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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Carbon dynamics in aboveground coarse wood biomass of wetland forests in thenorthern Pantanal, Brazil" *by* J. Schöngart et al.

Anonymous Referee #1

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The authors present a nice data set comprising a combination of inventories of four 1-ha-plots, estimates of wood density and dendochronological investigations allowing them to predict tree ages as well as recent and past growth rates. The work that has been done is impressive: Tree height was measured for about 80% of all trees, which in a tropical forest is not an easy thing to do; Species identification required the sampling of sterile and fertile material, wood density was measured for 48 tree species and 136 wood discs were analysed for age and growth rates. Especially the latter has to acknowledged, as identification of annual rings in tropical trees requires can only be done by specialists. All in all, this dataset represents a very valuable contribution to the literature. Unfortunately, the quality of the data analysis, especially the statistical treatment of the data, leaves room for improvement. A proper error propagation strategy is missing completely, the model selection procedures are not well described, and some calculations are simply wrong. However, I don't think that these mistakes and omissions affect the scientific story. I tend to disagree with some of the conclusions. The structure of the paper is more or less clear, the language needs to be checked by a native speakers (I don't do language corrections below). I suggest the manuscript for publication after major revision.

General statistical comments:

1. Allometric equations: There is no need to use (and even try) the equations by Brown and Chambers of you have the full set of predictors (D, H, rho). Also equation (3) by Chave is not a good choice. It does not contain height and tries to compensate for this lack of information by adding polynomial terms. Allometric equations with third order polynomial terms are dangerous, because they cannot be extrapolated. This leaves you with basically two useful equations (1 and 2). Concentrating on those, will make the paper simpler and will allow you to focus on the important bits. How do the ranges of your predictors compare with those in the datasets used to develop the equations? Please report.

2. Validation of allometric equations: On page 2114, line 27, you also state that equations 1 and 2 do the best job (see above), but how can you actually tell? You don't have validation data, have you?

3. Lack of error propagation: The only type of error you consider is a bias, introduced by picking different allometric equations. You do not estimate the uncertainty of the biomass estimates at the tree and stand-level, which is not good thing. There is whole branch of biometric research devoted to this sort of problem. The available techniques are not trivial (Parresol 1993, 1999; Fonseca & Parresol 2001; Parresol 2001; Wirth *et al.* 2004). I do not request to apply these,

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but please read the following papers that will help you to justify why you don't apply them (e.g. individual data for trees of the design matrix of the allometric equations not available?). At the moment, it is pretty clear that you are not aware of the problem. Application of equations 1 and 2 require a known height for every tree, which you don't have (but for many actually). Thus, you apply a predictor, that itself predicted. This is called a latent variable and its uncertainty increases the uncertainty of the final estimate. Since you have measured many heights and since you don't do uncertainty estimation anyway, this problem is not too severe. Just to make you aware of it: There are techniques to deal with this problem in a Bayesian context (Clark 2007).

4. Fitting data: You fit a number of equations yourself such as age = f(D,rho), D=f(age), H=f(D). Some of the fits are very poor, e.g. the H=f(D) in Figure 4, Stand 2 and 4. You would see why, if you would look at the residual plot. You need to find better functions and test their performance using for example the simply Akaike Information Criterion (AIC) or Cp (available in Statistica). Equation 10 (Age = f(D,rho) is probably hopelessly overparameterized and the terms do not seem to justified (why an interaction between D and the squared density? – this is really bizarre). AIC and Cp would probably tell you right away. Come up with an "informed" list of biologically sensible models and compare them as explained above. Don't use automated procedures.

5. Figure 5A: You really can't do this. Both dependent and independent variables were predicted from D. Therefore the tight relationship.

Other general comments:

1. Please place you story in the context of the Kyoto-Protocol. The new REDD mechanism (Mollicone *et al.* 2006), with the aim to include old-growth forests into

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the accounting scheme to avoid deforestation and which is heavily discussed at the moment, urgently requires carbon stock estimates from tropical forests.

2. Site selection: How were the sites chosen? Did you try to deliberately establish a chronosequence or did the age differences pop out a posteriori. Which information was your selection based on?

3. Chronosequence approach: You managed to establish a short chronosequence. Why not use it? You could present an estimate of carbon accumulation as the aboveground component of NECB (Chapin *et al.* 2006).

4. Growth: The relationship between diameter and age is often non-linear – most often a saturating function. If this is the case, the MDI overestimates the current increment in old trees significantly. You do have the ring width of the outer years, i. e. the current growth. Why don't you formulate a model of the type dDcurrent = f(D, and other potential predictors such as rho, species)? This you could then use to scale up the individual growth rates. I don't think the procedure outlined on page 2110 is the correct way to do it.

5. Turnover: This calculation is not valid. The calculation of turnover as biomass over growth is only valid in the equilibrium. Your stands may all be far from the equilibrium. Figure 5b is not correct. To prove that you oldest stands are in equilibrium please show whether you age-distributions (which you should show anyway) follow a negative exponential function. If this is the case, you are allowed to calculate turnover the way you did.

6. Out of interest: Are their papers on C-concentrations of tropical trees? I could imagine that this may vary way more than in temperate trees if there are inclusions of minerals.

Minor comments:

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P2108, L10: Why did you not measure all heights. 91 % is a pretty high fraction, not just a subsample

P2112, L1: Why does the percentage of Vochysia vary systematically?

P2112: Please present the stand age estimation and also an age histogram within the result section

P2112, L16: r needs to be negative!

P2114, L19-20: Not necessary to explain this. You say it in the next sentence too.

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Clark J.S. (2007) *Models in Ecology - An introduction*. Princeton University Press, Princeton and Oxford.

Fonseca T.J.F. & Parresol B.R. (2001) A new model for cork weight estimation in Northern Portugal with methodology for construction of confidence intervals. *Forest Ecology and Management*, 152, 131-139

Mollicone D., Achard F., Frederici S., Eva H.D., Grassi G., Belward A., Raes F., Seufert G., Matteuci C. & Schulze E.-D. (2006) Avoiding deforestation: An incentive accounting mechanism for avoided conversion of intact into non-intact forest. *Climatic Change*, 83, 477-493

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critical comparisons. *Forest Science*, 45, 573-593

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Interactive comment on Biogeosciences Discuss., 5, 2103, 2008.

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