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.

S. Lavorel (Referee)

sandra.lavorel@ujf-grenoble.fr

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Dear authors, This paper presents a nice follow up on the publication of the new implementation of the model JeDi (Kleidon & Mooney 2000) in GEB (2010). While that first paper focused on the summarized presentation of the model, and most importantly on the types of analyses that can be made of model runs in terms of functional diversity metrics and trait trade-offs, this second paper demonstrates an application of these methods to the case of future projections. The content of the paper is well in line with the title, with the proposed analysis for focus regions identified as 'hot spots' of change of underlying trait trade-offs. This is a very original approach and is extremely inter-

esting as such. I have only few comments that may especially contribute in furthering the interest of the paper to readers interested in the mechanisms underlying the observed responses rather than in the geographical responses per se, as well as two terminological comments.

- As the first reviewer, I would prefer if the term biodiversity was not used so generically, but rather using specific terminology, especially relating to different dimensions of functional diversity. It is not helpful to participate in the confusion of the term, nor does it serve the purpose of delivering unique information on the functional dimension of biodiversity.

- Although this terminology has already been established in the first published paper, I am having trouble with calling 'functional identity' the mean functional trait value of a pixel. Functional identity rather refers to the presence of a particular functional trait category or value.

- Biome shifts: to what extent are these shifts related to the magnitude of climate change in the sensitive regions (i.e. amount of exposure) (temperate-tundra and tundra-polar boundaries)? Although this is discussed very little (too little!) the general consistency of predicted changes with other models is likely to simply result from the effect of amount of exposure. In the case of hot / cold deserts the question may also be refined in terms not of magnitude only, but of crossing specific physiologically relevant thresholds (i.e. the climate becoming more 'livable' for plants). This could be discussed specifically in relation to the traits included in the model, and to those identified as relevant to explaining the observed shifts.

- Trait trade-offs: interestingly the trade-offs that are highlighted as responsible for changes in FR and FI, and discussed for the different focal regions seem to be quite different from those trade-offs identified as most important for the current distribution of different biomes. This is quite interesting and deserves more in depth discussion. In other terms, responses to climate change scenarios appear to relate to traits that

are not necessarily discriminating in present conditions. This is of particular interest because it suggests that diversity for such traits, rather than for the current 'response traits' is likely to become a key factor in the response to changing climate.

- The conclusion of the paper states that the observed patterns, while consistent with previous studies, are of greater interest because they can be explained mechanistically by trait responses rather than by empirically derived bioclimatic relationships. This is a bit of a short conclusion and it would be desirable to summarise how or where this approach brings significant improvements. In particular, such a mechanistic approach is likely to be important to address responses to novel combinations of climate parameters. This is well illustrated for the change in precipitation regimes in Central China, and would deserve more in depth discussion.

Best regards,

Sandra Lavorel