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Biogeosciences Discussions

Interactive comment on "Productivity of aboveground coarse wood biomass and stand age related to soil hydrology of Amazonian forests in the Purus-Madeira interfluvial area" *by* B. B. L. Cintra et al.

M. Larjavaara (Referee)

markku.larjavaara@gmail.com

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General comments

I congratulate the authors for a valuable dataset and potentially significant publication. The paper addresses significant questions within the scope of Biogeosciences and data and reports from this vast little studied area are of high interest. The overall presentation is in general well-structured and clear and therefore relatively fast to read. The field, lab and statistical methods seem to be valid and clearly outlined. The citing

style is appropriate and language is fluent and precise (certainly better than in this review).

I found different values reported in tables and the text. For example, on P31L22 (line 22 on page 6431) it is written that AGWB resulted in 233 Mgha−1, while in Table 8 the number is 252. This is probably caused by a different model used (Eq. 2 or 3) but is confusing for the readers. “AGWBP” on P31L25 and “AGWBPc” in Table 6 seem to refer to the same thing but the reported values differ.

I did not understand how the sampled trees with DBH > 30 cm were chosen (Table 3). This information is important. Secondly, it might be a good idea to give some information on the 22 sampled individuals for which growth rings could not be seen. E.g. information on their wood density might hint whether this exclusion over- or underestimates growth of all trees.

The term “productivity” is used in a misleading way. On P18L12 it refers to Mg/ha/yr as normally but later in the Abstract seems to refer to the Mg/yr per unit of basal area of trees. The conclusion of higher productivity in wetter sites seems to be caused by this unusual definition and unit of productivity. I recommend the authors to restrict to one and the traditional definition of productivity. It might be better to use “turnover” to describe productivity relative to basal area even though this term is normally restricted to productivity relative to biomass. Best would be to avoid using the variable productivity per unit basal area and compute the classic turnover instead.

It is great that the authors have had the data and used exceptionally the concept stand age for tropical rain forests. However, to make its computation faster to understand and more likely to get adapted in other pioneering studies it would be better to either compute it as average or all individuals with DBH > 10 cm or DBH > 30 cm or by weighing individuals based on their basal area for all trees with DBH > 10 cm. I assumed here that the “basal areas” on P29L5 refer to the total and not basal areas of individual trees.

It is always not clear for the reader whether trees with DBH < 10 cm are included (e.g.

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in Table 8) and whether woody structures with diameter < 10 cm of larger trees are included. I believe they are but the “coarse wood” in the title would indicate they are not. If the latter is true the title should be modified (it would be better to modify even if the former was true). What about “Productivity, biomass and stand age in Amazonian forests in the Purus-Madeira interfluvial area”? Or if you start using the term turnover this could be added to the title.

It is not easy to understand based on the Abstract and Introduction that the plots were actually not wetland plots but instead driest and wettest conditions from selected landscape were chosen (P23L6). Did I understand correctly? If yes please modify Abstract and Introduction. Use of terms wetland, seasonally waterlogged, seasonally flooded, floodplain forest, non-flooded, terra firme, várzea and Igapó is unclear. One option would be to add these terms to Table 1 so that readers would understand which plot belongs to which class and linking this information to the SWS rating. Another option could to add a textbook-style paragraph on definitions to the Introduction.

Simplifications could be done to facilitate fasten comprehension by readers. First, only one of the biomass models (Eqs. 2 and 3) could be chosen as the second gives exactly 8 % higher values always. If the authors want, the other could be simply mentioned in the discussion with one sentence. Second, some of the acronyms could be avoided. At least “HAND data” could be called e.g. simply “height above drainage data”.

The results of older trees, more biomass, less turnover and higher wood density (but to me no clear trend on productivity) on dryer soils could be explained by unusually long floods in wetter sites e.g. a few times per millennium which would have either directly killed the old and slow-growing high wood density species or indirectly by favouring pioneer species even unusual floods did not occur that or previous century. Similar productivity in wetter sites despite of lower biomass and tree species with more pioneer-characteristics would mean to me that the edaphic conditions are less suitable for growth in wetter sites.

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Specific comments and Technical corrections

P18L13: “There is . . .” is ambiguous. Should be clear on the direction of the relationship.

P18L15: The abstract should hint what the “hydromorphic properties” are. Now the reader does not know if increasing moisture increases or decreases productivity. (later reading revealed to me that these sentences are in addition of being ambiguous, misleading – see above)

P18L18: “enhanced soil hydromorphic condition” is unclear.

P18L24: Could “since there should be low restrictions for soil water availability” be converted to “since water is always available”?

P20L22: I would choose either “important” or “key” but not both.

Table 1: Modifications are needed. First, the caption does not describe columns from left to right. Please change order of SRTM and HAND either in the table or caption. Second, why are flood heights in brackets only for the first two sites (two other plots are flooded based on the SWS index) and was this the deepest flood of the year or decade or just flood depth at a typical day during the flooded season? Third, three neighbouring acronyms (SRTM, HAND and SWS) slow down reading. SRTM could be “Altitude above sea”, HAND “Elevation above drainage” and instead of SWS and a separate Table 2, you could combine these by bringing the definition to Table 1. This would certainly fasten reading but might as well save space. Fourth, dry season should be defined. Is the rightmost column the number of calendar months with lower precipitation than 100 mm? If you keep Table 2 and “SWS” column heading in Table 1 the caption of Table 2 should indicate that it is about the SWS index.

