Biogeosciences Discuss., 10, C7449–C7457, 2013
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Biogeosciences Discussions
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Interactive comment on "Relative roles of local disturbance, current climate and palaeoclimate in determining phylogenetic and functional diversity in Chinese forests" *by* G. Feng et al.

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Received and published: 18 December 2013

Dear Referee,

Firstly, thank you very much for giving us a very positive comment and lot of useful advice on how to improve this manuscript. Below are our responses (in blue) to your questions and suggestions.

1. Methods: Despite the small number of study sites I belief the results are robust, especially because the large size of the plots and the large amount of species included which should result in robust estimates of phylogenetic and functional diversity. However, I miss a clear justification why the authors used generalized linear models (GLM)

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with a Gaussian error distribution and not simply ordinary least's squares regression, from which they would get a proper R2. If there is not a clear justification to use GLMs that I cannot find in the main text, I recommend using linear models instead.

We will replace all the results related to GLM with ordinary least's squares regression (the results are similar and do not change any conclusions) in the revised manuscript.

2. Conclusions: The authors use maximum plant height to construct a one dimensional functional diversity measure, which essentially is the variation of this trait across communities. I belief, based on a single trait, one cannot conclude that functional diversity is driven by the considered environmental factors as is done in line 14-17 in the Abstract or in the first paragraph of the Discussion. I belief the lack of other components (i.e. traits) of functional diversity does not justify this overall conclusion. I would recommend being more careful with the interpretation of the results (i.e. using variation in max. height instead of functional diversity).

Related to this, I also find it difficult to infer disturbance from the proportion of light demanding species, especially when shrubs are considered. I speculate most of them are grouped as light demanding. Furthermore, rough terrain with potentially steep hill slopes or arid conditions may prevent a closed canopy thus favoring light demanding tree and shrub species, which doesn't necessarily relate to disturbance. In other words, I argue that the abundance of light demanding species may have other causes than disturbance. To me, it is also not clear what kind of disturbance you refer to? In that sense, I have doubts whether the conclusion with respect to disturbance is justified and I recommend that you provide more support for it and clarify what are the major disturbance regimes in those forests.

Concerning our usage of maximum canopy height diversity to represent functional diversity, we agree that this is a simple measure of functional diversity that does not include much functional variation that exists in the forest. However, we still believe that it can usefully be called a functional diversity measure (there is precedent for this, see

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for example Smith et al. 2013, Ecology), and have tried to be clear throughout that our functional diversity measure is based on maximum height.

However, we would pay attention to make this even clearer in the revised manuscript related to the interpretation of the results. For instance, line 14-17 in the Abstract we would say: "Hence, local contemporary and regional historical factors have highly contrasting importance for the geographic patterns of the functional (as represented by variation in maximum canopy height) and phylogenetic aspects of Chinese forest woody plant diversity."; in the first paragraph of discussion, the first sentence would be "For woody plant communities in forests across China, phylogenetic and functional (as represented by variation in maximum canopy height) community structure show highly divergent relations to potential driving factors, with the former most strongly constrained by palaeoclimate and the latter most strongly determined by contemporary local ecological dynamics."; also some other important places.

The proportion of light demanding species (LDSR) to infer disturbance was used in Molino and Sabatier (2001) and Feng et al (2013), where they argued that LDSR was even better than the proportion of pioneer species, because it not only reflects major canopy gap openings, but also reflects more fine-grained canopy heterogeneity that cause variability in the amount of light reaching the forest floor. We checked the growth form composition of the light-demanding species, and found only 58 are shrubs (of 167 shrub species in total). In contrast, 288 light demanding species are trees (of 570 trees in total). So the ratio is even higher for trees than shrubs. Furthermore, judging from the new version of Fig.2 with plot names on it and Fig.1, the four plots with highest LDSR are DJY, BSZ, GTS and DHS, which have very high precipitation. All plots were built in the core area of nature reserve or natural forest and the main disturbances would be natural fire, typhoons, ice storms etc. We will summarize this information and include it in the revised manuscript.

Minor comments by section:

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Abstract: Please revise last two sentences and being more specific with respect to the results. I belief, both sentences as stated here, are not supported by the results in general.

