Reviewer 1,

thank you very much for your detailed and constructive feedback, which helped us to improve the manuscript. You stated, that the study is somewhat descriptive. This is due to the fact that there is really scarce literature on the rewetting of coastal peatlands through dike removal. We see the value of our study in the fact that it is the first study that monitors the effect of coastal rewetting on several important variables with a before/after comparison. Thus, the study was not designed to examine individual processes in detail. However, there are many coastal peatlands in temperate latitudes that could be, and likely will be, rewetted with marine water, as several projects are already in the planning stages. Therefore, this study provides important data to evaluate this measure. While editing the manuscripts, we tried to find better ways to statistically elaborate the comparison of the pre- and post-rewetting situation. We think, that the manuscript improved substantially, and hope that it can now be published in Biogeosciences.

In the following, we have reposted the comments (in bold) and placed our responses below them.

Major comments:

- **Comparison of pre- and post-flooding data**

  The authors started their sampling roughly six months (in June 2019) before the flooding event (November 2019). They split their data set in two 'pre' flooding periods and four 'post' flooding periods (Table 1). However, it is not clear HOW they are using these periods in their statistical analysis or whether they take the temporal dynamics into account. Table 2, their main statistical results, seems to be a comparison between spatial means (bay and peatland) as opposed to a comparison of pre- and post flooding – which is the main question at hand. I should say that I am not a statistician myself, so I cannot give particular guidance on this analysis question, but I encourage the authors to reach out to someone about this question. It will help re-focusing some of the discussion on the actual impact of the flooding vs. general differences in concentrations of constituents between bay and coastal wetland.

Reply:

Thank you for your remark regarding the statistical analysis. We intentionally wanted to focus mostly on the differences between the rewetted peatland and the inner bay to investigate the terrestrial-marine connection and the potential impact of the rewetting onto both sides. However, to determine the impact of rewetting, one would naturally expect a comparison between pre- and post-rewetting conditions, but a comparison between the inner bay and the peatland after rewetting is also indicative of the effects of rewetting. Consequently, when we statistically examined the differences between the inner bay and the rewetted peatland, we found clear significant differences for many variables (i.e. variables influenced by biological activity, in particular in spring and summer), but not for physical water properties (i.e. surface water temperature, salinity, oxygen) and these results are summarized in Table 2. To make this focus clearer and understandable, we intend to rephrase the heading of Table 2 to:

"Seasonal comparison of the surface water means (± standard deviation) in the peatland ("peat") as opposed to the inner bay ("bay") for all in situ variables. The number of observations is shown in parentheses, and significant seasonal differences between the inner bay and the peatland are indicated in bold."

For us, it seems obvious to compare stations of both the peatland and the inner bay that were sampled before and after rewetting, but we initially considered a statistical analysis of pre- vs. post-rewetting nutrient concentrations to be difficult because of low data availability before rewetting.

However, after consulting a statistician, we now included a statistical pre- vs. post-rewetting analysis of nutrient and GHG flux data within the same seasons sampled before and after rewetting despite low data availability for nutrient data (summer and autumn 2019 vs. summer and autumn 2020; see new Table 3 in the supplement). We intend to adapt the material and method section 2.3 "Data processing, statistics, and definition of seasons and means" as follows in line 208:

"To describe temporal patterns during the entire sampling period, we defined two pre- and four post-rewetting periods, roughly akin to seasons (Table 1). For a direct comparison between the pre- and post-rewetting periods, we compared nutrient and GHG flux data from summer and autumn 2019 with those from summer and autumn 2020 (Table 3) by using the Mann-Whitney-U test."

The sentence "For direct comparisons between [...]" in lines 208 and 209 will be removed.

To strengthen the interpretation of the results with respect to the pre- vs. post-rewetting analysis, as suggested by the reviewer, we propose to make the following changes in the manuscript:

- To add in results section 3.1 "Surface water properties", line 334: "Additionally, no significant differences between summer and autumn 2019 and summer and autumn 2020 were found in the inner bay. After rewetting, temperature and salinity measurements near the peat surface [...]"
- To include the following table 3 (please see the supplement) right after Table 2:

"Table 3: Statistical comparison of pre- and post-rewetting nutrient concentrations and GHG fluxes. For pre- and post-phases, summer and autumn seasons were used (June to November 2019 and 2020, respectively). Nutrient concentrations are compared for the inner bay and GHG fluxes for the peatland site. *** and "n.s" indicate $p < 0.001$ and not significant, respectively."

variable	pre-rewetting		post-rewetting		p
	mean ± sd	n	mean ± sd	n	
NH_4^+ (µmol L^{-1})	2.6 ± 1.6	9	9.6 ± 17.7	17	n.s.
NO_3^- (µmol L^{-1})	1.9 ± 2.5	8	2.7 ± 3.3	8	n.s.
NO_2^- (µmol L^{-1})	0.2 ± 0.1	10	0.7 ± 1.1	16	n.s.
PO_4^{3-} (µmol L^{-1})	0.9 ± 1.6	6	0.4 ± 0.3	11	n.s.
CO_2 flux (transect + area, g m^{-2} h^{-1})	0.3 ± 0.8	330	0.3 ± 0.3	450	n.s.
CO_2 flux (ditch, g m^{-2} h^{-1})	0.3 ± 0.1	87	0.3 ± 0.3	92	n.s.
CH_4 flux (transect + area, mg m^{-2} h^{-1})	0.1 ± 1.0	97	1.7 ± 7.6	320	***
CH_4 flux (ditch, mg m^{-2} h^{-1})	11.4 ± 37.5	85	8.5 ± 26.9	92	***

