

Biogeosciences Discuss., referee comment RC3
<https://doi.org/10.5194/bg-2021-126-RC3>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on bg-2021-126

Anonymous Referee #3

Referee comment on "Net soil carbon balance in afforested peatlands and separating autotrophic and heterotrophic soil CO₂ effluxes" by Renée Hermans et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-126-RC3>, 2021

Comments on MS No. bg-2021-126- Hermans et al.: 'Separating autotrophic and heterotrophic soil CO₂ effluxes in afforested peatlands'

This is a well-presented and relevant study about soil respiration partitioning and the soil carbon (C) balance of drained afforested peatlands in temperate climates. Although soil respiration studies are common nowadays, the amount of data from these ecosystems and climatic zone is still scarce. The long-term consequences of draining and afforesting peatlands with conifers is still under debate and contradictory results (i.e. soils being carbon sinks or sources) can be found between study sites and years. In addition, results from this soil C balance study could help developing stronger national Tier 2 emission factors for this land use in the UK and also increase the number of study sites and data used to develop Tier 1 emission factors from the 2013 Wetlands Supplement. However, I have two important concerns that would need to be addressed and further discussed in the manuscript. I think that, once these two potential issues have been revised, it would make a nice paper well worth publishing in Biogeosciences.

General comments

Issue #1: soil C balance

The main issue I see in this manuscript is about the method used to calculate the soil C balance. In Line 61, it says that C inputs into the soil are represented by litterfall only. There is no mention to other C inputs such as organic matter from fine root and moss litter. If mosses are not present or they represent a very small fraction of the C inputs (lines 86 and 87), they should still be mentioned and a justification of why this has been omitted should be given. However, fine root litter (using the measured fine root biomass and an appropriate turnover rate) should be considered in the soil C balance because this is an important and significant C input. Not adding this C input would result in an underestimation of the soil C balance.

To facilitate the reader how this has been calculated, I would suggest adding an equation with all the components of the soil C balance and their uncertainty. Also, C outputs are represented by heterotrophic respiration and therefore, these fluxes should represent peat and litter decomposition. However, in multiple occasions, it is written as "heterotrophic (peat only) fluxes". When considering peat fluxes, please, mention it like that, peat respiration or peat fluxes and only use the "heterotrophic respiration" terms when both, litter and peat respiration are considered together.

In the discussion, line 309, it says that mass balance calculations indicate that soils are net sink of C but it does not specify any number (and ideally together with an error). In addition, this mass balance has not been presented in the methods neither in the results section. In my opinion, this mass balance calculation is one of the most important results from this study and therefore, it should be better explained and discussed.

Furthermore, from looking at figure 8, it seems that autotrophic respiration has been included in the soil C balance and this is not correct. This soil respiration component is not part of the soil C balance as this is not related to peat oxidation. This is part of the ecosystem respiration and net ecosystem exchange and it should be used to assess the net C balance of the plantation (i.e. when the C in the tree biomass is being considered) but it is not part of the soil C balance.

Finally, the soil C balance is compared with results from Minkkinen et al (2018) which found that the drained peatland forest was a net soil C sink of $-60 \text{ gC/m}^2/\text{y}$ (lines 344 to 347). However, this value from Minkkinen et al was derived using Eddy Covariance measurements. It would be more useful to compare the soil C balance with results calculated using similar methods like that from the same Minkkinen et al paper which is derived from chamber techniques. If using chamber techniques, Minkkinen et al reported that the site was a small soil C source. Similar results are also found in Ojanen et al.

Overall and as already pointed out, this is be the main objective of this manuscript and the method should be better described and the results and their implications further discussed. These results will define whether conifer plantations on drained peatlands are net soil C sources and sinks. Therefore, everything related to how this is calculated should be presented clearly. Some useful publications about soil C balance in forestry-drained and afforested peatlands:

Ojanen, P., Minkkinen, K., Lohila, A., Badorek, T. & Penttilä, T. 2012. Chamber measured soil respiration: A useful tool for estimating the carbon balance of peatland forest soils? *Forest Ecology and Management*, 277, 132-140.

Ojanen, P., Minkkinen, K. & Penttilä, T. 2013. The current greenhouse gas impact of forestry-drained boreal peatlands. *Forest Ecology and Management*, 289, 201-208
(already cited)

Ojanen, P., Lehtonen, A., Heikkinen, J., Penttilä, T. & Minkkinen, K. 2014. Soil CO₂ balance and its uncertainty in forestry-drained peatlands in Finland. *Forest Ecology and Management*, 325, 60-73

Minkkinen, K., Ojanen, P., Penttilä, T., Aurela, M., Laurila, T., Tuovinen, J. P. & Lohila, A. 2018. Persistent carbon sink at a boreal drained bog forest. *Biogeosciences*, 15, 3603-3624 **(already cited)**

Jovani-Sancho, A.J., Cummins, T. and Byrne, K.A. (2021), Soil carbon balance of afforested peatlands in the maritime temperate climatic zone. *Glob Change Biol*, 27: 3681-3698

Issue #2: soil CO₂ fluxes

My second concern is about the low soil CO₂ fluxes reported in this study. As it can be seen in Figure 9, both, heterotrophic respiration and total soil respiration, are half or even up to three times smaller (for total soil respiration) than fluxes from boreal forest on peat soils. The reason behind why the measured fluxes in this study are that low should be further explored and discussed. While trenching produces many uncertainties on measured heterotrophic (as pointed out by the authors) total soil respiration should allow an easier comparison between fluxes from Sitka spruce plantations across different study sites and environmental conditions.

