, and solid and dashed lines for model calculations obtained with two different parameterizations. However, we will work on a re-arrangement of Figures 4 and 5.

**Fig. 6** Yes, the sum of the black bars is 100%. Net assimilation means net photosynthesis rate (will be clarified).

## Response to comments by Peter Harley

p.409,l.19 Accepted, see response to Referee #1.

410, Fig. 3 *"Why [..] should shaded leaves in the upper half of the canopy be significantly warmer ($>$ 2C) than air temperature?"* Because they absorb a considerable amount of diffusive radiation, and the boundary-layer resistance to sensible heat exchange at 30 m is already relatively large, compared to the values at the canopy top.

p.415,l.9 Accepted.

17 (Fig. 9) Accepted.



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p.417,l.14 Accepted.

d p.421,l.6 Accepted, the reference to Guenther et al. 1999 should be added. Peter Harley is an expert on the field of isoprene emissions from plants and supports the idea, that emission potential varies with the light environment within the canopy, similarly as photosynthetic capacity. However, new ideas have to be promoted and need some time before they are widely accepted. Therefore we applied the “un-progressive” model comparison with G95.

p.429,l.8 Accepted.

p.422,l.11 Accepted.

p.423,l.3 See “Ozone”

p.424,l.17 Accepted.

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Interactive comment on Biogeosciences Discussions, 2, 399, 2005.