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Biogeosciences Discussions

Interactive comment on "Community shifts and carbon translocation within metabolically-active rhizosphere microorganisms in grasslands under elevated CO_2" *by* K. Denef et al.

Anonymous Referee #1

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Review of K. Denef et al. "Community shifts and carbon translocation within metabolically-active rhizosphere microorganisms in grasslands under elevated CO2"

The manuscript is reporting on a 13C-labeling experiment within the Giessen-FACE atmospheric CO2 enrichment study. Using the 13C signatures in phospholipid fatty acids (PLFA) from repeated sampling, carbon fluxes from roots to bacteria via fungi was tracked. Results suggest rhizodeposits are first taken up by fungi, before being transferred much more slowly into bacterial biomass. Elevated CO2 appears to increase relative abundance of some PLFA while decreasing others.

The quality of the manuscript as well as the degree of sophistication of the analyses

carried out speak well of the experience and competence of the authors.

Nevertheless, there are a couple of serious problems to be mentioned.

1) The experiment is set up to investigate the particularities of C-translocation under elevated CO2. Such CO2 enrichment is usually associated with increased productivity, but not in the plots that Denef et al. used for their experiment. Thus, CO2-effects in this experiment, if present, may reflect a quite untypical situation for future, increased CO2 concentrations. Certainly results can not be discussed with reference to a “typical” CO2-response of a grassland ecosystem. 2) Large parts of the CO2-effects on grassland communities are attributed to CO2-effects on stomatal conductance, triggered by internal leaf CO2 concentrations, altered vpd and other factors. Often microclimatic effects of the fumigation system (increased temperature, turbulence, etc.) add to affect stomatal conductance, to finally yield both changes in productivity and water use efficiency. The FACE technology minimizes such unwanted microclimatic effects. But the 80l chambers put on the grassland plots for six consecutive hours in order to label the system, must be suspected to create larger “cuvette-effects” than CO2-enrichment effects. 3) In “Conclusions” the authors demand more such studies, to test if their results can be reproduced in other ecosystems. I would be curious to know whether they can be reproduced in the GiFACE for a beginning, as the study reported on here lacks any form of replication. This also makes the interpretation of the inconsistent responses of fungal rhizodeposit-C assimilation difficult. 4) In the “Introduction”, the main hypothesis is stated as elevated CO2 having a larger impact on fungi than on bacterial communities. But in the manuscript I could not find any attempt to quantify the difference of fungal reponse vs. bacterial response.

In my opinion, the study presented by Denef et al. shows that A) C from rhizodeposits its first processed by the fungal community, and only much later by the bacterial community, indicating a strong retention effect of (presumably) AMF. B) Inconsistent responses of PFLA species to CO2 enrichment (two increase, one decreases) make inconsistent responses of different fungal species possible.

BGD

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Seen on the background of the drawbacks described above, these results do not justify publication of the manuscript.

ps: I do not understand the concept of analyzing metabolically active as opposed to inactive microbial communities. I suggest that in such an experiment you are always studying both types as soon as you are discussing responsiveness to any experimental treatment. It is always responsive (active) vs. not responsive (inactive) sources of PLFA.

Interactive comment on Biogeosciences Discuss., 4, 1437, 2007.