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Interactive comment on “The role of plant functional trade-offs for biodiversity changes and biome shifts under scenarios of global climatic change” by B. Reu et al.

H. Kreft (Referee)

hkreft@uni-goettingen.de

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Dear authors,

This is a timely and very interesting study and a logical follow-up of your recently published paper in GEB. I particularly like the way how you present and discuss the results for the three case study regions and how you are relating the model outputs to eco-physiological and eco-functional considerations and regional-scale studies. I strongly believe that more modeling effort of this kind is urgently needed.

I have only some minor comments. All I think are easy to implement in a revised version. The present paper contains only very limited information about the model

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itself, how it works and how well functional richness correlates with empirical patterns (several papers by Barthlott) or (geo-)statistically derived predictions (Kreft & Jetz 2007). Although you are referring to your GEB where you are discussing these issues in greater detail, I believe that this study would be stronger if you had produced a stand-alone paper with a few more general comments.

Use of the term ‘Biodiversity’ – Biodiversity is used in a very inflationary sense here. Biodiversity is such a broad concept making it almost impossible apply it in a straightforward way. We just need to be more explicit which component of biodiversity we are looking at to avoid confusion. What you are investigating here is functional richness and identify. Although there is arguably a relationship with species richness and identity, this relationship is not necessarily strong nor linear. So, I would urge you to reconsider your application of ‘biodiversity’ in the title and in the introduction. A similar mistake is often made with other surrogates of species richness. For instance, you are citing Francis & Currie (2003) in the context of species richness and environment. These authors have actually investigated angiosperm families. There is a discrepancy of two orders of magnitude between the number of families and species and the relationship between both is far from perfect and there is presumably a lot of important information in the deviation (e.g. how could some regions with rather few families produce so many species).

In the results and discussion you are very much focusing on cases and region that exhibit logical patterns. Some shifts in biome identity are actually rather unexpected, e.g. changes from temperate to tropical forests on the Iberian Peninsula. Is this based on your model or on your necessarily simplified biome delineation? Your paper would be stronger if you discussed some of these unexpected patterns. How did they come about and what they mean in terms of further improvements of your models.

Please check your references. The in-text citation are sometimes out of order. I have not checked the guidelines, but I assume that either a sorting by alphabet or year is required.

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Some important details are missing in the figure caption. For instance, the periods (1960-1989, 2070-2099) should be mentioned. Although it appears in your GEB paper, I think a map showing current patterns of FR would be helpful as a reference to interpret the changes. Figure 2 b and c: I think it would improve these panels be better if you would include the color intensities in the color legend at the bottom. In some case it is hard to tell the lighter shades apart (e.g. is Japan a lighter shade of temperate or tropical forests).

Amazon Basin, Sahel, Central China (e.g. Figure 3): I am not sure if these are geographically meaningful descriptions for the regions shown in Fig. 3. Is this the same analysis window for the results reported in Tab. 2? If so, this would not make much sense because you are including vastly differently climates, topographies, biomes (e.g. Andes, Atacama, Cerrado all summarized as Amazon basin?). Please clarify. Furthermore, it is not fully clear to me why you have actually chosen the Sahel. It seems like your predictions have relatively weak support there (Fig. 1 c,d).

quote in the introduction – Not sure if this is really needed to convince readers on the usefulness of models.

p 7452, l8 – “key fundamental functional trade-offs” read a bit clumsy. Delete either key or fundamental.

p 7452, l17 - Please include: “different [emission] scenarios”

p 7453, l4 Usually “grid cell” is not hyphenated. Please double-check.

p7455, l20. I am getting confused here. The $k=12$ is not the same like the k in your k -means clustering. Correct? In the latter the k should be 6, because you yielded six groups. Please rephrase.

p7456, l12 “we selected THREE regions”

p7458, l22-24 This sentence is cluttered. Please rephrase.

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p7460, l4 A reference at the end of this paragraph would be good.

p7460, l14. Same here. Please give a reference to support the q10 statement.

p7460, l21ff A limitation of your model is that you have not considered CO2 effects on photosynthesis and resource allocation. It might be worth discussing this here with a sentence or two.

Best regards,

Holger Kreft

Interactive comment on Biogeosciences Discuss., 7, 7449, 2010.

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