

Interactive  
Comment

***Interactive comment on “Modelling basin-wide variations in Amazon forest productivity – Part 1: Model calibration, evaluation and upscaling functions for canopy photosynthesis” by L. M. Mercado et al.***

**L. M. Mercado et al.**

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Response to specific comments by Referee 2.

Thank you very much for your comments.

1. Comment

Ecosystem respiration is determined by different methods in Manaus and Jaru from that in Tapajos and Caxiuana, but we are not told why. Generally, I think a plot of  $\log$  against night respiration in the Appendix would be a good idea as a means of telling us about the quality of the night flux data, which I presume is the reason behind the

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decision to have two approaches. This is all glossed over, and more detail ought to be included.

Answer

As mentioned in the submitted manuscript (page 2971 line 16).The choice of ecosystem respiration was based on availability of data. At Tapajos and Caxiuana, ecosystem respiration was directly provided from the eddy correlation investigators as part of the measurements available. This was in both cases inferred from night time net ecosystem exchange fluxes by filtering out data below particular  $U^*$  thresholds (Iwata et al. 2005 for Caxiuana and Hutyrá et al. 2008 and Restrepo-Coupe, in prep for Tapajos). As for Jaru, and both Manaus sites, ecosystem respiration was not provided along with the eddy correlation data, and was outside the scope of this study to produce estimates of ecosystem respiration based on eddy correlation. Instead, we used measurements and derived equations from single components of ecosystem respiration that were available for these sites. We do recognize in the discussion that the ecosystem respiration term adds to the uncertainties of this data-model evaluation.

2. Comment

On the calculated isotopic composition of leaves, the approach is to boldly go with Farquhar et al 1982. I think that rather small amounts of re-fixation of soil-respired C and plant-respired C will spoil the 'pure' Farquharian estimate. At the very least, the authors ought to mention the problems. There is a paper by Lloyd on this, which shows how much C is likely to have been refixed.

Answer

As pointed by the reviewer, our modelling approach does not include re-fixation of soil and plant respired CO<sub>2</sub>. However measured values of  $\delta^{13}C$  are from top of the canopy leaves (section 2.1.4, page 2972), and as shown by Lloyd et al. (1996), recycling at this level is minimal (<5%).

**BGD**

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The following sentence was added to section 2.2.4 on Isotopic composition of leaves  $\delta^{13}\text{C}$ :

'Equation 6 does not include re-fixation of soil and plant respired  $\text{CO}_2$ . However measured values of  $\delta^{13}\text{C}$  are from top of the canopy leaves and as shown by Lloyd et al. (1996), recycling at this level is minimal (<5%).'

### 3. Comment

I know that Mercado has worked with canopy light response curves under direct versus diffuse light, and I wonder why this is not an integral part of the simulation. Perhaps it is. The terms 'sunlit and shaded' as applied to leaves has no meaning when the sun is not shining, as is touched upon on page 2973. But how do we know whether the sun was shining. I don't think the solarimeters included a direct+diffuse instrument.

### Answer

The sun and shade model does include the input from direct and diffuse light on sunlit and shaded leaves, and the canopy leaf area in the sun and in the shade part of the canopy is defined using the geometry of the sun (De pury and Farquhar 1997). Lacking measurements of diffuse and direct radiation at the sites, we used simulated fields as explained in Mercado et al. (2006) using the model from Spitters et al. 1986. Again, the main scope of the study was to evaluate the model at 5 sites and to produce canopy scaling functions to be able to extrapolate to the Amazon region, so in this study we did not concentrate on the role of direct and diffuse radiation on photosynthesis at the different sites

### 4. Comment

Are the forests C-sinks? This question is avoided. Ideally, one would like to do an error analysis on the models and observations to see whether the question can be addressed.

### Answer

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This is a very relevant question, but it is really outside of the scope of this study. In this study we aimed only at simulating canopy photosynthetic uptake. To answer this question we would need to simulate and validate ecosystem respiration as well, which implies modelling resource allocation, turnover, and soil biochemistry, which could be the subject of a new study.

## 5. Comment

Limitations of soil hydraulics. My understanding from the work of Fisher at Caxi- uana is that soil hydraulics are rate-limiting, and cause stomatal closure towards the end of the day. Her work should at least be cited: it seems relevant to the basin-wide analysis.

## Answer

In page 2991, on the discussion about model evaluation, we do mention the need for greater understanding of the dynamics of water transport from the soil to the roots and leaves, i.e. inclusion of external environment (demand) and the supply of water from the roots. In this part we do refer the mechanistic approaches from Tyree, 2003 and from Buckley, 2005 that try to explain hydraulically related stomatal behaviours and now we added the reference suggested by the Referee.

## 6. Comment

Editorial/typographic errors Phosphorus is frequently misspelled Page 2969/line 6 'thus refine; should be 'thus to refine' Page 2973/line 5 'two sunlit and shaded components; is poor English as it suggests 4 components altogether. Better would be 'two components, sunlit and shaded'.

## Answer

Thanks for this. It has now been corrected.

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