Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-547-RC1, 2018

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Canopy Area of Large Trees Explains Aboveground Biomass Variations across Nine Neotropical Forest Landscapes" *by* Victoria Meyer et al.

Anonymous Referee #1

Received and published: 28 January 2018

General comments

This paper addresses an important and interested topic – the use of emergent crown area (from airborne LiDAR data) to estimate forest biomass. The abstract and introduction are very well written. Unfortunately, I get confused in the methods. From the abstract, it seems that the use of LCA to estimate biomass is going to be calibrated with ground AGB estimates. However, in sections 2.2 and 2.4 the authors estimate AGB from both LiDAR MCH and LCS. In the methods section, it is unclear whether they are predicting AGB_Lidar and AGB_LCA from an equation that already exists or whether they are doing a regression analysis to find values for parameters 'a' and 'b' in

Printer-friendly version

Discussion paper

Eqs. 1-3. If it's the former, show the actual values for 'a' and 'b'. Either way, it doesn't seem necessary to predict AGB from MCH other than to compare AGB estimates from LCA to those from MCH (eg, show improvement in new method). In section 2.3 the authors, say they have only 4 calibration sites (instead of 9 in the abstract). So, is AGB in the other five sights predicted by Eq 1 (MCH)?

This is important work, and I am happy to see it finally coming into fruition. However, I am strongly opposed to using estimates (ie, from MCH) to calibrate a new method. I suggest the authors remove AGB_Lidar estimates and focus on relating LCA metrics to AGB determined from ground inventories. This will also clean the paper - making it shorter and much easier to follow. Furthermore, I suggest trying to optimize AGB estimates from LiDAR by, for example, estimating AGB with both LCA and MCH. If I have misinterpreted the methods, please edit the manuscript accordingly. I look forward to seeing these improvements.

Specific comments and technical corrections

How is the LCA method weighted by WD if there isn't ground data at 5 sites? Line 104: what do you mean by 'unique'? Line 166: What model? Line 167: what data? Lines 203-4: This indicates that AGB_LCA is being tested against AGB_Lidar, where LiDAR is being treated as the reference. AGB_Lidar is only an estimate. Lines 205-6: Here you say that these results were compared to 'a traditional model relying on MCH to estimate AGB'. Isn't AGB_Lidar the model relying on MCH to estimate AGB? Section 2.5: Is it possible to apply the same methods to logged areas, since you may not know which areas have been harvested or not – or have before and after pictures? Line 269: Where did wood volume data come from? Lines 315-6: In what way does Antimary not represent Peruvian Amazon and Amazon-Andes gradients? Line 323: by how much does it explain the variation? Section 4.3: Would be helpful to refer to tables and figures Lines 344-6: This sentence is unclear to me, but it sounds like it supports my point that using AGB_Lidar as a reference is circular and not proving anything Line 374: Change 'only' to 'primarily' or something similar. Line 391: Change 'Any' to 'Most'

Lines 423-5: Maybe the relationship is not linear at the high end of LCA Line 467: If the relationship remains unique across forest types, is it not then broadly applicable? Fig 3: Clever way to find the optimal H threshold Fig 4b: This doesn't look like a perfectly fit Fig 5b: All calibration sites are above the 1:1 line. Why are Nouragues and Choco below the line? Fig 7: It would be helpful to see the actual data, not just regression lines.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-547, 2018.

Printer-friendly version

Discussion paper