

Interactive comment on “Vertical partitioning of CO₂ production in a Dystric Cambisol” by Patrick Wordell-Dietrich et al.

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1. Referee comment:

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

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have been very different.

Authors response:

Yes the reviewer is right that a schematic figure will be helpful. We will add the following two figures to the manuscript (Fig 1 and Fig 2) which clarify the experimental set up. In addition we will add 2-3 sentences in the method section where we explain the origin of the gas samples and that all measurements were done outside the subsoil observatory. The subsoil observatories contained the data logger and the power supply for the sensors as well as the endings of the stainless steel tubes of the gas samplers.

2. Referee comment:

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 (m^3m^{-2} , not m^3m^{-2}). 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?

Authors response:

We thank the reviewer for the comment and we agree that the CO₂ production expressed per area is a bit unusual. However, in the literature we found both expression per unit volume as well as area-based units see e.g. (Gaudinski et al. 2000; Hirano 2005; Fierer et al. 2005; Davidson et al. 2006; Hashimoto et al. 2007). Since SOC

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stocks are also reported on an area basis, we decided to stick with the expression of unit per area for the CO₂ production, which might be easier to understand for a broader audience. We assumed that the CO₂ production in a certain soil layer can be described as the difference between the flux at the top of the soil layer and at the top of the soil layer below (e.g. Gaudinski et al. 2000). E.g. to calculate the CO₂ production in 10-30 cm depth we calculated the CO₂ flux between the sensor in 10 cm and 30 cm depth, this would represent the flux leaving the soil layer. For the CO₂ flux entering the soil layer between 10-30 cm we used the CO₂ gradient between 30 and 50 cm depth. We are not sure if we understood the point you are making about the comparison of the different depth intervals. We don't see a problem by comparing different depth intervals, since we always name the specific depth interval.

3. Referee comment:

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?

Authors response:

The decrease in the surface fluxes derived from the gradient method of OB1 and OB3 can be explained by bioturbation (voles) in OB1 and OB3, which occurred in the second year, as tried to explain in the last sentence of section 4.2 (p.11 l30 – p.12 l1-3) and Fig 3a. In order to make things more clear, we will rephrase this paragraph and highlight more the problems of bioturbation which changed diffusivity in the first 10 cm of OB1 and OB3. The area around the CO₂ sensors were not affected by lysimeter installation.

4. Referee comment:

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In OB2 the gradient method overestimated the flux during the growing season, possibly due to incorrect parameterization of the model/diffusivity?

Authors response:

Yes the reviewer is right, the parametrization of the used diffusivity model in 10 cm depth at observatory 2 overestimated the fluxes. We reprocessed the data by using a fixed parametrization (without a distribution of the power fit function) of the diffusivity model for the specific depth and observatory. The total fluxes changed from 1080 g C m⁻² yr⁻¹ to 847 g C m⁻² yr⁻¹. We will change all figures and tables and the respective values in the text. Furthermore, in the final manuscript we will remove the distribution of the Ds model in the calculations for all observatories and depths and instead use the fixed parametrization set for each depth and observatory. This change will be made to be consistent with the data processing. The used parametrization values will be part of the supplement. However, there is still an overestimation of CO₂ flux at OB2 during the growing season. This could possibly be explained by the lower measured soil moisture during the growing season at OB2 in 10 cm depth. In addition, OB2 had the highest total porosity of all three observatories (51 % vs 46 % and 49 %). In consequence the diffusivity at OB2 is higher during the growing season. As discussed in section 4.2 the difference between chamber measurements and the gradient method must be attributed to the spatial resolution of the measurement. At each observatory soil respiration was measured at 5 spatial replicates with the chamber method. Therefore, chamber measurements accounted for the spatial variability in water content and CO₂ concentration below the chamber. However, there was no spatial replicate for the gradient method at the observatories.

5. Referee comment:

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?

