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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.

G. P. Asner et al.

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Dear Editors,

We thank the two reviewers for their thoughtful and constructive comments. We have addressed each point below, and have made updates to the paper to reflect our responses. As requested by reviewer 2, we also present two additional figures, both of which will be contributed as part of the revised supplemental text, or added to the main text at the editors' discretion.

Best wishes, Gregory P. Asner and co-authors

First reviewer (Emilio Chuvieco):

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Interactive Discussion

Discussion Paper

General comments: The paper includes an impressive amount of work and applies innovative ideas that were developed in previous papers by the same research group. It will greatly benefit carbon stocks inventories in remote areas.

Minor comments

Page 2448. The authors mention a universal Lidar equation to estimate ACD. However, the samples used for this equation were all derived from Tropical forest. Any references to Boreal or Temperate forests? Do you really mean "universal" for Tropical biomes?

Reply: We have clarified (revised line 107) that this is a universal tropical equation, although in principle it could be applied to other broadleaf forests. Non-tropical data would have to be ingested into the database to test its applicability there.

Page 2449. The authors do not mention potential problems of Lidar estimations derived from cloud coverage. Are they problematic in Tropical regions? Lidar does not penetrate clouds, and this is indeed a challenge in the humid tropics.

Reply: CAO makes a concerted effort to capture cloud-free imagery, and any cloud cover that makes it into imagery is clipped. In addition, we often fly under clouds during LiDAR mapping missions. Thus the extent of lidar coverage reported here is cloud-free imagery only, and we have clarified this (revised line 189).

Page 2448-50. The authors do not mention in the literature review the potential interest of discriminating between different fractions of aboveground biomass: foliar, branches, trunks. Some Lidar-base studies (Garcia et al., 2010 RSE), have successfully explored this possibility.

Reply: We have added a note that improved lidar analyses of forest structure may lead to lower errors in large-scale carbon mapping, citing Garcia as an example (revised lines 95-96).

Page 2451. Additionally to include full details in the supplement, here at least a brief description of the Lidar data acquired would help the reader.

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Reply: We have moved the details on the LiDAR data to the main text, and have maintained a reasonable word count for this section (revised lines 180-188).

Page 2457: Did the authors have soil maps of the region to account for its impact on spatial variation of ACD? Why did they not use human-related variables (distances to roads, populated areas)? Would it beneïňĄt the stratification/regression models?

Reply: One of our key goals was to utilize data that is available for most tropical regions (i.e., elevation, terrain variation, etc) to attempt to capture carbon stock variation controlled by soil type, etc for which data does not exist in most regions. The co-authors agreed that soil maps of the region were too coarse to be of use in the stratification – a problem common to many remote tropical regions.

Page 2463. The authors should include some ideas on how their work would benefit carbon inventories in other Tropical regions, and what are the main limitations of Lidar data to use it operationally within the REDD+ programs. My personal experience is that researchers/managers in Tropical countries are reluctant toward this technique for operational inventories.

Reply: We agree strongly with the reviewer, and we have noted several major advantages of LiDAR-based carbon accounting throughout the paper. However, we feel it important to keep advocacy in a policy forum, and have attempted to drive this message elsewhere (see, e.g., Asner 2011 Environmental Research Letters).

Second reviewer (anonymous):

The 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. We recognize that our group has conducted several carbon accounting projects that are superficially similar to the present manuscript, however, our approach and the study region differ in a number of important ways.

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 .

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.

Reply: We have clarified the caption of figure 2 to emphasize its importance (revised lines 707-714). However, it seems contradictory to suggest removing the schematic used to stratify the study area at each step of the project (i.e., figure 2), and later suggest there is no proof of design (see below).

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.

Reply: Both CLAS and CLASlite have an exceptional track record of delineating forest, non-forest, and degraded areas of tropical forests, with more than 15 peer-reviewed studies, applications in a variety of tropical countries, and a user base of more than 400 people in more than 170 institutions (http://claslite.ciw.edu). CLAS/CLASlite has proven accurate for forest cover, deforestation, degradation and regrowth mapping when used properly. However, the reviewer misstates what CLASlite does: it does not “detect” the age of forests, it ingests Landsat or other satellite imagery across multiple years to verifiably monitor deforestation or degradation from one image to the next. Forest that re-grows in previously deforested pixels is unambiguously secondary because the window of study is a matter of decades. These issues are detailed in the manuscript.

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.

Reply: We have added a figure that characterizes key components of the stratification process, both preliminary and final (Figure R1, below).

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.

Reply: As we noted originally (lines 2451.8-20), we designed the LiDAR data collection to achieve at least 1% coverage of each stratum in the preliminary stratification, but not surprisingly, other factors were found to influence those stocks once the LiDAR data were in hand, and these were subsequently incorporated into our final stratification. As we describe in the manuscript, the study area is one of many tropical forest regions worldwide that is exceptionally poorly studied (lines 2450.15-18), but as further study is applied (here, and elsewhere; e.g., Alvarez et al. 2012), future REDD projects will incorporate this new information.

