Referee 3

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.

Thank you for your positive and supportive feedback.

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.

> This is a good point and we're happy to elaborate on it. The site does have a sparse cover of mosses, which we assume makes only a small contribution to NPP, compared to the dense spruce/pine canopy and conifer needle input to surface litter. We did not quantify root production and turnover, as this was beyond the scope of our study, although we acknowledge that 'litter input' includes belowground as well as aboveground litter. We have used needle litter fall as a directly measurable C source for soil organic matter input, but acknowledge that this is a very conservative estimate due to the omission of root (and to a small extent moss) inputs.
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> We make this omission clear in the discussion (lines 330 – 336), where we also use literature estimates to provide outline estimates of the resulting under-estimation of C inputs, and highlight the fact that our estimated small carbon sink in these afforested peatland sols have to be considered conservative for that reason..

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.

This is a useful suggestion, and we have corrected our use of “heterotrophic fluxes” to refer to total fluxes (peat and litter) only, using “peat fluxes” or “peat decomposition” wherever appropriate.

In the discussion, line 309, in 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.

Figure 8 is a summary diagram using results presented in tables and text throughout the results section. As indicated above, we lack some terms of a complete C balance (root turnover), but even without this input, our results show a net sink of C in these soils. We now provide this estimate (with error) based on values in Figure 8. To avoid confusion, we have deleted the term “mass balance”, and instead present the “soil surface C balance”. This includes autotrophic respiration, as this is a key result of our study, aimed at partitioning soil CO_2 efflux. The following paragraph discusses values and clarifies the relevance of different terms to the soil C budget at our site.

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 gC/m2/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.

Thank you for pointing this out. We agree that the Minkkinen et al (2018) paper shows a slight C source: Litter input = 437 g C m^{-2} a^{-1} (no error presented) vs. heterotrophic CO_2 efflux of 475 ± 31 g C m^{-2} a^{-1}. We now indicate that the ‘headline figure’ of -60 g C m^{-2} a^{-1} is based on eddy covariance estimates, and that comparable chamber based estimate show a weak soil C sink.

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

Thank you for listing fill references, this has been useful.

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.

We have checked our flux calculations, and the rates are correct. We discuss this finding in context of literature reporting a range of fluxes and flux partitioning from temperate and boreal afforested peatlands. In this re-written section of the discussion, we address the referee's concern of methodological bias and include possible explanations or at least likely factors associated with an explanation for our relatively low flux sums. We note, however, that the flux ratios we report fit well with global patterns based on heterotrophic/total soil CO_2 flux, as shown in Figure 9.

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 and infrared gas analyser like the EGM-4 used in the present study. These differences is 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.

> The referee raises good points here. We now place our flux estimates incontext of literature. We decided to drop the reference to Hargreaves et al. (2003) (the only study reporting lower fluxes than ours); the authors used modelling to estimate flux contributions, but we were unsure about their site comparison to derive autotrophic and heterotrophic contributions.

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?

> We have included the issue of root decomposition and its effect on our estimates. Rather than attempting to estimate root decomposition derived fluxes for other studies, we highlight the magnitude of flux reduction that we applied to provide this information.

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.

> We appreciate the referee's comment here. After consideration, we decided to not broaden the comparison of flux partitioning to mineral soils, as this would open up a lengthy discussion of other literature from mineral soil based forestry, which is outside the scope of our study.

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 sever 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.

> We have checked our flux calculations again, including chamber dimensions, and are confident that there is no error. We are familiar with the issue of root cutting and hence potential artefacts of reduced CO_2 flux. The insertion depth was superficial (3 cm is a maximum estimate), and would have been no barrier to root re-growth under the collar in case of damage when collars were

established. Note also that collar insertion does not explain the low decomposition estimates, as there should be no impact in girdled plots (as roots are cut anyway).

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., Lame, 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.

These references have been very useful, and we appreciate the supportive approach of this referee to improve the paper.