Authors response:

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Thank you for pointing that out. The missing periods are also in figure 2. However these period are difficult to see, because they appear as a straight line. This is just a plotting issue of R. However, the figure 2, especially the graph of the flux gradient method will be changed. In consequence, missing periods will be better visible (similar to Fig 3).

6. Referee comment:

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: $((\text{prod-}^{13}\text{CO}_2/\text{prod-}^{12}\text{CO}_2)/R\text{-VPDB})-1$)*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.

Authors response:

We are happy for this comment, because it points out a mistake in our calculation of

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litter-derived CO₂ fluxes. As written in the manuscript we multiplied Eq. 9 with the absolute CO₂ concentration to distinguish between ¹²CO₂ and ¹³CO₂ and afterwards we calculated litter-derived C fluxes. However, as the reviewer mentioned this was wrong. Furthermore, we must first calculate the CO₂ fluxes / production in the respective layers for each sampling time. Then we must apply Eq. 9 on the CO₂ production to the amount of litter mineralisation in the certain layer. As a reference value we use the average delta value for each depth and observatory before the labelling experiment started assuming that it would not have changed. We tried the suggested calculation from the reviewer for each isotopologue, but the derived delta values based on that calculation was on average -50 ‰ with a range of -400 ‰ to 40 ‰ which seems not realistic when compared to SOC delta values of $\delta^{13}\text{C}$ 26.5 ‰. We think the Keeling plot approach for our soil profile is not suitable, since the diffusion offset of 4.4 ‰ is more theoretical and different from our field data. As shown Fig. 6a the delta values of CO₂ in all depths and observatories showed almost similar values around 24 ‰ and we could not observe a change with depth. In consequence, we used the calculation as described below to estimate the litter-derived CO₂ production. Changes in the manuscript: $\delta^{13}\text{C}$ Fig. 6c – Change title to Litter-derived C in CO₂ $\delta^{13}\text{C}$ Fig. 7 – will be replaced by absolute ¹³CO₂ fluxes $\delta^{13}\text{C}$ 2.5.3 Isotopic composition (p.7 l.25 ff.) "To determine the contribution of labelled litter-derived C to CO₂ (L) in the soil atmosphere we used the isotopic mixing equation (Eq. 9): where $\delta^{13}\text{C}_{\text{M}}$ is the isotopic signature of the soil atmosphere after the labelling, $\delta^{13}\text{C}_{\text{L}}$ is the isotopic signature of the labelled leaf litter (1241 ‰ for OB1 and 1880 ‰ OB2 and OB3) and $\delta^{13}\text{C}_{\text{B}}$ is the average isotopic signature of the soil atmosphere for each observatory and depth before the labelled leaf litter, which wouldn't change. Mineralisation of litter-derived C in each layer was calculated by multiplying the amount of litter-derived C with total CO₂ production in the specific layer. Absolute ¹³CO₂ concentration was calculated with isotopic signature of the soil atmosphere. Afterwards, ¹³CO₂ fluxes and productions were calculated using Eq. (2)-(8). To account for different effective diffusivities of ¹²CO₂ and ¹³CO₂, the effective diffusivity D_s for ¹³CO₂ was adjusted according to Cerling et al. (1991)"

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7. Referee comment:

I believe the surface litter removal experiment would greatly underestimate the contribution 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.

Authors response:

The contribution of the organic layer to total soil respiration is in the range as found in other studies. Litter-derived CO₂ accounts for 9.4 % to 37 % on total soil respiration as reported from litter manipulation experiments (Bowden et al. 1993; Nadelhoffer et al. 2004; Kim et al. 2005; Sulzman et al. 2005). However, we agree with the reviewer that the litter removal in the collar might underestimate the contribution of litter-derived CO₂. We will add a paragraph in the discussion section explaining the problem with the litter removal as already pointed out by the reviewer. Nevertheless, since our data fit in the range as reported in the literature it is still reasonable to report them in the paper even if we may underestimate the litter-derived CO₂.