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.

Reply: The reviewer's arithmetic is correct, and the issue is that the number of significant digits used in equation coefficients differed from what appeared in text. We have modified the text and verified subsequent analyses to reflect this change.

I also suggest they do not use the title "universal" for this equation. First, it is only used for tropics, even though 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.

Reply: This is untrue – and is a fundamental misstatement of Chave allometry. The Chave models all depend on knowledge of forest type (i.e., dry, moist, wet) as well as each tree's wood density; some also depend on knowledge of a tree's height.

As the coefïňĄcients of this equation change all the time, there is no reason to call it universal.

Reply: The equation coefficients do not change. The three variables put into the model must be assessed in a new region, just as they must be assessed for a new tree with the Chave model. As we noted originally (lines 2453.11-12), the equation is presented in simplified form as a courtesy.

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.

Reply: We agree that the form of the equation is based on allometric theory, as we reviewed extensively in the previous paper. We do not see how this can be construed as a shortcoming of the model itself or its application in the present study.

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 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.

Reply: We have clarified (revised lines 232-234) that the sample plots were used to validate the universal approach, and we acknowledge in the manuscript that the difficulty of placing vegetation plots in the study area was a limitation of our study. However, the reviewer misunderstands what it is we are validating. The universal approach allows for regional assessments of basal area and basal-area-weighted-wood-density to replace exhaustive tree-specific measurements. This is a significant departure from traditional inventory approaches to calibrate remote-sensing data. We have subsequently conducted further validation of this approach which we will present in a forthcoming 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.

Reply: As we noted in the original text, the regression model was fit only to pixels classified as forest. Thus, the regression approach is hybridized with some minimal stratification for non-forest regions where CLASlite demonstrates much higher confidence than regression approaches. We have clarified this (revised lines 277-282).

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.

Reply: We agree with the reviewer, and we noted that it is not a surprise that the mean values agree (lines 2460.15-18). However, there remains a need to present these results.

In fact, you need to also show that the Tier-1 method does not get you the right results.

Reply: Tier-1 mapping methods have been shown to fail for carbon predictions in previous studies because they are not high-resolution and may be biased (lines 2460.13-15). Thus, we feel it unnecessary to continuously demonstrate this. Indeed, current global biomass mapping techniques now substantially exceed Tier-1 capability (Saatchi et al. 2011, Baccini et al. 2012).

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.

Reply: We recognize that regression methods often yield the correct mean but produce bias in the spread of the data. We have added an analysis and a supplemental figure (Figure R2, below) that compares the average carbon stocks of all 38 flight polygons, which span a wide range of carbon stocks (but are more consistent in area). This analysis suggests no bias in predicted carbon stocks from stratification, and only a limited bias in the regression approaches. This analysis is also robust to a new, more conservative error estimate as outlined below.

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 estimate of errors and bias along the entire range of biomass variation.

Reply: We have redesigned the error analysis based on a 75% - 25% (training-testing) split of the LiDAR data. To do so, we limited all flight polygons to a width of 2600 m, which provided 75% of our total LiDAR coverage. We used the training data to produce both a stratification and regression-based regional carbon map (following the same methodology as for our primary maps). We then assessed the effectiveness of each technique in the 25% testing data that was set aside.

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 at 0.28 ha. They need to use an independent dataset to arrive at this error.

Reply: As we noted it the original text (lines 2455.22-24), we rely on a completely independent demonstration of LiDAR calibration error scaling patterns in a tropical forest in Panama in order to estimate pixel level error. These data are derived from the same sensor in similar forest. We have clarified this (revised lines 451-460).

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.

Reply: We have clarified that the validation of the universal model was a validation of a technique. For LiDAR calibration errors, we rely on the originally discussed empirical analysis.

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.

Reply: Mascaro et al. demonstrated that LiDAR-carbon errors scaled in this manner empirically (to at least 1-ha resolution) in Panama using a large plot with mapped trees. This pattern has subsequently been verified in a second completely independent large-scale plot, and these results confirm 10% error at 1-ha resolution. The results will be presented in a forthcoming paper.

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.

Reply: As noted above, the regression modeling was applied only to forested pixels, while non-forest carbon stocks were caried from the stratification technique. Thus, the regression-based carbon map is more accurate than would be predicted from looking at forested pixels alone. We have clarified this point (revised lines 277-282). We have also modified the error analyses to include a more conservative estimation of upscaling errors (i.e., with training and testing data as noted above).

References:

Alvarez, E., Duque, A., Saldarriaga, J., Cabrera, K., de las Salas, G., del Valle, I., Lema, A., Moreno, F., Orrego, S., and Rodríguez, L.: Tree aboveground biomass allometries for carbon stocks estimation in the natural forests of Colombia, Forest Ecology and Management, in press, 2012.

Asner, G. P.: Painting the world REDD: addressing scientific barriers to monitoring emissions from tropical forests, Environ. Res. Lett., 6, 021002, 2011.

