Reviewer comments in *italics;* Responses in **bold**)

<u>Response to Anonymous Referee #3</u> Received and published: 25 July 2017

General Comment: *This manuscript mainly focuses on Amazon-Cerrado transitional vegetation. For this region, a mechanistic model is used to determine the effects of various processes on aboveground biomass (AGB). In particular, the effects of fire phosphorus (P) limitation, and interannual climate variability are evaluated. It is concluded that all of these effects are important, but that fire is the main driver of vegetation change along the transition. The manuscript also reports that the model simulates >80% of the spatial variability in AGB in the transition zone.*
Understanding the spatial distribution of tree biomass in the tropics is a very active area of research. The questions asked by the authors, especially in regard to P limitation, are open ones and quite worthy of investigation. It is very reasonable to approach questions about mechanisms, such as those asked by the authors, using a mechanistic model. Nevertheless, I was unconvinced by the analysis that was presented. I have major concerns about the implementation of P limitation and the statistical analysis.
The phenology scheme was not described in much detail, but could strongly influence the results. Several claims in the discussion were weakly, if at all, supported by the results.

Specific Comments

I do not think that the authors really implemented P-limitation in their model. As I understood the manuscript, simulated P dynamics do not affect vegetation biomass. Instead, the authors prescribe a map of Vmax based on a statistical regression between Vmax and soil P. As such, there is no mechanistic representation of P limitation in this model. Without a mechanistic link, I do not think it is correct to ascribe variation in AGB to variations in P. This manuscript can be improved by investigating the effects of different mechanistic implementations of P cycling and P limitation on the simulated vegetation.

Indeed, we did not implement a full P cycle model, but rather we parametrized what we believe is the most important relationship between soil P and photosynthesis. In this study, we only use the P-limitation through the linear relation developed by Castanho et al. (2013) to evaluate the biomass along the Amazon-Cerrado transition. The P cycle is slow, and a full representation of the soil P may be advantageous only over a time scale of several decades to centuries. In the shorter term (a few decades), starting from a known point in soil fertility as we did, seems to be a more appropriate approach. The values of P in the map influence Vmax, which mechanistically influences NPP and biomass. This has been demonstrated before by Castanho et al. (2013).
The implementation of a full P cycling and is out of the scope of this manuscript. The main objective of this manuscript is investigate the influence of P-limitation, climate and fire occurrence on AGB, and not implement P cycling.

1. *Ptotal may indeed have some positive relation with Vmax, but Equation (1) still seems problematic to me. What happens when Ptotal is very large, and vegetation is presumably no longer limited by P? This equation would say that Vmax would still increase, but surely there must be some maximum value when other factors become limiting.*

Equation (1), being an empirical equation, represents the relationship between P and V_{cmax} in the range where there are observed values. In the hypothetical case of a very high Ptotal, and a consequent very high Vmax, other bottlenecks in the model code (limitation by light or by water or by temperature) would limit gross photosynthesis. However, this is unlikely, given that the Amazon and Amazon-Cerrado transition soils are known for being nutrient poor.

More generally, I was not convinced that the most important way P affects plants is through Vmax. For example, what about maintenance of some approximate C:N:P stoichiometry, carbon costs of P acquisition, etc.?

Castanho et al. (2013) provided empirical evidence that Vmax is influenced by P-total. The other processes described by the reviewer, along with other process not described, may also be important, but they have yet to be implemented and tested. Our study, however, falls in line with the common use of parameterizations in the modeling literature, to quickly test some effects before a full and more complex model is implemented.

2. *The statistical analyses are inappropriate and do not support the conclusions. The statistical tests used by the authors are only appropriate when there is some random variable. I did not identify anything in the simulation design that could lead to a random effect (for example, some stochastic process). I recommend cutting the whole statistical analysis.*

   Response: With all due respect to the reviewer, we believe that this point is not correct. There are several sources of random variability in the data, such as (from lower to higher): spatial variability in the soils texture (in both CA and CV simulations), spatial variability in soil fertility (in PR and PG simulations), the interannual climate variability (in the CV case), and the random ignition in the fire module.

