Author's response

Reviewer (R#1) comments and author responses and changes to ms bg-2020-230

We highly appreciate the very helpful and constructive comments of the anonymous referee, which helped us to further improve the manuscript. We tried to consider all of them.

Reviewer comments are given in italic and with author responses in normal style

The authors improved their manuscript. They now describe their reason for this study adequately. However, I have still several concerns that should be addressed before reconsideration for publication in Biogeosciences. Especially the discussion part needs to be revised as some discussion points are not well-thought-out and references are imprecise or wrongly cited.

N2O data
Hypothesis 3 cannot be answered with this data set. With a low frequency of sampling, it cannot be excluded that peak emission were missed. In order to answer this question I would suggest to have at least weekly N2O measurement (with higher resolution in situation with high N2O emission risk or continuous data from Eddy-Covariance systems. As the authors stated in their response, it was intended to measure N2O at a higher frequency. Unfortunately, it failed. I would suggest deleting hypothesis 3 instead of trying it to answer it with in inadequate data set. I suggest considering to change ghg to carbon emissions in the title. In the abstract the frequency for N2O measurements are missing, but the authors stated, that the N2O emission were lower in 2017 than in 2018. In addition, N2O data are included in the ghg budget (241, 455). This implies that there is a sufficient data base for calculation of N2O budget. In addition, N2O emissions could also be underestimated by missing peak emission in summer (466). Therefore, it is not easy to estimate the contribution of N2O to ghg budget. Please add number of N2O (and CH4) measurements in Table 4.
According to the method section site D 17 measurement campaigns took place. However, I count 13 data points for SII.
L462:The resolution of N2O data is too low to detect peak emissions. For N2O peaks not only fertilization events but also soil humidity is essential. The soil humidity is changed by treatments and could differ. The soil moisture in one treatment could be high enough for N2O emissions, but not in the control. The N2O emissions could also occur one different days due to different moisture regimes in soil. Therefore, it is not possible to state that no peak emission were missed.

> **Response:** We recognize that the data for N2O is insufficient to make a good estimate for a N2O balance. We changed our title to Carbon emissions, and removed hypothesis 3 from the manuscript. Now the focused for N2O is on the direct comparison of measured fluxes between SSI and control, which is sufficient in showing the absent of treatment effects. It is also addressed in discussion that we have too few measurements to engage gap-filling and statistical analysis of year budgets for N2O.

Experimental set up.
My second concern is the frequency of grass mowing within the chambers in 2017. The grass was cut 8 times, which is considerably higher than in 2018 and for the whole pasture (4-5 times) and as generally applied for intensively used pastures. For me it is not clear why the grass was cut in 2017 at this high frequency. The frequency of mowing affects the grass development and thus yield. Therefore, comparison of yield may be biased by different management and not only by climatic conditions, as the authors interpret it.

> **Response:** We made an additional cut in the start of May 2017, because of the fast grass growth and grass height exceeding 30 cm. which resulted in an out of sync mowing regime compared to the surrounding farmland, we adjusted for this. The month of September and

October were extremely wet, this resulted that most farmers decided not to harvest the grass. We however included this harvest. At the end of the year, there was a relatively high amount of biomass remained in the field, so we added an extra harvest to close the Carbon balance of the year. This was not necessary during the drought of 2018.

I still wonder why a considerably CO2 uptake can be observed after grass cuts, as the CO2 uptake is generally considerably reduced after mowing not only for organic soils, but also for mineral soils (Beetz et al. 2013, Poyada et al. 2016, Eickenscheidt et al. 2015, Schmitt et al., 2010). This question was also addressed in the first review. One reason may be that the modelling/gap-filing is of too low quality to capture the management events of the grassland or the grass was only cut slightly, so considerably amount of CO2 can still be taken up after harvest. However, other grassland with 4-5 cuts per year show the reduction of CO2 uptake after mowing.

Response: This is now addressed in the Method section with **"**Models developed for the campaign before harvesting were then corrected using the slopes of the linear regressions as the models after the harvest to be applied in the extrapolation. The loss of biomass was therefore accounted according to lowered grass height, different from the studies where model parameters are to zero after harvest (e.g. Beetz et al. 2013). "

We chose not to set the parameters (GPPmax, alpha, etc.) to near zero after harvest as did in the cited studies, because at the start of the experiment we had some moments that we measured after harvesting. There was indeed a drop in CO_2 uptake, however not to 0. And the biomass was not completely removed but with a small amount of residual with grass height of 5 – 7 cm. The grass height has good agreement with the GPP estimations; therefore, we used this relationship to correct for the harvest rather than a manual reset.

Discussion

Especially, the whole discussion session needs to be carefully revised. The argumentation and wording are not always straightforward or even false. It is not clear, what the land use, peatland type, management and origin of the cited sites are. In addition, it is often not distinguished between field experiment or laboratory studies.

