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Biogeosciences Discussions

Interactive comment on "Regional and large-scale patterns in Amazon forest structure and function are mediated by variations in soil physical and chemical properties" *by* C. A. Quesada et al.

Anonymous Referee #3

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This paper explores the relationships between soil properties and aboveground forest biomass, biomass production rate, and individual tree turnover times across the Amazon Basin. It uses the large data set for soil physical and chemical properties that are reported in a related paper (the first one submitted in this set of three) and looks for correlations to assess relationship between various forest attributes and derived indices that summarize soil physical characteristics together (e.g. soil depth, slope,). Overall the work demonstrates the importance of physical soil characteristics in defining vegetation structure and dynamics. A second major point of the paper is the need for correction of correlation statistics if there is autocorrelation with spatial distribution.

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Interactive Discussion
Discussion Paper

Overall, the soils are a valuable data set as are the spatial patterns of aboveground properties and the derived relationships between vegetation and soils across the Amazon region. However, there are some major points that need to be resolved to make the paper complete and publishable. I suggest specifically a reorganization of the Methods and Results sections that include how the major independent variables discussed here are derived (e.g. from primary data collected) 8211; see specifics for what this means in the comments below.

First, and most important, there is no description in the methods section of how the 8216;independent8217; variables in this study were derived (these are turnover, AGP and AGP rate). Earlier papers are referenced (as for the soil methods), and contain the necessarily details, but nonetheless the reader here deserves some description as the methods may influence the results. For example, was a single allometric equation used to derive biomass across the entire basin, or were site- or region-specific allometries used? Was the information on density variation part of the calculation of AGB? If so, how important was its variation compared to other components (e.g. mean stem diameter, mean tree height?). For AGB production rate 8211; was this based on recruitment as well as mortality or is the stand assumed to be at steady state (recruitment = mortality). Did the treatment of standing dead versus fallen dead trees differ (since the authors make this distinction later in the paper).

Second, variables like turnover time (but also AGB) are derived from other, directly measured variables and distributions may be complicated by interactions among them. For example, turnover (I am assuming this is stem turnover and not biomass turnover, though the authors don8217;t say) is presumably calculated from the number of individuals per hectare and their mortality rate (individuals/ha/year). One can imagine a basin-wide gradient in turnover arising from (in one extreme) constant mortality rates and variable stand densities or (in the other extreme) constant stand densities and variable mortality rates. The authors have written most of the discussion as if mortality rates are responsible for all of the variation in turnover across the landscape, but they

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do not give any evidence that stand density variations are not also varying across the basin. It would be nice to know. Similarly, AGB and AGB production are derived from measures of stem diameter and diameter increment, and perhaps height (these could be given as means or medians or both). Density is already treated separately, though we are not informed as to whether it is used to calculate AGB. Would it not be better to give the relationships to the primary (measured) variables first? This gives more meaning to the relationships to the derived variables.

More detailed comments.

Abstract. Considerable importance is given in the text to the need for correcting regressions for spatial autocorrelation. This is not reflected at all in the abstract 8211; especially the fact that the importance of different factors change with the analysis. Perhaps it is worth including a sentence on this.

Line 17. A 8216;new hypothesis8217; (line 17). I think the idea of self maintaining forest feedback mechanisms initiated by edaphic conditions is quite old - what is 8216;new8217; here is the evidence that supports this hypothesis.

Introduction. Line 23. Perhaps a brief indication of what the authors mean when they say 8216;soil fertility8217; would be useful (sum of base cations?). This term is used a lot in the paper and can mean different things to different communities.

Methods.

Lines 3-5. Again, no methods are reported for how data for turnover, AGB and AGB production were obtained, even though data are apparently updated from previous papers. In this case, there is no mention of what kind of error is associated with the turnover rates given (to two significant figures!) 8211; and whether these are multiyear averages that contain information on interannual variation (which at one site is probably at least as big as a factor of 2, given recent Phillips et al Science paper on the effect of the 2005 drought?).

