Biogeosciences Discuss., 4, S590–S594, 2007
www.biogeosciences-discuss.net/4/S590/2007/

Biogeosciences Discussions

Interactive comment on "Long-term steady state ^{13}C labelling to investigate carbon turnover in plant soil systems" *by* K. Klumpp et al.

K. Klumpp et al.

Received and published: 30 May 2007

1. Particularly the use and interpretation of data describing the C-flow between different soil OM fractions needs to be strengthened and improved. The flow of 13C between the different fractions is barely discussed although this was the main objective of the current study. Please, see specific comments in the following. -The objectives of the paper are to show for the first time the feasibility of changing the 13C isotopic signature of soil organic matter by steady state labelling and to apply this method to the study of soil carbon turnover in the litter pathway. Given size limitations, we do not deal here with the analysis of carbon fluxes between OM fractions. This requires modelling (see e.g. Personeni and Loiseau, 2004) and given space limitation a separate manuscript will be needed. Personeni, E. & Loiseau, P:. How does the nature of living and dead roots affect the residence time of carbon in the root litter continuum? Plant and Soil,

267, 129-141, 2004.

2. The title needs to be more specific referring to grassland. Certainly the described method could be applied to other soil-plant systems, but it has its limit not being useful for forest ecosystem studies. -We have now revised the title to include grasslands.

3. The Materials and Methods section needs to be strengthened e.g. by avoiding too much repetition. One suggestion is to organize the text for Figure 1 in a more tabulated manner, for example listing the different compartments by succeeding order downstream in the air flow direction. Supplier names can be included in this list, and removed from the body text to facilitate reading. -Legend of Figure 1 was reordered in a tabulated manner. This section was also shortened in some parts to avoid unnecessary details.

Specific comments Materials and methods. 1. Section 2.1.1 Line 8. In Figure 1 it appears that the air flow enters (IA) and leaves (OA) in close proximity on the same side of the enclosure. How was a complete mixing of air in the enclosure ensured. Was the ventilator actually a fan mixing the air inside? Please, specify. -Indeed, air mixing was provided by a fan. This is now explained in the text 2. Line 8. Was PAR only measured externally? -PAR was measured several times inside the enclosures and outdoors to determine the fraction of PAR absorbed by the enclosures. 3. Section 2.1.2 Line 4 and throughout. It would be very helpful if the dimensional numbers were supplemented by also the directions, i.e. W, L and H It is unclear how a 0.30 m3 belowground compartment can be subdivided into three 0.14 m3 units. -We added dimensions of enclosures, monoliths and belowground compartement thoughout the M&M section. 4. Line 15. What was the rationale for actively pumping air through the soil column? This is a rather artificial condition that may affect soil gas characteristics such as aeration significantly. This needs to be commented further in the text. Moreover, it is not clear whether the air pumped through the soil was mixed with effluent chamber air and thus included in the overall C- and 13C-mass balances or if the soil-air was analyzed separately. This also needs to be specified -The soil air was analysed separately. A

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small pressure head was maintained by the open-flow system. This pressure head and the depression caused by the pump eliminated back-diffusion of CO2 from the soil (e.g. Casella and Soussana, 1997). The flow of the pump was adjusted to suppress diffusion of soil CO2 to the aboveground compartment. We clarified this in the text and added further comments on the degree of disturbance of the soil atmosphere. Casella, EE. and Soussana, J.F.: Long-term effects of CO2 and temperature increase on the carbon balance of a temperate grass sward. J. Exp. Bot. 48, 1309-1321.1997.