Response: We worked through the discussion to rewrite the argumentation and the wording and to make sure that the cited studies are properly placed and described. As we recognize that there was a lack of consistency between our arguments with the supporting references in the previous version of the manuscript.

Here are some examples:
Line 127ff:
I guess that the site had been drained for long-term (highly decomposed material line 444). Thus, the top 30-40 cm have under oxidized condition for long-term and easily decomposable organic material may have been already decomposed. Accordingly, relative stable organic matter may have been accumulated at the top 30-40 cm which is in contrast to the argumentation in line 127ff.

Response: As a pilot tryout for this study, we incubated soils from the field from different depths under ideal oxidation conditions, we did not observe lowered decomposition activity from topsoil layers or increased emissions from deeper/more pristine layers. Furthermore, our fluxes measurements did not show a low emission in situation where only the topsoil was exposed and drained. Therefore, our arguments did not go to the direction of the reviewer's assumption.

In addition, it is not clear if the water content of deeper layers is saturated and how the O2 saturation is, as these parameters are site-specific. The annual averaged soil temperature at deeper layers may be the same as at top layers.

> **Response:** We only measured the moisture content for the deeper layers with physical samples. Saturation of the deeper layers and the O2 saturation would be the factors that are of interest to follow and see how they are influenced by SSI. However, we did not include this in our setup.

Line117ff:
After Tiemeyer et al. 2020 there is a general dependency of groundwater table and CO2-emissions rates for all land use classes, most strongly at water levels between -20 and -50. The dependency of deeper groundwater level on CO2 emissions is less clear. The findings of the authors are in agreement with recent literature.

> **Response**: We removed the statement that our findings are contrary to the general assumption

L 405-409: here are results presented not discussion

> **Response:** This is removed from the discussion.

L422-423:
The soil moisture data shows differences between 33 and 90% (Appendix). Not sure, if I would classify these differences as small variations.

> **Response**: "This lack of effect is explained by the fact that there is only a small difference in soil moisture values above the GWT" Is an explanation of what was found in literature, by Lafleur et al., (2005);Nieveen et al., (2005);Parmentier et al., (2009).

L423-425:
I can only guess the meaning of this sentence. Please reformulate.

> **Response:** We reformulated the sentence, what we were trying to say is that ,the lower CO_2 emissions reported with structurally elevated GWT often have a vegetation and land use that are more adapted for the higher water table.

L 427: Please reformulate the sentence. What is effect size

> **Response**: We reformulated the sentence, "treatment effect on measured R_{eco}"

L 473:
In Tiemeyer et al. 2016 it can be clearly seen that there are two sites with NEE > 60 t CO2 (Fig 1), so there have been higher values measured before. The cited values (Tiemeyer at al 2020) are the new German EF based on published data. The German EF are self-evident lower than the highest measured values.

> **Response:** Agreed, we adjusted this in the text.

Soil moisture

The authors now include soil moisture from one sampling day in August 2017. Unfortunately, the groundwater was quite similar between treatments in 2017. I asked for the soil moisture data from sensors. If these data are not included in the manuscript, than they can be deleted from the material and method section.

> **Response**: The soil moisture sensors where often the cause of the malfunction of the sensor station. In the summer the sensor would give faulty data, because of the depth of the sensor and the soil properties of the clay layer. This resulted in incomplete time series, that is why we decided not to include this in manuscript. It is now deleted form the material and method section.

T soil is used for the modeling. How is the data coverage of the 5 cm soil data, as it can be expected that malfunction of sensors was both for soil moisture and soil temperature? When was the Hobo sensor installed? Which data was used for modelling 5 cm or 10 cm Hobo data? How much uncertainty add the missing soil data to the NEE? Which data was used, when there was a data gap?

> **Response:** The sensors were installed at the start of the experiment, and the problems occurred for both sensors quite early into the experiment. That is why we installed multiple sensors that were more robust to the field sites. The main data used for the modeling of NEE is from the 5 cm sensor of the main station, but if there was a malfunction data from the additional sensors that were installed was used.

Soil properties are now included in the Appendix. I would suggest to incorporate the information about C content (g/kg) of different layers in Table 1. The unit g/l is not often used. It is rather carbon stock than carbon content?

> **Response:** Table 1 and appendix are now combined to provide additional information. In the table we display carbon content and carbon stock. This will help to understand the conditions in the field and the properties of the high organic clay cover that is present in our field sites.

Please provide also errors of NECB and c-export and display them in Table 3 (Also for Manure)

> **Response:** The errors are now included in the table for the NECB, c-export and manure.

Schmitt, M., M. Bahn, G. Wohlfahrt, U. Tappeiner & A. Cernusca. 2010: Land use affects the net ecosystem CO2 exchange and its components in mountain grasslands, Biogeosciences, 7, 2297-2309