Anonymous Referee #1, 03 Jan 2024

We would like to thank Referee #1 for his/her effort to review our paper. We are happy to hear that the reviewer finds the study interesting. We agree with most of the comments and suggestions that are given. In the comments below we will elaborate on how we want to incorporate the suggested changes in our manuscript. Also, if we disagreed with the reviewer, we will elaborate on the argumentation why we do not share his/her vision.

General: This manuscript describes an interesting case study on the effects of top soil removal, rewetting and the use of different wetland plant species on greenhouse gas emissions. The emissions fluxes were compared between these different plant species, but also with a reference grassland site. It would be nice to mention the total greenhouse gas balance in the abstract also, in CO2 equivalents, so that the reader can directly see the effect of the treatments on the greenhouse gas balance.

Thank you for the suggestion. We will add the total GHG balances of the treatment and reference site in CO_2 equivalents to the abstract.

Introduction:

L75. This is the first time that I hear that vegetated conditions may have higher CH4 emissions than non-vegetated conditions. Moreover, in the paper of Antonijevic it is stated that the period with elevated CH4 emissions ended with the occurence of cattail. So please correct that reference. And why are there no measurements of non-vegetated conditions in this experiment?

There are many studies that show that vegetation leads to higher emission (Bodmer et al., 2024; Bastviken et al., 2023; Zhang et al., 2019; Hendriks et al., 2010; Kankaala et al., 2003) with the most important reason the carbon substrate input in the system for methanogens, and plant CH_4 transport. However, the oxygen transport to the rootzone also increases CH_4 oxidation, which in some cases leads to lower CH_4 emission (e.g. Vroom et al. 2018, van der Nat et al., 1998). But indeed, the study of Antonijevic was not the correct reference to back-up our point. We will change the text with including the argumentation and references above.
We conducted also CH_4 measurements in non-vegetation conditions, but only with the manual chambers, so therefore we did not include it in this paper (but is described in Vroom et al. in review). These data also showed that the treatment without vegetation had the lowest CH_4 emission of all treatments.

L95. No CH4 measurements at the reference site?

There were CH_4 measurements done on the reference site in a different year (2019) with a different chamber system (manual), therefore we did not include the results in this paper. The data are however described in a report. We refer to this report in the discussion.

Methods: are the two Typha compartments 430 m2 in total, or are they each 430 m2?

They are 430 m^2 each. We will describe this clearer in the methods.

Figure 1C: indicate the inlet ditch and the water flow.

In Figure 1 we aim to visualise the general overview of the field site and its geographic location. In Figure 2 the water flow, including inlet and outlet ditch is further described. We do not see added value in including these details in Figure 1.

L135. Is it realistic to provide only inorganic fertilizer to the reference site? Does this give an underestimation of carbon fluxes to the atmosphere?

The decision for inorganic fertilizer was made to prevent to have an extra carbon source for the carbon balance. If organic fertilizer was used, carbon input from organic fertilizer should be subtracted from the carbon and GHG balance. However, the carbon content of manure can vary to some extent, and it is unclear how long it exactly takes for the manure to be decomposed again. This results in higher uncertainties in the carbon and GHG balance. This uncertainty is not present with inorganic fertilizer, since there is no extra carbon input which needs to be corrected for.

L180. It is a weak point that CH4 fluxes have appartently not been measured in the reference site. This flux could be zero of course, but then the authors should mention this. Also no N2O emissions were measured, which could have a major effect on GHG emissions, especially on the reference site. Please discuss the importance of N2O emissions somewhere in the introduction or discussion.

As mentioned above as well, we will refer to the measured CH_4 emission that are described in a report in the discussion. The emissions were very low, hardly contributing to the total GHG emission.
N_2O is indeed a missing GHG. With complete inundation of the soil, we do not expect much N_2O from the paludiculture fields. From the reference site, N_2O emission will most likely contribute significantly to the total GHG balance. Therefore, we expect even a larger reduction of GHG emissions from the paludiculture fields. We will mention this in the discussion.

Results: it would be good to provide the actual biomass harvest values (per m2 or per ha). Now this is only mentioned in the discussion.

We will add this to the results as well.

Fig.7 typo (And).

Will be removed.

L370. Table 3. Figure 9. Why is all harvested biomass (C-export) considered as CO2 loss and thus as GHG flux? This totally depends on the biomass use. The grass from the reference site will partly be converted in CH4 by cows and the Typha biomass will for example only be converted to CO2 after a long time if it used as building or insulation material. This seems to be an important disclaimer here. The authors mention this in the discussion, but the disclaimer can also be mentioned here already.

We will add the disclaimer about the use of the biomass already in the results.

Discussion: how do Typha roots supply easily degradable carbon to the sediment? And is this in a significant order of magnitude to have effects on CH4 production?

Roots lose carbon by root exudates, which is an easily degradable substrate. This is a very relevant carbon source for CH_4 production and thus emission (Bastviken et al. 2023). We will elaborate on this in the discussion.

L408-410: several typos.

Typos will be corrected.

L410: I think that the damage to the T. latifolia plants is een important thing to mention, also in the abstract and conclusions, as it seems to be the reason for the very high methane emissions.

Herbivory is already mentioned in the abstract and discussion as possible cause for the higher emissions. We will also write it in the conclusions.

L451-453: the authors mention the CO2 emissions for cultivating and processing Typha here, but do not mention the CO2 (and CH4) emissions for the reference site, i.e. the cultivating and processing of grass, milk, etc. This probably also (more than) compensates for the grass biomass harvest. So please make a fair comparison, or leave the statement about CO2 emissions for cultivating and processing Typha out of the text.

This is a good point. We will mention the extra emissions for cultivation and processing of grass biomass harvest as well.

L456: if the topsoil would have been stored under anoxic conditions, much more CH4 would have been emitted in CO2-equivalents than the 557 t CO2 per ha under oxic conditions, based on the papers of Harpenslager et al., (2015) and Quadra et al. (2023). The authors also mention this in line 468. So in that sense, the authors could be more positive, or less negative, about topsoil removal here.

We were not necessarily negative about topsoil removal we only point out that there is a huge amount of carbon removed with the topsoil and this should be considered in the GHG balance. How much more CH_4 we would have gotten without topsoil removal is of course hard to say. We will mention the trade-off between potential higher CH4 emissions and CO2 reduction with respect to top-soil removal.

L468: typo

Typo will be corrected.

L475-478: the highest chloride concentrations measured in the surface water were 62 mmol/l, which is equivalent to 2.2 g/l. This is in the range of the upper limit for T. latifolia and far under the upper limit of T. angustifolia. So the statements made here are not true.

Indeed, the made statements are incorrect, thank you for noticing it, and will be rephrased as follows:
'... which is similar to the concentrations we observed and may partly explain the inhibited growth. For T. angustifolia our measured concentrations were lower than the upper limit of 7.2-8.8 g l-1 (Sinicrope et al., 1990).

L482: typos.

Typos not found.

Conclusions: please rephrase based on the feedback given above.