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Interactive comment on "Relative contribution of stand characteristics on carbon stocks in subtropical secondary forests in Eastern China" *by* A. Ali et al.

Anonymous Referee #2

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General comments

In general, I consider the MS has great potential in providing a strong contribution to ecological literature by assessing the relative role of different predictors and particularly of structural and species diversity on carbon stocks in subtropical secondary forests. This is a topic of active research today. However, I consider the current version is still away from publishable in Biogeosciences. I have five main comments on this:

1) First, Rather than providing a strong conceptual approach for framing their aim, that is, testing the role of structural diversity on aboveground biomass, authors made a long but not structured literature review of the many variables that could explain variation in AGC stocks, of course making particular emphasis on those the will further

test. After such review, there are no clear stated hypothesis guiding the application of statistical methods and their prediction is so general and non-exclusive that it could be demonstrable almost with any result. I consider the conceptual model in Figure 1 is a good starting point, but such a model should be clearly sustained in the introduction. It could serve as the hypothesis to be tested. Another argument on favor of this critique is that soil carbon stocks are almost no introduced and furthermore, authors pretend to explain them with the same set of predictors than used for the AGC case. This shows a naive approach that does not take into account the vast literature on the factors influencing C stocks in (tropical) soils.

2) In accordance with the unstructured introduction, authors present a wide range of statistical tests for testing basically the same idea. They use simple linear regression, multiple regression and SEM to test the same predictors each time. If you have worked to present a conceptual model like that in Figure 1, why to use approximations do does not allow to test it? Moreover, simple and multiple regressions ended providing almost the same results that SEM, with the exception of two new significant interactions in the SEM model, which are then undervalued by the authors. So I would suggest that according to the idea of a very clearly presented unique hypothesis, a unique analysis should be presented, in which case SEM seems to be the best option.

3) There are some parts of the discussion where authors present possible explanations to their results, but they do not realize that their own results (particularly the SEM) provide no support for such explanations. I consider that a more careful interpretation of such a model should be done.

4) Authors sometimes cite references that are not appropriate or even not refer to the point under discussion. See several specific comments below.

5) I consider the inclusion of site productivity as a predictor should be reconsidered (see specific comments below).

Specific comments

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Line 54. Replace ", and store" by "by capturing ". Yu et al 2014 highlight the capture capability rather than the currents C stocks.

Lines 58-59. Authors assert site productivity impact C stocks. However, Lohbeck et al. did't tested the effect of site productivity on biomass or carbon stocks, they tested the reverse. A recent test of the effect of productivity on biomass can be found in Grace et al. 2016 Nature for grasslands, or the general hypothesis for the causal relations between productivity and biomass in tropical forests can be found in Quesada et al. 2012 Biogeosciences or Malhi et al 2012 J. of Ecology

Line 60. Does species diversity impact C stocks? The reference provided (Con et al. 2013) does not seem to provide conclusive evidence. I suggest to soften this assertion and to look for additional literature to sustain it. See for example Cardinale et al. 2011. Am. J. Bot.

Lines 61-63. Although authors use consistently a definition of "stand structural characteristics" throughout the MS which includes both "structural" and "diversity" variables, I consider this concept does not provide to the reader a complete idea of what is being tested here, and could hamper the interest on the work. The role of biodiversity has been the subject of much research in the last two decades and stating it separately may make more appealing the work to a broader audience. Therefore, I would suggest to use different concepts for structure and diversity.

Line 66. Include the recent work from Poorter et al. 2016 in Nature "Biomass resilience of secondary forests"

Line 69. I would say that Age is a variable that summarizes or reflects the action of several processes. Probably the authors need to rethink how age is included in their conceptual model. Particullarly, which would be the direct effect of stand age on carbon stocks? What is the ecological mechanisms behind such effect?

Lines 78-82. Soil C is an important component of the study. However, it is just briefly

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introduced and the ecological mechanisms linking aboveground biomass or productivity with soil C stocks are not explained here. Therefore, your questions regarding soil C are not fully understandable.

Lines 83-90. These lines say the same than previous paragraphs, no? Probably better to merge them with previous paragraphs and to try to focus more on the general hypothesis regarding the effects of forest age, stand structure and stand diversity.

Line 98. What is C synthesis?

Lines 110-111. So anything could explain C stocks? Isn't there a hypothesis on which of this potential explanatory variables could be more important? Also, what is stand density? Isn't it included within stand structure in general?

Line 112. What is a direct effect of stand age? Isn't it mediated always by stand charachteristics? Which is its ecological basis?

Lines 114-115. This generalization applies only for wet forest, probably not for dry forests. Please be specific.

Lines 117-118. That is not an adequate prediction, that is a "all matters" scenario. Rather, say that you tested the contribution of different predictors.

Line 122. Randomly? Within the entire landscape? How were you sure they represented all the successional gradient possible? There were no mature forests, conserved and/or degraded? Did you use a GIS to select them? Please elaborate on site selection.