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Line 12. Tree mortality and turnover rates. The authors seem to use these terms interchangeably, implying that all of the variation in turnover rates is due to mortality 8211; but the reader is given no evidence that this is true (see comments above).

Line 25. 8216;physically adverse soil conditions8217;. This is the first time this term is used in this paper, and perhaps it requires some definition (e.g. soils in which physical properties limit root growth, rather than chemical properties that limit access to nutrients?) Geographical associations and spatial autocorrelation. There authors may wish to cite papers by Holmes et al. (Biogeochemistry (2005) 74: 1738211;203 and GLOBAL BIOGEOCHEMICAL CYCLES, VOL. 20, GB3004, doi:10.1029/2005GB002507, 2006) as examples where recent studies have dealt with spatial distribution of soil properties (e.g. for mapping soil C change with land use at large spatial scales). These papers point out how 8220;the magnitude and sign of correlations among soil properties changed with scale, indicating major shifts in distribution and soil dynamics depending on the scale of observation and analysis8221; (from the abstract of Holmes et al.. 2006).

Page 4000, last line 8220;Moran8217;s correlograms8221; 8211; Is there a citation for this?)

Page 4007. It should be explicitly noted somewhere in the paper that AGB gain is not necessarily linearly related to variation in overall net primary productivity. In the central Amazon, leaf litterfall is larger than AGB gain as an overall flux 8211; perhaps the ratio of biomass to leaves:stems:roots varies across the Amazon and that is one reason for the trends in AGB gain (whereas NPP could in theory be constant). The authors do not necessarily say anything wrong, but the reader might make an incorrect assumption if this is not pointed out.

Page 4008. AGB divided by AGB-gain is another measure of turnover (biomass turnover). It would be interesting to know how biomass turnover relates to individual stem turnover (which I am assuming is the turnover used in the rest of the paper,

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given the emphasis on mortality rates?) across the basin.

Page 4009. Wood density correlations are introduced only in the discussion. We are not told whether or not these data are used in the calculation of AGB, only that it explains variations in AGB. It would be cleaner to list all of the 8216;independent variables8217; 8211; wood density, stem density, mortality rate, mean diameter, height, diameter increment and briefly describe the methods used to measure them, then the derived variables 8211; AGB, AGB-increment, stem turnover. That way it is a little more clear what factors underlying the variability in the derived variables comes from. For example, AGB is a function of stem density, diameter, height (maybe not available?), and density. Discussion about what underlying factors cause variations in AGB would follow more logically if the other variables were discussed first.

Page 4011. I am not quite sure what the authors mean when they say (lines 13-15) that 8216;no relationship was found with any edaphic variable when spatial filters were applied8217;. If (for example) P is correlated with space because of the underlying distribution of bedrock across the basin, does this mean P can not be said to be controlling vegetation characteristics or does it mean that you cannot say P is controlling because it correlates with space? I guess what is puzzling is the 8216;wrong in principle8217; statement in line 19. Again, the results of Holmes et al. (see above for citations) may be worth citing here, since I think the authors are finding some of the same things.

Page 4012. The discussion of 8216;standing dead8217; is difficult to understand given that the authors have not said how they derive mortality in the first place (see general comments above). Presumably they counted standing dead in mortality.

Page 4015. 8220;anoxic conditions8221; . Is this based on data from 0-30cm soil depth, or the Indices that are obviously calculated using the complete soil column information?

Pages 4016-7. Nothing is said here about the possibility of changes in allocation (leaves and roots versus stems) as being a potential reason for variation in AGB/AGB

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production across the gradient 8211; clearly that is an additional 8216;growth strategy8217;

Page 4019. The missing link in the discussion of kinds of gaps here are data that show the types of gaps are distributed according to soil physical properties 8211; as noted above, these are introduced after the methods/results sections and there is not even a citation.

Interactive comment on Biogeosciences Discuss., 6, 3993, 2009.