Reviewer (R#2) comments and author responses to ms bg-2020-230

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

Sub-soil irrigation does not lower greenhouse gas emission from drained peat meadows

by Stefan Weideveld et al.

Generally, the manuscript will be of interest for readers of Biogeosciences, and the topic of adequate mitigation strategies for drained organic soils is one of high relevance. While the overall result that there is no difference in GHG emissions of this sub-surface irrigation (SSI)system and the control seems to be robust, there are, in my opinion, still four major issues which need to be solved before the manuscript could be considered for publication in BG:

> **Response (1)** We thank the reviewer for the positive comments and constructive inputs. This will help us improve the manuscript.

• *The authors appear to be surprised that SSI does not result in lowered GHG emissions, but this "surprise" is rather unfounded as the water table is raised only slightly towards a target level of 60 cm below ground, which I would –in line with the IPCC Wetlands Supplement (IPCC, 2014) –still regard as "deeply drained".*

> **Response (2)** We recognize that part of the questions raised are a result of an inadequate framing of the experiment. This is an important factor for the paper, to improve this the current state of the SSI technique will be given. In the Netherlands, the aim of the government is to reduce CO_2 emission from peat meadow areas by 1 M t by 2030, from which halve is expected to be achieved with the SSI technique (PBL, 2018). To come to this reduction, an area of 50.000 ha with SSI drainage pipes are planned and a CO_2 reduction of 50% is expected from this area. However, the current design of the SSI technique aims to increase the lowest water table while maintaining the agricultural function as "business as usual". Also, this technique has never been validated by measured CO_2 emission data. Expectations are based on pilots with only soil subsidence measurements. In these pilots a relation between lowest GWT and soil subsidence is found, therefore the elevation of summer GWT is expected to contribute most to the reduction of CO_2 emission. The current set-up tested in our experiment aims to explore the effectiveness of SSI on GHG emission for the first time by measurements, also on a large scale on sites representative for the Frisian peat meadows. Therefore, our hypothesis is based on the state of the SSI technique according to policy in practice. And our set-up was made based on the current policy status rather than the scientific exploration of the optimal use of rewetting to mitigate the emissions.

• *According to Figure C1, there were partially only 7 measurement dates for N2O in 2017 and afterwards a gap of five months. Given the highly episodic nature of N2O fluxes, this is absolutely inadequate for the calculation of annual balances in a strongly fertilized grassland.*

> **Response (3):** In 2017 we experienced infrastructural constraints to measure N_2O fluxes more frequently. The extended winter gap is a consequence of mal-functioning of the Picarro 2508 under field conditions with low temperature. We agree that 7 flux days and 90 measurements are too few for year budget estimation. The methods and results will be adjusted so that it becomes clear that the year budget of 2017 is a rough estimation based on average fluxes from 7 flux days. We will discuss the importance of generally higher winter emissions that were not measured for year budget estimates, which made our estimation a conservative underestimation. However, we believe that the measured data is still valuable for evaluating

the N_2O emissions under influence of SSI. The results show no structural higher or lower N_2O emissions between the control and SSI sites. The measured data fits our expectations and references of these types of systems. Therefore, we would like to keep the annual budget estimation for the general discussion of the total GWP. But clarification will be added to methodology and discussion the stress the low temporal resolution of our measurements, and daily measured data will be presented. The moments between frost and thaw was measured for Farm B and C in the beginning March 2018. However due to technical difficulties with low temperatures and the gas measure equipment these moments were still sparse.

- *For the interpolation of GPP, all measurement campaigns have been pooled for 2017 and harvests have not been accounted for when interpolating GPP despite the large influence of above ground biomass on maximum photosynthetic rates.*

Response (4): We appreciate the reviewer's suggestions for improved gap-filling strategies. The data for GPP gap-filling is available for a recalculation of the GPP balance for 2017 for a better estimate of the GPP. Pooling of measurement campaigns will be improved based on conditions during the measurements. The amount of biomass and the harvest are key in understanding the GPP flux. We will include the harvest moments in the interpolations of GPP, and consider possible need of correction for the Interpolated Reco regarding effect of the amount of biomass on dark respiration.