Line 122. Stand age in relation to what? What kind of disturbance?

Lines 124-130. Questions should be rephrased, their actual form is not appealing (they seem barely descriptive). Also, questions 1 and 2 are the same but in their discrete and continuous forms, respectively.

Line 140. The "consequently" is not clear. Authors asserted "there were different

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intensities of human disturbances (typically logging)" Do they refer to different types of disturbance, different intensities of logging, or both? This is quite important since recent studies on succession have highlighted the relevance of different types of previous land-use or land-use intensities for the unfold of succession (Mesquita et al. BioSciences 2015, Arroyo-Rodríguez 2015 Biological Reviews). Moreover, it is particullarly relevant the authors provide a detailed description of the disturbance history of the region and of the related criteria for selecting plots in particular.

Line 141. Rather than developmental stages, which may refer to a departure from a clear-cutted forests, authors could use "stands with different levels of degradation" or "stands with different level of perturbation"

Line 142. Does this mean that there was previously a landscape characterization of different landcover types from which it was possible to filter only successional forests and to select randomly the location of the plots?

Line 143. Any kind of disturbance? Excluding only recent human disturbance? What do authors mean exactly by "recent"?

Line 148. What do the authors mean by "typical habitats"? Did the authors include plots in different environmental conditions? Or do they refer to different successional habitats all in under the same environmental conditions?

Line 152. It is interesting that until here I assumed the authors constructed a chronosequence of sites derived from a pulse-type disturbance. This was probably because of the use of the terms forest age and secondary forests, which are commonly used in the literature to refer to clear-cutted sites. However, after looking at Table 1, I figured out that sites were assigned to one of three different "development stages", which seems to be different in the intensity of previous logging. Therefore, sites were not clear-cutted but instead affected by a pressure-disturbance like continuous logging. Therefore, I suggest the authors provide their working definition of secondary forest, or, alternatively, use the term "degradation level", "degradation intensity" or simply "log-

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ging intensity" to refer to their different levels of logging. Authors can look at several references for the definitions of secondary forest and degraded forest (Chazdon 2014 Second Growth, Chapter 1; Chokkalingam & de Jong 2001 International Forestry Review; Putz & Redford 2010 Biotropica).

Line 169. Which stages? You have not defined such stages here.

Lines 170-171. Ok, so it is an indirect measure of productivity. Much more is therefore required on the definition of the disturbance regime to which such plots were subjected. Was the initial point (year 0) a clear-cutted forest for all? Or a selectively logged forest as suggested by Table 1?

Line 176. Which one of these references was used to calculate biomass? Please be specific.

Lines 175-184. Why is this paragraph here? A portion could be used during model framing in the introduction section.

Lines 188-189. This is not an argument to exclude height from biomass calculation. See for example Chave et al. 2014 GCB for a detailed discussion on height inclusion in allometric equations.

Line 192. First sentence is not clear: what kind of uncertainty is avoided and why?

Line 193. second sentence should be re-written

Line 196-197. what are D-H models?

Lines 210-211. Why you did not use the Chave et al. 2014 equation, which seems to improve Chave′s et al 2005 equations?

Line 2015. Therefore, which equation you used? I suggest all this discussion could go in an Annex or supplementary material, leaving here in the methods only the descripcion of the equation finnally used

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Line 216. Why you did not used the Alí et al. 2015 equation for all the tree community?

Line 239. Does this values refers to the number of categories, the range of the categories or the limits of the categories?

Lines 244-245. Why to use correlated DBH-height classes if you then want to assess their explanatory ability in a unique multiple regression model? Should not the categories be selected based on their correlation to the variables you want to explain, i.e. biomass? You could simply try to test correlation between diversity and biomass and select those categorizations given the maximum correlation.

Line 251. Mathematical notation is wrong. x should denote only one thing: or the number of different attributes evaluated (3) or the number of classes within a attribute. Furthermore, sub-index for p should be i (pi), because the proportion is evaluated for each i class within 1 and x (if x is the total number of classes).

Line 270. Please say explicitely at the beggining of the secition 2.3 which C pools are considered in this study: "two carbon pools were assessed in this study: aboveground living biomass of the tree community (excluding lianas and herbs, no'), and soil organic C in the top 20 cm of soil").

Lines 270-276. Probably better to summarize lines 270-276 by saying that for each series, al the possible variable combinations and interactions were tested (a fully ...model) and the best model was selected using AIC.

Line 291. If you have previously settled a hypothesis of a hierarchy of effects acting on C stocks, why to use simple and multiple linear models and not going directly to the SEM? What is the original hypothesis? Doesn't SEM allows you to test the same that multiple regression model allows, that is, which are the structural determinants of the C stocks?

Line 304. Age is not expected to be linearly related to AGC. Also, from Figure 2 it seems that some of the relations could be better explained using a non-linear (but

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probabliy linearizable) model.

Lines 307-310. So, really the logic behind fitting such models was to select the best to use in SEM? Why not allowing SEM to test the whole model? Why testing two different models if you can test only one?

