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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In their paper 'Relative roles of local disturbance, current climate and palaeoclimate in determining phylogenetic and functional diversity in Chinese forests' Feng and colleagues evaluate the role of local and regional factors structuring the phylogenetic and functional composition of Chinese forest communities. The authors use regression analysis to identify the most important drivers, such as disturbance and climatic variables (i.e. current and paleo-climate) determining the phylogenetic and functional diversity of these forests, taking different factors (growth forms: shrubs vs. trees, phylogenetic groups: angiosperms vs. gymnosperms, as well as abundance weighted

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measures vs. presence absence measures) into account. They find that the phylogenetic diversity can be best explained by climate, while functional diversity is related to disturbance. I belief title and abstract do a good job introducing the content of the paper.

Because this study aims for indentifying the environmental drivers of functional and phylogenetic diversity over a large region, which is central to studies in macro-ecology and questions in evolutionary ecology it lies well within the scope of Biogeosciences. The study presents a considerable amount of new data, which may help to extend databases on functional traits and test trait-based ecosystem models. However I have some major comments with respect to the conclusions reached and the methods used:

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 phylogentic and functional diversity. However, I miss a clear justification why the authors used generalized linear models (GLM) 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.

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

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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.

Overall, I find the manuscript is well structured and easy to read. Because of several typos and grammar issues, I recommend further language checking. Furthermore, I belief some redundancies could be removed (e.g. in the very last paragraph) and the Results and Discussions section could be strengthened and elaborated a bit more. For example, in the discussion the study is well placed in the context of similar studies, but lacks ecological explanations in the context of the study region and the discussion of other unconsidered factors (e.g. topography, environmental heterogeneity, land use) that may affect the presented results.

Minor comments by section:

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.

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.

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

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

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

Line 15: delete 'for'

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.

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.

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).

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 rain forests, but may not apply for your study region (i.e. considering the effect of aridity or topography).

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)?

Section 2.2.

Line 26 (14664): You used GLM with Gaussian error distribution. Why not a linear model (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?

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

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.

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.

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 (∼2.8° resolution); Did downscaling account for topography?)

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

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

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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.

Line 18 (14669) Please name the different determining factors

Figures:

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

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?

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

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