Title and assumption that this specific SSI system would lower GHG emissions

The SSI system studied here has a target water level of -60 cm. Given the limited hydraulic conductivity of the peat and the "exit resistance" of the pipes, a water level of -60 cm in the ditches results in even deeper field water levels in summer. This target seems to be based on the assumption that CO2emissions originate from deeper peat (see below). Thus, the authors state that a WT rise of 6-18 cm in summer compared to an even lower level "unexpectedly" (line 22) or "contrary to our expectations" (line 29) does not lower GHG emissions. In my opinion, this is absolutely no surprise, but should be expected as laboratory studies often show highest respiration rates at medium water content and as field studies, on average, showed an asymptotic rather than a linear response of CO2 emissions to water table depth (too dry, no more peat exposed, Tiemeyer et al., 2020).

Response (5): See response(2) for a full response. This is not our own expectation, but the expectations are based on previous pilot studies and now common accepted in policy.

Thus, the title needs to be changed to "Sub-soil irrigation with target water levels of 60 cm does not lower carbon dioxide emissions from drained peat meadows" or something similar, as the experiments do not allow for conclusions on SSI in general. Further, if the authors are really surprised by their results, they will need to convince the reader why. In this context, it also needs to be discussed why such low target water levels have been chosen at all. At least for meadow use as in 2018, such low water levels are technically not needed when adequate machinery (low weight, double tyres, etc.) is used.

Response (6): A change will be made in the title and conclusion, to clarify that the current design of SSI is the commonly applied compromise between additional drainage and increased infiltration during summer and that this technique may fall short to have a significant effect on the GHG balance. Furthermore, information will be added in regard to the average ditch water level to indicate that the goal of a water table of -60 was further promoted by raising the ditchwater in de summer periods, on average the ditch water level connected to the SSI was closer to -40 cm rather than -60 cm.

Peat layers below -70 cm contribute most to GHG emissions

In the introduction, there is no reasoning why this should be the case at all. Many studies have shown that top soils show higher respiration rates than subsoils e.g. due to higher nutrient contents or generally more favourable conditions for microbial activity(e.g. Bader et al., 2018). This is indeed briefly discussed on page 22, but the whole "story"of the manuscript (and probably also the design of the sub-surface irrigation system) builds on this assumption. Thus, either it needs to be substantiated by peer-reviewed (!) literature, or the manuscript needs to be restructured based on more adequate hypotheses.

> **Response (7):** We agree with the reviewer that the manuscript needs a clear distinction between current knowledge in peatland sciences and (current) assumptions of land authorities and Dutch governmental institutions responsible for emissions reporting from peatlands. In our own scientific reporting (van den Berg et al. 2018) we show that the top 20 cm of peat revealed the highest CO_2 production potential. In contrast, the current estimation methods in the Netherlands make no use of CO_2 flux data but rely on soil volume – soil carbon models. It is assumed that soil subsidence is quasi 1:1 related to carbon losses in form of CO_2 without taking volume changes of the peat and changes in the carbon density into account. Based on that 1:1 soil subsidence-soil carbon relationship it has been inferred that soil subsidence is stronger when groundwater resides during summer

Frequency of N2O flux measurements

According to FigureC1, there seem to be only 7 measurement dates for N2O in some cases in 2017, then a gap of more than 5 months in winter and finally a further gap of two months at the end of the study period. This contradicts the text that N2O was measured at each campaign, i.e. supposedly bi-weekly in summer and monthly in winter(page 8). If Figure C1 is actually correct, this data may not be used for the calculation of annual balances as effects of fertilisation cannot be captured adequately with such a low temporal resolution. Further, I would suspect that the first fertilisation event took place before April and was thus missed by the campaigns. In any case, fertilisation dates should be indicated in Figure C1.

Even more important, it is well-known that high N2O emissions may occur when temperatures change between frost and thaw(e.g. Koponen and Martikainen, 2004), especially under wetter conditions, and that maximum N2O fluxes of drained peatlands may occur in winter also under temperate climatic conditions (e.g. Flessa et al., 1988). Therefore, the authors should refrain from calculating annual balances from a dataset without winter data. The N2O data could, however, be used to compare treatment effects on the basis of campaigns. In consequence, this means that GHG balances cannot be calculated from the presented data, but only C balances.

> **Response (8):** See response (3) for the elaboration about the N_2O choices that were made in the manuscript and the changes that we will make.

GPP modelling

In my opinion, pooling all summer data as done for 2017 is not an adequate gap-filling strategy as GPP max and α strongly depend on vegetation development. This strategy of pooling might be valid for (semi-)natural vegetation, but no for intensively used grasslands with frequent harvests.