Lines 314-320. This paragraph is very difficult to grasp. Does the second sentence mean that rather the structural diversity, the proportion of big trees could alternatively explain biomass?

Lines 315-318. If I understood well, this is the same problem with analyzing Shannon index results for species diversity: we do not actually know if an increased diversity is caused by increased number of categories (which in this case means increased number of big trees) or by a more even distribution among categories (that is, basal area is more equitatively distributed among dbh categories). If you want to dissect such effects, then wouldn′t be easier to have from the beginning to different predictors indicating directly such different possible explanations? Moreover, previous findings would allow authors to hypothesize that the amount of big trees is an important predictor of forest biomass (Slik et al. 2013 Global Ecol. Biogeo.), so authors could use some indicator of the size of the biggest trees as a predictor of biomass.

Line 322. I′m not completely sure that a higher correlation with CV means that dominance of big trees is not important. Higher CV values means that deviation from the mean DBH or H increases, which can happen if bigger trees are present but there is an uneven size distribution.

Line 332. Most of the significant relations seems to violate linear regression assumptions, particularlly that the straight line is an adequate representation of the relationchip or that variance is homogeneous. Authors do not clarify through the text or in the supplemetary tables if other relationships were tested or if variables were transformed to meet assumptions

Line 334. Species density? Stand density?

Line 341. What is the positive variation?

Lines 360-363. Probably, the synthetic models are not necesary. Authors can check that the relative importance of variables in the synthetic modelcorrelates negatively but perfectly to the p values associated to each of the variables in the best-fit model. So probably that part could be taken to the supplementary material.

Lines 368-369. As expected, there is no direct functional relation between stand characteristics and C stocks. This only reflects the poor literature review on the mechanisms that drive C accumulation in tropical forests soils.

Lines 377-379. There is no sense in having these two alternative models, at least if there are no competing hypothesis grounded on strong ecological knowledge.

Lines 380-381. I really have a doubt on the meaning of the variable "productivity" here. As defined, productivity is calculated on the basis of stand volume divided by forest age. Stand volume is another measure of biomass (the volume of a forest is filled with biomass, so as it is bigger, biomass is bigger), rather than an "independent" structural measurement. I really think that it is an spurious relation and that the authors should consider to exclude it from the model.

Lines 382-383. What is the difference between this model and the multiple regression model?

Lines 410-411. This last sentence evidence the poor literature review made by the authors on the ecological and physical processes controlling C stocks in soils. I suggest to not include soil C stock estimation in the model, but rather to provide their estimates as supplementary material.

Lines 419-420. Such argument would imply that higher species diversity have incidence on higher structural diversity. However, there is no association between species and DBH diversity, so data does not support such possibility.

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Line 433. If such argument was true, a significant relation between species diversity and stand age should arise.

Line 449. Uncertain? It seems authors are "averaging" results from two different approaches and therefore saying that there is no conclusive evidence, even with the same data! That's why it is important to have a clearly stated hypothesis from the beginning and to use the adequate analytical framework to test it.

Line 451. A similar argument was raised by Grace et al. 2016 Nature

Lines 467-468. Site productivity does not mediate such relation according to SEM. Please rephrase.

Line 481. Dupuy et al. 2012 do not test age as a predictor of biomass. Please see Hernández-Stefanoni et al. 2010 Landscape Ecology for the adequate reference. There are a lot more of references on the recovery of biomass or AGC stock during succession in both wet and dry tropical forests. See also Poorter et al. 2016 Nature for a recent compendium.

Lines 485-487. this argument is not right. Although it is true that at tree level bigger trees acumulate more carbon, at the stand level it is not true if we have a gradient of forest age, for which maximum accumulation commonly occurs early in succession. See Mora et al. 2016 Biotropica, Vargas et al 2008 GCB or Yang et al. 2011 New Phytologist for how expected rates of change should be higher in the first decades of succession.

Line 488. Not pretty sure of this since CV test does not seem to be the best indicator.

Line 499. Lohbeck et al. 2015 never tested productivity as a predictor of biomass, but the reverse (biomass as a predictor of productivity).

Lines 500-504. In the model site productivity is not affected by forest age, so this argument does not march data.

Line 514-516. This argument is not clear at all

Lines 536-537. Please elaborate more on how stand diversity could be improved based on your results.

Line 790. Why should soil organic C depend on structural stand variables? There are many ecological process between C accumulation in the aboveground biomass and its accumulation in soil (literfall, biomass decay, microbial growth), plus a set of factors that may have greater potential impact (soil type, bulk density, previous land use, etc). For the case of soil organic C, this model seems very naive.

Specific comments

Line 123. Replace “in accordance to” by “regarding the” or “about the”

Line 230. Delete “in”

Line 247. Please modify to ".. diversities were calculated for each plot using equation 3".

Line 254. Replace “analysis” by “calculation”

Line 512. Replace by "effect"

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