Review

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:

- 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".
- This target water level seems to be based on the assumption that the majority of the CO_2 emissions originates from deeper peat, but no reasons are given for this assumption.
- According to Figure C1, there were partially only 7 measurement dates for N_2O in 2017 and afterwards a gap of five months. Given the highly episodic nature of N_2O fluxes, this is absolutely inadequate for the calculation of annual balances in a strongly fertilized grassland.
- 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 aboveground biomass on maximum photosynthetic rates.

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 CO_2 emissions 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 CO_2 emissions to water table depth (too dry, no more peat exposed, Tiemeyer *et al.*, 2020).

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.

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 topsoils 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.

Frequency of N_2O flux measurements

According to Figure C1, there seem to be only 7 measurement dates for N_2O 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 N_2O 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 N_2O emissions may occur when temperatures change between frost and thaw (e.g. Koponen and Martikainen, 2004), especially under wetter conditions, and that maximum N_2O 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 N_2O 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.

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.

Further comments

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

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 topsoil and on depths where the water level/moisture changes actually occurred.

- How comparable are SSI and control when they partially strongly differ in SOM content (location D) or C:N ratio (location A)?
- 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. Does the "schalter" layer have any effect on the sites' hydrology?

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

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?
- Was PAR outside the chamber corrected for the light transmittance of the chamber before interpolating GPP? Which light transmittance was assumed/measured?
- Was there any quality control procedure for flux calculation (linearity, outliers, leakage…)?
- Was there any minimum temperature difference within one R_{eco} campaign to avoid artefacts due to extrapolation of Eq. 2?
- Which R_{eco} (nearest?) was subtracted from NEE to yield GPP (line 192)?
- The unit of α is wrong (line 200), it should be mg CO_2-C m^{-2} h^{-1} / µmol m^{-2} s^{-1}
- Why did you choose to interpolate parameters and not weighted fluxes (line 204)?
- Why did you use GPP_{max} and not GPP_{opt} (Falge *et al.*, 2001) which is less susceptible to extrapolation errors?

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 R_{eco}. 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!

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?

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

Table 2 and Table 3

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

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.

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

Line 451: What do you mean with “abnormal data points”?

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”.

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)
- 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.

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).

References

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Flessa, 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.x

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