We will correct this.

Introduction:

Line 22 – 2 (next page): I think, both, local and regional processes are equally considered. However, the first being more studied by community ecologists, while the latter by biogeographers. Biogeography has a long history about the causes of biodiversity (see von Humboldt, A.R. Wallace). There is definitely no need to state that the latter are increasingly considered.

Although biogeography has a long history, we think it is reasonable to state that regional and historical factors are increasingly considered in the field, as this reflects the rapidly rising availability of molecular, palaeoclimatic, and palaeoecological data, most of which had very low availability just a few decades ago.

Line 20: 'not at all'; Line 22: 'a broad'; Line 2 (14661): delete 'been' and rephrase: ': : :, but the focus of the present study'.

We will change all of these technical in the revised manuscript.

Line 13 (14661): not sure if 'limited' is the right word here. Maybe try 'constrained'

We will change it to 'constrained'.

Line 14: 'Therefore: : :' I cannot understand how the content of this sentence results logically from the previous one. Please consider further explanation.

Will be carefully revised.

Line 15: delete 'for'

Will delete it.

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Methods: Section: 2.1.2. Phylogenetic tree: You constructed a phylogenetic tree for all 1102 species. When you did the tree separately for tree and shrub species, you only included 570 tree species and 167 shrub species, which makes 737 species. Please clarify what happened with the remaining species.

This is the same situation with the functional tree in comment 1, i.e., some species are existed both as shrub and tree in the flora and where not included in the growth-form specific analyses. So for phylogenetic tree, in total, we have 1102 species, hereof 570 tree, 167 shrub and 365 undefined species. We will mention this in the revised manuscript.

Section 2.1.3. As mentioned earlier, I belief maximum height is not sufficient to refer to it as functional diversity measure. I can see the effort collecting trait information for over a thousand species, but wouldn't it be possible to include e.g. seed size, which is eventually available in other databases. I am not suggesting to do this, but ideally you would include seed size and a leaf trait such as SLA. This would reflect fundamental dimensions of plant strategies (i.e. LHS-scheme by Mark Westoby), which would allow you to refer to it as functional diversity.

Comprehensive data on additional traits are not available for many of the study species, so it is not possible to do so at present, though it would certainly be an interesting next step.

Section 2.1.4.

Please provide the spatial resolution of the climate data. Line 18: How was the temperature anomaly calculated? (Note that present-day MAT simulated by the Earth System Model may differ from observed present day MAT. This may be crucial for the anomaly).

We used a cell size of 0.00833 degrees for the current climate and 0.04167 degrees for palaeoclimate, and will add this to the revised manuscript. The temperature anomaly was calculated by comparing modern interpolated climate surfaces (from Worldclim)

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to modeled palaeoclimate surfaces. The reviewer is correct that this may introduce artifacts in that model errors may be misinterpreted as anomalies. While palaeoclimate projections are certainly uncertain, the uncertainty is probably small relative to the magnitude of the estimated anomalies

Line 3: As mentioned earlier: The abundance of light demanding species may have other causes than disturbance. This may be true in homogenous plots in tropical rainforests, but may not apply for your study region (i.e. considering the effect of aridity or topography).

We also answered this before. Although the four plots with highest LDSR are located at steep area, the precipitations in the four plots with are higher than other plots. Anyway, we would argue the disturbance is likely to be a major cause of the variation in LDSR.

Last sentence. Please clarify how you included plot area. Did you include it as a covariate in the modeling of each predictor variable (i.e. the two-predictor GLMs)?

Plot area here was treated the same as other variables, not a covariate.

Section 2.2. Line 26 (14664): You used GLM with Gaussian error distribution. Why not a linear model (ordinary least's squares regression)?

We will change this into ordinary least's squares regression.

Line 4 (14665): Please explain how the pseudo R2 was calculated and why you didn't choose the normal R2 and linear regression analysis?

We will use adjusted R2 of ordinary least's squares regression to replace the pseudo R2 in the revised manuscript.

Line 6 (14665): Please explain how summed AIC's were calculated.

