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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.

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## 1 General comments

### 1.1 Paragraph 3, page C4533

*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)*

It is well known that the quality of any satellite-derived data is affected by atmospheric



conditions i.e. cloud cover and aerosol loading and needs to be filtered. The MODIS LAI product already includes atmospheric corrections (Knyazikhin et al., 1999) and we have also filtered the data using the provided quality flags prior to our analysis.

*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.*

Asner and Alencar (2010) state that lower LAI values can be observed in areas with higher sub-pixel cloud cover, but they conclude that they are unable to differentiate between cloud contamination and canopy greenness in response to sunlight. Since our model predicts an increase in LAI in response to PAR we do not contradict their findings.

*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*

Several studies do indeed relate the observed increase in canopy greenness to a flush of new leaves at the top of the canopy, but a very recent paper (Samanta et al., 2012) shows that the observed increase in reflectance cannot be attributed only to the emergence of new leaves. Such a flush of leaves must be reflected, to a greater or lesser extent, in overall canopy LAI. Furthermore our model predicts an increase in leaf gain during the dry season in response to an increase in PAR, which would be consistent with this hypothesis.

*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.*

The amplitude and magnitude of ground based measurements in LAI varies widely with location and the method employed (Asner et al., 2003; Malhado et al., 2009). Furthermore, continuous, long-term records of LAI are very rare and often suffer from the same limitations as space based observations, especially if optical methods are employed (e.g LAi-2000). Flux measurements in the Amazon (e.g. Hutyra et al. (2007)) have shown an increase in GPP during the dry season, which is not explained by current models and which could indicate the absence of water stress for forests in this area. As such, ground-based studies do not either prove or disprove the seasonal cycle observed in the MODIS data. In this paper we offer a simple physical explanation for this seasonal cycle, which indeed is yet to be independently verified..

1.2 Paragraph 2, page C4534

*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.*

This was only given as an example of other unexplained processes that exhibit the same seasonal cycle and not as a justification for the use of this particular dataset.

*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.*

The main justification for using satellite based data is its spatial and temporal conti-



nuity, as we have stated in the original manuscript. The data section has now been extended to include a further discussion of LAI seasonality.

1.3 Paragraph 3, page C4534

*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.*

We were fully aware of this issue and have consequently used a strict filtering criteria adequate for the atmospheric effects associated with data quality over tropical regions. Further information about how the quality flags were used has been included in the data section.

1.4 Paragraph 2, page C4535

*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.*

Our modelling approach involves building a mechanistic model of leaf phenology which we then optimise using a flexible fitting method (the Metropolis-Hastings algorithm). We obtain parameter sets which, given the environmental variables for each location,

result in the observed seasonal cycle. In this type of inverse analysis the relationship between the data and parameter estimates is non-trivial and parameter values are affected by more than one aspect of the seasonal cycle.

*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?*

The locations for Fig. 7 are the same as those for the seasonal cycles in Fig. 4. We have now explicitly stated this in the manuscript, according to the referee's suggestion.

## 2 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.*

We have now added further references following the referee's input.

**P10391 L 1:** *[...] It is arguably the case that deep root access minimises 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.*

The Harper et al. (2010) reference is provided as a study of soil water retention in the Amazon. We have now added further references on rooting depths.



*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.*

Our model mainly refers to average dry seasons and does not necessarily reflect responses to unusually dry periods. Furthermore, our study period does not include 2010, but we have not observed any significant difference between 2005 (which was also a drought year) and the other years. A more detailed drought response is important and will be included in our future work.

**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.*

We agree with the referee that construction costs can indeed play a very important role in phenological behaviour and our model could benefit from a further construction cost component, which will be part of a future study.

**P10392 L 22:** *That MODIS LAI uses assumptions about vegetation structure that depends on 'biome specific' [...]. More explanation about the MODIS main algorithm and the provenance of the 'biome specific' inputs would therefore be appropriate at this point.*

We have included more references for a more detailed explanation of the MODIS LAI algorithm. More information is available from the MOD15 Algorithm Theoretical Basis Document (Knyazikhin et al., 1999).

**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.*

We chose to use the reanalysis soil moisture data set as other water availability metrics often include assumptions about vegetation cover and LAI to derive ET and throughfall, which would lead to a circular argument. Soil moisture data provides information about aspects such as water retention in the soil and the surface hydrology which would not necessarily be included in precipitation derived metrics. Soil moisture is also a good measure of the long term (days to weeks) available water, which is more likely to be a driver fro leaf phenology than precipitation or VPD.

More generally, our principal finding is that Amazon phenology can be explained by a simple model driven only by light. If we were to extend our model to more water sensitive areas a detailed analysis of available moisture and precipitation data sets would indeed be useful.

**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?*

This equation is derived from Beer's law as the number of leaf layers which result in a light level equal to the leaf compensation point at the bottom of the canopy (now



clarified in the text)

**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?*

We do not simply use the sum for direct and diffuse PAR because the two streams have different attenuation coefficients through the canopy and they affect sunlit and shaded leaves differently (dePury and Farquhar, 1997). Also, direct and diffuse radiation has different effects on photosynthesis (Brodersen et al., 2008). The subject of sunlit and shaded leaves and their different photosynthetic response is still unresolved and a more complex canopy representation within our model could be a further improvement.

**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?*

Within our model, the leaves that are higher than the water-limited target, as would be the case in a drought, are indeed dropped to avoid whole plant carbon and water loss. Leaves that are above the light-limited target are not dropped instantly because we consider that they would be unlikely to cause whole plant carbon starvation in the short term. Furthermore, during a drought the canopy is unlikely to have a LAI higher than the light limited target, as a drought would lead to higher levels of direct solar radiation because of reduced cloud cover which would lead to a much higher LAI target.

**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.*

A leaf cohort is simply all leaves in the same age class. A clearer explanation of how

we use leaf cohorts has now been added to the manuscript.

**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.*

The production term (P) refers to leaves gained in response to solar radiation according to Eq. 1. The loss term L refers to leaves lost as a function of soil moisture and leaf ageing. We express both production and loss process fro each time step at each location.

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

The predicted amplitude and timing of LAI are constrained by the model structure as both depend on the amplitude and timing of the input variables, in this case solar radiation. if, fro example, we calculate the maximum and minimum light-limited target LAI (Eq. 1) given maximum and minimum solar radiation we obtain a value that is independent of parameter values (in this case the compensation point $C$).

**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.*

The fitting method we use does indeed provide a full posterior distribution for all parameters which allows us to derive confidence intervals. A further figure and discussion have now been added to the manuscript.

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

The median value for the $p$ parameter is 15 days ($\pm$ 3 days).

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

We have now included a clearer explanation.

**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. . .*

The leaf lifetime values have now been included in the text.

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

Now modified accordingly.

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

We have now included further references.

**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.*

We present a simple physical model to explain the observed variation in LAI which has so far only been explained as correlations. Our model is consistent with ground based observations of litterfall, leaf lifetime and GPP seasonality.

**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 ageing 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).*

The carbon assimilation component is currently presented for illustration purposes only and an example of the possible uses of our phenology model. Future work will include coupling our model with a full-scale carbon assimilation model which would then be compared to flux measurements.

**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.*

We cite this paper as an example of a study that outlines the need for better phenology models.

**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?*

We have not analysed the statistical correlation between LAI and soil moisture. The parameters resulting after model fitting indicate that soil moisture does not affect LAI in that the estimated water limited LAI target is higher than the LAI value throughout the year. This is consistent with the fact that the peak MODIS LAI occurs during the dry season, when soil moisture is at its lowest while PAR is at its maximum.

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