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Biogeosciences Discussions

Interactive comment on "Mapping landscape scale variations of forest structure, biomass, and productivity in Amazonia" *by* S. Saatchi et al.

Anonymous Referee #2

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General comments:

This paper compiled information on vegetation structure and productivity from 226 RAINFOR plots and extrapolated these results to the Amazon basin with the aid of remote sensing techniques. Their analysis confirmed and mapped previously published results from Malhi et al., (2004; 2006) and Baker, et al. (2004) with respect to vegetation structure and productivity in Amazonia, but also introduced new distribution maps for the basal area of large trees and palms, which are very interesting. The paper also intended to evaluate the importance of soil type and precipitation on the vegetation parameters. However, the paper needs some work to properly describe the effect of soils on forest structure and productivity. Soil classes as they are now are not very informative and the authors are loosing a great opportunity to develop a nice work in

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relation to soils. This is detailed in the specific comments below.

Specific comments:

- Figure 4. This being the “soil map of Amazonia” I suggest that the map should be limited to the Amazon instead of covering half of South-America and Caribbean. Perhaps the authors could consider using the Amazon border definition found in Soares-Filho et al., (2006).

- Figure 4 and subsequent analysis in pages 5483-5486. The choice of following the soil class scheme used by Sombroek (2000) is problematic. Basically, it groups together soils that are completely different in terms of both chemistry and physics (i.e. Class 1 groups Podzols/Arenosols with Regosols, Class 4 groups Acrisols and Cambisols, and Class 6 groups Acrisols, Plinthosols, Gleysols, Luvisols and Histosols!) while separate soils that are not different in chemistry and physics at all (“old” and “young” Ferralsols). This creates some confusion and lead to information loss. It assumes that soils evolving in a similar geomorphological area would have similar chemical and physical properties, this despite of occupying totally different soil classes, which is simply not true. Despite of geological history, soils amalgamated in Sombroek’s classes have evolved in totally different routes and will certainly influence vegetation in very distinct ways (see Quesada et al. (2009a), in the same special issue). Therefore, I suggest that the authors should consider revising this map and subsequent analysis by using proper WRB soil classes as available in the SOTERLAC and EMBRAPA databases. In addition, the analysis of soil-vegetation relationships as presented here are mostly a geographical representation of results published by Malhi et al., in 2004. What would be really interesting to see is a mapping of relevant soil attributes such as phosphorus and sum of bases, which in turn could be related to forest structure and productivity (these forests are certainly limited by phosphorus). This could be done by associating these attributes to WRB soil classes

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and I understand that the necessary data is already available for the RAINFOR network. This would be really novel and perhaps would result in a much better picture of the basin wide relationships between soil properties and productivity, wood density and forest structure.

- The authors often relate their study parameters (i.e. Fraction of palms, wood density, etc) with remote sensing layers (MODIS maximum LAI, STRM surface ruggedness etc) however their meaning are not straight forward to readers which are not familiar with remote sensing. The authors also give little or no discussion about the ecological meaning of these parameters (in fact thet are not even mentioned in the discussion), thus I suggest that more effort in discussing these variables should be made in favour of broader audiences.

- Discussion, page 5483 line 7. The authors say that there are areas in the Amazon where above ground biomass is not related to either basal area or wood density. It would be great to have some more detail on this matter, perhaps indicating its geographic distribution and for which reason the authors believe this occurs.

- Discussion, page 5485. The authors say that in general wood density is lower in more fertile soils and higher in infertile ones, and that is correct. However, they were not able to show this relationship with the soil classes as used here. Again, this occurs due to mixing totally different soils into the same class, which results in a bad averaging of wood density per soil class. This is made evident in the RAINFOR dataset itself. For instance, average wood density ranges from 0.49 in Fluvisols, 0.54 in Alisols, 0.58 in Cambisols and Plinthosols, 0.63 in Acrisols, 0.64 in Ferralsols and 0.68 in Podzols. Thus showing clear relationship with soil type and weathering degree once soil classes are properly separated. Again, I suggest the authors should consider doing this analysis again, this time using WRB soil types.

- Discussion page 5486, line 3. The authors suggest that soil texture and cations

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may be important in determining forest productivity. Although this may be true in some places, limitation of forest productivity by soil phosphorus has been shown to account for a great proportion of variation in forest productivity in the Amazon and abroad (Quesada et al., 2009; Paoli and Curran, 2007; Silver, 1994). This is another reason why forest structure and productivity should be overlaid on soil P maps.

Minor comments:

Page 5470 line 18. Cambisols and Acrisols in the WRB taxonomy are equivalent to Inceptsols and Ultisols in the US. Soil Taxonomy

Page 5484 line 2. “In general, the poorest soils are found in central and eastern Amazonia and the richer soils are in the west” Is there a reference for this? It is not shown in your data.

Page 5486. line 17. Missing word after ”..and standard deviation of..”?

Interactive comment on Biogeosciences Discuss., 6, 5461, 2009.