- In results section 3.2.1 "Pre- and post-rewetting spatio-temporal dynamics and comparison with a nearby monitoring station" we will replace the sentence "However, as there were fewer measurements before rewetting, [...]" in line 358 with: "However, this finding could not be confirmed statistically (Mann-Whitney-U-test, see Table 3)."
- In results section 3.4.2 "Pre- and post-rewetting GHG fluxes" we will refer to the new Table 3 and add:
- in line 477: "After rewetting, formerly terrestrial CO_2 fluxes decreased in amplitude (-0.5 to 1.4 g m^{-2} h^{-1}), while the summer and autumn averages were unchanged compared to the pre-rewetting fluxes (Table 3)."
- in line 484:"In summer and autumn 2020, after rewetting, average CH_4 fluxes on formerly terrestrial land increased slightly but significantly (1.74 ± 7.59 mg m^{-2} h^{-1}), whereas in the ditch they decreased significantly (8.5 ± 26.9 mg m^{-2} h^{-1})."

Furthermore, we applied a more robust statistical analysis to investigate the influence of temporal vs. spatial dynamics that justified the usage of means for the peatland and the inner bay, respectively. Thus, we suggest to add in line 213:

"The difference between spatial (sampling stations) and temporal (sampling seasons) data variability was tested by using a Two-Way ANOVA and showed a higher temporal variability ($p < 0.05$)."

Finally, we are planning to include the following changes within the discussion:

- In line 683: "At our study site, [...] terrestrial locations increased significantly by 1 order of magnitude, the overall increase [...]."
- Finally, we need to correct a small mistake in the standard deviation of the pre CO_2 flux in the lines 28 and 475: [...] 0.29 ± 0.82 g m^{-2} h^{-1}. (former value: 0.29 ± 0.74 g m^{-2} h^{-1}).

- **Calculation of lateral transport rates**

Sampling and quantifying lateral fluxes in coastal systems is a difficult task, given the potentially huge temporal (and spatial) hydrological variability. This system is not tidal, but still exhibits considerable temporal variability in water level, possibly wind-driven. The authors use a combination of hydrological and topographical information to estimate discharge in relatively high temporal resolution. It is less clear, though, how the manually sampled water constituent data is integrated with this discharge time series. Given the temporal variation in water level, was this taken into account for the water sampling? Or do the authors calculate seasonal or general concentration means? The export rates are given with uncertainty ranges, but it is not explained how this uncertainty range is generated. The uncertainty range is quite high, typically of equal order of magnitude as the mean export rate. I believe that that is indeed realistic and raises the question of how confident we can be about the quantification of these fluxes. Finally, the sign convention for import and export fluxes in equations 4 and 5 are not well explained. I thought that the Qin is a positive flux and Qout negative (equations 2 and 3). However, in equations 4 and 5, this seems to have been flipped: Qin is explicitly a negative flux, and presumably Qout is positive, although that is not clearly defined. This reverse step seems unnecessary and potentially confusing to me.

Reply:

Thank you very much for your valuable feedback on this topic. Concerning the high temporal variations of the water level, water levels ranged from ~ -0.1 to 0.6 m above sea level during our study period (Figure A1). For the calculation of the export rates, we summarized the nutrient concentrations into seasonal concentration means for all nutrient species (DIN-N, PO_4-P) each for the peatland, the inner bay, and the reference station (central bay) separately. To make this clearer within the method section 2.4.3 "Nutrient transport calculation", we intend to adjust the sentence starting in line 252:
"Seasonal mean values of nutrient concentrations (DIN and PO_4^{3-}) were calculated and converted from µmol L^{-1} to kg m^{-3} by using the molecular masses of the basic elements N and P to derive DIN-N and PO_4-P."

To better explain the calculation of the uncertainty range, we propose to add the following text in line 259:

“Uncertainty ranges for the seasonal NNTs (u_{NNT}, as 95 % confidence level) were calculated as standard errors (SE) by using an error propagation according to Eq. (6):

$$u_{NNT} = \sqrt{\left(c_{bay}\, dt\, u_{Qin}\right)^2 + \left(c_{peat}\, dt\, u_{Qout}\right)^2 + \left(Q_{out}\, dt\, u_{cpeat}\right)^2 + \left(Q_{in}\, dt\, u_{cbay}\right)^2}$$

where terms with “u” denote the respective SE as 95 % confidence level. To gain the annual SE of the NNT, all seasonal SE were added up.”

It is right that the uncertainty range is high and “raises the question of how confident we can be about the quantification of these fluxes”. Since another of your comments further below addresses this topic as well, we will give a combined answer to both comments and post it after the second comment in the “minor comments” part (concerning lines 522-533).

