Biogeosciences Discuss., 7, C2603–C2614, 2010
www.biogeosciences-discuss.net/7/C2603/2010/

Biogeosciences
Discussions

Interactive comment on " Mortality as a key driver of the spatial distribution of aboveground biomass in Amazonian forests: results from a Dynamic Vegetation Model" *by* N. Delbart et al.

N. Delbart et al.

nicolas.delbart@lsce.ipsl.fr

Received and published: 30 August 2010

Answer to Reviewer 1 Chris Huntingford. Thanks a lot for your comments, your interest and your enthusiasm on our manuscript . In the following we detail the answers to all your comments. We hope that our answers are satisfying and we are willing to adjust our modifications if you consider they are insufficient.

Reviewer comment: (a) There are quite a lot of plots, and so the main message can get lost slightly. I found it difficult at first to move between the plots whilst simultane-

ously moving around the paper. For instance, Figure 2(a) shows very clearly the over-estimate by ORCHIDEE of NPP. But then one jumps to Fig 3(a) and the opposite is true. (OK, so find in text that Fig 3a is only NPP for "above and below ground wood and to fruits", but this is still "overestimated". Figure 3a looks like a model under-estimate to me? Is there a typo around page 3105, line 11?).

Answer: To make clearer the distinction between the results on the above ground woody NPP (NPPAGW) and the total NPP, we made the following changes:

-we changed the x-label in figure 3b, and we replaced the former "branch and trunk" by "AGW", to make the link clearer to figure 2b. We also changed the figure caption to make it apparent that AGW stands for "above ground wood". We also precised in the caption when "NPP" concerns the total NPP.

- we modified the result section as follows :

"Despite overestimation of NPPAGW (Fig. 2, Table 1), total NPP (above and below ground) was found to be underestimated by 25% (Fig. 3a). This is explained by the fact that the allocation fraction to above ground wood was overestimated (Fig. 3b, Table 1) in the model compared to empirical data (Aragão et al 2009, Chave et al. 2010). Allocation to below ground wood and to fruits was also overestimated (Fig 3b-c, Table 1). By contrast, allocation to leaves was underestimated by 34%, and allocation to fine roots by 84 % in ORCHIDEE. For none of the tested parameters was there either a significant correlation or a linear regression slope that is close to 1 (Table 1), showing the model cannot reproduce the observed spatial patterns. The simulated NPPleaf+fruit is equal to 0.54 NPPAGW, whereas the ground measurements indicate that NPPleaf+fruit = 1.67 NPPAGW on average."

Reviewer comment: (b) I'd be very tempted to split up Figure 5 in to two separate plots. The reason for this is that Fig 5(a) is an important part of the modelling exercise and is adopted from elsewhere. In fact, without this, the new modelling cannot be completed. Then Figures 5(b)-5(e) are the overall model new verification, which is a separate issue. Also, by removing Fig 5(a) to a separate panel, it will also allow Fig 5(b)-(e) to be bigger. This is important – I'd like to see how the red and black plots overlay each other in more detail i.e. make 5(d)-(e) larger. It is these two sub-figures that show the new theories work!

Answer: Thanks for this comment, we agree on it and we separated the former figure 5 into figures 5 and 6.

Reviewer comment: (c) Many of the plots in the paper present, as a way of differentiating geographically, values against Longitude. I did wonder if this was implying that we should introduce a longitude-based variation in NPP and/or other parameters if we were to build a predictive model?

(d) I'd like the authors to comment a bit more on exactly what would be required to make a predictive pan-Amazon set of simulations i.e. introduce geographical variation. The authors demonstrate (very clearly) that NPP is incorrectly modelled, but even if it was correct, then AGWB is wrong. To get this correct requires the earlier discovered relation between turnover rate and NPP. So, if we assume the latter relationship is robust everywhere (??), then am I correct in thinking that GCMs would only then require new geographical variation introduced in to NPP. However, if we don't have observations of AGWB, what do we do to get this NPP variation introduced in to geochemical models of the land surface?

(e) So following on from the above, the authors suggest that we might be able to resolve the geographical uncertainties if we knew nutrient availability. Can we relate this to other work regarding phosphorus availability? Aren't these things slowly becoming available?

