

Interactive comment on “Predicting tree heights for biomass estimates in tropical forests” by Q. Molto et al.

Q. Molto et al.

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We thank the reviewer for his constructive comments. We generally agree with the asked modifications and provide further discussion where needed.

Some remarks are identical between the two reviewers. When necessary, the response made to the other reviewer is duplicated here.

Anonymous Referee #1 General comments Molto et al. develop height-diameter relationships for tropical forests in French Guiana. Their three main aims are clear and concise. They investigate different functional forms for the height-diameter relationship and test the effect of including forest structure and environmental predictors. They conclude that a Michaelis-Menten equation was the most appropriate functional type

C5053

and that the basic height-diameter relationship can be improved by including forest structure variables, and to a lesser extent by adding environmental variables. Although the results are not particularly novel, the generation of accurate height-diameter relationships for tropical forests is an important area of research, especially in data-poor regions such as French Guiana. I have four general comments that need addressing before this manuscript can be published.

R1Q1. The introduction fails to mention some key references in tropical forest height-diameter relationships (e.g. Feldpausch et al. 2011, cited later on in the text) and as such fails to accurately place the current research within the context of existing work. Do height-diameter relationships exist already that can be applied to the study region? It would have been good for the authors to compare the accuracy of existing relationships with those generated from this particular study.

R1A1 We agree with the reviewer. The citation of Feldpausch et al. 2011 could appear earlier in the paper. We believe that we have not missed some other literature presenting height-DBH relationships in our context. See also the response R2A1 to the reviewer #2: “R2A1 Sadly, it appears that the given information in Feldpausch et al. 2011 in the section working equations is not sufficient to apply their model since they do not supply the error terms. The errors terms are required to convert $\log(H)$ to H because the base e exponential of a normal distribution does not mean to 0. In addition, our conservative attitude considering uncertainty propagation makes that we need the error terms to compare the model outcomes. Thus, were not able to apply their equations to our data. We agree that some of our work duplicates questions addressed by Feldpausch et al. 2011. However, our hierarchical modeling methods and the way we compared models are radically different: First, they had trouble fitting the models they wanted to test (“The greatest constraint on non-linear models was that they frequently did not converge (e.g., 30% of the time for the Weibull function for plot-level fits)”). We show that with convenient weighting and fitting methods, this is not a constraint. Second, they add the environmental information in their model as an addi-

C5054

tive effect: $\text{Log}(H) = f(\log(\text{dbh})) + \text{error}$ Becomes $\text{Log}(H) = f(\log(\text{dbh})) + \text{environment} + \text{error}$ Using multilevel modeling techniques described by (Snijders and Bosker, 1999). We prefer to apply the environment effect on the H-dbh model parameters themselves. In other words, the model parameters are function of environmental and structure variables. It seems more logical to us that the environment can directly affect the model parameters, e.g. such as the maximum height. Overall, we do not think to our work as an “improvement in height models” but as an improvement in the way of handling the height model question. Of course our results in French Guiana are not as general as the results found worldwide by Feldpausch et al. 2011 but we still think that our modeling approach is worth reading by height-dbh modelers.”

R1Q2. Methods are missing key relevant information. The authors should state how many stems were recorded in the dataset, and what the species distribution was like (if this was known). How many species or community types did the plots sample? Species identity is one of the key predictors of height-diameter relationships (as the authors mention in the discussion) and as such some sort of information on species distribution across plots is needed in the current study. Species traits and community composition will have very strong effects on maximum height reached within any plot (i.e. the alpha parameter).

R1A2 The plots are described in a table in the appendix. We could add a short paragraph to summarize this table. The plots are also extensively described in Baraloto 2011, but we could repeat some short information on the species composition. Like the reviewer, we think that community species composition (eventually through the trait composition) may have a strong effect on the height-DBH relationships. Because our recorded datasets are very complete, we could have addressed this interesting ecological question. But we believe that this is beyond the specific scope of this paper and we will keep it in mind for future work. Our specific goal is to provide and methods for height predictions that are to be applied to various forest inventories. These inventories include large and/or old inventories that lack detailed information such as species iden-

C5055

tifications or functional trait values, but they may be considered as important source of information for carbon stock assessments. Therefore we kept the model as simple as possible and did not use the species identifications.

R1Q3. Results are incomplete. I was expecting figures showing the raw distribution of the height-diameter data and the comparing the four different functional forms. This would visually show the reader how much difference the functional form had on the fit of the relationship, and how much scatter there was in the data. Many of the results statements are currently subjective and need to be backed up by hard statistics. For example “RMSEs being quite similar” (P8621); the results associated with Figure 4; and “part of the variability in the height-DBH relationship was mainly explained by the maturity of the stand” (Discussion, P8625). Without statistical tests or plots of the fitted relationships it is hard for the reader to judge if these differences (e.g. Fig 4) are significant or not. I was also expecting the model parameters of the best fitting model to be presented somewhere (e.g. supporting information or does Table 1 do this?) so that the models can be used by other authors working in this area. I would also have liked to see information of how the different height models affect the plot-level above-ground biomass estimate.

R1A3 On the plots: We agree with the reviewer. We should add a figure showing the raw data and the different model shapes in two or three forest plots, see figure 1 of this discussion for example. (Shape is Michaelis-Menten, plain line: median, dotted lines: 95% Bayesian confidence interval). However, you would see that the different shapes are not distinguishable when plotted on the same figure – this comes along with the fact that they perform almost equally well. On the tests (figure 2, figure 4): It is not really in the Bayesian culture to perform statistical tests. However, you can consider parameters to have “significant” different values when their confidence intervals do not cross each-other. This is obviously not the case here. Table one gives the working parameters of the best model.

