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Biogeosciences Discussions

Interactive comment on "Influence of climate variability, fire and phosphorus limitation on the vegetation structure and dynamics in the Amazon-Cerrado border" *by* Emily Ane Dionizio da Silva et al.

Anonymous Referee #3

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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

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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 1. 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.

2. 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. 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.?

3. 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.

4. 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.

5. 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?

6. 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?

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

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?

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?

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

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

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

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

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

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

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

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

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