Interactive comment on “Greenhouse gas and energy fluxes in a boreal peatland forest after clearcutting” by Mika Korkiakoski et al.

Response to Reviewer #2

The study tested the impact of a disturbance (clearcutting) on surface greenhouse gas (CO2, CH4, and N20) fluxes in a forested boreal peatland in southern Finland. Over a 2 year period they used both eddy covariance and chamber based methods to measure GHG fluxes, as well as a number of environmental variables known to drive gas fluxes (water table, soil temperatures). The study shows a increase in CH4 and N20 emissions following disturbance, however the authors deem the CH4 to not be of importance regarding GWP. The paper is well written and contains valuable information regarding the impacts of forest management practices on the carbon balance of boreal peatlands.

My main concern is the conclusion drawn from the data presented here that clearcutting results in the forest turning in to a large CO2 source. The study includes 2 years of CO2 flux data collected after the clearcutting and presents the trends of these 2 years well. However, there is no reference data of the CO2 balance of an undisturbed site and as such the study does not show that there has been an increase in a CO2 source effect. The study mentions unpublished data that provides this information. This data needs to be either published and referenced or including in this study for the conclusions being drawn here to be substantiated and allowed. The study site is also compared to a control, undisturbed, site where there has been water table and soil temperature collected. There is no CO2 flux data provided from this site, if data is provided from this site as reference point of the annual NEE budget of an undisturbed site in the area. A more detailed data set from this control site (including flux data as well as environmental variables, seasonal weather data) would allow for it to be used as a reference site and then an impact to the source/sink function of the site be commented upon. A map the shows the location of both sites would also be useful. I would suggest either including the unpublished data for a more complete study or a more detailed description of the control site as described above if the authors wish to maintain their conclusion that there is an increased source effect. As is presented here this conclusion cannot be claimed.

1. Thank you for the comment, and we understand the problem about referencing unpublished data. Referee #1 made the same comment (please see our reply #1 to referee #1). In short, we have no reference data for CO_2 fluxes from the control site (which is too small for eddy covariance) and also cannot use the CO_2 flux data from the original EC tower as a control dataset; therefore, we have decided to only discuss the post-harvest balances and remove all the references to the unpublished data from the revised manuscript.

   We have made chamber measurements similar to the clear-cut area within the control area and will add them to the revised manuscript (please see our reply #2 to referee #1).

   The map included in the manuscript (Fig. 1) already shows the location of the control area, but we will add borders around it to make it more distinctive.

My secondary concern is regards to the modelling of Reco. There is no influence of water table included in this model (Equation A3) and I would like to know what is the justification of this? The study has a good data set on water table depth throughout the years and a number of times in the paper it is mentioned that this rising water table may be influential on CO2 fluxes and suppress them. If it is being used as reasoning for low fluxes, then it should be including in the modelling attempts. Also, what was the justification to use air temperatures and not soil temperatures in the part equation A3 that is acting similar to a Q10 value?

2. The referee is right that water table level affects the decomposition of organic matter and thus should affect CO_2 fluxes from the soil. However, it should be noted that Reco, as defined here, combines two processes: forest floor respiration (mainly soil) and the decomposition of logging residues. Water table depth affects the former but not the latter, which is mostly controlled by temperature. According to our chamber measurements (with no above-ground residues), the CO_2 emissions from the forest floor were 32% (2016) and 57% (2017) of Reco (P. 12, L. 11-17), meaning that the logging residues contribute markedly to Reco. One should also take into account that the chamber measurements also include below-ground residues (e.g. roots), so in reality the contribution of the residues to Reco is even larger.

   We plotted residuals of the respiration fit (Eq. A3) against WTL for both summers (Fig. 1) and found no significant relationship between respiration and WTL. This is discussed in the original manuscript (P. 13, L. 9-14). Thus we did not include WTL as an explanatory variable in the Reco model. We will add a comment on this and the residual-vs-WTL plot to the revised version.

3. Regarding the temperature issue, please see our response to Referee #1 (reply #15). In short, we actually did use soil temperature in Eq. A3, but forgot to update this into the text and figure captions. We apologize for confusion and will fix this in the revised manuscript.