Answer to the last three questions: There are some clear longitudinal variations in above ground woody NPP as shown in Malhi et al 2004. The variations are due to variations in climate, type of soil and nutrient availability, and finally to the adaptation of the floristic composition to the local climates and soil types. It may possible to in-

troduce geographical variations in NPPAGW in our model, for example by introducing variations in photosynthesis efficiency parameters. This is what we would like to do by ingesting remote sensing biomass map. Nevertheless, it may also be possible to introduce geographical variations in these parameters by interpolating the NPPAGW measurements from Malhi et al 2004, but then we would get high uncertainties due to sparse distribution of the measurements (then we should also take into account the latitudinal variations that are not shown in our graphs). Another possibility may be to refine our analysis in order to consider the relationship between the NPPAGW/tresidence couple and the type of soil. This is linked to your suggestion about nutrient availaibility. Thus we re-analysed our data, and found that the NPPAGW/tresidence couple is quite determined by the soil type. We added another paragraph to the discussion, and another figure (new fig C1, former fig C1 is now removed).

New paragraph :

"Soil type is an important factor influencing NPPAGW and tresidence, as shown in Fig. C1. For example, forests with low NPPAGW and long tresidence are favoured on older oxisol, whereas forests with high NPPAGW and short tresidence are favoured on entisol. Based on ground measurements, Quesada et al. (2009) analysed the impact of soil properties on the mortality rate and on NPPAGW. The mortality rate was found essentially influenced by the soil physical properties (topography, soil depth, structure), whereas NPPAGW was found primarily driven by fertility parameters, essentially phosphorus availability. The authors proposed that AGWB gradients can be explained by the ecosystem dynamics that is essentially driven by these soil properties. In Western Amazonia, poor soil physical properties (steep slope, shallow soils) favour high mortality rate, which favours early-successional species with low wood density, whereas meanwhile the high phosphorus availability induces higher NPPAGW. On the contrary, on central Amazonia, ecosystems are less dynamic, with better soil physical properties and lower fertility inducing respectively a lower mortality rate and a lower NPPAGW. These two factors favour high wood density late-successional species, which ends up in higher AGWB. Eq. (7) is a in line with this explanation, as long as physical properties and fertility properties co-vary, which appears to be the case from the soil properties measurements reported in Quesada et al. (2010). This strong influence of soil properties could be a key issue when modelling the future evolution of Amazonian forests under a climate change scenario, as soil type may limit the floristic composition change that we suggest to model through Eq. (7). However, this may also allow deriving maps of average tresidence, NPPAGW and thus AGWB from a soil type map."

Reviewer comment: (f) I wondered if the authors might like to comment on the very large general overestimate by the models of NPP? I only mention this because there is so much interest in the "die-back" possibility for future climates. Does having in fact lower NPP values than expected bring us nearer to potential thresholds of the sort first considered by Cox et al (2000)? I'm certainly not suggesting a long excursion in to future predictions with this paper – it is not what it is about – but could we make some sort of link to Malhi, PNAS 2009 for instance?

Answer: Malhi et al. (2009) et al. show that tropical forests and savannah are established in regions that are different in terms of annual precipitation and water deficit. These observations are online with the modeling exercises by Cox et al. 2000 or Huntingford et al 2008 : the precipitation decrease in the future, the Amazonian forest could be replaced by savannahs. Our results and the observations such as in Malhi et al. 2004 show something different: highest biomass are not found for the forests with high productivity, because of their high rate of mortality. This could suggest that a future decrease in precipitation could favor species with low mortality rate. Our results are based on observations concerning only the forest ecosystems. Therefore they are about the intra-class variability within the evergreen forest "cloud" of points in figure 1 in Malhi et al. 2009, whereas dieback simulations are about the inter-class variability in the same figure. The end of the second paragraph of the discussion has then been modified:

"However, moderate and progressive decrease in precipitation may favour slow-growing species, with a low turnover rate and a high biomass. This point was ignored

in previous Amazonian forest dieback simulations (Cox et al. 2004, Huntingford et al. 2008) and could be modelled through Eq. (7). Nevertheless, as indicated by the observations of current biome spatial distribution (Malhi et al. 2009b), forest might be replaced by savannah if a large decrease in precipitations is experienced in the future in the Amazonian region."

