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Biogeosciences
Discussions

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

A. Ali et al.

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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:

=> We are grateful to referee #2 for providing constructive comments on our manuscript. According to your comments, we will thoroughly revise the MS both in theoretical and analytical aspects. Please finding our responses to your specific comments below.

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.

=> Thanks for this constructive comments. We agree with your concerns that the research aims are not well structured in our previous manuscript. In the new revision, we will clearly introduce our conceptual model in the introduction for driving the specific hypothesis. We will test new conceptual models (please attached figure) in the revised manuscript, by considering your suggested papers in the following comments. The best SEM model among all combinations of DBH and height diversity based on different discrete classes and/or three types of paths between species diversity and structural diversity will be selected on the basis of lowest AIC. We will exclude stand density (trees per hectare) and site productivity from our conceptual model, as you have suggested in the following comments. => In addition, we have re-analyzed our data with SEM models before we make this reply and we believe that revise MS will be improved thoroughly by testing the models below.

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.

=> We agreed with your comments about statistical analysis. We will use SEM models by testing different combinations of height and DBH diversities based on different discrete classes, and then select the best model through AIC. In this way, we believe that our proposed hypothesis and conceptual model will be clear than the last version of the MS. Actually, we have finished the data analysis by following the approach above when we write this reply. We found that this approach is feasible and reasonable for testing the specific hypothesis.

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.

=> We apologize for the lack of clarity in the discussion section. We will clearly discuss our model with sound evidences in this study and other studies. Thank you.

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

=> We will avoid such mistakes in the revised MS. We apologize for inappropriate citations in the discussion section.

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

=> Thanks for your constructive comment. We will follow your suggested paper (Grace et al. 2016 Nature for grasslands) for making a new conceptual model, as already shown above. Therefore, site productivity will be excluded as a predictor.

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

=> We will correct in the revised MS. Thank you.

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

=> Thanks for pointing it out. Actually, this is a wrong citation. We will revise this and make new concept model by considering your suggested papers in the revised MS. Thank you.

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.

=> Thanks for your constructive comment here, and we will soften this assertion and revised it by following your suggestion accordingly.

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.

=> This comment is very constructive. We will follow your suggestion by considering stand structural diversity as a latent variable including DBH and height diversity, while species diversity as a separate variable, as shown in the above mentioned conceptual models. Thank you.

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

=> We will consider their recent work, and thanks for suggesting this significant paper.

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?

=> Indeed, Age is a variable that summarizes processes such as growth, ingrowth and mortality. Our data do not include processes-based measurements, and we wish to use age to summarize multiple processes responsible for standing above-ground carbon. We will use one type of complex conceptual model in the revised MS. In the last version of MS, it was quite a big confusion by using two different models, such as age model and stand characteristics model. We will avoid such type of confusion in the revised model. Thank you.

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

=> After careful consideration, we feel that it may be best to exclude SOC component since data associated with many drivers such as local site condition, past disturbance

history as well as litterfall (leaves and roots) feedback for SOC are not available. We would like to use much of our efforts on AGC stock by testing numbers of SEM models, in order to clarify the effects of stand age, stand structural diversity and species diversity on AGC stock.

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.

=> We will introduce our new hypothesis based on our new proposed conceptual models, as you have suggested in the last comments. Thank you.

Line 98. What is C synthesis?

=> We apologize for using different terminology here. Actually, we meant C stock or storage.

Lines 110-111. So anything could explain C stocks? Isn0t 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?

=> Thanks for your constructive comment. In the revised MS, we will consider stand structural diversity including DBH and height diversity, and species diversity as potential explanatory variables, when assessing the residual effect of stand age on both of them. Stand density is the number of trees per hectare. Yes, it is included within stand structure in general. We will avoid this variable in the revised conceptual models.

Line 112. What is a direct effect of stand age? Isn0t it mediated always by stand charachteristics? Which is its ecological basis?

=> With increased stand age, biomass accumulation will increase by following stand development, tree growth and increased stand structural diversity. Therefore, stand age can act as a driver for improving carbon stocks. In the revised MS, we will use one complex conceptual model. In the last version of MS, it was quite a big confusion by using two different models, such as age model and stand characteristics model. We will avoid such type of confusion in the revised model. Thank you.

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

=> Yes, we will correct here.

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.

=> We will design a new conceptual model by considering all your comments on the previous conceptual models. Thank you.

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.

=> Thanks for pointing it out. ‘Randomly’ is not an appropriate description of site selection. Actually, we selected site and plot through both field survey and local forestry inventory that used for classifying regional vegetation types. We will further elaborate on site selection in the revised MS.

