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Biogeosciences Discussions

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.

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We thank Holger Kreft for his constructive comments and believe that they have helped us to make the manuscript clearer.

RC: refree comment, AC: author comment

RC: The present paper contains only very limited information about the model 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.

AC: We described the basic functioning of the JeDi model in Section 2.2. Since this paper presents an application of the JeDi model, which has been documented (Kleidon Mooney 2000) and evaluated (Reu et al. 2010) previously, we consider this basic description as sufficient in order to avoid redundancy with previous publications and to keep this manuscript streamlined. In particular, we did compare our results to the patterns of Barthlott et al. (2005) or Kreft and Jetz (2007), because these are based on the same data set used to evaluate the model in Reu et al. (2010). However, since the simulated changes in plant functional richness are central to this study, we agree with the referee that more information should be given to the reader about the performance of the model and how well it reproduces observed patterns. We therefore added the map of simulated plant functional richness under contemporary climate (Fig. 1) and report on how well this patterns correlate with observed patterns in species richness:

In Reu et al. (2010) we have shown that the global distribution of FR simulated by JeDi corresponds well with observed species richness (Kier et al. 2005) at the level of an ecoregion (Pearson's r=0.75).

RC: 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 straight- forward 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 indentify. 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.

AC: For sake of brevity we kept the term "biodiversity" in the title. However in the abstract, main text and figure captions, we replaced the term "biodiversity" and used the term "plant functional richness" instead to avoid confusion.

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RC: 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).

AC: This is a valid point. Since JeDi only considers the functional aspect of plant growth and its diversity, a discussion about evolutionary driven or taxonomic aspects of biodiversity (e.g. species richness vs. family richness) would go beyond the scope of this paper. Therefore, we replaced the reference Francis and Currie 2003 with Barthlott et al. 1996, Kreft and Jetz 2007 (see Introduction, second paragraph). These references relate to species richness, against which the JeDi model has been evaluated.

RC: 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.

AC: The biome delineation as presented in this study is based on the approach taken in Reu et al. 2010, which is a simplified approach to biomes. It is intended to serve as a means of evaluation and for comparison with previous work on broad scale climate-driven biome shifts. Its results depend on the choice of the number of biome clusters when performing the cluster analysis. In the case of six biomes, the analysis did not reveal the Mediterranean biome, which may lie somewhere in between a temperate, a savanna and a tropical biome. For the resulting biome shift this means that few grid-cells of the northern part of Iberian Penninsula that were classified as temperate

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biome moved closer to the tropical biome, which has to do with the location of the cluster centers in the trait space originating from the number of clusters. Since the Mediterranean biome is not adequately resolved under this setup, we did not further investigate a biome shift in this transition region. However, JeDi allows considering specific changes in various aspects of vegetation characteristics, as presented in the case studies using plant functional traits. Such an approach may therefore better suited because it allows identifying gradual changes, which may go beyond a simple biome classification. To clarify this point we added the following sentence to the discussion (Section 4.2):

This simplified method to delineate biomes helps to identify and visualize major biome shifts, whereas more information about plant functional trade-offs driving these shifts can be obtained from directly analyzing trait value changes. We do this in the following for the three focus regions.

RC: 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.

AC: We checked and revised

RC: Some important details are missing in the figure caption. For instance, the periods (1960-1989, 2070-2099) should be mentioned.

AC: We included the time periods in the figure caption.

RC: 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.

AC: We included this map as Figure 1. (see also first point)

RC: 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 of

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tropical forests.

AC: We added a panel to the legend that incorporates the color intensity.

RC: 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.

AC: The mismatch between the geographic window used to produce Table 2 and the naming of the focus regions originated from the introduction of Table 2 in a very final stage of the manuscript. We adjusted the description of the focus regions to: Tropical South-America, Sahel, Southeast China. In fact each region comprises different biomes and vegetation zones (as it is probably the case for some single grid cells too, e.g. in mountain regions). For the sake of consistency, Table 2 summarizes the geographic regions displayed in Fig.3. We did not delineate these regions based on biome boundaries, because biomes are in transition, hence we opted for something easy and tractable.

RC: 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).

AC: We choose the Sahel because it is an interesting example for a "positive" biome shift where a scarcely vegetate region increases in vegetation cover and functional richness. Furthermore, it is, at least to the author's knowledge, an unexpected biome shift, for which rather few documentation exists compared to high latitude biome shifts. At least for the western part of the Sahel the trend among models is consistent.

RC: quote in the introduction – Not sure if this is really needed to convince read-

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ers on the usefulness of models.

AC: We removed the quotation.

RC: p 7452, l8 – "key fundamental functional trade-offs" read a bit clumsy. Delete either key or fundamental.

AC: We deleted the "key"

RC: p 7452, l17 - Please include: "different [emission] scenarios"

AC: Included

RC: p 7453, l4 Usually "grid cell" is not hyphenated. Please double-check.

AC: Checked and replaced

RC: 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.

AC: As explained in the description of the equation (Section 2.5) the k stands for the 12 functional traits. To avoid confusion we replaced the k by an n.

RC: P7456, l12 "we selected THREE regions" p7458, l22-24 This sentence is cluttered. Please rephrase.

AC: We inserted the "three" and rephrased the sentence:

Despite this general consistency, our projected changes differ considerably for individual regions from the projections by Sommer et al. (2010), who suggested a loss of species richness in the Sahel and an increase in Southeast China, while we find an increase in FR in the Sahel zone and a significant decrease in Southeast China.

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

AC: We added the reference IPCC (2007).

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

AC: We added the references Amthor (1984).

RC: 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.

AC: This is a valid point. We inserted a paragraph (Section 4.3.2) to explain why JeDi does not consider effects of ambient CO2 on plant growth and survival:

Whether changes in physical climate lead to increased ecosystem water stress further depends on the effect of atmospheric CO_2 on plant water use. Water-use efficiency, the amount of carbon assimilation per unit of transpired water, generally increases under elevated levels of atmospheric CO_2 (Medlyn et al. 2011). While the physiological effects of CO_2 at the leaf level are relatively well understood (Amthor et al. 1995, Ainsworth et al. 2005), their effects on especially biologically diverse ecosystems are still under debate (Koerner 2009, Medlyn et al. 2011). Since plant growth involves many other processes than photosynthesis (such as nutrient acquisition and competitive interactions), different plant growth strategies may differ in their responses to elevated CO_2 levels, which might affect long-term ecosystem dynamics and species composition changes (Koerner 2009, Ellsworth et al. 2004). The trade-offs involved in community responses to elevated CO_2 are not sufficiently understood to be included in JeDi, though these effects will be addressed in a future version of the model. As a consequence, we can currently not quantify the relative importance of atmospheric CO_2 changes on the FR and FI metrics. However, if CO_2 fertilization had an important role in tropical carbon assimilation, or water use, the predicted high loss rate in FR might be substantially reduced.

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