3. *The model description incomplete. Is the source code available somewhere? Exactly how does this version of the model differ from previous versions? Any new equations or new parameter values need to be documented here.*

   Response: The source code can be downloaded from http://biosfera.dea.ufv.br/en-US, clicking on models and, then, on INLAND. The INLAND project was mainly a revision of the IBIS code, through assembly and standardization of different IBIS versions, and improvements in software engineering. We used the version described by Senna et al. (2009) as starting point for INLAND. No changes in tuning were done since that paper, except the addition of the P parameterization, which was described in detail. Some of the key equations and parameterizations, however, were described by Foley et al. (1996) and Kucharik et al. (2000).

4. *The manuscript indicates (lines 120-121) that a temperature-based phenology scheme was used. But a drought phenology scheme is more appropriate for the tropics. I can imagine that the results would change dramatically if a drought phenology scheme were used. The original IBIS model had a drought phenology scheme, right? I guess that was not implemented here?*

   Response: We agree and thanks the reviewer for this commentary. That was our mistake. This sentence has been modified. The INLAND, such as IBIS, has phenology scheme based on winter-deciduous and drought-deciduous behavior of particular PFTs. For winter-deciduous plants (outside the area of study), the temperature based is used, while for tropical drought-deciduous plants, the PFTs drop their leaves during the least productive months of the year, defined in terms of the previous year's carbon balance. This phenology scheme is extensively described in Foley et al. (1996) and Kucharik et al. (2000). Lines 120-121 were changed to "The vegetation phenology module simulates the processes such as budding and senescence based on drought phenology scheme for tropical deciduous trees."

5. *I was surprised that the manuscript did not discuss alternative stable states in terms of either the AGB database or the simulations. How was the AGB database constructed, given that there may be alternative stable states? I found it remarkable that the model was able to capture 80% of the variability. Would this result indicate that the idea of alternative stable states is not really appropriate in the Amazon-Cerrado transition?*

   Response: We are not sure we understand this comment. Alternatives states in this region would arise as a consequence of climate change and antropogenic disturbance. This is clearly outside the scope of this study, which aims at representing the actual non-disturbed status of the vegetation, and attribute the processes responsible for its present status, in terms of climate variability, fire and P limitation.

Additional comments

Lines 88-89: This is too vague. A discussion of the failures would be welcome.

Response: In accordance with the referees's wishes, we included references and have now changed this paragraph to "Currently, no model has demonstrated to be able to accurately simulate the vegetation transition between Amazon and Cerrado. In general the DGVMs simulate evergreen forest along the Amazon-Cerrado border neglect savanna occurrence (Botta and Foley, 2002; Bond et al., 2005; Salazar et al., 2007; Smith et al., 2014). This difficulty may be due to absence or not well represented disturbances such as fire, nutritional limitation or soil proprieties. Thus, we need a better understanding of the drivers on transitional vegetation to determine the parameters and establish relations between the environmental and transitional vegetation physiognomies."

Line 155: This equation needs more description. Is the same Vmax assigned to all PFTs? Is it meters square of leaf area or meters squared of ground?

Response: There is reference to equation and specified the PFTs limited by phosphorus on the manuscript. Please check Line 157: "This equation has been developed by Castanho et al. (2013) based on data for tropical evergreen and deciduous trees, and is applied only to these two PFTs in the model."

Lines 269-276: It is arbitrary as to whether there are increases or decreases. Whether there is an increase or a decrease depends on the chosen baseline. Also on this paragraph, I am wondering whether tree biomass simply follows soil P?

Response: The increase or decrease biomass in PG and PR was evaluated from PC using the subtraction between the simulations: (CV+PR)−(CV+PC) and (CV+PG)–(CV+PC). These differences represents the isolated effect of P limitations on tree biomass along the simulated area.

Line 280: Note that the word "inflammable" actually means easily ignited.

Response: We agree with the reviewer. This sentence was changed to "The small or null fire effect in the Central Amazon rainforest is related the greater water availability on the Amazonia, which makes the forest naturally not flammable as well as a gradient towards seasonally dryer climate increases the intensity and magnitude of fire effects towards the Cerrado (Figure 4d)."

Lines 278-281: Not justified. Where is water availability shown, and how is it defined?

Response: We agree with the reviewer. This sentence was changed to "The small or null fire effect in the Central Amazon rainforest is related the greater water availability on the Amazonia, which makes the forest naturally not flammable as well as a gradient towards seasonally dryer climate increases the intensity and magnitude of fire effects towards the Cerrado (Figure 4d)."

Lines 300-302: Why does fire cause LAI to increase?