Reviewer comment: The authors might like to make a quick sweep through the paper, tidying up a couple of notation things. For instance, where an acronym is introduced, usually describe with capitals e.g in Abstract, "Above Ground Woody Biomass (AGWB)". Check consistency everywhere through the manuscript.

Answer: We went through the manuscript again, and found a few problems like giving the AGWB acronym meaning again (for instance, page 3100). We tried to correct these things as much as we could.

Reviewer comment: Could the symbols in Fig 1 be made slightly bigger? It is difficult to differentiate between the different shapes/colours. Done.

Reviewer comment: I always think a paper looks better if units are given everywhere, even if they are obvious. Related to the above, mortality is usually in units of (/yr) i.e. fractional turnover? However, from Equation (3), for this paper it is (ton C/ha/yr). That's fine, but best to state this.

We rewrote : "The amount of biomass allocated to each organ is calculated from the following equation: NPPorgan= falloc-organ $\times$ NPP (1).

with NPPorgan and NPP expressed in mass of carbon per time unit and surface unit, hereafter in tonsC/ha/year, and falloc-organ being a dimensionless fraction ranging from 0 to 1.

At year n, AGWB is given (in mass of carbon per surface unit, hereafter in tonsC/ha) by:

AGWB(n)=AGWB(n-1) + NPPAGW(n-1) – mortality(n-1) (2) where mortality (in mass of carbon per time unit and surface unit, hereafter in tonsC/ha/year) equals

(3),

with tresidence (in years) being the time of residence of carbon in wood. Note that the inverse of tresidence is equal to the rate of mortality, i.e. the fraction of AGWB lost annually via mortality.

"

Interactive comment on Biogeosciences Discuss., 7, 3095, 2010.