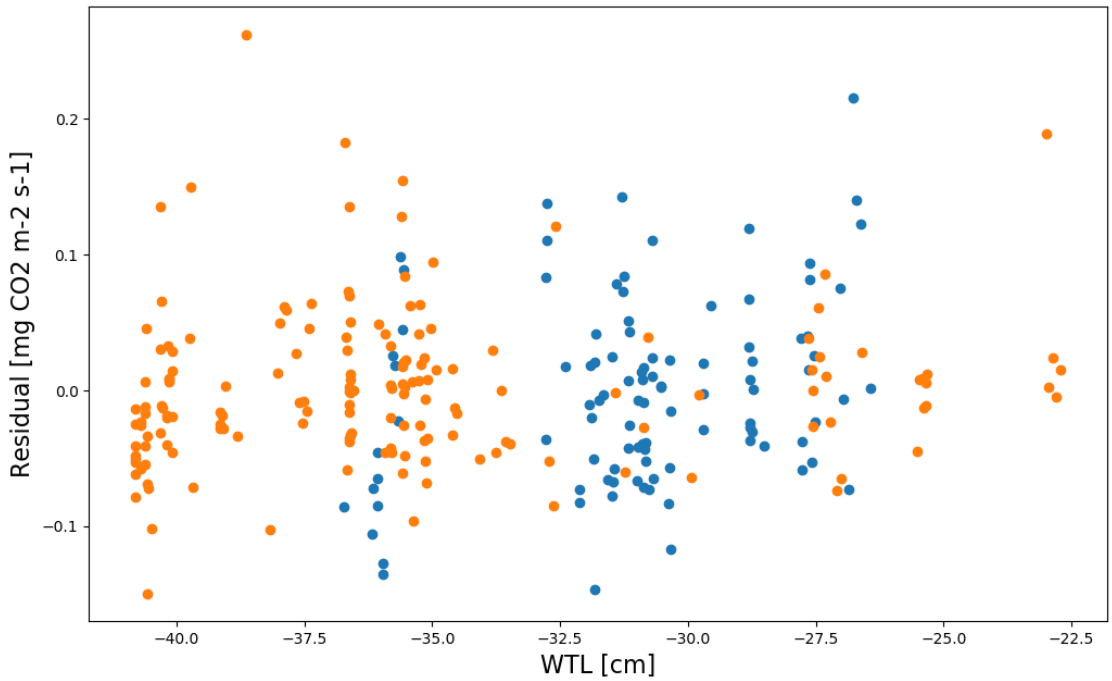

Figure 1. Hourly mean residual of the respiration fit vs. hourly mean water table level in Jun-Aug 2016 (blue) and 2017 (orange).

Reco is represented well in the paper and seems to be the driving factor in annual NEE budgets that show a loss of CO2 to the atmosphere. However it's is unclear what proportion of Reco is made up of Rff and CO2 lost due to enhanced mineralization of the clearcutting leftovers/debris. Some kind of graphical representation of the contribution of these two CO2 sources to Reco would be necessary to comment on the long-term impact of peat carbon following clearcutting and not just the recently felled organic matter at the surface.

4. As indicated above (reply #2), the CO_2 emissions from the forest floor were 32-57% of Reco, while the logging residues probably account for the rest. The aim of our study was to investigate the short-term impacts of clearcutting, and discussion of any long-term impacts would be mostly speculative. Likely, the CO_2 emissions from the logging residues will decrease in the next few years as they decompose. Also, the planted trees and the ground vegetation will grow; therefore, the transpiration will increase causing the WTL to decrease. This will again likely increase CO_2 emission from the soil to the same level as before the clearcutting at some point. Anyhow, our focus is on the CO_2 emissions right after the clear-cut, so we would prefer not to add any further discussion on longer-term effects.
   The idea of graphical presentation is great, and we will add a bar plot to the revised manuscript, which shows the distribution of estimated Reco to $R_{residues}$ and Rff for both summers.

Pg. 2 L. 13 nitrogen should be nitrous?

5. Yes you're right. This will be corrected in the revised manuscript.

Pg. 6. L. 15 – 16 what was the justification of using a closure time of 10 – 11 mins? For N20 closure time can be very site dependent, was this tested at this site to ensure the time was enough to capture representative flux measurements?

6. Yes, we tested the chamber system at our site before the clearcutting in 2015 with a longer closure time (20 min) and noticed that by selecting only the first 10 min of the data gave very similar results to using the whole 20 min dataset. Because of this, we decided to limit the closure time to 10-11 min to allow more spatial replicates to be measured during the same day. After clearcutting, the N_2O fluxes increased so much that we could have actually reduced the closure time down to 5 min. However, due to the fact that the absolute CH_4 flux decreased (from sink to small source) after clearcutting, we still had to use a closure time of 10-11 min to measure the CH_4 fluxes with sufficient accuracy. Also, keeping the closure time the same allowed a more consistent comparison between the years.

Reporting of GPP data is not consistent in the text. Pg. 9 L. 13 – 14 GPP rates are reported using positive values. Compare this to Pg. 10 L. 10 where GPP balances are reported using negative values. I would suggest using negative values for both as this paper is addressing NEE not NEP.

7. Thank you for pointing this out. We decided to use the absolute value of GPP (|GPP|) in the text to avoid the confusion that occurs commonly when talking about increasing/decreasing GPP. However, we accidentally left some negative values in one section, as pointed out by the referee. The values in that section will be changed from negative to positive in the revised manuscript.