Further, it seems that parameters are generally interpolated across harvests which does not capture the effects on GPP, which should be very low after harvests. Harvests are unfortunately not indicated in Figure 7 and Appendix B. I would strongly suggest using an interpolation approach suited for highly managed systems (e.g. Eickenscheidt et al., 2015). If this should not be possible due to inadequate PAR ranges during measuring campaigns, only campaign data (instead of annual balances) may be evaluated.

> **Response (9):** We will re-calculate the GPP of 2017 campaign-wise. We will also include the cutting events as reducing the GPP. This will result in a different GPP value and different figures 7 and appendix. We agree that the effect of the biomass is important for the total value of the GPP and Reco. In order to see the effect of cutting a correction for both interpolations is needed. Seeing the effect of the higher biomass on the dark respiration of the plants. The harvest dates will be included in the figures to visualize these moments. And to give a better estimate for the total emission.

Further comments

• Line 59: Better cite the most recent Dutch inventory data instead of an "old" (2009) paper.

> **Response(10):** The most recent Dutch inventory will be used to have an indication of the national emissions from drained peatlands.

Table 1:

• Details (e.g. SOC, clay content) on the "mineral top layer" would be helpful.• Soil properties averaged for 0 to 70 cm are not really informative, better provide data on the top soil and on depths where the water level/moisture changes actually occurred.

> **Response(11):** We agree that the current table is inadequate. In the revised version we will replace the averaged soil properties for a higher resolution per soil layer. More details will be provided on the mineral cover layer, the schalter layer, the degraded peat layer and the less degraded peat layer. The mineral content was determined, however the fractions of the mineral top layer where not determined.

• How comparable are SSI and control when they partially strongly differ in SOM content (location D) or C:N ratio (location A)?

> **Response(12):** The differences in organic matter is largely due to the thickness of the mineral top layer. However, for the soil organic carbon stock is of a similar size for both sites. The soil organic matter will be indicated as g/l soil.

• Data on hydraulic conductivity or at least on the degree on decomposition are needed to discuss the contrasting hydrologic effects of SSI at the four locations.

> **Response**(13): The hydraulic conductivity was not measured during the experiment. However, the dip wells that we used to measure the water table for the different frames could give an indication for the processes in the field. Only one location had a good horizontal water flow in the peat layer, that was location B. Location A saw a strong effect of the SSI in the water table. However, in some places there was a large difference in the water table at different distances from the pipes.

• Does the "schalter" layer have any effect on the sites' hydrology?

Response(14): Schalter is known to limit vertical water flow, due to its laminated structure. However, there is little documented about the properties and processes. In our case, the locations with "schalter" seem to have lower effects from the SSI

• Line 140: How was the C-export actually determined? For the frames (line 166 ff) or for the whole field as it is implied here? Were the frames fenced off from grazing?

Response(15): The experiment sites were fenced off from grazing. The C-Export was determined inside the frames. However, on harvest days the whole experimental site was cut.

• If the C-export had to be excluded from statistical analysis, how could the GHG balance containing the C-export be analysed?

Response(16): The C-Export has been set for a similar amount for both fields. The NEE, CH_4 and N_2O budgets will be focused in the statistical analysis instead of the total GHG balance.

Line 145: Flux measurements and modelling

While I understand that not all details can be provided to limit the length of the manuscript, lots of information is missing which would allow assessing the quality of the data.

• Were the chambers cooled and vented for pressure equilibration?

Response(17): The chambers where not cooled. The pressure inside was equilibrated when placing the chamber on the frames.

• Was PAR outside the chamber corrected for the light transmittance of the chamber before interpolating GPP? Which light transmittance was assumed/measured?

Response(18): We corrected the PAR values outside the chamber since the acrylic glass of the transparent chambers reflected or absorbed at least 8% of the incoming radiation

• Was there any quality control procedure for flux calculation (linearity, outliers, leakage...)?

Response(19): Each flux was checked, for the dark measurements a only fluxes with a R-squared of 0.99 or higher where used. For the light measurements the majority of the fluxes that were used had a R-squared of 0.95. The exception where the fluxes with slopes close to zero or zero (equilibrium between gross primary production – GPP – and R_{ECO}) were not discarded.

• Was there any minimum temperature difference within one Reco campaign to avoid artefacts due to extrapolation of Eq. 2?

Response(20): There was no minimum temperature difference set within the Reco campaign. The measurement campaigns where planned to have a range in light variation for the GPP calculations, this resulted in a good temperature range during the day.

