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Biogeosciences Discussions

Interactive comment on "Controls over aboveground forest carbon density on Barro Colorado Island, Panama" *by* J. Mascaro et al.

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General Comments:

"Controls over ... carbon density on [BCI]" presents an aerial lidar-derived regional estimate of aboveground carbon stocks and carbon density (ACD) as well as spatial structure in ACD. Furthermore, this paper addresses the question of controls over the spatial structure of ACD on Barro Colorado Island; factors addressed can be grouped into physiography and land-use history categories. In the physiography category maps of continuous slope estimates and categorical parent materials and soil type/textural variables are considered while land-use history–available through the long record of research at BCI–includes regions of old growth and relatively old second growth forest (two categories spanning 80 - 130yrs). Results show that both basic categories are

important for the distribution of aboveground biomass on the island, with slope followed by forest age and soil categories predicting up to 30% of variability in ACD (at the largest scale of spatial aggregation considered).

The paper is well structured and clearly presented. The findings that slope, soil factors, and stand age are related to ACD over this spatial extent are novel and important and have not been possible before the development of lidar for this application.

Specific Comments:

In some tropical regions, e.g., the central Amazon, the height and seasonal dynamics of the local water table may be important to forest structure and function. In the extreme these soil hydrological factors may be associated with specialized riparian communities like igapo and varzea but they may also play a role in terra firme forests of different elevations and water table characteristics. While this study describes the drainage of soil types in table 2 there is relatively little discussion of potential hydrological factors. One factor that might capture something about soil hydrology on BCI is simply elevation above the lake; in the Amazon, for example, height above the nearest drainage (HAND, derived from radar, Renno et al 2008, doi:10.1016/j.rse.2008.03.018) has been correlated with the depth of the water table. Would this add predictability to the model or is there a good argument to leave out this factor? Perhaps in future studies mapping the distribution of deciduous trees and potentially lianas that might be expected to respond strongly to local hydrology–along with measurable hydrological factors–might help inform estimates of hydrological effects on ACD.

A potentially important citation for this paper is Castilho et al. 2006 "Variation in aboveground tree live biomass in a central Amazonian Forest: Effects of soil and topography." This study analyzes a series of 1ha plots arrayed on a 'mega-grid' covering 10X10km of forest. Plots are stratified by elevation–and follow elevation isocontours–in drainage-associated 'micro' topography typical of the region. Castilho et al. find that 20% of variation in ACD is related to topography and soil but they do not find a slope effect. I

am not sure where their slope plots fall in the spectrum considered by this study, some are certainly very steep but not all. I am also not sure if these slopes 'expose' bedrock to the same extent nor do I know if they are associated with the same nutrient dynamics as in BCI–the reference explicitly addresses the nutrient availabilities though.

The correlation of predictors presented in Table 5 does not seem to control for spatial structure as the rarefaction method used in the main analysis does. I do not believe, however, that the resolution of this question is central to the article nor the main conclusions of the article.

Were any interaction terms considered in the main statistical analysis? My limited knowledge of linear modeling suggest that significant correlations can influence estimates of main effects, and it seems that some of these factors might interact. The general conclusion of the paper is likely insensitive to this potential analysis issue, though.

While Agua Salud is used, along with the BCI 50ha plot, as training data for the lidar biomass prediction, it seemed that the Agua Salud sites were not considered in the analysis of ACD controls. I wonder if such 'low end' biomass and height value sites would be represented on BCI? What happens to the r2 and RMSE of the mean canopy height vs. biomass relationship if only the BCI plots are included?

Technical Corrections:

Table 5 - model 'parameters' should be changed to model 'variables.'

'Imperfect' is used to describe the drainage of shallow mottled clay in table 2. I suggest choosing a different term unless this is technical.

On pg 8832 line 19 add of between 'some' and 'these'.

Interactive comment on Biogeosciences Discuss., 7, 8817, 2010.