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Biogeosciences Discussions

Interactive comment on "Vertical partitioning of CO_2 production in a Dystric Cambisol" *by* Patrick Wordell-Dietrich et al.

Anonymous Referee #1

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This manuscript describes an investigation into sources of respiration of CO2 in soil profiles in a beech forest. I believe the measurements were generally done well and constitute a rare and valuable dataset, but I do have several questions and comments about the study setup and the data analysis that I would like to see addressed. I think if these concerns are addressed and data are reprocessed more appropriately (the production and isotope calculations in particular), this would be a unique and well-cited paper.

My understanding from reading the Methods several times is that Lysimeter tubes were temporarily installed in a beech tree forest soil, the soil inside was excavated away and used for sampling, and then the tubes were replaced with a large, solid polyethylene plug. The experimental treatments and measurements were then conducted in the area

immediately around the plugged hole. I may have this wrong, and I think a schematic figure showing the physical structure of the experimental setup would really help, or at a minimum clarifying the text. My initial impression at first reading was that the lysimeters remained and some experiments were done inside and others outside, which would have been very different.

The calculations of production rate and units were confusing (e.g. section 2.5.2, Fig's 3-5). I believe production needs to be expressed in conventional terms of unit volume, not area (mˆ3, not mˆ2). To calculate production with the gradient method, you need a difference between fluxes at two depths, and therefore must divide by the difference in depth, and end up with a unit volume in the denominator. You cannot use the gradient method to calculate production at a single depth because any horizontal plane with only two dimensions at some arbitrary soil depth only has one concentration gradient and one diffusivity, and so there is only one flux, and therefore zero production. When you want to sum the production at depth intervals to get the steady-state surface flux per unit area, you must multiply each each production value by the depth increment. If you apply your Eqn 8 to your modeled depths (without dividing) you would be comparing 10 cm to 40 cm depth intervals equally. Will you please clarify this?

Agreement between the profile method and the chamber measurements was off by quite a lot over large sections of time (Fig. 2), and could use more attention in the discussion. For OB1 and OB3 it looks like the modeled fluxes decrease relative to the surface fluxes over the course of the experiment. Could it possibly be that the flux gradient measurement area was impacted by the lysimeter installation (e.g. severed roots) in ways the surface fluxes were not? In OB2 the gradient method overestimated the flux during the growing season, possibly due to incorrect paramaterization of the model/diffusivity?

Why are there missing periods in the CO2 profile data (Fig. 1c) but not in the flux gradient model results (Fig. 2)? Was there gap filling of some kind?

Decreasing concentrations with depth from 30-50 cm does not indicate that production was highest at these depths (section 4.1), but indicates non-steady state diffusion, possibly due to very wet soil and very low diffusivity (Fig. 1b). There would be a very slow net downward flux below these depths, but the source could still be the surface. Similarly, the "downward diffusion of 13CO2" after litter addition is a non-steady state phenomenon, and has nothing to do with the total CO2 gradient (Abstract, section 4.4.3).

For the isotope calculations, it appears you report the effect of label additions on delta-13C of CO2 at different depths. If I am mistaken about this I apologize and please clarify this in the text, but in Eqn 9, delta-13CM refers to a "gas sample", and Fig. 6c presents "litter-derived CO2". The isotope ratio of CO2 at a given depth does not tell you much of anything about production. It completely ignores the physics of diffusion.

Instead, the authors should calculate the isotope ratio of production at different depths (apply the gradient method to each isotopologue), or of the cumulative soil profile (Keeling method). For the gradient method, you would have to calculate fluxes and production of 12CO2 and 13CO2 separately throughout the profile, and then calculate the isotope ratio of production for each zone using the ratios of 13CO2 and 12CO2 produced per unit time: ((prod-13CO2/prod-12CO2)/R-VPDB)-1)*1000 per mil

Alternatively, you can use a Keeling plot approach for the whole profile), with a diffusion offset of 4.4 per mil on the offset to calculate the production signature of the entire profile (using all depths, so does not give information within the profile).

Then, after either of these, to know percent of label you would want to compare labelled and unlabelled plots over time to have the unlabelled endmember for a 2 source mixing model (use these values in equation 9 instead of the gas sample value). But, since there are no unlabelled plots, you will have to use the average or seasonal values from pre-treatment and state that you assume it would not have changed.

I believe the surface litter removal experiment would greatly underestimate the contri-

bution of litter to CO2 production. The insertion depth was 5 cm and the diameter of the chamber was 10.4 cm. The unsaturated layer of soil is at least two meters deep, and the CO mole fraction is tens of thousands of ppm at relatively shallow depths (Fig1c). Molecules of CO2 are moving in all directions under the soil and reflecting back off the lower boundary. Therefore, the volume of soil affecting the measurement made by the chamber is much larger than the volume of soil within the collar, and you would have to remove litter from a much larger area to see the effect in a surface flux measurement. For the same reason, it would be good to know the treatment area for the isotope-labelled litter addition. If the treatment area is small relative to the depth of the soil, the signal will disperse like a drop of ink into the ocean.

Lastly, I would consider changing the title to remove "in a dystric cambisol" and maybe instead using words that are more broadly relevant to raise the reach of the paper. If the soil type is important enough to put in the title, then I think there should be more text in the paper explaining the importance of the soil type for the contribution of this paper.

There is an impressive amount of CO2 profile and isotope profile data in this paper, and clearing up these analysis questions will make this a highly citable paper.

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