• Which Reco(nearest?) was subtracted from NEE to yield GPP (line 192)?

Response(21): The Reco closest in time was used for subtraction of NEE to yield GPP. During the campaigns light and dark measurements were always conducted in the same time frame.

• *The unit of α is wrong (line 200), it should be mg CO2-C m-2h-1/ µmol m-2s-1*

Response(22): Is adjusted in the material and methods.

• *Why did you choose to interpolate parameters and not weighted fluxes(line 204)?*

Response(23): We interpolated the parameters by assuming a linear development of the parameters between two measurement campaigns. E.g. a linear development of the temperature sensitivity of R_{eco}.

• *Why did you use GPPmax and not GPPopt(Falge et al., 2001)which is less susceptible to extrapolation errors?*

Response (24): According to Falge et al. (2001), GPPmax (or saturation value of GPP) has less explanatory worth for real systems since PAR will not reach infinite, therefore the author switched to GPPopt which provides a reference value at a certain PAR level. GPPmax was used in numerous fluxes modeling works, and we did not find argument from literature stating significantly larger uncertainty from the use of GPPmax. Thereby, if a GPPopt is used, there should have enough data in a specific PAR value (e.g. 2000 µmol m^{-2} s^{-1}). With eddy covariance (where this gap filling paper of Falge et al. is written for) this is not a problem, but with chamber measurement data is limited. The accuracy will therefore be better to fit the light response curve with the GPPmax.

Line 254 ff: "Drainage" and "irrigation" periods

• *From my understanding, "drainage" and "irrigation" periods are not defined correctly. While it remains unclear which WT (0.5, 1.5 or 3.0 m from the pipe) was used for this calculation, it is of course useful to differentiate whether SSI was dryer or wetter than the control when comparing Reco. However, "drainage" and "irrigation" periods can only be identified by using absolute heads, i.e. by comparing the field WT to the ditch water level!*

Response(25): We will clarify the definition of these periods. 'Drainage' periods refer to moments when there is drainage to the ditch and 'irrigation' periods refer to moments with water infiltration from the ditch to the field. Here the aim is to differentiate between SSI and control.

• *In this context, it also remains unclear why the SSI system works better at some of the fields in terms of hydrology –is there always enough water in the ditches, how is the hydraulic conductivity or at least the degree of decomposition of the peat, or are there strong differences in WT at 0.5, 1.5 and 3.0 m difference to the pipes which could be used to deduce information on the hydraulic conductivity?*

Response(26): The water table in the ditch was maintained at a level between -60 to -20 cm from the soil surface. It was never a limiting factor for the functioning of SSI. The hydraulic conductivity of the peat soil was not measured during the experiment. However the functioning of the SSI gives an indication of the conductivity. This is closely related to the type of peat present. Farm A, C and D all have Sphagnum peat, with the layer where the pipe is present being moderately decomposed (H5-H7). We suspect there are some macro cracks in the peat soil of farm A, that help infiltration. For location B the peat soil consists of Alder peat. The layer where the pipe is present is moderately decomposed but with a large presence of wood/branches. For this location the SSI seems to work best. With a strong drainage and infiltration effect.

• Furthermore, it is rather difficult to compare results to other studies. Therefore, please give numbers for the mean and the summer mean water level.

Response(27): The mean annual average GWT table will be given to increase the comparability between the different sites and to other studies.

Table 2 and Table 3

•Should be merged and N-fertilisation should be added. •Uncertainties should be added.

Response(28): The tables will be merged and N-fertilisation is added to table 2/3. Modeling and gap-filling uncertainties will be added to R_{eco}, GPP and NEE.

*• **Line 425 ff:** There are some comparisons to other studies, but the authors do not try to explain the differences in emissions between their sites.*

Response(29): The comparison between the sites is expanded upon. The differences between the sites are largely because of the soil conditions. The locations with a mineral topsoil seem to respond stronger to drought. Furthermore, there was a difference between the starting conditions of the sites. The sites A and B where grazed before the experiment and site C and D where only mown. This resulted in a difference in the grass structure, where the grazed grass forms a more dense vegetation structure than the mown grass.

*• **Line 439 :** How do you know that moisture conditions were optimal in 2017?*

Response(30): The indication of 'optimal' come from observation of the conditions in the field, for example the grass growth that we observed during the field experiment. This was also determined in contact with the farmers who judged a better year for grass growth. We will reconsider the wording 'optimal', but the point was that we expect that the moisture levels were not a limiting factor during this summer period.