Baccini, A., Goetz, S. J., Walker, W. S., Laporte, N. T., Sun, M., Sulla-Menashe, D., Hackler, J., Beck, P. S. A., Dubayah, R., Friedl, M. A., Samanta, S., and Houghton, R. A.: Estimated carbon dioxide emissions from tropical deforestation improved by carbon-density maps, Nature Clim. Change, doi:10.1038/nclimate1354, 2012.

Saatchi, S. S., Harris, N. L., Brown, S., Lefsky, M., Mitchard, E. T. A., Salas, W., Zutta, B. R., Buermann, W., Lewis, S. L., Hagen, S., Petrova, S., White, L., Silman, M., and

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Morel, A.: Benchmark map of forest carbon stocks in tropical regions across three continents, P. Natl. A. Sci., 108, 9899-9904, 2011.

Figure R1. Selected variables used to stratify the study area: (a) digital elevation model (DEM) derived from the NASA Shuttle Radar Topography Mission (STRM), (b) terrain ruggedness index (TRI), (c) fractional cover of photosynthetic vegetation (PV) derived from CLASlite, and (d) drainage catchments. Elevation and PV were also used as inputs to the regression approach.

Figure R2. Mean aboveground carbon density (ACD; Mg/ha) as predicted by the stratification and regression approaches to upscaling, compared to the observed estimate derived from airborne LiDAR. Each point represents the validation region of one of 38 flight polygons. These areas comprise the 25% of the LiDAR coverage remaining after excluding a 2600 m strip down the center of each polygon that was used to train the upscaling models. The line depicts a 1:1 relationship.

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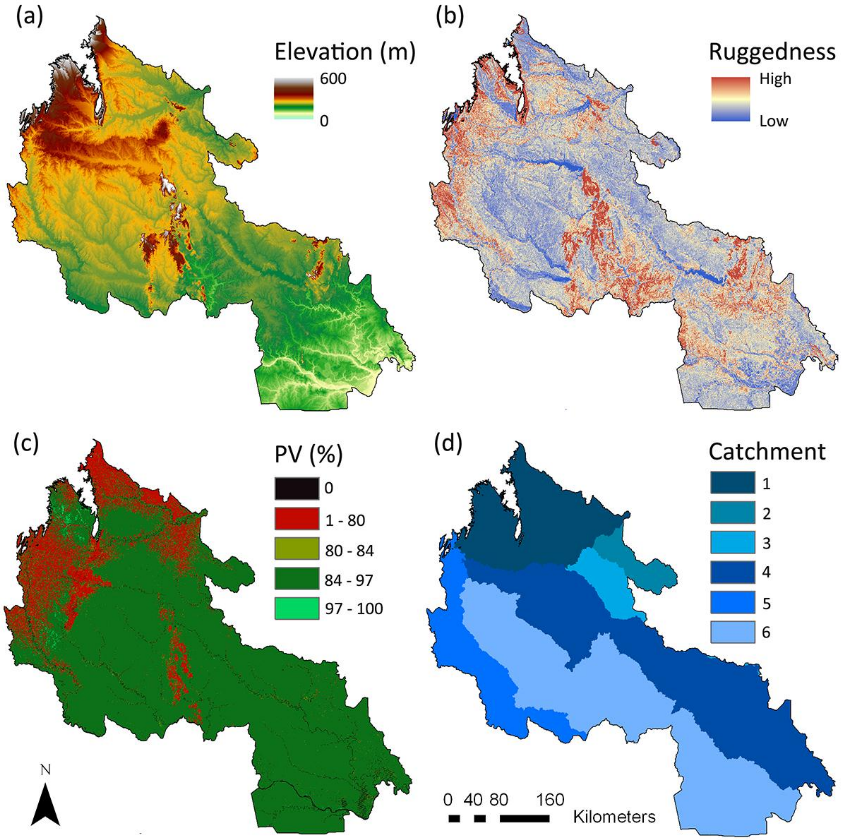

Fig. 1.

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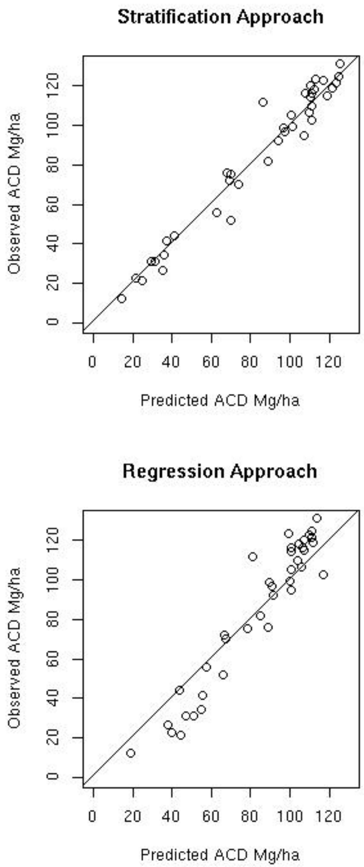

Fig. 2.

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