

Interactive comment on “Estimation of Coarse Woody Debris Stocks in Intact and Degraded Forests in the Brazilian Amazon Using Airborne Lidar” by Marcos A. S. Scaranello et al.

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A note upfront from the submitting person: This review was prepared by Alice Gargano and Marc Grob, both master students in geography at the University of Zurich. The review was part of an exercise during a second semester master level seminar on “the biogeochemistry of plant-soil systems in a changing world”, which I organize. We would like to highlight that the depth of scientific knowledge and technical understanding of these reviewers represents that of master students. We enjoyed discussing the manuscript in the seminar, and hope that our comments will be helpful for the authors.

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C1

The study by Scaranello et al. deals with a new approach to quantify coarse dead wood using lidar measurements combined with Landsat images and forest inventory data. The study area consists of 14 test sites and 103 transects with a total length of 48 kilometers in the Brazilian Amazon. The newly suggested approach should make it possible to gather the amount of coarse dead wood in intact, logged, burned or logged and burned forests and help to estimate their importance as carbon stocks.

We appreciate the chosen topic for its originality, which in our opinion justifies conducting the study. It is interesting to see how the problem of quantifying coarse dead wood has been identified and how a new approach can be adopted using remote sensing to complement the information gap. The comparison of the collected remote sensing data with Landsat time series and forest inventory samples seems comprehensible and is well described, despite its technical and interdisciplinary complexity. The structure of the paper is generally well chosen and the language as well as the abbreviations are consistent and of good quality.

Generally, the paper is very interesting to read and contains a detailed description, yet the abundance of numbers in % or meters per each dataset makes it hard for the reader to fully understand and keep track. It is very technical, mainly discusses the lidar-predictor model and has only a limited part involving the dead wood's contribution to carbon storages, which makes us, as non-experts, reflect whether it really belongs to a biogeosciences journal or rather to a remote sensing one. However, we agree with the importance of the topic and think the paper can be published after some modifications.

— At the beginning of the paper we would have appreciated a short section on the detailed contribution of dead wood to carbon storage and the related processes. It would be a good introduction to the topic and could better justify the importance of the study.

Moreover, the paper does not describe clear hypotheses and expectations regarding data collection and results. This part is particularly important due to the existing uncer-

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C2

tainties and unexploredness regarding coarse dead wood. We would have welcomed more information from such a pioneering study on unexplored aspects of the contribution of coarse dead wood to the carbon storage. In contrast, the section on the tested lidar metrics is extremely detailed in our opinion. Although this aspect is important to describe, it can be challenging for remote sensing non-specialists to follow and too technical, we would suggest the addition of figures (for example after line 6 in page 6, guiding figures would help). The overweight of the material and methods section compared to the results makes us understand that this paper works as a preliminary study in a new research field. Due to the limited predictors and their overall performance (page 11, line 23) the question arises whether the study was conducted too early.

Other uncertainties we came across are concerning the justification of the chosen test sites. The Amazonian Forest is extremely large and your study focuses only on a small tract of the its area (according to the stated lidar-data used). How representative is this, given the not very high performance of the predictors?

In the results section, you could add a small table summarizing the results you compare, the best resulting predictors and comparison between lidar-only and historical models. Moreover, in the discussion you accentuate that the differences in site-specific characteristics are uncovered by the slight improvement achieved by the historical model. The RMSE decreased of only 1% making it questionable whether historical scenarios are detailed enough.

— The following specifications and questions also emerged after reading the paper:

Page 2, line 11: What are the structural variables exactly? Maybe you could give a short definition.

Page 3, line 4: The exact value of 60 Pg-C is in our opinion not appropriate, due to uncertainty. You could either give a range or use “approximately” to reduce the anchoring effect of the absolute value.

C3

Page 3, line 10f.: Too many percentages are reducing the readability.

Page 3, line 25f.: Considering that the study was conducted during three years, the question arises whether any feedback or temporary changes during this period have been considered.

Page 4, line 10: Why did you choose this period? How can that be justified?

Page 4, line 25f.: Were the assumptions for regression met? Normality, homoscedasticity, etc.

Page 7, line 14: Why was the subset selection approach chosen?

Page 10, line 8: What is the single event (here mentioned) exactly?

Page 19, Figure 1: Figure 1 does not really help nor guide the reader, we believe it could be improved by adding an inset map to facilitate the orientation and readability of the location of the test sites and points for each site instead of the abbreviations of the test sites. Furthermore, the canopy height map colours are hard too identify regarding the visual differentiation on the small map as well as the tiny legend. We suggest to either enlarge the map and legend or change at least the colourway. The small figure on the right top shows a rather unnatural pattern between fallen and standing wood, which we cannot explain. The explanation of the used statistical approaches could contain a bit more details, like why the subset selection approach was used and whether all regression assumptions were met.

Page 23, Figure 5: The colourway of the figure is cartographically questionable and the legends have different ranges, making comparison challenging. Furthermore, the predicted mean (red dotted line) and the field-based mean (black dotted line) are hard to detect and not explained in the legend. Moreover, the arrangement of the graphs makes it hard for the reader to compare the different graphs, maybe you use a consistent arrangement for all figures and graphs.

C4

C5