Biogeosciences Discuss.,
doi:10.5194/bg-2016-510-RC2, 2017

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 #2

Received and published: 3 February 2017

This study analyses the INLAND vegetation model with the purpose of discerning the relative impacts of fire, empirical phosphorus limitation and climate variability on predictions of ecosystem structure across forest-cerrado transitions in S. America.

In common with reviewer #1, I think that the text requires careful editing, particularly for (mostly minor but widespread) grammatical errors.

The model description is extremely vague, and and parameterization and calibration carried out prior to these experiments is omitted. I am skeptical that the model simply performed reasonably the first time that it was run. What uncertainties do you need

to grapple with before the model output falls within the sensible range? Without this information, the reader might assume that goodness-of-fit tests between the models and the observations might have been substantially affected by undisclosed model calibration.

Given that, I find the comparison of different influences over model outputs (fire, phosphorus, etc.) to be somewhat predictable and not very interesting. The reliance on statistical tests is distracting. A better analysis of the consequences of and the uncertainty in the impacts would be much more useful.

The discussion section contains numerous logical errors confusing the output of the model and the behaviour of the ecosystem in real life. Until these are rectified, I do not think that this paper is of sufficiently high scientific standard to be published.

Specific Comments.

L112: Is Kucharik (200) really the most recent reference for the INLAND model? I'm fairly sure this isn't the case. To be repeatable, this model description needs to provide at a minimum references to the most recent version, along with specific descriptions of the model equations and parameters if they have been modified since the last publication. Many EGU journals stipulate that directions to the code used are also included. I do not know if this applies to BGD, but it would be good practice to do so.

L115: Does this mean the vegetation types compete for light, or for water & nutrients? The mechanisms of competition and dynamic vegetation are a critical part of a model of this type. I am surprised you skipped over this so briefly.

L116: This classification seems arbitrary to me. Why not just report the LAI numbers?

L122: Again, I'm not sure of the need for this cross-referencing of PFTs, 'vegetation types' (why not ecosystem type - that would be less confusing) and then names for the ecosystems. The purpose of a mechanistic model is to describe the system quantitatively and in multiple dimensions. Introducing a simplistic written classification scheme does not seem. to add any extra information.

L129: How is it similar to Century and how is it different? Small differences can be important in dynamical non-linear models.

L136: From where did this relationship between P and Vcmax arise? Is it sensible to use is across this biome? More detail is needed in addition to giving the reference, given how central this relationship is to the rest of the analysis.

L140: Again, how it is similar to CTEM? How dos the arbitrary ignition scheme work? If this is covered later in the text, it should be referenced here.

L145: Why bring up the two options if only CTEM is used in this study?

L185-192: This description seems more like a discussion than methods. Also, can you clarify the impact of land use on these transitions?

L194-197: I'm not sure what point you are trying to make here.

L212: This description of the model experiments needs cleaning up. Only the P limitation scenarios seems to have label (PC, PR, etc.) and what the combinations are is not discussed at all in the text, nor are the number of scenarios, etc.

L220: It is not yet clear how the model distinguishes upper and lower canopy LAI? Therefore this distinction is not useful yet as a diagnostic.

L235: Given these are deterministic model outputs, why conduct these statistic tests? Only one instance of the atmospheric forcing, boundary conditions, parameters, and model structure is sampled, so what does it tell you if the difference between one model run or another is 'significant'? This might make sense if applied to ensembles of runs, but to compare one run against another it seems inappropriate.

L275: This is over-stating the conclusions of the model. No real evidence is presented here that it correctly simulates the complex biophysics of forest flammability, so to draw this conclusion (that the model 'shows that the Amazonia(n) forest is naturally

inflammable) is not defensible.

L279: It seems strange to me that, in the absence of a detailed illustration that the model functions appropriately in these regions, there is no investigation of any type of within-model variability, and the structure and parameterizations of the model are assumed to be fixed. I see that this study aims to look at large modifications in model scope, but I find it unusual that no other model features are brought into question at all, particularly with regard to the strength of the conclusions.

L309: My reading of figure 5 is that the full model, with all elements, under-predicts biomass significantly over much of the transition region (transect 1 and 4 in particular). Table 7 only presents correlations and not biases, so this feature is glossed over.

L313: How does this finding relate to those of the Senna and Castanho studies? This is too vague a reference.

L320: This is a highly complex system and biases can and do arise from a huge number of sources. It is not necessarily a local problem, nor anything to do with moisture stress - those are both unfounded speculations.

L337: These conclusions - that phosphorus limitation and fire tend to reduce vegetation biomass, are pretty self-evident and not very interesting.

L339: The word 'robust' here is problematic. The model does not show deviate through time in these fields, but 'robust' is normally used to describe a simulation which is physically plausible, and I don't think that applies here necessarily.

L363: Again, changing the model drastically 'led to significantly different average biomass' is not a very interesting conclusion from a piece of science. I do not think there is any debate about whether fire reduces forest biomass where it occurs, nor whether introducing a universally lower Vcmax might reduce vegetation productivity?

L369: Is climatic inter-annual variability in the CRU dataset realistic? There are other climate reanalyses that one might test it against, as well as station-level meteorological records. Given the incompleteness of met station data across this domain, it would seem likely that it underestimates variability somewhat.

L372: This is a very old reference for this very active field.

L413: How is the adaptation of savanna species to P-limitation represented in the model? As far as I can tell, the impact of P on Vcmax was universal and not PFT specific?

L426: These outputs do not actually show that understanding phosphorus limitation is associated with reliability of databases, it just shows that the databases are different. An alternative model structure that is not so sensitive to overall soil P, for example, might conclude that the database discrepancy doesn't matter? That is a hypothetical example, but the logic of this sentence is unconvincing.

L432: References to the state of the art in nutrient cycle modeling should be included here.

L444: This logic (it is clear that phosphorus has a significant effect on woody biomass) is also unconvincing. It simply shows that the (simplistic) model predicts this, not that it happens in real life.

L447: Again, this simply shows that this fire model does not burn the intact forest, and this cannot be used to conclusively state anything about real intact forest.

L459: Are the physiological differences between cerrado and other vegetation types depicted in the CTEM model? Again, the sparse model description does not allow this to be determined.

L491: Given that there is no indication of how the parameterization for rainforest vegetation came to be in the first instance, one cannot say whether the P limitation should necessarily be an improvement. LAI in biosphere models can be modified trivially by changing the leaf lifespan and/or specific leaf area or leaf allocation scheme. All of these features are variable in observational datasets, and so the initial LAI predictions

can, I am pretty certain, be modified massively. Whether the model over or under-predicts LAI in the first instance is therefore a matter of parameter choice, and therefore whether the P limitation improves or degrades the model is also a feature of that choice.

L519: You predict that the vegetation distribution is affected by these things, not observe.

L540: This is an extraordinarily grandiose and unneeded claim. I'm pretty sure that, for example, Levine et al. (2016) might disagree.

L555: Bringing up the need for greater constraint on the uncertain model parameters at this point seems a bit too-little too-late.

Figure 1) I don't see how the transects are dilineated in this figure?

Figure 2) Definition of 'new' is ambiguous in the legend. As is the use of the '-' sign to denote PR and PG. Unclear if it means 'minus' or not.

Figure 3) What is figure b? It doesn't say in the legend.

Table 7: Why only correlation coefficients and not also biases?

Levine, Naomi M., et al. "Ecosystem heterogeneity determines the ecological resilience of the Amazon to climate change." Proceedings of the National Academy of Sciences 113.3 (2016): 793-797.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-510, 2016.