The summed AIC weights (w) were calculated based on both single and two predictor models here. We first built all possible models (single and two predictors, which are 6+15=21 models). So each variable will occur in six (one single by itself and five other

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two predictors) models. The summed AIC weights of a variable were calculated based on the mean AIC of all the models it occurs. This was done using the “importance” function in “MuMIn” R package, which we will mention this in the revised manuscript.

Section 3 Results

In the presentation of the results, please also make clear which relationships were significant providing the p-value and not only which ones weren’t significant.

It is generally advised not to mix AIC and p-values (e.g., cf. papers by Burnham and Anderson).

Section 4 Discussions:

For Hmax you use the terms maximum plant height, canopy height or stem height inter-changeably. Please unify the terminology and use only one of the three.

The Hmax used was maximum canopy height, we will unify this in the revised manuscript.

Line 25 (14666): ‘Hence, these two key aspects of woody plant diversity appear to be predominantly shaped by divergent assembly mechanisms acting on very different spatiotemporal scales.’ Please clarify what you mean by ’divergent assembly mechanisms’ and how you infer them from your results.

The divergent assembly mechanisms here should be climatic niche conservation for phylogenetic diversity and disturbance for functional diversity, which we will clarify in the revised manuscript. The significantly increasing phylogenetic clustering with increasing MAT anomaly might suggest the stable climate would promote the diversification and large climate change would filter the community, leaving only certain closely related clades. And the increasing LDSR indicates more light in the forest floor, which will promote the establishment and growth of canopy or sub canopy species and further promote the clustered Hmax structure.

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I miss a critical evaluation of the paleo-climate data. From when are the CCSM3 climate data? Are they still state of the art? How was statistical downscaling achieved (NOTE: The Earth System Models run on a T42 grid ($\sim$ 2.8 °resolution); Did downscaling account for topography?)

The CCSM3 climate data is from Collins et al., 2006, which used T85 grid for land (about 1.4 degree resolution). Downscaling was a simple statistical downscaling of anomalies from the modern climate surface, which thereby takes into account anomalies.

Line 9 (14669) Please name the divergent processes and how you infer them from your results.

We have answered this in the “line 25 (14666)” related question and we will add more information here like what we did before.

Line 13 (14669) Which theoretical expectations? Please name then again, because it is not clear which ones you exactly mean. For me it is also not clear how disturbance comes in here. E.g. Purschke et al. 2013 investigated assembly mechanisms after disturbance and during succession. I can understand how successional dynamics drive functional diversity, however I am not sure if I understand how you define disturbance and how it drives functional diversity.

We mentioned these expectations in the introduction line 16-20 page 14660, that phylogenetic patterns reflect biogeography history and functional structure reflect ongoing ecological processes. We will add this in the conclusion here in the revised manuscript.

As we mentioned before, the disturbance here is defined by LDSR and it might be caused by nature fire, typhoon, ice storm etc., which would produce canopy gap or smaller gap by broken branches. These gaps will increase the light reaching the forest floor, and promote the establishment and growth of canopy or sub canopy species, which further promote the clustered Hmax structure.

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Line 18 (14669) Please name the different determining factors

This sentence would be like this “Other aspects of functional diversity may well reflect different determining factors, e.g., interactions among traits or climate (Wright et al., 2007; Swenson and Weiser, 2010).”

Figures:

Table A1: What is the unit of area, hundreds of square meters? Please use m2 or ha.

It should be ha. We will change it in the revised manuscript.

Table 3: Please rephrase the caption of Table 3. What do the AIC weights tell us? Which ones are for the single predictor model and which ones are for the two-predictor model?

The summed AIC weights (w) in Table 3 were calculated based on both single and two predictor models, which we mentioned in line 5-7 page 14665. We built all possible models (single and two predictors, which are 6+15=21 models). w tells us the relative importance of each variable in all the models it occurs. So generally, the higher the value, the more important this variable is.

Fig A1 and A2: Both figures a very difficult to read. Please consider presenting the trees in a ‘round’ arrangement.

These two figures will be put in the supplement and only available online as electronic, so readers can download the figures and zoom in to better check them.

Yours sincerely, Gang Feng, on behalf of all authors

Interactive comment on Biogeosciences Discuss., 10, 14657, 2013.