Response: In INLAND the vegetation structure is represented by two layers: upper and lower canopy. The fire occurrence reduces the upper canopy decreasing the trees LAI (LAI upper), opening the canopy, and leading to more luminosity available to the lower layer, and consequently increasing the photosynthesis rates by grasses (increase of LAI lower). In general, the LAI lower increases quickly after the fire occurrence contributing with the increases of the LAI total that represents the sum of the two layers.

Line 409-411: There are exceptions (Goll et al, Yang et al).

Response: We agree to the reviewer. This sentence was changed.

Lines 416-424: This paragraph seems too speculative given the model results.

Response: We excluded this paragraph.

Lines 425-428: This is also not strongly supported.

Response: This is supported by Tables 3, 4 and 5.

Lines 460-462: But does it help explain the spatial variability?

Response: Yes. Patterns along the eastern part of the transects are determined by fire, which is less important in the western part of the transects.

Line 502: Showing the climate data would make this point more convincing.

Response: This is well known in the literature, but anyway. We provide three figures in this response to clarify this point to the reviewer. The seasonality of precipitation for Amazon and Cerrado biomes used in this study is shown in Figure R1. The dry season duration is larger in the Cerrado domain (Figure R1a) than in the Amazonia domain (Figure R1b). In the Cerrado, dry season comprise a period of about 6 months with little or no rain.

Spatial variability of precipitation and temperature are shown in Figures R2 and R3, respectively. These figures plot the difference between the average of 1999-2008 (a subset of CV) and CA (average of 1961-1990) highlighting the spatial variability of these climate variable throughout the study area. Comparing the interannual climate variability with the average climate, precipitation decreases (Figure R2) and temperature increases up 1.5°C (Figure R3) in central Cerrado in October, November, December and January. The lower precipitation associated with higher temperatures in central Cerrado can explain a low biomass, low LAI vegetation and savanna existence without fire disturbance. Note that this is a 10-year subset of the CV database. The actual year-to-year variations present much more intense amplitudes.

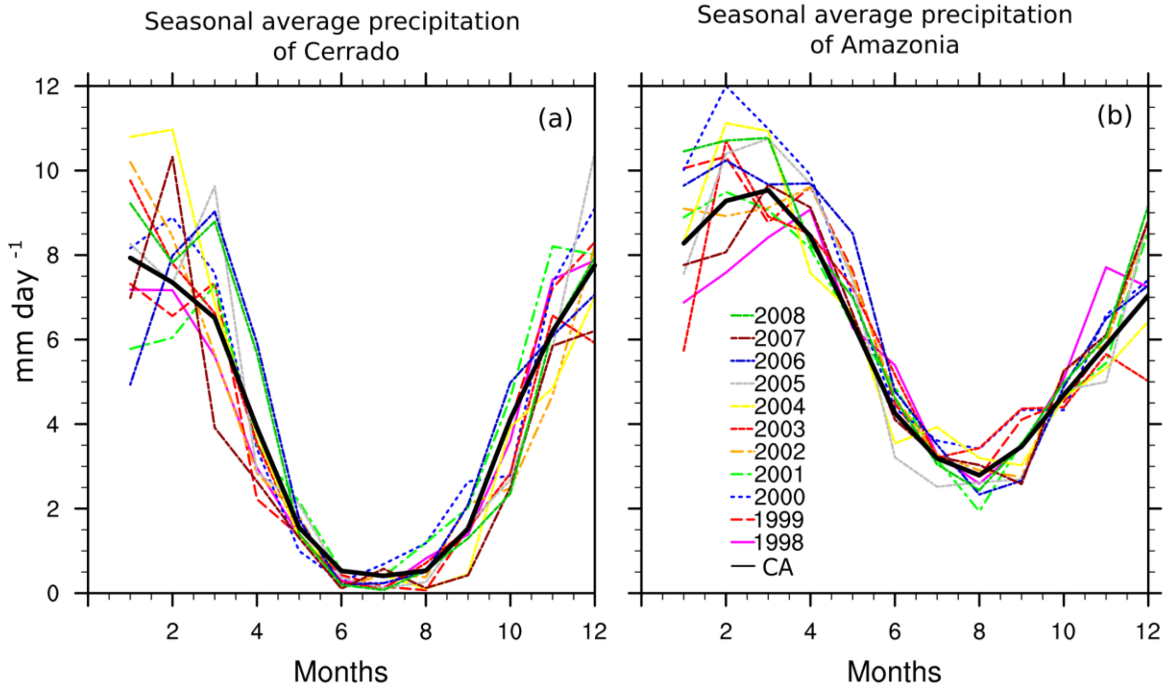

Figure R1. Seasonal of precipitation for Amazon and Cerrado domains for average climate (CA) - black line - and the last ten years of interannual- climate variability (CV) – color lines.