*• **Line 451:** What do you mean with "abnormal data points"?*

Response(31): The "abnormal data points" refer to measurement that did not fit into the temperature dependent function of R_{eco} or light response curve of GPP, due to the extreme drought that limited soil respiration's response to higher temperature, or reduced the photosynthetic rate.

*• **Line 486** : Effects of land-use intensity and land-use history should be discussed in the context of general emission level (section 4.3) as these aspects do not fit to the section "costs and benefits SSI".*

Response (32): We chose to discuss about land use in the 'cost and benefit' section because of the possibility of SSI to be beneficial for the intensive land use. Due to the increased load boarding capacity of the fields and the drainage in Spring and Autumn, it is possible to extend the periods that the field can be managed. We consider this as a possible benefit from the SSI, however we didn't observe this during the experiment.

• Besides methodological issues, the manuscript seems to be hastily prepared which results in many inaccuracies especially regarding the references (list might not be exhaustive): • Several references mentioned in the text are not in the list of references (Hoffmann et al., 2015, Tiemeyer et al., 2020)• One reference appears twice (Berglund and Berglund, 2011)•References are incomplete (Couwenberg, 2009, Tanneberger et al., 2017)

Response(33): The references will be updated and check more thoroughly.

• *Generally, there is some tendency to cite non-peer reviewed literature (Joosten and Clarke, 2002, Joosten, 2009, Jurasinski et al., 2016, Hendriks et al., 2007b, Hoving et al., 2015, van den Akker et al., 2008, van den Born et al., 2016). In many cases, peer-reviewed papers could easily be found and should be cited instead.*

> **Response(34) :** The current references will be updated. The choice for the non-peer reviewed literature is largely due to the current condition that many of the decisions made for the SSI-experiment by the local government were based on these references. And some indicate the aim of the national and provincial government to implement SSI on a large scale as a way of mitigating problems that occur with management of these Peat meadows.

• *Furthermore, table and figure headings are often very brief or contain abbreviations, sometimes also such which are not used in the manuscript(e.g. location "Ger" in Appendix B).*

> **Response(35):** The table and figure headings are expanded upon the improve the understandability of the figures and the abbreviations will be written full out in the headings.

References

Bader, C., Müller, M., Schulin, R., Leifeld, J., 2018. Peat decomposability in managedorganic soils in relation to land-use, organic matter composition and temperature.Biogeosciences 15, 703–719. https://doi.org/10.5194/bg-15-703-2018Eickenscheidt, T., Heinichen, J., Drösler, M., 2015. The greenhouse gas balance of a drained fen peatland is mainly controlled by land-use rather than soil organic carbon content. Biogeosciences 12, 5161-5184, https://doi.org/10.5194/bg-12-5161-2015Flessa, H., Wild, U., Klemisch, M., Pfadenhauer, J.P., 1998. Nitrous oxide and methane fluxes from organic soils under agriculture. European Journal of Soil Science 49, 327-325, https://doi.org/10.1046/j.1365-2389.1998.00156.xIPCC (Intergovernmental Panel on Climate Change), 2014. 2013 Supplement to the 2006 IPCC Guidelines for National Greenhouse Gas Inventories: Wetlands, Hiraishi, T., Krug,T., Tanabe, K., Srivastava, N., Baasansuren, J., Fukuda, M., Troxler, T.G. (eds), IPCC, Switzerland.Koponen, H.T. and Martikainen, P.J., 2004. Soil water content and freezing temperature affect freeze–thaw relatedN2O production in organic soil. Nutrient Cycling in Agroecosystems 69,213–219, https://doi.org/10.1023/B:FRES.0000035172.37839.24Tiemeyer, B., Freibauer, F., Albiac Borraz, E., Augustin, J., Bechtold, M., Beetz, S., Beyer, C., Ebli, M., Eickenscheidt, T., Fiedler, S., Förster, C., Gensior, A., Giebels, M., Glatzel, S., Heinichen, J., Hoffmann, M., Höper, H., Jurasinski, G., Laggner, A., Leiber-Sauheitl, K., Peichl-Brak, M. & M. Drösler, 2020. A new methodology for organic soils in national greenhouse gas inventories: data synthesis, derivation and application. Ecological Indicators 105838, https://doi.org/10.1016/j.ecolind.2019.105838