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Biogeosciences Discussions

Interactive comment on "Plant communities as drivers of soil respiration: pathways, mechanisms, and significance for global change" *by* D. B. Metcalfe et al.

B. Reu (Referee)

breu@bgc-jena.mpg.de

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In their paper, Metcalfe et al. review how plant traits affect soil carbon cycling, especially soil respiration (R), via the quality and quantity of litter and the amount of carbon plants allocate below-ground. They further discuss the role of community composition and diversity on R and highlight those processes that may become relevant under global warming including the potential for positive feedbacks within the Earth system. Finally they describe how these processes are captured by dynamic global vegetation models (or not) and identify research needs, also in terms of model development.

The paper is well written and gives a nice overview on diverse aspects of plant func-

tioning relevant for soil carbon cycling. Especially the link between current state of knowledge and how it is treated in global climate models is interesting for Biogeosciences readers. I have only few comments that may help to improve the readability of the paper.

In general, I think the focus of the paper needs to be made more clear. Furthermore, the text could be sharpened, since some information is scattered throughout the manuscript and, finally, some parts need more detailed explanation (see specific comments below). Especially,from the abstract it was not entirely clear for me what follows, also because some statements seemed rather disconnected from the rest.

SPECIFIC COMMENTS:

p 2146, l 4-7: I would move this sentence more to the end of the abstract. Before, it may be helpful to motivate the need for such a review, for example by showing the discrepancy between state of knowledge about the diversity of pathways plant communities can affect R and how this knowledge is represented in vegetation models. After this, I would shorten, but better connect your findings.

p 2146, l 9-11: This sentence does not logically connect to the previous one and it remains unclear why a shift towards fast growing plants with nutrient rich litter could provide a positive feedback mechanism, which I think is also not the case (see below).

p 2146, l 21-22: For me it remains unclear and vague how experimental and field studies would improve model accuracy. This sentence calls for a very concrete outline of experiments and field studies in your paper, which I think is not given (or scattered throughout the text). Either remove this sentence, or, more interestingly outline how such experiments shall be designed.

p 2150, l 15-17: I cannot follow the point that shifts in plant community composition towards greater dominance of faster growing species or those with rapid turnover would provide a positive feedback. This can only be the case if C efflux from the soil is greater

than the C flux into the soil. The only process I could imagine would be a priming effect and the release of old carbon stored in the soil, however I cannot find priming mentioned in your text.

p 2151, l 7-18: What about warm and infertile sites such as tropical lowlands, where soil nutrient status and herbivory favor plants maximizing resource retention? E.g. Fine et al. (2004) Herbivores Promote Habitat Specialization by Trees in Amazonian Forests, Science 305, 663-665. In this case R may not be amplified, but dampened by the plant community.

p 2151, l 23-28: You make the point here that plant strategies that maximize resource retention are those which also allocate a greater portion of their assimilates to below-ground organs. I wonder if this relationship really exists. You refer to Figure 1 here, but I think a reference would be important. For example, in nutrient poor tropical lowlands plant strategies invest a lot of resources into fine root growth, but at the same time may have high root turnover rates through increased rates of herbivory or not if they invest in protective traits. Since both may happen under similar environmental conditions, I think this needs further explanation. E.g. Fine et al. (2004) Herbivores Promote Habitat Specialization by Trees in Amazonian Forests, Science 305, 663-665.

p 2154, l 4-8: Please explain how you derive your hypothesis that TBCF dedicated to mycorrhizae originates from the geographic variation of TBCF across forests. I wonder if this hypothesis might be biased because of a geographic bias in studies towards high and mid latitudes. What about mycorrhiza in tropical nutrient poor soils (e.g. in the Amazon Basin where P seems to be a limiting nutrient). I wonder if there is some literature from the tropics to include here (e.g. Hättenschwiler et al. 2011: Leaf traits and decomposition in tropical rainforests: revisiting some commonly held views and towards a new hypothesis, New Phytologist, 189: 950–965), in case there are no studies on mycorrhiza in the tropics, it might be worth pointing out this research need.