R1Q4 In places the language (mainly tense and grammar) is poor and needs significant

C5056

(professional) editing. In places the language makes it difficult to understand what the authors mean. Some examples are given below in the specific comments.

R1A4 This was also coined by the second reviewer and we will use a professional editing service before re-submitting.

Specific comments: R1Q5 Title –I suggest narrowing the title to “Predicting tree heights for biomass estimates in tropical forests in French Guiana”

R1A5 Why not, but the methods presented here (weighing of the trees according to their biomass and using Bayesian methods and Monte-Carlo simulations to propagate uncertainty to an upper-level model using basic forest structure descriptors to predict the parameter of the height-dbh model) is also applicable elsewhere. The values of the model parameters found in French Guiana are secondary for most readers. We prefer keeping the original title but this may be finally decided by the editor.

R1Q6 Abstract, lines 4-6. This is an example of a poorly worded sentence. Currently “Even if tree height is a crucial variable to compute the above-ground forest biomass tree heights are rarely measured in large-scale forest census because it requires consequent extra effort”. Could rephrase this to be “Tree height is a crucial variable for computing above-ground forest biomass. However, tree height is rarely measured in large-scale forest surveys because of the effort required.”

R1A6 Yes, this will be changed.

R1Q7 Abstract line 7 “10cm of diameter” delete “of”

R1A7 Ok.

R1Q8 Abstract, line 14-15. The wording here is strange and I suggest revision – what do you mean by “to affect the ABG predictions”

R1A8 Yes, poor use of the word “affect”. We meant “to improve the AGB predictions”.

R1Q9 Methods page 8615, line 19-20 “Heights were measured with various methods,
C5057

mainly lasers and ropes when climbers could approach the top of the trees” – this needs more elaboration and clearer wording.

R1A9 Yes. We could add a sentence like “These practices are usual good practices in tropical forestry.”

R1Q10 Methods page 8616, line 6-7. “To avoid mathematical singularity, the proportion of stems between 20 and 40 cm was discarded from the data” – what do you mean here? Did you remove all stems between 20 and 40 cm from the data? Did you not use this size class in your models? This needs more explanation.

R1A10 In a forest plot, the proportion of stems in the different classes always sum to one. This property makes the variables “proportion of stems in class X” impossible to use in a linear model. Thus, in the dataset describing the forest plots, we have to drop one variable. We decided (arbitrarily) to drop “proportion of stems in class 20-40”.

R1Q11 Methods, page 8618 line 11 – “5” needs deleting?

R1A11 Should be “(Eqn. 6)”. Thank you for pointing this out.

R1Q12 Methods, page 8619 line 2 – “7” needs deleting?

R1A12 Should be “(Eqn. 8)”. Note that if you click on the number you are redirected to the appropriate place, so it should be some LaTeX compilation failure.

R1Q13 Discussion, page 8623, lines 13-15. How much of the variability among plots in model parameters was driven by species turnover? Do you have any information of species composition that you can provide?

R1A13 See response R1A2 to your second major point considering species composition.

R1Q14 Discussion, page 8624, line 26. “clearly unexpected”. Delete clearly as this is subjective and the result may not be clear to some readers.

R1A14 Ok.

R1Q15 Discussion, page 8626, lines 11-15. These claims (e.g. “mainly explained” and “did not find them crucial”) need more statistical backing in the results section to be supported here. Hard numbers would be preferable. Figure 2. It seems there is not much difference among models. Some statistics comparing these models and/or figures showing the fitted relationships are required.

R1A15 This is the point. We compared models using RMSEP and we found no significant differences between the four tested shapes. This is discussed lines 1-12 page 8623. Maybe it is not clear enough?

Interactive comment on Biogeosciences Discuss., 10, 8611, 2013.

C5059

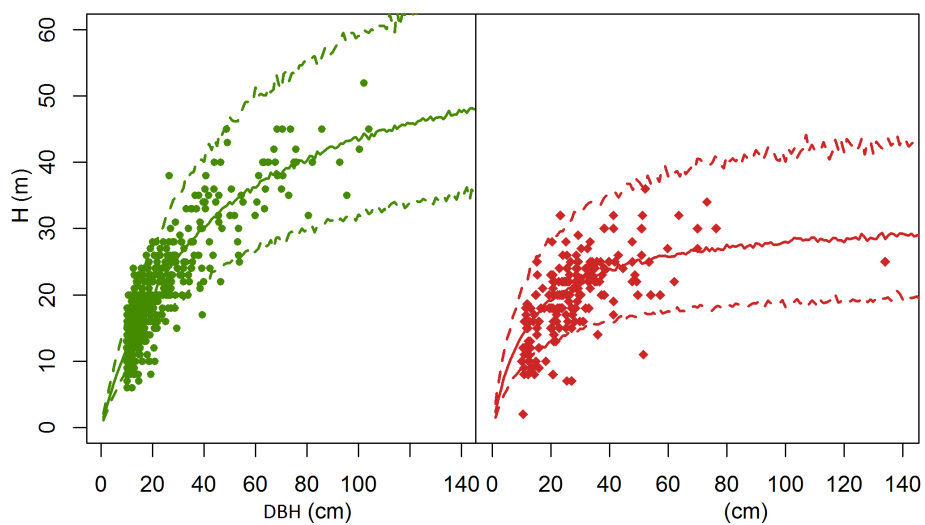


Fig. 1. Height-DBH relationship plotted in two different forest plots

C5060