8. Referee comment:

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.

Authors response:

The treatment area of the labelled litter was 6.6 m². This information will be in the schematic figure and also written in the method section as written above.

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9. Referee comment:

Lastly, I would consider changing the title to remove “in a dystic 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.

Authors response:

We agree with the reviewer to remove the soil type in the title. The title will be changed to “Vertical partitioning of CO₂ production in a forest soil”.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-143>, 2019.

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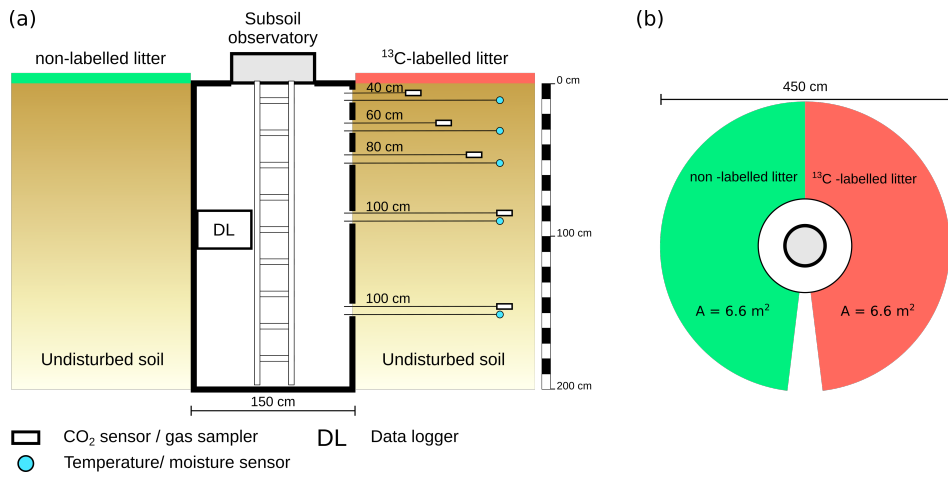


Fig. 1. Schematic presentation of the subsoil observatories and the installed sensors and the labelling experiment (a) side view of the subsoil observatory and (b) topview of the labelled and control area.

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Fig. 2. Photograph of the used lysimeter vessel to drill the whole for the subsoil observatory.

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Fig. 3. Photograph of the used polyethylene shaft inserted thereafter.

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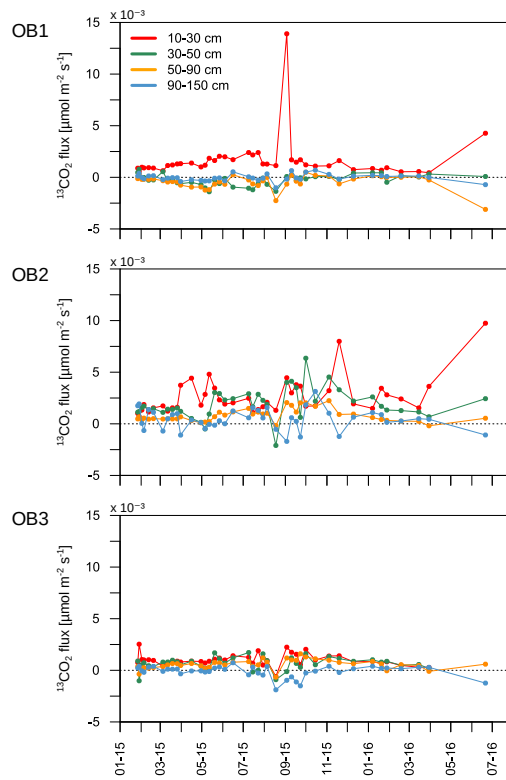


Fig. 4. Absolute $^{13}\text{CO}_2$ flux for each observatory and depth. Positive fluxes represents an upward diffusion of $^{13}\text{CO}_2$ and negative fluxes represents downwards diffusion of $^{13}\text{CO}_2$

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