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Biogeosciences Discussions

Interactive comment on "Technical Note: A combined soil/canopy chamber system for tracing δ^{13}C in soil respiration after a $^{13}CO_2$ canopy pulse labelling" *by* M. Barthel et al.

Anonymous Referee #2

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General Comments

The manuscript describes a supposedly novel pulse-labelling chamber, where above-ground parts of the plant (here a beech sapling) are physically separated from the soil components to avoid direct tracer diffusion into soil pores. The necessity to separate the soil from the labelling atmosphere is indeed important, as the physical diffusion into the soil creates an artefact that is difficult (if not impossible) to separate from the isotope signal derived from within-plant belowground allocation. However, I am not convinced that this is a truly novel approach. Work by Cheng and Kuzyakov have used physically separated canopy and soil compartments for over 15 years. (e.g. Cheng et

al., 1993, Soil Biol. Biochem 25, 1189-1196; Kuzyakov et al. 1999, Plant and Soil, 213, 127-136). The "novelty" here is that rather than using grasses, the design here addresses tree saplings, but I don't consider this to be truly an innovation.

Regarding the experiment itself, two of 6 beech saplings were exclude from the study on the basis that they were obtained from a different tree nursery, and had been planted in different soil. This considerably reduces the value of the study, as no statistically sound comparison between the droughted and watered saplings can be made, and the results can only be regarded as "proof of concept". The fact that the authors have submitted this as a "Technical Note" probably reflects this realisation. To that end, the exact origin of the saplings and the soil type should not matter, as the objective is to show the potential of the novel chamber design for labelling, with no direct contamination of the soil compartment by atmospheric label diffusion.

Somewhat more worrying is the fact that in one of the remaining four pots (and seemingly also in one of the excluded ones) the critical seal between canopy and soil compartment was not adequate, resulting in contamination. If this system has a "failure" rate of 1 in 3, it will be of little use. The authors should indicate the likely source of the problem, and whether this is an inherent risk of the set-up, or if it can be avoided in the future.

In conclusion, I think that the chamber design is not sufficiently new to warrant a dedicated publication. I have no doubt that the authors have put the effort into building this in order to address scientific questions regarding plant C transfer. With the presented data, there are too many problems to draw firm conclusions but more work will surely be done using this set-up. I recommend that the technical aspects of the labelling set-up are included in the methods section of a more rigorous study in terms of replication of treatment, and that this takes reference to previously existing chamber systems. I hope that the detailed comments below will be useful to that end.

Detailed comments

1604, 8/9: Remove manufacturer's names in the abstract.

1605, 12: 50% is a fairly approximate number, and the actual range is wider, dependent on ecosystem season, etc. I would prefer to see reviews or meta analyses cited to make this general point, as there are a large number of studies on this and it isn't clear why you choose to use these particular ones.

1605, 13: "plant's" or "plants"', rather than "plants"

1605, 13: A citation is needed for the assertion regarding the plant carbon budget.

1605, 29: "determining" is not true, I suggest substituting with "influencing". Delete "even".

1607, 17-19: Delete sentence. This specific detail is explained later, and is not relevant to th experimental design described in this paragraph.

1608, 16: "flow-through" rather than "through-flow" (?).

1608, 20: Please clarify if the soil compartment was sealed against the wooden boards, or if these were structural parts only, with no gas seals formed between wood and PVC or Plexiglas. Wood is not a good material if you want to achieve gas tightness, and if wet, can be a source of CO2.

1608, 21: Delete "Because of the canopy".

1608, 22: Did you seal the joint between the two semi-circular discs?

1609, 5-9: Move these sentences to the Results section.

1609, 10/11: I don't think the exact model of fan is necessary to include. Also: 16.5 l/min seems an extremely high flowrate.

1609, 11: Replace "a well" with "good"

1609, 24: State manufacturers of laser spectrometers here.

1610, 21: What is SERTOflex? Tubing?

1610, 26: Delete "chemically".

1611, 1: Not strictly true; what do you suppose happened to the sulphate and the sodium?

1611, 5: This is new to me. From own experience, I know that our Li-Cor gas analysers (admittedly a different model) "misses" about 2/3 of all 13CO2. Did you test the 13C sensitivity of this analyser yourself? I would be surprised if this particular model was more sensitive to the isotope than others by Li-Cor.

1611, 21: Delete "commercially available"

1612, 17: As d13C is always expressed as per mille, please include "x 1000" in your equation for completeness.

1613, 14: "replicates" rather than "samples".

1614, 14: Better expressed in hours than minutes

1614, 17-18: This s quite speculative (which is ok), but physiologically not very plausible. Phloem transport is diffusive, rather than a mass flow (mainly), and xylem pressure should not influence it by much. (In fact, high transpiration should lower xylem pressure, as this is how water moves in the stem.) Unless you have evidence from literature that these correlate, I think that your data are only very poor proof for such a mechanism.

1614, 22/23: You never describe how you measure conductance or transpiration, but it is necessary to understand this here. If transpiration correlates with phloem transport speed, but photosynthesis doesn't, does this mean that water use efficiencies differed between replicates?

1615, 4/5: I can't see any evidence for photosynthesis driving the diurnal 13C pattern. The phase of the fluctuation not 24 hours, but the light cycle is, and the insert in Fig 5

does not illustrate very well what you describe here.

1615, 10-17: don't quite follow how the starch/remobilisation issue is relevant here. Transitory starch reserves in leaf chloroplasts will certainly be labelled, but if they were activated at night tine, it would take about 20 hours for them to be visible in soil respiration. If the Tcherkez et al model relates to sucrose being enriched over natural abundance levels of bulk starch, your enrichments are several orders of magnitude greater than that, so that this effect is not relevant. The Gessler and Koduma study likewise.

1615, 28-1616, 6: There may be a more consistent pattern if you calculated label-derived 13CO2 soil efflux. A higher 13C abundance in non-watered samples appears to be coupled with a reduced soil CO2 efflux rate, so that the total amount of label returned this way may be similar?

1618, 14-17: I don't think you can conclude starch dynamics on the basis of your results (see above), and this speculation is not required in the conclusion.

Figure 1: "iso = isolation"? Do you mean "seal"?

Figure 4: What are the dotted vertical lines? It would be interesting to know soil temperature also.

Interactive comment on Biogeosciences Discuss., 7, 1603, 2010.