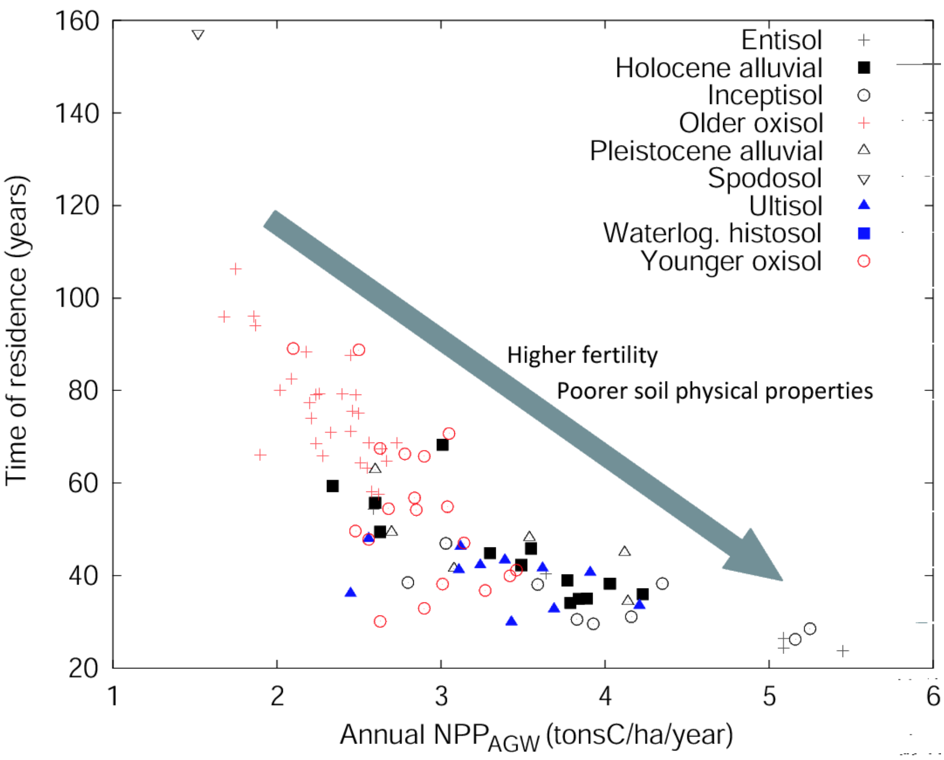

Fig. 1. figure C1

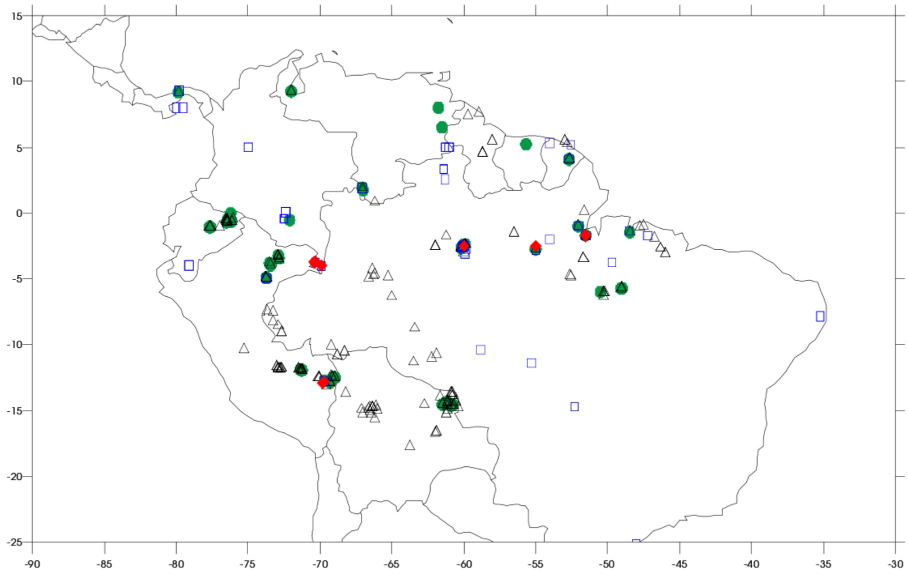

Fig. 2. figure 1

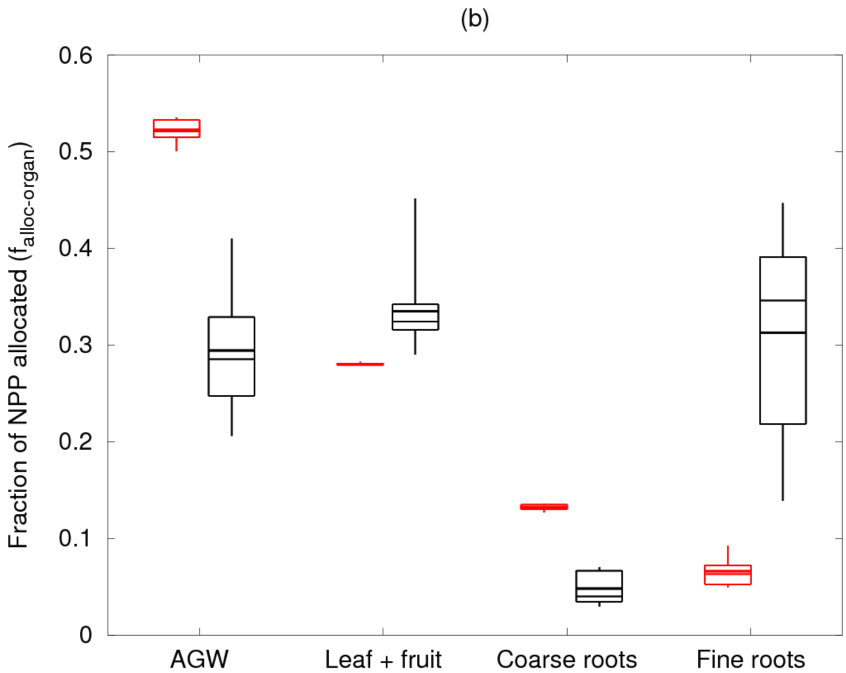

