

Interactive comment on “Rapid transfer of photosynthetic carbon through the plant-soil system in differently managed grasslands” by G. B. De Deyn et al.

Anonymous Referee #3

Received and published: 1 April 2011

General comments

This paper describes a “classical” $^{13}\text{CO}_2$ pulse labeling study and assess the rhizodeposit C transfer to soil microorganisms. Although the paper is well written and does not have major flaws I have serious problems with the level of innovation and the level of discussion that has been provided. I think that a high quality journal like Biogeosciences deserves better science than what is presented here.

Further I have some major comments with respect to the experimental design which did, not allow to TEST the hypothesis put forward by the authors (which in a way they also acknowledge). The latter especially concerns the level of N fertilization

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



tested: 25 kg N per ha is simply a bit over background N deposition of the experimental site, which is in the UK or NL I suppose because it is not mentioned in the materials and methods.

In the conclusion section the LIMITED results from this research are over-emphasized or SIMPLY NOT SUPORTED BY THE DATA: there are no “striking differences observed” from these data, the outcome for the saprotrophic fungi is only based on one (1) data point!!, and ESPECIALLY the conclusion that C transfer from plants to microbes is common across plant species and is unaffected by management is not corroborated by the experimental design since a very limited amount of N was applied with respect to background N deposition (although it was unclear where the study was carried out); AND IN CONTRADICTION WITH THE CONTENT OF THE ABSTRACT.

Specific comments

Page 5, line 3-4: we deal with 3 or 4 treatments? I guess it is 3? Page 8, Line 7: what is plant biomass distribution? Is this the same as biodiversity? Page 9, line 17-19: this statement is much too strong for the results presented in this MS. The information given from page 9, line 20 to page 10, line 16 is very trivial and is therefore not essential for the MS. Page 11, line 20-21: Due to the low biomass of mosses its role in C sequestration is largely overestimated and fully biases some major conclusions of this MS. Fig 3: why 6 species in the legend and 5 in the figure caption? Fig 4: I really wonder why no PCA or Canonical discriminant analyses was used to assess the effect of ¹³C allocation in function of time?

Interactive comment on Biogeosciences Discuss., 8, 921, 2011.

BGD

8, C394–C395, 2011

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

