Biogeosciences Discussions, 2, S208–S213, 2005
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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.

Anonymous Referee #1

Received and published: 25 May 2005

General comments

In a previous paper, the authors parameterized a one-dimensional multilayer model (CANVEG) used to calculate water-carbon exchange between the vegetation and the atmosphere in the Amazon rain forest. In this paper, the authors validate this scheme on independent data and applied the model to better understand the processes involved in carbon and exchange. They carefully look at numerical problems and model convergence, vertical profile and vertical source/sink distribution of different trace gas and energy, as well as diurnal pattern and seasonal trend in the flux calculations. This careful evaluation gives detailed overview of the ability and failure of the model to re-

produce the different processes involved, and gives some insights into the processes themselves. With the exception of ozone chemical sinks, the authors present adequate results to support the interpretations and conclusions. With the exception of respiration (soil, leaf, stem) and ozone chemical sinks processes, the methodology for modeling is mostly valid and present no major flaws. The authors finish by doing some predictions to doubled atmospheric CO2, which might be a little bit off topic. The paper might be too long, and I do some suggestions in the Specific comments below. In addition, clarity of the text, Figures and Tables must be improved.

Specific comments

1/ The paper lacks some clarity in the text, Figures and Tables. Some statements are not supported by adequate references to Figures or Tables. It is left to the reader to find the Figure that supports the text. Some figures area really too small, with confusing choice for lines and points, and confusing legend. Not all the abbreviations and variables in equations are defined in the text. A list of symbols with definitions might be necessary.

2/ The authors state that chemical reactions with ozone are negligible in comparison to the turbulent transport, which seems to be supported by potential reactions and rates given in the literature. However these rates have not been inferred from observations at their site, so that the presence of chemical loss can not be totally excluded. In addition such process could actually explain some discrepancies between observations and predictions by the model. This possibility should at least remain open for discussion.

3/ It is true that large uncertainties remain in nighttime EC measurements. However, nighttime fluxes are mainly underestimated. This means that the real flux would even be larger than model predictions of nighttime fluxes. Obviously there is a problem in computing respiration from soil, stem, leaf at the ecosystem level and this need to be discussed.

4/ The paper is too long and should be reduced. I think that the part on prediction to

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doubling CO2 could be omitted because it is a little bit off topic considering the main objectives of the paper. In addition, these predictions only take into account some physiological responses and exclude processes such as acclimation, change in LAI and biomass, change in carbon allocation,...

Technical questions and corrections

1/ Page 401, line 14: I'm not sure if models can be used as a substitution to costly field measurements. They can certainly be used diagnostically to give insight about processes, but they will never replace actual measurements of these processes.

2/ Page 403, line 28: Are structural changes really addressed in this paper?

3/ Page 405, line 1: Is CO2 concentration at the top of the canopy an input of the model?

4/ Page 405, line 14: I'm not sure if the storage flux of ozone can be calculated like it is for CO2 because ozone is a reactive compound. Changes in ozone concentration inside the canopy can be due to chemical loss which, if not taking into account, can lead to overestimation of storage flux.

5/ Page 405, line 23: Please keep the same units for meteorological parameters in between the text, the Figure 1 and the Table 1.

6/ Page 406, line 2-3: "The noon time values of relative humidity...."

7/ Page 406, lines 20-23: It seems that optimization of activation energy and entropy was not reported in Simon et al. (2005a). Other parameters were calibrated, but not these ones. In addition, activation energy and entropy needs careful leaf chamber temperature response data that I don't remember to be presented in Simon et al. (2005a).

8/ Page 406, line 24-Page 407, line 5: I still don't understand the concept underlying the parameterization procedure here. Ideally, the uncertainty range of a parameter must be less important than its seasonal variation, otherwise the seasonal variation is not

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significant within the uncertainty range of the parameter (which is actually very often the case). However, the authors seem to confound uncertainty and seasonal variation.

9/ Page 407, line 11: Is the concept of dry deposition valid for a reactive compound like ozone?

10/ Page 408, line 2: Give detailed definition for variables and parameters in Eq (3-4-5), what is ra, rleaf,...?

11/ Page 408, line 20: The authors state that chemical reactions with ozone are negligible in comparison to the turbulent transport, which seems to be supported by potential reactions and rates given in the literature. However, these have not been observed in situ at their site, so that the presence of chemical loss can not be totally excluded. In addition such processes could actually explain some discrepancies between observations and model predictions. This possibility should at least remain open for discussion.

12/ Page 409, line 19: What is the surface, is it leaf surface? In addition, a list of symbols and explanation including ALL the abbreviations in the text is probably needed.

13/ Page 409, line 24: The authors should better explain, in this paper or in the companion paper, what are these iterations for? What is the target variable and the process through which convergence is achieved?

14/ Page 410, line 18: Not all physiological processes follow an exponential or Q10 relationship with temperature.

15/ Page 411, line 1: "The diel pattern...." of what?

16/ Page 411, line 22: From Fig 4, it seems that most of the available energy is NOT converted into latent heat. Bowen ratio goes from 1 to 0.2, and is about 0.5 at noon.

17/ Page 412, line 10: "...lower assimilation rates (in relation to the incoming radiation).". From which Figure or data are you drawing these conclusions?

18/ Page 412, line 13: Increasing stomatal conductance leads also to overestimation

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of LE.

19/ Page 412, line 19: Isn't it an increase in NEE? (NEE being negative when there is a net uptake by the vegetation?).

20/ Page 412, line 22-25: Is there a lot of sunlit leaves in the lower canopy ($<$0.25 z/hc)? In addition: please refer more clearly to Figures to support your points.

21/ Page 413, line 10: It is true that large uncertainties remain in nighttime EC measurements. However, nighttime fluxes are mainly underestimated. This means that the real flux would even be larger than model predictions of nighttime fluxes. Obviously there is a problem in computing respiration from soil, stem, leaf at the ecosystem level and this need to be discussed.

22/ Page 414, line 13: "...shape" Please refer more clearly to Figure (8a here) to support the text.

23/ Page 414, line 13-14: "The observed concentrations are much higher as model predictions". Is there a problem with calculations of respiration?

24/ Page 414, line 14-17: I don't follow your point here.

25/ Page 418, line 4-5: At which height?

26/ Page 421, line 6: 95.8 in Table 3.

27/ Page 421, line 11: "...agrees well with...": Is 64.4 vs 84 a good agreement?

28/ Page 421, line 27: Give the equation too.

29/ Page 422, line 7: What about chemical loss?

30/ Page 423, line 23-24: I'm not convinced that chemical sinks are insufficient AND that it would affect both seasonal periods similarly. If no data are available, no definite conclusions can be drawn at this point, and this should be open for discussion.

Table 1: Please keep same units as in the text and Figure 1.

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Table 2: aN is NOT a coefficient of correlation.

Fig 2: change (Fig. 1a, c, f) to (Fig. 1a, c, e)

Fig 4 is really confusing, both the figure (choice of lines and points) and the legend. I spent 15 minutes on the figure alone. Plus, it had to be larger, and this is a general comment for ALL the figures, they are too small.

Fig 5: legend is un-understandable.

Fig 6: The sum of the black bars should be = 100%. Is it the case? You speak about relative source distribution, isn't it relative source/sink strength? What is net assimilation? NEE or GPP?

Fig 10: legend not clear.

Fir 13: what is canopy net assimilation? NEE or GPP? Make it clear in the text also.

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