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Biogeosciences Discussions

Interactive comment on "Inferring Amazon leaf demography from satellite observations of leaf area index" *by* S. Caldararu et al.

Anonymous Referee #1

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The idea that leaf demographics that leaf demographics might potentially be inferred from seasonal variations in satellite observations of canopy greenness is intriguing, and deserves thoughtful consideration. This paper presents a detailed analysis of timeseries data on canopy greenness from Amazonia and a sophisticated inverse calibration of a simple model of LAI dynamics and finds some systematic differences between different areas of Amazonia.

My major problem with this paper is that I think the complexity of the analysis suggests a level of confidence in the data that seems at odds with the degree of confidence that past studies have shown might be attributed to these data.

The MODIS canopy greenness data (even before the LAI algorithm comes into account), and issues surrounding their interpretation, are the subject of a very large

quantity of discussion, and the potential for contamination of the wet season signal by clouds and the dry season signal by aerosols is very high. It is the case that some researchers have detected large seasonal swings in the MODIS product with declines in the dry season (e.g. Myneni et al. PNAS 2007; Huete et al. GRL 2006; Doughty and Goulden JGR 2008) but it is well known that the result, and the magnitude of the change in EVI that is detected during the dry season and/or drought periods is strongly dependant on the manner in which data are quality checked and filtered for cloud contamination, as demonstrated by Brando et al. (PNAS 2010, Fig 4a). Further to this, Asner et al. (New Phytologist 2010, figure 2) demonstrate that even with no change in underlying canopy greenness, sub-pixel level changes in cloud cover in the dry season are likely to return an increase in MODIS observed EVI. Brando et al. and Doughty and Goulden also note that changes in detected canopy greenness are more likely to be linked to flushing of new leaves than from changes in LAI per se. Long term observations of LAI at plot-scale are rare, but those from the (control plots of) Amazon drought experiments show variation of less that 1m2 m-2 (Brando et al. PNAS 2010) or no detectable seasonal cycle at all (Metcalfe et al. New Phyt 2010), so the seasonal MODIS signals used here are, in my view, still speculative and untested.

That this debate is controversial is well known, yet the authors have chosen not to justify their use of the MODIS LAI product in any way other than to indicate that the seasonal cycle is consistent with satellite observations of biogenic trace gases (pg 10391 L25). This is a very unusual argument, given that we have extremely limited understanding of the biophysical controls on biogenic flux emissions. The absence of detailed discussion of this matter is highly problematic, given the trust in the seasonal cycle which is implied by the fitting of a 9 parameter model to the observed timeseries.

In order for this data to be used for the purpose proposed here, I would want to see a much greater emphasis and effort put towards detecting the robustness of the MODIS signal to the filtering of data for cloud cover. It is apparent that those data with lower quality flags typically show lower values, but no information is given here concerning how the quality flags were used or otherwise.

In addition, should the data be demonstrated as robust, the paper would benefit from a more clear discussion of which parts of the detected seasonal cycle can be used to infer leaf turnover, and how the outcomes are dependant upon the methodologies to obtain them. Specifically, I can see how the relationship between the seasonal magnitude of LAI and the baseline might be an indicator of turnover. E.g. LAI which changes from 2 to 4 each dry season implies that half the leaves are lost every year and therefore that leaf turnover is at least 0.5 yr-1, but some further discussion of the basic principles of how evergreen leaf turnover can be estimated from LAI is needed in the current version. In figure 7, you could, for example, accompany the frequency distributions of leaf age with examples of the typical annual cycles that give rise to these outcomes, and discuss the underlying reasons for these outcomes?

Specific Comments.

P10391 L 19: The ground based studies you report here are for semi-deciduous forests, so the fact that the lose leaves in the dry season is unsurprising, but not applicable to the rest of the Amazon basin.

P10391 L 1: "under these circumstances, we expect that light availability is the primary controlling factor determining changes in leaf area" is a very strong statement given that it is only backed up by the Harper et al. paper, which is a modeling paper with data from a single site (Tapajos). It is arguably the case that deep root access minimizes dry season stress in many areas during normal dry seasons, but this statement should be defended by the observations from flux towers (except Malhi et al. JGR 1998, who clearly show a drought stress signal) and the physiology papers from the drought experiments, and the early work on deep rooting by Hodnett and Tomasella, and not just by a single modeling study. Furthermore, Xu et al (GRL 2010) show very clear correlations between the drought of 2010 and the same MODIS observations used here, so the idea that soil moisture drivers can be wholly discounted for the entire

timeseries is not well supported.

