Anonymous Referee #2, 03 Jan 2024

We would like to thank Referee #2 for his/her effort to review our paper, and for the positive feedback, and points to improve or clarify. We agree with most of the comments that are given, and the suggested changes will help the paper to get to a higher quality and better readability. In the comments below we will elaborate on how we want to incorporate the suggested changes.

General comments:

This is a comprehensive and well written paper which constitutes an important contribution about species-specific GHG balances from species relevant for paludiculture. The measurements have been conducted over a single year with limited frequency, which is common practice with this type of studies, but remains an important limitation. The study is ambitious as it aims to capture the fluxes of both CO2, CH4 (both diffusive and ebullitive fluxes) as well as important soil and water chemistry. There were also mentions of DOC, but these fluxes have not been reported. Inclusion of N_2O would have strengthened the study further.

We are happy to read the positive feedback about the relevance and quality of the paper. We agree that the measurement frequency is a limitation for the diffusive fluxes. The only other method that would overcome that problem would be the eddy covariance method but cannot be used on such a small scale as this experiment. One year of data is indeed also a limitation. We started a year earlier, but then the vegetation was not sufficiently developed, so we decided not to use the data. Extending the measurement period was financially not possible. DOC concentrations were indeed measured monthly, but since we do not know the exact water flow rate from the inflow ditch to the outflow ditch, we cannot calculate fluxes. 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. This makes that we expect even a larger reduction of GHG emissions from the paludiculture fields. We will mention this in the discussion.

The paper concludes that rewetting (flooding) with paludiculture reduces GHG emissions compared to an intensively used drained fen grassland, and that the choice of species is relevant for the success. Moreover, the paper discusses the pros and cons of topsoil removal at this site, and seem to suggest that topsoil removal may not have been positive here due to the removal of soil C, high CH4 fluxes, and possibly P-limitation for the plants. This is an important consideration for future studies as topsoil removal has been suggested to decrease CH4 emissions and limit P-leakage, without much consideration of where this soil should be stored to avoid continued soil C-oxidation off-site.

We are happy to hear that the reviewer shares our concerns of topsoil removal.

However, the study cannot determine the role of topsoil removal itself on post-rewetting GHG emissions as they have no reference site for this particular question.

It is indeed a good point that we cannot exclude the single effect of topsoil removal on CH_4 emissions. The CH_4 emissions could have been even higher without topsoil removal, but we do not know. However, the fluxes we measured are in the upper limit of what we found in *literature from sites without topsoil removal, so it is the question if much higher emissions are realistic. We will address this point in the discussion.*

The study is also unable to answer questions about how GHG emissions from rewetting may be managed by the choice of water table depth, which asserts strong control on CH4 emissions in particular. This was briefly mentioned in the discussion but deserve further consideration due to its importance.

We did study that too, together with different forms of manure applications, in the other basins (Figure 1C). We came to the conclusion that it would have been too much to elaborate on that in this paper, so these results are described in a separate paper (Vroom et al. in review). We will mention that we measured it and refer to the other paper more clearly.

All in all, the study clearly answers the questions they set out to investigate, which is well-described by the title.

We are happy to hear that.

Scientific comments:

Material and methods:

There is a mention in the methods that Azolla died off and was replaced by *Lemna spp*. This is not discussed further anywhere. How fast did the Azolla deteriorate, was there x% left in September/October when the data in figure 6 suggest high concentrations of ammonium and total phosphorus in the pore water? And isn't that 'weakness' something worth mentioning when suggesting Azolla as a paludiculture species?

We agree that this is not enough discussed yet. Azolla *started to decline substantially from October (70% coverage by the end of October to completely disappeared in December), which does not completely coincide with the high ammonium and phosphorus pore water concentrations.*
We will add the coverage of Azolla *and* Lemna *spp. over time in the appendix and we will mention the risks of herbivory (for* Typha *as well) in the discussion.*

The reference site is presented in this section, and I wonder to what extent this site is representative of the conditions before topsoil removal and rewetting. The text does not discuss why this site was chosen, and the choice is not defended anywhere. Primarily the site is described to be different (intensively used, grazed, fertilized), hence my worry.

We understand the concerns. The reference site is indeed not representing completely the situation before rewetting, since the land management is different at the reference site. However, the soil profile and water management are very similar. The reference site is a site that consists of conditions that are representative for drained peatlands in this area. We agree that it would be good to discuss this point and give a better description so that the reader better knows what the reference site represents.

The study mention that diurnal fluxes were captured, but this data is not presented anywhere, which is unfortunate. There are several studies showing diurnal patterns of CH4 emissions from plants, but more data is needed to confirm which species this is relevant for. It would have been a nice addition to the supplementary material to visualize diurnal patterns, both if there are clear patterns or not. There is also no further mention if these diurnal patterns were included to interpolate data or to correct it.

Indeed, diurnal patterns are expected, especially from the Typha *species with the pressurized flow. We did look into this, but it is described in the other paper of Vroom et al.*

I am somewhat unfamiliar with bubble traps and would have liked a reference to the method to indicate if this is standard measurement practice.

This method is used more often, and we will add a reference for the bubble trap.

There seems to be no measurements of CH4 from the reference site, although emissions may have been negligible from the soil, they could have been rather large from ditches (if present). This is not discussed fully anywhere.