Fig. 3. figure 3b

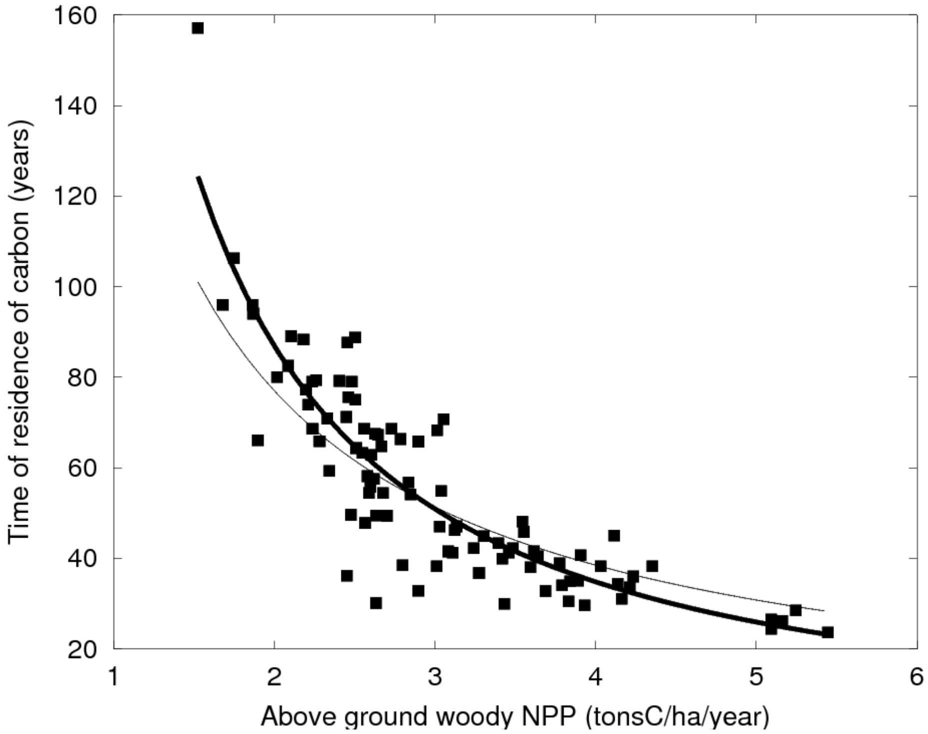

Fig. 4. figure 5

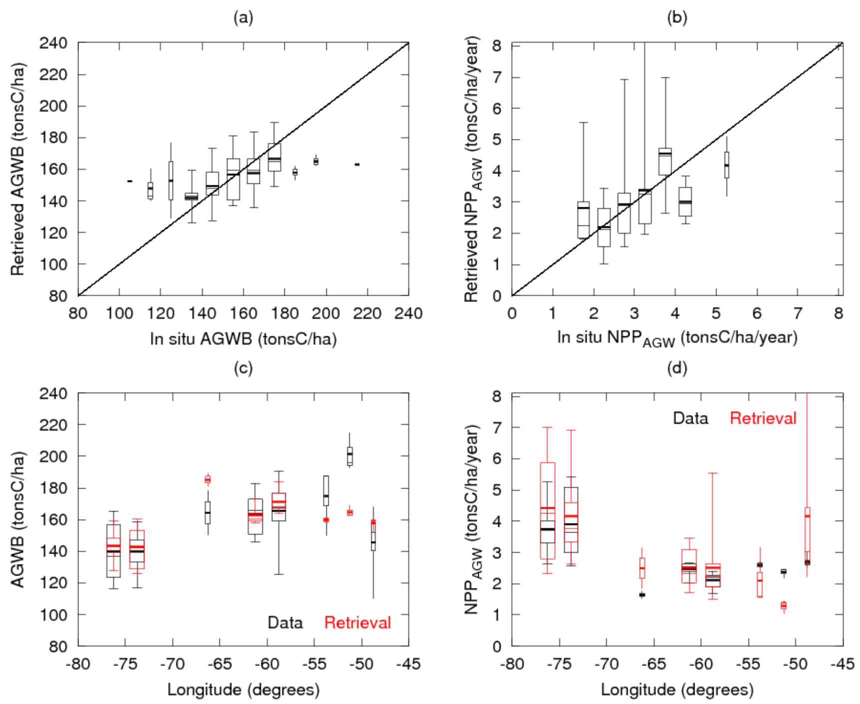

Fig. 5. figure 6