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Discussions

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

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We were rather taken aback by this review, which we found to be unnecessarily negative in tone, at times incorrect, and generally lacking in detail. Despite some very strong statements, there is a general lack of rationale for the criticisms, and the tone is in complete contrast to the supportive and constructive comments of the other two referees, who provided us with well thought criticisms for us to address. We especially refute the suggestion that our work does not represent quality science, which as noted above is also in contrast to the views of the other referees and is not substantiated in the reviewer's comments. Nonetheless, we have responded to this reviewer's comments

as follows:

(1) The main criticism of our work expressed by referee 3 is the low level of N fertilisation (25 kg N/ha/year) used as level of continued N fertiliser management in the grassland system. Although this level may seem to be low relative to productive, lowland pastures, it is the level of fertilisation used in species-rich grasslands of Environmentally Sensitive Areas in England, as recommended by the Ministry of Agriculture, Fisheries and Food (1992), and its long-term implementation at our experimental site has been shown to cause substantial changes in vegetation composition and productivity, soil microbial communities, and soil C and N stocks and fluxes, as reported in a series of papers published in the Journal of Applied Ecology, which is a leading ecological journal (Smith et al. 2003, 2008; De Deyn et al. 2011). In view of these arguments, we believe that this treatment is representative of the grassland management practice of extensive species-rich grasslands, and that our hypotheses are valid given that past studies have shown significant vegetation and soil responses to this treatment.

Despite the above, we now better emphasise the fact that the results may have been different with a higher dose of N and highlight the differences in N treatments between previous studies and ours in our discussion. We did this by reformulation of the original text (P931, lines 6-12) into: 'The reason why we did not find effects of fertiliser use on 13C enrichment may due to the time elapsed since application fertiliser application in May with pulse labelling last day of August, and the modest addition rate (25 kg/ha 20:10:10 N:P:K) of the fertiliser. In the aforementioned studies of Bradley et al. (2006) and Denef et al. (2009) the levels of N applications that did cause significant shifts in soil PLFA abundances, irrespective of time since application, and their signature of new photosynthate-C were much higher: ranging from 225 to 450 kg N/ha/year, suggesting that the responses might also be strongly dose dependent.' (see also response to specific comment three of referee 1).

De Deyn, G. B., Shiel, R. S., Ostle, N. J., McNamara, N. P., Oakley, S., Young, I., Freeman, C., Fenner, N., Quirk, H., and Bardgett, R. D.: Additional carbon seques-

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(2) The statement of the referee that the outcome for the saprotrophic fungi is only based on “one (1) data point!!” is incorrect. For each response variable, each value at a certain point in time represents the average over 12 data points (each field plot is a data point) and its variation. If the referee is alluding to point in time rather than to data point then we would like to draw attention to the fact that also after 24h saprophytic fungi show a steep increase in 13C enrichment.

(3) We maintain our conclusion that C transfer from plants to microbes is common across plant species and is unaffected by the management applied in our system. However, we now do this with more caution in that we stress that the results hold for the range of fertiliser treatments used in our study, and highlight that we cannot extrapolate (only speculate) what might be found with higher doses of fertiliser. In the conclusion, we therefore nuanced our point by including the notion on the level of fertiliser application by stating: ‘Together, our findings suggests that the rapid assimilation, turnover and transfer of C from plants to microbes is common across plant species and that this short-term C cycling is unaffected by management change at least for low levels of fertiliser application.’

(4) To clarify where the experiment was carried out we have included more details in the material and method section in the form of following text: ‘Measurements were made

in four treatments arranged in three blocks in a long-term (since 1990) multi-factorial grassland restoration experiment (Smith et al., 2008). The study site, Colt Park meadows, is located in north west England in the Ingleborough National Nature Reserve (latitude 54° 12’ N, longitude 2° 21’ W) on Lolium perenne-Cynosorus cristatus grassland. The soil is a shallow brown earth over limestone of moderate-high residual fertility (15 mg P2O2 L-1), with 19% organic matter and a C% of 7.7 and N% of 0.75 and average pH of 5.5. All plots were grazed in autumn and spring and cut for hay on 21st of July since 1999.’ We recognise that the level of N addition was low, but even with background N deposition the addition of 25 kg per ha per year is still a treatment on top of that background, which led to many changes in vegetation and belowground properties (see point (1) above).

Specific comments Page 5, line 3-4: we deal with 3 or 4 treatments? I guess it is 3?

Reply: We are dealing with four treatments namely: no seed addition and no fertiliser use, no seed addition and fertiliser use, seed addition and no fertiliser use, seed addition and fertiliser use. This information was provided on line 9-10 of the same page.

Page 8, Line 7: what is plant biomass distribution? Is this the same as biodiversity?

Reply: With ‘plant biomass distribution’ we mean the amount of biomass that was produced by each of the groups. This is not the same as biodiversity because biodiversity entails several aspects, such as the richness (i.e. the number of species) present as well as their abundance or biomass. Biodiversity indices like for example Simpson’s evenness index uses both characteristics of diversity in a community to calculate an index that reflects the biodiversity taking both aspects into account. We replaced ’plant biomass distribution’ by ’the biomass produced by each plant group’ to clarify what we meant.

Page 9, line 17-19: this statement is much too strong for the results presented in this MS.

Reply: We do not think that all aspects of our statement 'Overall our results support the hypothesis that in the field plant species differ markedly in the rate of assimilation, retention and translocation of recently photosynthesised C to soil.' are too strong. But given the comments of the other referees we acknowledge that we should be cautious with the statement on the retention of C. Therefore, we have reworded the statement which now reads as: 'Overall our results support the hypothesis that in the field plant species differ markedly in the rate of assimilation and translocation of recently photosynthesised C to soil.'.

The information given from page 9, line 20 to page 10, line 16 is very trivial and is therefore not essential for the MS.

Reply: The information on page 9 (line 20) to 10 (line) may be trivial to this referee, but may not be all that trivial to many readers. Moreover the other referees did not mention this section as being redundant, and therefore we retained the paragraph.

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.

Reply: We do not agree with the referee and would like to draw the attention to the results shown in figure 1. There we show that the biomass of moss in the unfertilized plots represented the largest biomass of the plant community in the February sampling (equal to grass biomass in unfertilized plots in September).

Fig 3: why 6 species in the legend and 5 in the figure caption?

Reply: The referee is right that we needed to include the sixth species in the legend of figure three. We have included 'one legume species' in the legend.

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?

Reply: We choose to show the actual levels of delta 13C (y-axis) in relation to time (x-axis) rather than using PCA analysis because with the latter analysis we get a sense of

shifts in position in multidimensional space, but that analysis does not show the actual levels.

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