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Biogeosciences Discussions

Interactive comment on "High-resolution Mapping of Forest Carbon Stocks in the Colombian Amazon" *by* G. P. Asner et al.

Anonymous Referee #1

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TThe paper follows the approach the first author has used in other publications as in their work in Peru, Panama and Madagascar. The amount of data collection, processing, and all the ancillary work to achieve the results are very impressive. However, given the fact that the authors have published similar papers over other study areas, the paper does not contribute significantly to the literature as an innovative scientific research result. It seems as if they are reporting the results of a project similar to ones they have done before. Having said this, I feel the community interested in REDD projects for assessing and monitoring forest carbon would be interested to read the paper. I recommend the paper for publication after major revisions.

The following series of questions and suggestions need to be considered while revising the paper.

1. The authors discuss the design of the Lidar data collection based on a landscape stratification using Landsat and SRTM data. However, what they show in Figure 1 is a Landsat image colored based on PV, NPV, and soil signal from their TM end member analysis. In Figure 2, they show a flowchart that is not at all interesting. It does not say anything but just few boxes with names and no image to demonstrate the result of implementing the flowchart. I think, the authors are really exaggerating the use of Landsat for unambiguously detecting the age of forests and if they are secondary or degraded. I suggest, they remove their flowchart and instead show clear landscape stratification with few classes distinctly related to topography, ruggedness, and some segmentation of PV, NPV, and soil. I think, this is an important part of their Lidar acquisition design and they need to demonstrate the segmented landscape and the design of their Lidar data collection. So far, it appears that the collection had nothing to do with the segmentation and the gradients of landscape and vegetation features over the study area. There is no proof of a rigorous design in the paper. Any future REDD projects following their footstep need to know this. 2. Regarding equations 1 and 2. If one follows the steps they describe, and if I have not made a mistake, after substitutions, equation 2 will become ACD=1.931*MCH1.382. It would be great, if the authors check and make sure the equation is correct. I also suggest, they do not use the title “universal” for this equation. First, it is only used for tropics, even thought it can be extended to other forests as well. Second, this is as universal as any allometry. Chave’s allometry is more universal that this equation because it is used without any changes for all tropical forests. As the coefficients of this equation change all the time, there is no reason to call it universal. The form of the equation has also been around for ages. A power law relationship between biomass and height has a history of almost a century and it has also been used in metabolic scaling theory for decades. The importance of the equation is its application on a plot or pixel instead of tree level and it was demonstrated in their previous paper. 3. I do not understand how the authors claim the 11 plots to be their validation plots when they used exactly the same plots to determine the coefficients and then to develop equation (2). May

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be, this is the reason why figure 3 looks so unbelievably great! Using an independent validation data that is not used in calibration could've been a more realistic test of the model performance. If the validation data is different, they need to clarify this in the paper. 4. For the regression method in upscaling Lidar data, the authors show Figure 5. As they mention in the text, the elevation is the best parameter and explains only 19% of the variations. The rest of the variables explain another 10-12%. So, I am surprised to see a very low RMSE in the application of the regression model. 5. The differences between the two upscaling approaches are interesting. One of the main characteristics of regression models is the tendency to predict the mean value over the domain of its application correctly. However, it may not predict the distribution right. The fact that there is a general agreement between the two methods is related to this effect (The difference over a large area is about 10-20% of the mean carbon numbers. There is no surprise that you get 1.497 Pg and 1.499 Pg using the two methods. In fact, you need to also show that the Tier-1 method does not get you the right results. 6. I also suggest another approach to convince the readers that the methodologies work. I would calculate the total carbon in all 136 final segmented areas from method 1 and 2 (stratification and regression) and plot them against each other. This plot will be able to show a better comparison in terms of how the data are spread. You may have bias in the estimation. Overestimation the low biomass values and underestimation of high biomass values can always give you the right mean and no bias. 7. A best test of validation would be to use few of the Lidar data sets and predict the pixel level forest biomass using their regression models. I am surprised the authors do not show this. They have 38 areas and a total area of 462000 ha of Lidar coverage. I would use 2/3 of the data to build the models and prediction and use of 1/3 as an independent test and then show how well the predicted biomass from the regression compares with biomass calculated from Lidar data. This will allow a more rigorous estimates of errors and bias along the entire range of biomass variation. 8. The uncertainty analysis requires some changes. These include: a. Reporting uncertainty at 30 m and 100 m scales is a bit misleading. The authors refer to the results shown in figure 3 to estimate the 20% error

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at 0.28 ha. They need to use an independent dataset to arrive at this error. 11 data points used both as calibration and validation will always give good error estimates regardless of what type of cross validation method is used. b. In addition, it is not clear how they calculate errors at 1-ha without having 1-ha plots. They mention they have shown this in their previous paper and it seems all they have done is to reduce the errors by dividing the error by sqrt(N), and N being the effective number of pixels 0.28 ha pixels in a 1-ha pixel. c. In the method section, they mention they have calculated errors by evaluating the application of regression equation on Lidar pixels. However, they do not show any results to demonstrate this. For regression models that can only explain 30% of the variation cannot estimate the forest carbon with about 15% accuracy at 1-ha scale. The authors need to justify the numbers in table 1.

Interactive comment on Biogeosciences Discuss., 9, 2445, 2012.