p 2157, l 1: Please explain further why invaded ecosystems would have higher R,

this seems very speculative to me and may be context dependent. Again, only what has been assimilated can be respired, otherwise there must be a depletion of the soil carbon stock through a shift in community composition. However, you do not mention this and it remains unclear for me, what the underlying mechanisms would be. Please clarify.

p 2158, l 20-27: I think from natural gradients it is impossible to say that diversity doesn't matter, since diversity effects can only be tested within similar vegetation types, because of confounding effects across vegetation types, which may result from different environmental constraints (e.g. differences in climate, soil nutrient status, disturbance regime). Moreover, I think the latitudinal diversity gradient is not a good example here, since it is subject to ongoing debate and multiple hypothesis for its origin exist. In most cases productivity is only used as an explanatory variable, however experiments in grasslands show a clear relationship between diversity and productivity (within one vegetation type). See e.g. Marquard et al. (2009) Positive biodiversity–productivity relationship due to increased plant density. Journal of Ecology 97, 696–704. You may also consider that Fig. 2 shows mainly data for above ground production, while data on below ground production hardly exist. Also the loss of above ground carbon through insect herbivory in the tropics is probably not considered in Figure 2, which can be quiet large in the tropics.

p 2161 Section 5: I belief this section would benefit from a table summarizing which plant traits in general have an effect on R, via which process, in which DGVM (land surface model) this process is represented and how, and maybe the potential for a positive or negative feedback. The white areas of such a table would immediately show needs for future research.

p 2165, l 5-7: Again, I cannot follow this point, because increased assimilation and respiration balance each out, which is not a positive feedback, please clarify.

p 2165, l 19-22: I have difficulties to follow this point, maybe you can explain a bit

further. What about this relationship across vegetation types? In general, I think the distinction between within and across vegetation types is important to make, since it may cause some confusion. You do this in the paper in some cases but not in others, I would recommend to be more consistent, especially in the abstract and the conclusion. This is particular important when referring to plant traits, which are usually measured at the individual plant level and also for discussing the effect of trait diversity. As mentioned before, diversity effects can only be identified within similar vegetation types.

MINOR:

p 2147, l 2: "feedbacks", please explain between which components feedbacks occur, and how R impacts and feed backs. Or do you mean "impacts" on R?

p 2147, l 2: here it might be worth mentioning that there are feedbacks of terrestrial ecosystem to the climate system, and that not only climate change impacts terrestrial ecosystems.

p 2148, l 1: "conflicting results" in terms of what? please explain, which are the "conflicting results" that you are referring to.

p 2149, l 21: Do you mean "Effects of aboveground litter quantity and quality" on R

p 2151, l 19: Do you mean "Effects of plant allocation below-ground" on R or the effects of plant communities on plant allocation below-ground?

p 2152, l 14: "towards" more photosynthetically active ...

p 2153, l 23: Which aboveground plant properties do you mean?

p 2155, l 16: "Effects OF microclimate ..."?

p 2161, l 14: You forgot to mention the SEIB-DGVM here, to which you are referring later.

p 2163, l 4: you mention land surface models here. Do you mean DGVMs or land surface models that run as part of climate models (e.g. CLM or JSBACH). Please clarify and/or give a reference.

p 2164, l 18: It may be more appropriate to cite here: Reu et al. (2010) The role of climate and plant functional trade-offs in shaping global biome and biodiversity patterns. Global Ecology and Biogeography. DOI: 10.1111/j.1466-8238.2010.00621.x

In general there is a large body of literature on plant functional trade-offs (more than acquisitive vs conservative plant growth strategies) and allocation patterns, especially relevant for section 2.2 where you address below-ground allocation. Here I also miss some theoretical considerations, this may be relevant since below-ground allocation is hard to measure. You may find some of the pioneering work in:

Bazzaz, F., Grace, J., and Raven, J.: Plant resource allocation, Academic Press, San Diego, California, USA, 1997 Grime, J.: Plant strategies, vegetation processes, and ecosystem properties, John Wiley & Sons Inc, Chichester & New York, 2001

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