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Biogeosciences Discussions

Interactive comment on "Seasonal variations of belowground carbon transfer assessed by in situ $^{13}CO_2$ pulse labelling of trees" *by* D. Epron et al.

Anonymous Referee #2

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This paper is the third in a series from this group describing an elegant and complicated experiment designed to examine transport (primarily) and allocation (to a limited extent) of carbon in mature trees following stable carbon isotope labeling. The experimental approach is not entirely novel, as a similarly elegant experiment has been carried out by Peter Hogberg's group in Sweden with several papers in the last few years. The present authors are to be commended for achieving some incredibly difficult science. The paper does not suffer from serious flaws, but the amount of new knowledge reported here is small relative to the complexity of the experiment. We have basically learned that there are different time lags between photosynthetic labeling and the appearance of the label in soil respiration or microbial biomass. We knew these facts already. Yes this paper is pointing out some seasonal variability but given the massive effort the amount of new knowledge is a bit disappointing. That doesn't

mean it shouldn't be published, but I don't feel this paper will move the field forward like Hogberg's girdling experiment did a decade ago (just one example). The use of the multi-pool model and especially the subsequent discussion could really be improved by reading and incorporating into your thinking the extensive set of experiments done by Hans Schnyder's group. Those papers (by Lehmeier, Gamnitzer, etc) are not even mentioned here. That work was done on small-statured plants – there may be some interesting contrasts with the woody species in your study. I recommend this paper be published in BG after the authors consider these comments and the minor ones below.

Minor:

P 887 Line 5: only 1 objective is listed here (but objectives is plural)

16: the amount of . . . was estimated – why not report it here in the abstract?

18: seasonal patterns differed – why not report some detail in the abstract?

25: telluric is not a common word in English, not even in a scientific use

P 888 line 2: I would argue this is not "autotrophic" but rhizospheric respiration – "autotroph" has a meaning derived from Latin and this use violates it

5: rhizosphere and mycorhizosphere – can associations with fungi be distinguished from those with other organisms? not from the measurement approaches described in this paper – I don't see that a new word needs to be coined here, fungi interacting with roots are still part of the rhizosphere

7: the Bahn and Epron papers don't make the first point of the sentence as clearly as other papers would

12: ". . ." leaves too much to the reader's imagination – this does not belong in a scientific publication

6-17 Hogberg's girdling paper in Nature (2001) belongs in this list as it was the beginning of the recent focus on coupling between photosynthesis and respiration – there

were earlier papers but this was a very important one

22: the recent paper about phloem transport (Mencuccini and Holtta 2010 New Phytologist) might be a useful addition here, as would the review of Kuzyakov and Gavrichkova (2010) Global Change Biology

P 889 line 12: “the season” is vague

9: Hogberg et al. (2008) New Phytologist should be cited here

13: “tunable” not tuneable

16: “the transfer time” is vague

19: “first” recovered here because you didn’t look in the leaves or the phloem – Hogberg et al. (2008) showed of course that these pools show the label first

P 890 line 4: “leafy season” is vague for an evergreen – did you do this in winter?

10-13: can you estimate how much the living root biomass was reduced by the trenching for each tree? 10 %? 50 %? 90 %?

20: “permits to include” is poor grammar – reword

21: this viewpoint is too simplistic and reflects a plant-centric viewpoint: there are other organisms smaller than 30 microns in the soil and many of them are motile (eg, bacteria with flagella) – diffusion also operates, so you are incorrect to assume “the only carbon” passage is via the hyphae – your point is that roots were excluded, but fungi and other soil organisms were present – you cannot assume the mycorrhizal fungi only – remember that saprotrophs also have hyphae and will grow into your cores but will not be connected to the roots!

24-26: at this point in the paper it’s not clear why the rainfall exclusion was required

P 891 first paragraph: why were different sensors and depths used for water content in your different treatments? Without explanation this just appears to be poor experimen-

tal design

Section 2.2: it would be helpful to refer to the Plain and Dannoura papers earlier so that readers know there are other papers from the same experiment – the labeling was described much better in the plain paper than it is here

14: polyane is not a familiar material to me

15-16: to control and maintain constant temperature in a huge transparent chamber is not trivial – more detail is needed

18: is this particular IRGA equally responsive to the isotopes of CO2? Usually this is not the case – it would be helpful for you to report how much of the label was 13CO2 versus 12CO2 – was the entire amount 13CO2? If so, how did you remove the atmospheric contribution (which is mostly 12CO2)? What was the total CO2 mole fraction 12CO2 + 13CO2??? – more detail is needed – perhaps refer to the Plain paper if that will resolve these questions

P 892 line 21: this value of VPDB can be problematic if you report 12CO2 and 13CO2 mole fractions rather than isotope ratios (as little delta) – there are 2 different values for this and they differ by the equivalent of 5 permil – this may or may not influence results with a big isotope label like you have – presumably the authors know this since they cite Griffis et al. (2004) on the next page, but it deserves some discussion here – I think that this is likely to have an effect, maybe a big one, on eq 4 but without some real values it’s hard to tell

P 893: line 10: “root fragments” (not roots)

P 894 line 8: this isn’t my area but most people try to do the processing for microbial biomass C much more quickly, no? is the 4-day delay a worry?

P 900 line 6: dry mass “DM” should probably be defined at first use

P 906 10-13: our community should be past the point where we need to continually

promote the usefulness of the TDL – we've been using them to do stable isotope work for nearly a decade now (the study of Bowling et al. 2003 was conducted in 2001) – we don't talk about how girdling or trenching or electronic soil moisture measurements are promising new tools, but these are just as useful – in my opinion the method you have devised to label a tree taller than my home is the promising new tool!

Figures 2-6 are hard to read and would be easier with the addition of color – keep the symbols though so they can be seen by color-blind people

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Interactive comment on Biogeosciences Discuss., 8, 885, 2011.