There were CH_4 measurements done in the year before (2019) with a different chamber system (manual) and therefore we did not include it in this paper. The data are however described in a report. We refer to this report in the discussion.
We did not consider fluxes from ditches, also not at the paludiculture site, but only soil fluxes. We decided that that would be outside the scope of the study. We will mention this in the text.

The authors use the term soil T when measurements are done in the 'soil' beneath a water column. It may be clearer to use the term sediment T when the site is flooded as in this case. I leave this to the authors' discretion, however.

We agree that the term sediment T could also be used, but in our opinion soil T is more suitable as the material is not of sedimentary nature but rather inundated soil.

The interpolation of CH4 based on soil T (or water T in case of Azolla) seems rather risky (which figure 7 clearly shows). The R-square is not high. Could it be possible to reach a higher R-square if more environmental parameters are included? This methodology is also not discussed, and no references to other studies applying this method are made. Is there a risk of an overestimation or underestimation of annual emissions?

The method we used is based on the same idea as the temperature interpolation of ecosystem respiration (R_{eco}) with the Lloyd-Taylor function. With R_{eco} we also know that there are more variables related than temperature (like vegetation). But we assume that the other factors are captured in the mean measured CO_2 fluxes (and thus also CH_4) every 2-3 weeks and are linearly changing over time. So we used the mean measured CH_4 flux per campaign, calculated this back to a reference temperature with the gained temperature relation (Fig. 7), linearly interpolated these reference emissions and then calculated the actual emission with the measured temperature and the temperature relation from this reference emission. This is not well explained in the text, so we will adapt the text to make this clearer. Furthermore, we will discuss this method by comparing it with other methods from literature in the Discussion section.

Results:

It would have been interesting if the authors had supplied a simple RF-modeling to describe the GHG balances for the different species and the reference site (see Günter et al. 2020 – code freely available). This could also have included the C losses from the topsoil removal. I personally think that better visualizes and describes the net GHG impact from rewetting, where CH_4 release is mitigated by CO_2 uptake. However, I concede that in this particular instance, where measurements have only been made over a single year, with insect infestations etc. it may not be prudent to extrapolate emissions over several years (which is done in RF-modeling). This is perhaps something the authors could discuss though. Also, for future studies.

Thank you for this suggestion. We were not aware of the model code, and we are very much interested in applying it to our dataset. If we decide that the results make sense, considering the restrictions of our dataset, then we will include it in the Results section 3.5.

Discussion:

The discussion is overall very good with much to consider and some helpful guidance to understand the results. I have mentioned some parts which are not discussed, which the authors may want to consider including for a strengthened paper.

We are happy to hear that the discussion is considered to be very strong.

I am very pleased to see that the authors mention that high DOC inputs may have influenced the CH_4 emissions, but would also have liked to hear more about the authors' thoughts on its influence on CO_2. Is it possible that NEE measurements were contaminated by allochthonous C? If the inflow had high concentrations of DOC, some of it may have been oxidized to CO_2. Would the contamination be negligible or not?

This is a good point that we have not really considered. We will mention in the discussion the possible overestimation of CO_2 as well.

The authors make a good point when they question to what extent (typo in text) the carbon storage in Typha will continue in the future. These studies should ideally cover more years than they frequently do…

Typo will be corrected. The point of a study covering several years will be added to the discussion.

I am very glad to see that the authors have included numbers on the potential C oxidation from the topsoil removal and how many years it would take to reach the same numbers from the reference site. This is often overlooked.

We are happy to hear this point is received well.

Technical issues

Abstract:

Please note that the species-specific CH_4 fluxes do not match those in table 2.

Indeed, thank you for noticing this. We will change this in the revised version.

L30 “Azolla and T. angustifolia seem to have the highest potential in reducing…” (of these three species)

Will be changed.

L30 “complete rewetting” please consider using the term flooding.

Will be considered.

Introduction:

L43 The increase of CH_4 emissions after rewetting depends primarily on the water table depth. This should be introduced here.

We will introduce this.

L43 “this gives an extra impulse” please reword to incentive.

We agree with the rewording and change it accordingly.

L44 “Rewetting 60%” This sentence does not describe what the reference presents. Clarify this statement. I.e. Rewetting 60% of the drained organic soils would turn the global land system into a net C sink by 2100, as opposed to a net C source as projected.

We will rephrase the sentence as proposed.

L51. It is quite possible that the degree of degradation (increased bulk density and thus higher SOC content per cm3) of the topsoil is important along with the nutrient status when it comes to the CH_4 emissions. https://doi.org/10.1016/j.agee.2016.01.008

Thank you for this additional reference, with more factors that relate to CH_4 emission after rewetting. We will include it in the introduction.

Material and methods:

Figure 1B is very hazy. Is it possible to produce a map with higher dpi?

We will increase the resolution.

Equation 1, the minus sign within the brackets is difficult to see.

We will add spaces next to the minus signs to make it more clear.

Discussion:

L410 Please capitalize the two Wainscot bugs.

We will do so.

L452-453 Typha as insulation material, emissions…? I do not understand this sentence. Is it possible to clarify?

We will change the sentence to: '…but if biomass is used sustainably for long term storage such as building material, this C-export should not be accounted for in the carbon/GHG balance.'