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

=> We define stand age as time since last stand replacing disturbance, which includes clearcutting, reclamation from agriculture, and typhoon. This will be clarified.

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.

=> We will revise the proposed questions according to the new hypothesis and conceptual model. Thank you.

Line 140. The "consequently" is not clear. Authors asserted "there were different 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 particullarlly relevant the authors provide a detailed description of the disturbance history of the region and of the related criteria for selecting plots in particular.

=> Yes, different types of disturbances such as logging, land conversion, typhoon etc, as well as different intensities of logging at different sites were happened in the history. We will clarify here in the revised MS. We will provide a detailed description of the disturbance history and criteria for plot selection. Thank you.

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"

=> We will clarify here in the revised MS. Thank you.

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?

=> Yes, more exactly saying, there was a landscape characterization of different forest use types, i.e., secondary shrublands, mature forests protected from clearcutting or logging, and logging forest. We will clarify this section in the revised MS. Actually, the detail description of the study area was not included in the last version of MS, which make reviewers doubtful.

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

=> We will clarify the kind of disturbance in the new revision. Recent means for the last 3 decades according records from local government. Thank you.

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?

=> Sorry for the vague wording, we will rephrased this statement. Thank you.

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 "logging 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).

=> Thanks for your constructive and helpful comments on site selection. We will clarify here in the revised MS, by following your comments and suggested papers for definitions.

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

=> Developmental stages such as young, pre-mature and mature forests. We will define here in the revise MS.

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?

=> Yes, we have indirectly estimated the site productivity by reviewing the official documents of Ningbo Forestry Bureau, Zhejiang Province, to collect relevant data about the disturbances for each site in the study area. The study plots included both clear-cutted forests and selectively logged forests. More exactly saying, there was a landscape characterization of different forest use types, i.e., secondary shrublands, mature forests protected from clearcutting or logging, and logging forest.

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

=> We used both references because Brown's (1989) equation only covers trees with DBH > 5 cm while Ali et al. (2015) equation was developed for small trees and shrubs. Thank you.

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

=> Thanks for constructive suggestion here. We will revise MS as recommended.

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.

=> We will use Chave et al. (2014) GCB equation for the estimation of AGB in the revised MS. Thank you.

Line 192. First sentence is not clear: what kind of uncertainty is avoided and why? Line 193. second sentence should be re-written

=> These sentences will be rewritten. Actually, most of the generalized allometric equations are for tropical forests instead of subtropical forests. Therefore, we compared different models to avoid uncertainty. We will used Chave et al. 2014 equation, which is extendable to subtropical regions. Thank you.

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

=> Model using DBH and height as predictors for estimation of AGB. We will clarify in the revised MS.

Lines 210-211. Why you did not use the Chave et al. 2014 equation, which seems to improve Chave0s et al 2005 equations?

=> We will use Chave et al. 2014 equation in the revised MS. Thank you.

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 description of the equation finnally used

=> Brown's equation was used for the estimation of AGB of big trees. Now, we will use Chave et al. 2014 equation in the revised MS. Thank you.

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

=> Ali et al. 2015 equations were only developed for small trees and shrubs.

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

=> These values refers to the limits of the categories. For example, for DBH 0 – 2 cm, 2.1 – 4 cm etc.

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.

=> Thanks for your constructive comment. By following this comment, we can not get any good fit for SEM model, as we have tried. Therefore, it is better to test different SEM models instead of just focusing on correlations. In the new revision, we will test a number of SEM models through combinations of different DBH and height diversities based on different discrete classes, and then select the best model through AIC. In order to make things clear, we will provide statistics of all SEM models in one table and more details for selected best model. Thanks.

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).

=> We will correct the equation form in the revised MS. Thank you for pointing it out.

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").

=> We will include the beginning statement for C stocks.

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 onC stocks, why to use simple and multiple linear models and not going directly to the SEM? What is the original hypothesis? Doesn0t SEM allows you to test the same that multiple regression model allows, that is, which are the structural determinants of the C stocks?

=> By considering your all comments on the conceptual model, we will only employ

SEM model in the revised MS. In addition, the bivariate relationships will be included in the appendix file or even main text. Thank you.

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 probabliy linearizable) model.

=> We will consider your suggestion in the revised MS, by assessing both linear and several linearizable forms (log, exponential and 2nd order polynomial) and choose the one with lowest AIC in our revised SEM. Thank you.

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?

=> We will use one SEM model and access the whole model as well as the best model based on AIC. Thanks for helpful suggestion.

