Biogeosciences Discuss., 10, C4092–C4096, 2013
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Biogeosciences Discussions
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Interactive comment on "Biological and physical influences on soil $^{14}CO_2$ seasonal dynamics in a temperate hardwood forest" *by* C. L. Phillips et al.

Anonymous Referee #1

Received and published: 6 August 2013

This study investigates seasonal variation in the concentration and 13C and 14C content of soil pore space CO2 and soil surface CO2 emissions over the course of a growing season in both trenched plots and control to assess 1) seasonal shifts in the contributions of auto- and heterotrophic respiration to total soil respiration, 2) shifts in contributions from CO2 produced in shallow vs. deep soil and 3) seasonal shifts in the 14C content of heterotrophic respiration. The authors also investigated how atmospheric CO2 invasion, diffusive mixing, and the 14C content of CO2 produced at depth impact the 14CO2 profile of soils – to explain the mismatch between the 14CO2 data observed in the trenched plot and lab incubations.

In my opinion, this is a well-designed and -presented study. As the authors point out, in-

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vestigating the seasonal variability in the 14C content of heterotrophically-respired CO2 was long-overdue. Many studies rely on one-time incubations to assess the 14C content of microbial respiration, because these incubations require soil samples which may disturb/destroy experimental plots, and 14C analyses are time consuming and expensive. Furthermore, the comparison of non-isotope methods (trenching) with 14C-based partitioning of respiration provided new insights into the strength and shortcomings of both methods.

The work provides additional compelling evidence that roots take a long time to die and the CO2 flux from dying roots has a large effect on the 14C signature of soil surface CO2 fluxes and pore space CO2 – this has been discussed before as a problem in interpreting 14C data from trenching and girdling studies. On the other hand, the 14C-based approach clearly does not partition between root and microbial respiration, but 'fast' vs. 'slowly' cycling C; microbes decomposition root exudates respired CO2 with a 14C signature similar to that of roots.

I find it refreshing that the authors present a lot of 'failed hypotheses'. It is very interesting to see that changes in soil moisture where unable to explain the observed shifts in surface flux 14CO2 content.

Lastly, the data presentation and some wording should be improved, but this can be easily addressed in a revision.

Major comments.

10723L6 I don't understand why the 14C content of SOM is declining with depth due to decomposition. Are the authors implying that 14C is preferentially decomposed? Maybe limit this statement to '[. . .] deplete in 14C due to radioactive decay, [. . .]'

Reporting 14C data. Radiocarbon terminology is notoriously confusing. The authors explain how the 14C data is reported in the method section, but I find it confusing that in 10723L20+ the authors discuss other work and talk about changes in '14CO2' in 'per

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mill'. '14CO2' is not a common term used to report 14C data - that would be fraction modern, radiocarbon age or various delta values. Here, it should be DELTA14CO2 in per mill. In other sections of the paper, the authors use the Delta 14CO2. I recommend using the same abbreviation throughout the manuscript, incl. the figure captions.

Missing data. The authors state 10731L18+ that several dates lacked observations from the shallowest gas well and the next deeper well was used to infer surface fluxes of 14C and 13C. What is the uncertainty associated with this process? I am wondering if it could be estimated by calculating the isoflux two-ways (using the 7 or the 14 cm well) on days where both the shallowest and next deeper well provided data.

Root incubations. Roots were only incubated once to estimate autotrophic respiration in August 2011, the year before all other measurements were conduction in the field. Earlier studies (e.g. Schuur & Trumbore 2006) show that in some systems, the 14C content of roots is also variable over time. I wonder if the authors are overstating the effects of not knowing the temporal variability in Rh by ignoring potential changes in Ra.

10743 (1) The authors recommend that in future studies, rather than incubations, field measurements of early spring soil respiration and air/root respiration could be used to infer the heterotrophic endmember. This conclusion seems a little abrupt and is not well worked out in the previous sections. i.e. 3.2 and, or 4.1.

10743 (3) I think the idea to measure 14C of ecosystem respiration at least 3 times to capture annual variability works in some ecosystems, but not in others. In non-mesic systems, 14C of soil respiration barely changes throughout the growing season (e.g. Czimczik & Welker 2010, AAAR), but strongly from year to year (manuscripts in review). Further, we don't know much about the 14C content of ecosystem respiration during the wintertime, so I suggest restricting this statement to 'growing season' rather than 'annual'.

Minor comments.

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Plot 4. Since there is only one trenched plot, I suggest referring to the trenched plot throughout the manuscript as 'trenched plot' rather than 'plot 4' – this would make it easier for the reader to follow.

Format. When reporting data in percent, degree or per mill using the signs, e.g. %, by convention there is no space between the value and the unit. Please remove all spaces throughout the manuscript (text, tables) in the 14C data reported in per mill.

10722L11-13 "[. . .] shifting from an older C composition that contained more bomb 14C, to a younger [. . .]" In this sentence, the comma seems out of place. It should be removed or there should be an additional comma after 'C composition'

10723L1 Replace 'radiocarbon' with 14C

10723L8 missing 'point' after '[. . .]Trumbore, 2000)'

10725L25 It would be more correct to include the authors (in small caps, not italized) with the scientific names of the tree species as many plants have various different names. The complete names can be found e.g. at www.plants.usda.gov

10727L7 '5 mil polyethylene' – what is the unit? Is this supposed to be '5 mm thick'?

10728 '21 m a.g.l.' what unit is this? 'above ground level'?

10733L15 and Equ. 9 - 'Rtot' is not being introduced as an abbreviation (I think)

10736L2 As the authors are presenting two types of microbially-respired CO2 measurements, please specify if the root respiration data is being compared to the trenched plot CO2 or the lab incubations.

10737L21 Calculation of production of CO2 at depth is referred to as '(Eq. 9)'; it should be 'Eq. 8' (see 10733)

10742L14 'OM' not introduced as an abbreviation

Table 1 Remove spaces between value and % or per mill (97%, not 97 %) The unit

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for ‘Atm DELTA14C’ should be ‘per mill’, not ‘per mill Delta14C’ ‘Production vertical distribution’ should be ‘CO2 production vertical distribution’?

Figures

Figure 1. On the background that there are 10 Figures, I am not sure Fig.1 is necessary. The sampling design is described well in the method section. I find it confusing that the plots 1-4 are being displayed by different symbols in various figures. For example, Plot 3 in Fig. 3 is a full diamond, in Fig. 4 a ‘x’, in Fig. 5 an open square.

Figure 5. The panels are referred to as 5a-c in the caption and in the text (10735L11-15), but the panels of the Figure are not labeled. Please include a-c in the Figure. The authors argue that the flux decreased in the trenched plot over the course of 2012, but not in the untrenched plot. This figure does not support this claim, because one can only see plot 3 vs. 4. I suggest either showing the average of the flux in plot 1-3 vs. plot 4 or using more panels.

Figure 6. Here (as well as in the text referring to this figure), the surface flux is ‘Rtot’. I am confused why in Fig 3. the flux is referred to as ‘surface flux’ and here as ‘Rtot’. Are these two fluxes the same or not? If they are, please use ONE consistent term and one abbreviation.

Figure 10. The Y axis label needs a unit (cm)

Interactive comment on Biogeosciences Discuss., 10, 10721, 2013.