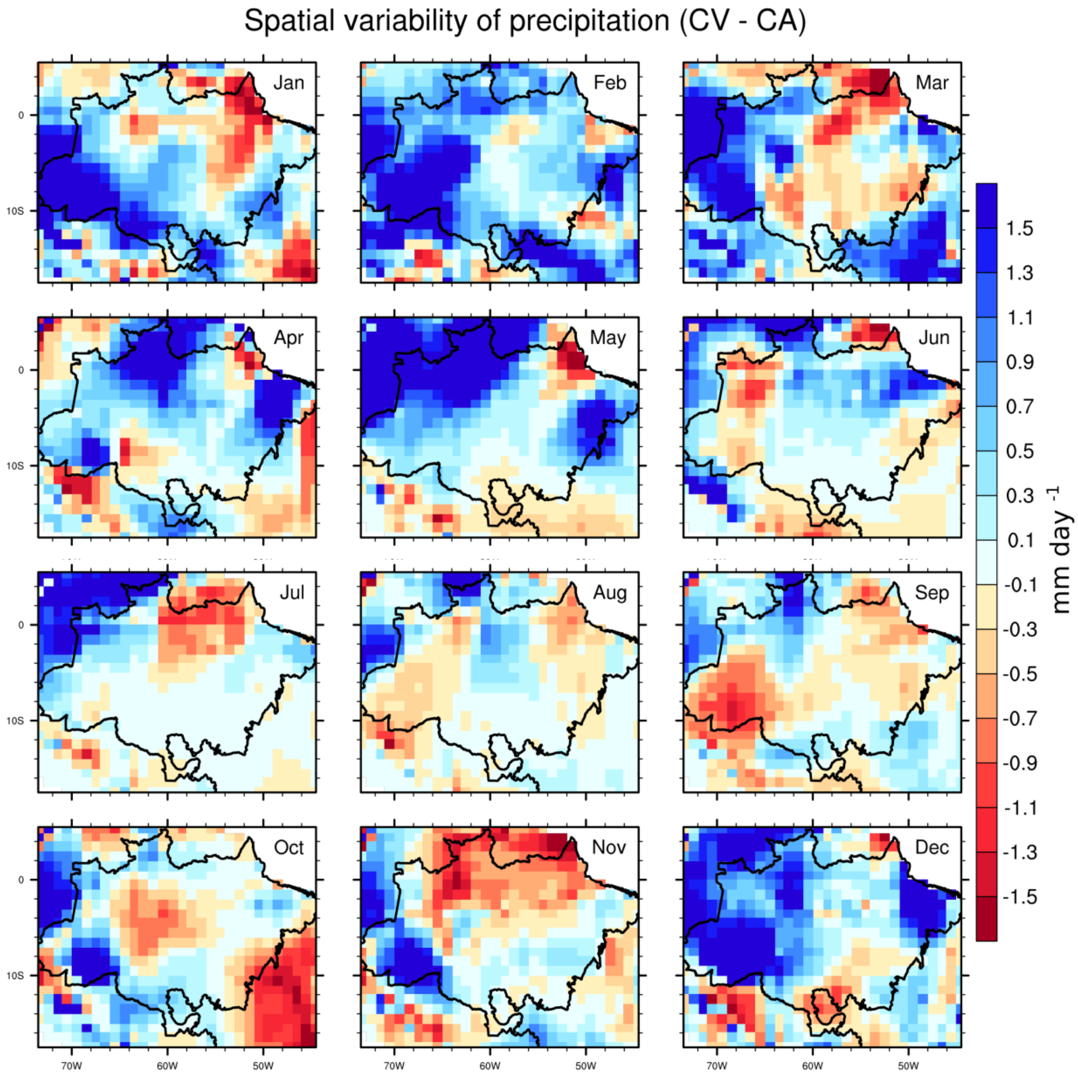

Figure R2. Spatial variability of precipitation for study area considering the average of the last 10 years of CV (1999-2008) and average climate CA (1961-1990).