5. Line 15. Suppose it should read the CO2 in the vials was analyzed for d13C. How long time were the air samples stored prior to analysis? Please, provide this kind of information as it is well known that long-term storage of air samples in rubber sealed vials can be problematic causing changes in concentration and isotopic composition. -Vials were measured monthly, as a batch since adjustments in the mass spectrometer method were required. We added this information in the text. 6. Section 3.1.1 It is mentioned that the quality and isotopic composition of influent and effluent air is measured as a control on a regular time scale. In addition to this, it is stated that the isotopic composition of influent air was checked on a temporal resolution of 30 minutes according to the mass balance calculations. However, these calculations were not accompanied by isotopic measurements but presumably based on IRGA determinations of the CO2 concentrations combined with assumptions about the 13C enrichment of the CO2. Thus, from a rational point of view this calculation can not provide a check on the labeling as stated (p 808, line 1) but rather provide interpolated values for the influent air quality as determined at the bi-monthly observations. This needs to be specified more clearly in the text. I also recommend that the calculations and equation on p 808 is moved to the Materials and Methods section. -We agree, that this method does not provide a verification, but is useful to interpolate in between direct measurements of the 13C signature of the labelling atmosphere. The sentences were revised.

7. Section 3.1.2 Line 12 (p 809). A fluctuation between -49.9 o/oo and -40.4 o/oo seems rather significant, perhaps more than a “small seasonal pattern” as stated in

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the text. However, a reasonable explanation for this observation is given. You refer to Fig. 2d apparently visualizing this pattern, but then it is stated in parenthesis that data are not shown (line 13). This is confusing. Nevertheless, the apparent winter depletion of -49.9 is not obvious from the Figure? -Data not shown referred to results from a repeated measure ANOVA. However, the mean delta 13C in winter was actually -44 per mil (and not -49.9 per mil) and was therefore less negative than mentioned earlier in the text. This has been corrected. 8. The initial offset in d13C between the two treatments pictured in Fig. 3 is quite interesting, and the authors propose a couple of explanations for this observation. However, influences of previous as well as current dung and urine depositions by grazing animals and artificial urine on the distribution of 13C among various soil C-compartments is not mentioned at all. This should be considered also in the discussion of the results. -The urine deposition affects vegetation and microbial community, which in turn affect delta 13C of the soil organic matter fractions. The amount of C provided by urine application was very small (4 g m-2 yr-1) compared to the amount of C in the soil fractions analysed. The urine application by grazing and cutting, thus, only affects indirectly the measured delta 13C. We further developed this aspect in the MS. 9. A draw back in the interpretations of the soil C dynamic is the apparent exclusion of the 13C data obtained in the different OM fractions. Apart from the graphical presentation of the AOM data in Fig. 3 it is basically only data from the SOM ($>$0.2 mm) fraction that is discussed. The authors suggest that it is likely that a large part of the “new” C which has deposited into the soil compartment vanished into smaller particle sizes ($<$0.2mm) (p 812, line 25). But as the smaller fractions 13C was analyzed, why are the data not considered? Obviously a lot of effort has been put into these analyses, and this data material would improve the quality of the discussions strongly and should be included. -The objectives of the present MS were to introduce the labelling facility and to show one of the main results obtained. Obviously there are further results on the effects of disturbance on litter decomposition in semi natural grasslands. This however would make the MS too long and will be prepared for a separate paper. 10. Concerning the between treatment

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offset in d13C in SOM and AOM, shown in Fig. 3 and discussed p 811, line7, I think the authors need to emphasize the temporal evolution in the data. As pictured, the two treatments were initially different, and it can be difficult to verify from the graphs only to which extent the discrepancy in d13C changed over time. -We agree, it would be misleading to use delta 13C directly to interpret the time dynamics of soil organic matter fractions. Rather, we have calculate the changes in the amount of unlabelled C in the soil fractions. Thus, first order exponential models were fitted to the decay of old C (g C g-1 soil) and to the accumulation of new C (Table 1, Fig 4). 11. Section 4 I do not agree on the last conclusion. See comment above. -The conclusion has been extensively revised to better reflect the findings on the role of disturbance on soil organic carbon turnover and possible consequences for carbon sequestration.

Interactive comment on Biogeosciences Discuss., 4, 797, 2007.