P22L12: Is it already paved?

P23L13: Air dried in air-conditioned room or in tropical humidity?

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Table 3: I would merge this with Table 1. Even if you do not this I recommend you to remove the third column as repetition of information and potentially the percentage information on the second column.

P25L9: Please describe better how height was measured? How was the distance to the tree measured and how experienced were the technicians? Measuring tree height in a tropical rain forest is prone to bias of several meters and much larger more random errors.

P25L12: What was the coring height for buttressed trees?

P25L15: Stored for how long?

P25L20: Why do you mention about Botanical data if you did not use it for this study?

P26L12: If you describe the method in detail like this I recommend that you modify the wording of the sentence starting "The sample . . ." At least units for volume and weight should be added.

Eqs. 2 and 3: The current way of presenting these is misleading as it first seems that the form of the models is different. However, by multiplying PI, 1/2^2 and F, I got 0.0471. Therefore, it would be better to simply present one equation and write about varying value of the parameter (0.0471 in Cannell and 0.0509 in Chave), or better even to just use one of the models and potentially mention the deviance to the other (e.g. Chave gives 8 % higher values than Cannell) in the discussion.

Eq. 4: I plotted the equation and it gives low C content of only 44 – 46 % for normal wood densities. I checked the original publication (Elias and Potvin 2003) and found your equation in the caption of Fig. 4. I compared the regression shown in their Fig. 4 to my plot and there seems to be difference. My guess is that there is typological error in their caption of Fig. 4 but there could very well be something that I do not understand here. Please check and potentially contact the authors. If the equation was wrong you would need to redo all carbon calculations.

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P27L24: Typical errors are probably much more than 1 % when the tree is buttressed.

Table 4: I am sorry but I am confused with terminology. I could go back and search whether the terms are defined earlier but I would prefer each figure and table to be understandable alone. Basal area is for trees with DBH > 10 cm I presume. It is better to define this in the leftmost column. Writing about diameter class DBH < 30 cm is misleading if tree smaller than 10 cm in DBH are missing. What is mean canopy height? Is it the same as mean tree height? When reading canopy height I start to think of mean leaf height. Are the mean for height and wood density without weighing with basal area? Is the mean tree age (in caption) or age (in the table) the same as stand age?

Fig. 2: It would be valuable if you could add discussion on causes of uncertainty in tropical dendrochronological studies. Not in the caption but to the main text.

P31L7: Please repeat the number of sampled trees again here.

Fig. 3: The journal might want the (a) and (b) included in the figure itself – even though it is obvious that a is in the left and b in the right. Why there are cores of less than 50 mm long if all sampled trees were at least 100 mm in DBH? Does this figure include "estimated decayed rings"? The manuscript contains both mm and cm for length of the core sample and DBH. It would have been more natural to use e.g. mm for both.

Table 6: There seems to be errors either here or in the text P31L25 and P31L28. Please check all – including tables, figures and text.

P31L24: What are the "two seasonally flooded forests"? According to Table 1 three of the eight plots have the SWS index 3 corresponding to seasonally flooded. Again in P33L21. Another comment is that Eq. 4 should probably be 3 and 3 should be 2.

Table 7: Are the decimals missing for clay content for fourth and fifth of the plots as they are zeros? Better to include these as in the rightmost column.

Fig. 5a: Please add M01 datapoints to the plot. You can still report the regression

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excluding these from the model if you mention this in the caption. You can plot these two datapoints with another symbol than black dot.

P32L27: I would not present data in the results with is not part of the described dataset in Material and methods. If you do not agree you could mark the eight plots described in your dataset with another symbol and you should mention in the Material and methods that all these plots were used for this analysis.

P33L7: Is the reference to Table 6?

P33L16: I do not understand how the observation that trees start growing during a flood indicates that flooding is unfavourable for tree growth.

P34L2: It might be a good idea to try to quantify the "smooth topography" already in the Material and methods e.g. by reporting in addition to the "HAND" value the distance to the nearest drainage. This would be give an idea on how steep the slopes are.

P35L4: Wood density is an important factor in addition to productivity that influences DBH growth. The lower wood density is the more DBH increases with a given productivity.

Table 8: This is a valuable table. It seems to me that there is a mismatch between the 252 reported in this table and the values reported in Table 6. Is the 252 based on Eq. 2 or 3? Is the DBH limit below which trees were excluded the same (10 cm) in all studies? (more on this in General comments above)

P36L6: It could also be that the height measurements are easily so biased that their additional value is often low.

P36L23: If sustainability is defined in the classic forestry way (extraction is equal or lower relative to growth after extraction) then extraction can be sustainable from any forest ecosystem. What is your definition of sustainable here and prior when you used the term?

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P37L22: Did you attempt to correlate ring width with precipitation of the year? This might reveal patterns that are different on dry and wet sites.

I wish that the authors will continue and carry out similar valuable studies in other parts of the tropics.

Markku Larjavaara

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Finnish Forest Research Institute

markku.larjavaara@gmail.com

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