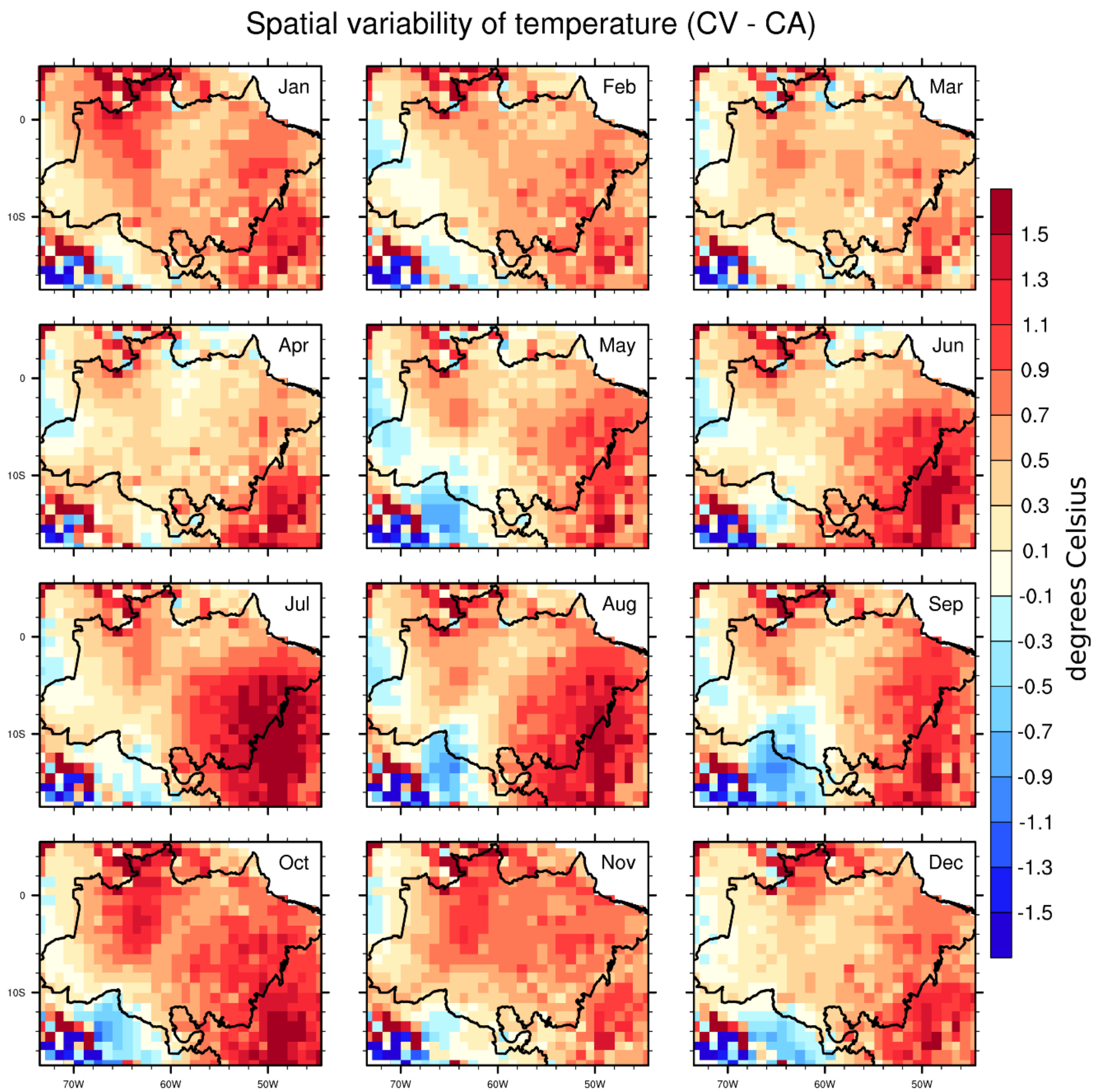

Figure R3. Spatial variability of temperatures for the study area considering the average of the last 10 years of CV (1999-2008) and average climate CA (1961-1990).

References:

Bond, W. J., Woodward, F. I. and Midgley, G. F.: The global distribution of ecosystems in a world without fire, New Phytol., 165(2), 525–538, doi:10.1111/j.1469-8137.2004.01252.x, 2005

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an individual-based dynamic vegetation model, Biogeosciences, 11(7), 2027–2054, doi:10.5194/bg-11-2027-2014, 2014