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?

=> Yes, you are right. We will correct here in the revised MS.

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 wouldn0t 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.

=> Thanks for your constructive comments here. We will use SEM model to test different combination of DBH and height diversities based on different discrete classes, to know whether increased diversity caused by increased number of categories has any different effect on C stock.

Line 322. I0m 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.

=> We will use the alternative approach, as you have suggested above. Thank you.

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 supplementary tables if other relationships were tested or if variables were transformed to meet assumptions

=> We will provide such results or explanations in the revised MS. Thank you. Line 334. Species density? Stand density?

=> Actually, it was species diversity and stand density (trees per hectare). We will clarify this.

Line 341. What is the positive variation?

=> Means positive linear relationship. We will modify this.

Lines 360-363. Probably, the synthetic models are not necesary. Authors can check that the relative importance of variables in the synthetic model correlates 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.

=> Thanks for constructive comment here.

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.

=> We will include more potential literatures about AGC stock, while we would like to drop SOC component in the revised MS. Thank you.

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.

=> We will redesign a new conceptual model by considering your all comments. Therefore, this part will be updated.

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.

=> We agree with your suggestion to exclude productivity from our conceptual model. Thank you.

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

=> Sorry for providing double proof of the results. We will only consider SEM model in the revised MS.

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.

=> OK, we will follow your comments in the revised MS. Thank you.

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.

=> We will revise here according to the new analysis. Thank you.

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

=> We will revise here accordingly. We apologize for doubtful statement here.

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! That0s why it is important to have a clearly stated hypothesis from the beginning and to use the adequate analytical framework to test it.

=> We apologize for such doubtful argument, it will be clarified in the revised MS. We acknowledge the uncertainty due to using of three types of statistical models. We will focus on SEM model in the revised MS, as you have suggested in above comments.

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

=> We will include their argument to support our statement.

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.

=> We will revise here according to our new conceptual model. Thank you.

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.

=> We will correct here in the revised MS. But see Stephenson et al., 2014 (Nature) for such augments, which is really interesting.

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

=> For CV of DBH as a good predictor of AGB, please see Zhang & Chen 2015 (J Ecol.). Thank you.

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

=> We will revise it. Thank you.

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

=> We will exclude productivity from our conceptual model, as you have suggested in the one of the above comments. Thank you.

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

=> We will clarify here in the revised MS.

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

=> We will put more light on the importance of stand diversity in the revised MS. Thank

you.

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.

=> Thanks for your constructive comment here. Actually, we are interested that whether and how stand characteristic affect SOC stock. We will drop SOC component from our analysis, as explained in the above response. We believe that focusing on AGC stock by testing several SEM models in order to clarify the ecological mechanisms may be sufficient for this study.

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"

=> We will correct the above mistakes in the revised MS. Thank you.

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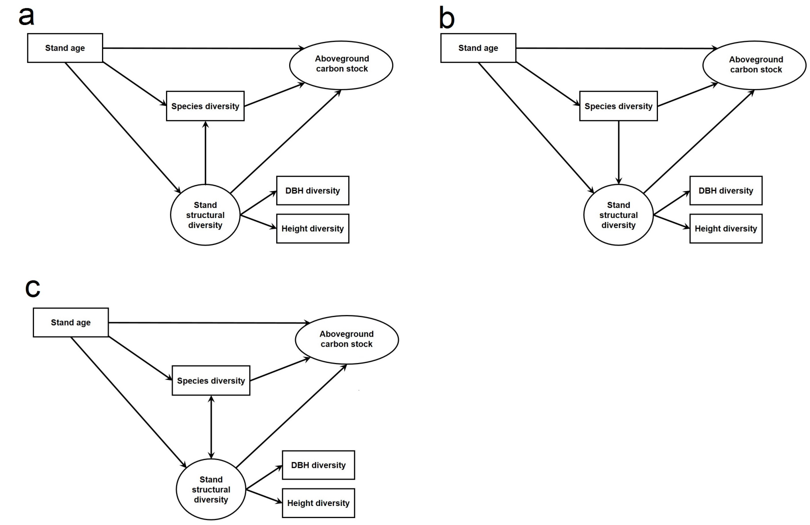
a
Stand age
Aboveground carbon stock
Species diversity
Stand structural diversity
DBH diversity
Height diversity
b
Stand age
Aboveground carbon stock
Species diversity
Stand structural diversity
DBH diversity
Height diversity
c
Stand age
Aboveground carbon stock
Species diversity
Stand structural diversity
DBH diversity
Height diversity

Fig. 1.