P10391 L 3: It might be the case that seasonal cycles in radiation promote the growth of extra canopy leaf area, but only if the construction costs of the leaves are outweighed by the additional photosynthetic benefit from increasing canopy cover temporarily in the growing season. This is a hypothesis to be tested, and has not been clearly demonstrated by the information presented so far.

P10392 L 22: That MODIS LAI uses assumptions about vegetation structure that depends on 'biome specific' inputs is the reason why most studies of this kind use NDVI or EVI and why MODIS LAI is not generally considered to be a robust test of vegetation models output (e.g. Randerson et al. GCB 2009; Quaife et al. remote Sensing of the Env 2008). More explanation about the MODIS main algorithm and the provenance of the 'biome specific' inputs would therefore be appropriate at this point.

10394 L 10: The use of soil moisture products that are reanalyses makes me worry that the soil moisture inputs might be subject to model assumptions that are poorly tested in the Amazon. Most studies looking into soil moisture dynamics in this region have used data to generate a cumulative precip – max ET predictor of the timing of drought stress (Brando et al. PNAS 2010, Philips et al. New Phyt 2010, Lewis et al. Science 2011, Fisher et al. 2008, Malhi et al. PNAS 2009). This avoids the possibility of model assumptions of soil texture and depth (which are largely unknown) affecting the projected soil moisture product. I do not know whether the NCAR/NCEP product suffers from this issue, but given the scarcity of actual soil moisture data in the Amazon, I would rather trust a more transparent data-driven estimate of soil moisture variability.

P10394 L19: The provenance of this equation is not clear. Can you explain how it is derived and what assumption are used to construct it?

P10395 L1: Why is the compensation point the minimum of the diffuse and direct radiation? Shouldn't it be the sum of the two, as absorbed PAR used for photosynthesis is the sum of the direct and diffuse streams?

P 10395 L 15: In a drought, the leaves that are higher than the target LAI will be respiring, and therefore detrimental to plant carbon balance, so, it could be argued that they would be dropped as well?

P 10395 L 17: At this point, it becomes apparent that the model tracks leaf age, and later on, the division of leaves into 'cohorts' is repeatedly alluded to but never explained in the main text.

P10395 L 21: Using function notation for P and L is slightly confusing here, as the location of a description of the meaning of P and L is not clear from the text, and there are no units. This needs more explanation in the main text, as opposed to the appendices.

P 10396 L 27: What do you mean by 'constrained' in this context?

P10397 L 7: The values of the parameters are reported without any error estimates, throughout the results section, but the calibration process must have returned some estimate of how well constrained the parameters were by the timeseries data. I am curious as to how well each parameter was constrained, given that there are 9 free parameters being simultaneously fitted to a single timeseries in each location. It would also be interesting to report trade-offs in the fitting of different parameters, and to explain which qualities of the timeseries (maximum, minimum, amplitude, shape) constrained the different parameters. That would make the discussion of he inferences of the model a lot more tangible.

P10397 L 10: From the figure, 'p' looks highly variable and appears to be highly than 14 days in a majority of places?

P10397 L 20: How was this equation derived? What is a cohort of leaves?

P10397 L25: What are the actual values for leaf turnover reported by these studies and where are they reported for? See also Metcalfe et al. New Phyt 2010, Malhado et al. Forest Ecol & Manag. 2008…

P10398 L18: There needs to be a reference to the appendix here, otherwise the leaf aging model is unexplained.

P10398 L 25: Are there references for the studies that have failed to predict the pattern?

P10398 L 27: Nothing is ever 'proven'. This study might support the emergence of leaves in the dry season, but that also needs to be more clearly demonstrated, and in any case the support would be based on the same MODIS data as Hutyra etc., and is therefore still the same hypothesis that has been proposed to explain the same apparent seasonal cycle.

P10399 L 8: This model has not been compared against any carbon cycle data, so it is not clear how it can be shown to have 'improved predictions of the seasonal carbon cycle'. The leaf aging algorithm changed the output of the GPP model, but this is a long way from demonstrating that it has been improved? There is no illustration that this explains the observed decrease in assimilation (nor any indication of where these data might come from that need explaining).

P10399 L 12: It is strange to cite the Bounoua paper in this paper, because the model of Bounoua is directly conflicting with the light-limited-LAI idea proposed in this paper. They assume that LAI increases as CO2 increases because down-regulation of maximum Vcmax allows redistribution of N to leaves lower in the canopy, leading to LAI values that would be extremely high, ignoring the possible impacts of light limitation. Implementation of this model would quickly disprove the conclusions of the Bounoua paper, which are unsupported by any physiological theory and directly conflict with the outcomes of CO2 fertilisation experiments.

P10400 L 24: This discussion of LAI water, targ is a bit obscure. What is it about the data that make soil moisture unimportant? Are LAI and soil moisture simply not correlated at all?

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