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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.

Anonymous Referee #2

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"Controls over aboveground forest carbon density on Barro Colorado Island, Panama" By; Mascaro et al. General Comments: The paper discusses different controls on the aboveground biomass carbon density (ACD) in Barro Colorado island. The biomass density is derived from airborne lidar measurements calibrated with height-biomass allometry developed from various plot data in the study area and mapped over the entire island at different spatial resolution. The controlling variables are slope angle, soil texture, bedrock, and forest age. The paper has the potential of providing interesting information about the landscape variations of biomass and approaches that can be implemented in other regions. However, the paper suffers from major methodological errors in analyzing the lidar data that may impact the magnitude of biomass distribution and probably change the entire results and findings of the paper. Detail Comments: 1. There is a body of literature on the biotic and abiotic factors that impact forest structure,

such as size-frequency distribution of trees, and spatial organization of structure, gap size and dynamics, and wood density, all variables that define forest biomass variations. It is important that the authors refer to these literature and highlight their findings as some are relevant and others may be different and in contradiction to what they have found in their analysis. I will point these out later. It is true that most of these results (e.g. Clark & Clark, 2000 ; Castiilho et al., 2006; Chave et al., 2001; Laurance et al., 1999; Stagen et al., 2009; etc.) are based on plot data. However, it is not clear why plots well-designed across the soil, elevation, slope, or other factors may not be the best way to address this problem. I will try to explain this in other points. 2. The authors show how biomass varies over the landscape and how the variations can be explained by several variables. However, there is hardly any ecological, geological, and historical reasons are tested or discussed for these variations. For example, the authors mention that the reason biomass is larger on steeper slopes is because erosion rates may exceed weathering rates. However, they do not produce any evidence or show any data to support this. Therefore, it could not be used a sentence in the abstract. One can suggest this as a potential hypothesis in the discussion.

3. Mapping forest Biomass from Lidar data has several methodological problems that need to be addressed. It appears that the authors treated this part of the problem casually. However, this is an important part of the paper as they claim that the Lidar captures the biomass and its variations better than well-designed plots. Here are some of my main concerns: a. Height metrics calculated at 5 m resolution is meaningless when calculating forest biomass. The auto-correlation length of forest structure variations in BCI or similar forests is between 10-20 m about the average size of crowns. Waveforms or height metrics are meaningful if they are calculated beyond the scale of canopy gap dynamics, such as 0.25. It is important to analyze the height metrics and biomass at a scale where the structure is stable. In this study, the calibration plots are much larger than 5 m to start with, so I do not understand why the authors calculated the waveforms at 5 x 5 m resolutions. b. As mentioned above, biomass estimation are better at a scale where both the biomass and the allometries are stable. This scale

varies in forests but they are greater than 50 m resolution (0.25 ha), much larger than the average crown size or the scale of canopy gap dynamics. The analysis performed by Chave et al., have shown the scale where biomass is beyond the scale of gap dynamics and stable in BCI. At small scales (5-20 m) the forest structure and biomass are extremely dynamic and can very a lot from year to year because of the natural disturbance and changes within the canopy. So, the ground data at 50 ha plot in 2005 will be very different than the lidar data in 2009 at the scale of less than 0.25 ha (if not, definitely different at 20 m or 5 m, resolutions). For biomass mapping, I recommend to redo the analysis for at least 0.25 ha. c. The height metric used in this study is not as convincing as one expect. The authors have a powerful waveform data and they only used one metric that actually may be not the best to predict the biomass. Recent results from Lefsky et al. 2010 and others have shown a combination of metrics will provide higher accuracy in estimating biomass. MCH is also very sensitive to changes of forest structure at high resolution. In fact, RH100 can be more stable through time than MCH (as smaller, below canopy trees, affecting the overall forest structure, are more dynamic in the forest than then tall emergents). Figure 2 also confirms that the variations of the height metric at the BCI plots are much larger. Without the Agua Salud data, the plot would be different, suggesting that one metric alone will large errors in estimating the biomass in BCI. d. The authors completely ignore the scale of analysis. They use biomass estimated at various size plots and calibrate the lidar data at 5 m resolution. Again, Chave's paper and also other ecological papers including some from the co-author Muller-Landau will show that allometries are scale dependent unless applied at a scale biomass and structure are stable. As this is not an individual tree allometry, relationships developed at one plot size on the ground between height and biomass cannot be applied at the different resolution lidar data. I can understand combining different plot sizes to reduce the heteroscedasticity, but the algorithm developed cannot be applied reliably at the 5 m data. It would be great to test what would happen if lidar data was aggregated to different resolutions (5 m, 10 m, 20m, 50 m) and the same calibration equations were applied. I am sure, you will definitely find difference

in the final results. e. How about slopes? Have you considered using plots at different slopes to calibrate the lidar data? Will there be the same calibration between height metric and biomass regardless of slope? How do you know that you are not creating a bias in your relationship when you use a calibration equation from flat area (e.g 50 ha plot is located on a flat area relative to the rest of the island). Let's imagine, tree diameters are smaller on higher slope but the tree heights are not. This means that you may be over-estimating the biomass on higher slopes. f. Finally, you use the lidar height data and map the biomass. It appears that both your ground and lidar map are not area corrected. In general, after calibrating your lidar data to biomass, you need to correct for the area over slopes. Otherwise, the biomass values are higher over steeper slopes. The biomass results are impacted by not including the area correction. In BCI, you can easily over-estimate the area of a 1-ha plot by 1-20% depending on the slope and if not corrected, you will end up comparing biomass over a larger area (on the slopes) with smaller area (flat surface). I suggest, the authors look at this problem carefully. In mapping biomass density, area correction is important. g. The analysis of 2-7 m and 12-17 m layers of the height profile is confusing. It is not clear where these numbers come from. What is the statistical significance of the selected height ranges when looking at the forest structure at different scales? What happens if you use slightly two different ranges? I recommend choosing height metrics such as percentiles of energy to compare forest structure over the landscape instead of fixed height ranges.

4. I think most of the results may be impacted by the problems with the methodology described above. I am afraid; it is very difficult to evaluate the paper and its importance without correcting the errors associated with the data analysis. Since slope angle is the most significant predictor of biomass variation on the island as mentioned by the authors, I think most of the results presented under forest age, soil texture and bedrock are also impacted by problems of mapping biomass from lidar. 5. The authors mention that the reason biomass is larger on steeper slopes is because erosion rates may exceed weathering rates. However, they do not produce any evidence or show any

data to support this. Therefore, it could not be used as a sentence in the abstract. One can suggest this as a potential hypothesis in the discussion. I assume, if the analysis changes according to the suggestions and then biomass variation turn out to less dependent on the slope, the hypothesis will not be true.

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