In Lines 334 to 339, the authors compare the CO₂ fluxes with results from Byrne and Farrell (2005) and Hargreaves (2003), studies with similar CO₂ fluxes for total soil respiration and peat oxidation, respectively. Although Byrne and Farrell (2005) is a very nice and useful study, the method used to measure soil CO₂ was based on soda-lime technique which is clearly not comparable with results from an infrared gas analyser like the EGM-4 used in the present study. These differences in the methods is highly relevant for potential readers and it should be clearly stated. In addition, there are other very interesting soil respiration partitioning studies (see Makiranta et al 2008) or heterotrophic respiration from drained peatland forests (Minkkinen et al 2007) that could be used to compare the peat, litter and root respiration values. While comparisons with results from Jovani-Sancho et al (2018) only focused on total soil respiration, other useful results from peat and litter respiration are provided in such study. Yamulki et al also provides useful soil respiration data for drained afforested peatlands with lodgepole pine. I would suggest

a broader comparison with other soil respiration studies on both, temperate and boreal peatlands. It is likely that such comparisons with the mentioned studies (or others selected by the authors) would show large differences in peat respiration. My question is, could all these differences be explained by the artefact of the dead root biomass and not having applied the "C flux from dead roots" correction? This is briefly mentioned in line 350-351. Could this flux correction be applied to one or two studies and see how the soil CO₂ fluxes would vary?

Finally, something to point out is that the reported total soil respiration (342.5 gC/m²/year; lines 334 and 336) are much lower than modelled total soil respiration (between 556 and 991 gC/m²/year) a Sitka spruce chronosequence on mineral soils in Ireland (see Saiz et al 2006). This could perhaps be explained by the fine root biomass, climate or nutrient content. But also, modelled heterotrophic respiration from the same Saiz et al study (between 240 and 403 gC/m²/year) seems to be much higher than heterotrophic respiration from Hermans et al (115 gC/m²/year). I would imagine that heterotrophic respiration would be greater in a drained peatlands than in wet mineral gley soils.

I would suggest a final check on the flux calculations to make sure that everything is correct. In line 121 says that collars of 10 cm collars were inserted 3 cm into the peat. Were the remaining 7 cm of the collar added to the 5 cm of the chamber when calculating the chamber's headspace? If so, the total dimensions would be a height of 12 and a diameter of 20 cm. Does this diameter refer to the internal dimensions of the chamber? Knowing the exact dimensions and volume of the chamber would be useful. And finally, could the 3 cm insertion depth have severed some of the fine root located at the top of the floor surface? Did you have surface roots (below or growing through the fresh litter) on your study sites Sitka spruce on afforested peatland have most of the fine roots located on the top cm of the soil. Please, see Heinemeyer et al 20011 and Jovani-Sancho et al 2017 for peatland-specific studies about this effect and Jian et al 2020 for a global review.

Mäkiranta, P., Minkkinen, K., Hytönen, J. & Laine, J. 2008. Factors causing temporal and spatial variation in heterotrophic and rhizospheric components of soil respiration in afforested organic soil croplands in Finland. *Soil Biology and Biochemistry*, 40, 1592-1600.

Minkkinen, K., Laine, J., Shurpali, N. J., Mäkiranta, P., Alm, J. & Penttilä, T. 2007. Heterotrophic soil respiration in forestry-drained peatlands. *Boreal Environment Research*, 12.

Saiz, G., Byrne, K. A., Butterbach-Bahl, K., Kiese, R., Blujdea, V. & Farrell, E. P. 2006. Stand age-related effects on soil respiration in a first rotation Sitka spruce chronosequence in central Ireland. *Global Change Biology*, 12, 1007-1020.

Collar insertion effects

Heinemeyer, A., Di Bene, C., Lloyd, A.R., Tortorella, D., Baxter, R., Huntley, B., Gelsomino, A. and Ineson, P. (2011), Soil respiration: implications of the plant-soil continuum and respiration chamber collar-insertion depth on measurement and modelling of soil CO₂ efflux rates in three ecosystems. *European Journal of Soil Science*, 62: 82-94.

Jovani-Sancho AJ, Cummins T, Byrne KA (2017b) Collar insertion depth effects on soil respiration in afforested peatlands. *Biol Fertil Soils* 53:677–689.

Jian, J., Gough, C., Sihi, D., Hopple, A. M., & Bond-Lamberty, B. (2020). Collar properties and measurement time confer minimal bias overall on annual soil respiration estimates in a global database. *Journal of Geophysical Research: Biogeosciences*, 125, e2020JG006066.