Concerning the sign convention of equations 4+5, we double-checked these and removed the minus signs in both equations because these were simply wrong. Thank you very much for pointing that out.

- **Use of reference data (2016-2020)**

  The authors state that they use 4 years of data from the monitoring station in the central bay, but it is not clear to me how several years are being used as opposed to ‘only’ the 2019 and 2020 data used in the results. Given the short sampling period, it may be helpful to see how much inter-annual variability occurs in the water chemistry in the central bay and whether the concentrations can possibly get as high as in the peatland area.

Reply:

Thank you for pointing out that this seems confusing and hard to understand. The inter-annual variability you mentioned is exactly the reason why we chose to consider more years for the reference data instead of only the two sampling years we have for the study site. Accordingly, in section 3.2.1, we compared our 2019/2020 nutrient data of the inner bay with 5-year reference data (2016-2020) of the nearby monitoring station (“central bay”). By doing so, it became visible that we could neglect the inter-annual variability and focus on the effect of rewetting only. We will clarify this by adding the following sentences in results section 3.2.1 “Pre- and post-rewetting spatio-temporal dynamics and comparison with a nearby monitoring station” starting in line 368:
“Nutrient concentrations of the monitoring station (“central bay”) showed a low inter-annual variability during the years 2016-2020 and often lower concentrations than the inner bay (Figure 6). A detailed comparison of nutrient data from the monitoring station with those from the inner bay showed that before rewetting, only the NH_4^+ concentrations were significantly higher in the inner bay. “

The sentence “Compared to the monitoring station, [...] shortly before rewetting (Figure 6)” will be removed.

Additionally, we also used these 5-year-data in Figure 6, line 380. We stressed out in the figure caption that 5-year-data are shown for the central bay. However, we intend to adapt the figure legend of the central bay to “central bay (2016-2020)” to additionally highlight that there are 5-year-data included for this area.

- **Air-sea exchange**

The authors describe very late in the paper (section 3.4.1), that they compared their methods in determining air-sea exchange, i.e. comparing floating chamber estimates to k based estimates. This should be moved up from the results into the methods section. I may even suggest to put the method comparison in the appendix and only note in the methods that they have done this comparison, with reasonable agreement.

Reply

Thank you very much for this good suggestion. Since we had the same discussion among the authors about the best position in the manuscript prior to submission, we will follow the advice to move section 3.4.1 into the appendix C “Appendix C: Comparability of two independent approaches to atmospheric flux determination”. Accordingly, the results section will continue with section 3.4 “Pre- and post-rewetting GHG fluxes (CO_2, CH_4, N_2O)”.

Moreover, we intend to include a short notice about this comparison in the methods section and create a new headline within section 2.5.3 (line 329) “Comparability of two independent approaches to atmospheric flux determination” with the following content:

“We evaluated the comparability of the two previously described methods by comparing the results of a representative station (BTD7) for each post-rewetting season. The comparison showed no significant differences between the fluxes of CO_2 and CH_4 derived with the different methods and therefore, it seems appropriate to combine the fluxes for each GHG into one pooled post-rewetting data set. The pooled post-rewetting flux values were compared with the pre-rewetting values to investigate the effect of rewetting on CH_4 and CO_2 fluxes (Table 3). For more details concerning the comparability approach, see Appendix C.“

- **Peatland CO_2 fluxes**

From the information given, it is not clear to me how much of the vascular vegetation remains after flooding and how their possible disappearance is taken into account: The authors take light, dark and shaded measurements before the flooding, presumably when the vegetation was active. They stop doing that after flooding, presumably because no vegetation has survived the flooding. However, in the analysis of the fluxes, it looks to me, that this impact on CO_2 fluxes (more directly on the vegetation itself) is not analyzed or discussed at all. Are the CO_2 fluxes prior to flooding just taken as an average? And – given the light dependence – would it not make sense that those values are more variable than after flooding?

Reply:

Thank you for this comment. Yes, vascular vegetation died completely after the flooding. Therefore, we assumed that photosynthesis by the macroflora would not significantly take place after rewetting. This was also the main reason why we conducted the post-rewetting measurements with opaque floating chambers. We will make that clearer in the method section and change the sentence in line 199 to:

“Since the flooding caused most plants to die, and almost all measurement locations were covered by water during the study period, we skipped the NEE measurements with transparent chambers.”

The sentence “Since transparent chambers were no longer used, PPFD variation was no longer considered” will be removed.

We indeed used averages for the gas fluxes, but we included the ranges too, see lines 474-475. In this way, we can show the variability of the CO_2 fluxes. It is also displayed in Figure 10a. We also mentioned in the text that the amplitude of the CO_2 fluxes decreased after rewetting (line 477). Thus, we indirectly take the vegetational die-back into account. If we would exclude the pre-rewetting transparent measurements and only take opaque chamber data into account, we would have an average of 0.62 ± 0.63 $g*m^{-2}*h^{-1}$ from June-November pre-rewetting instead of 0.29 ± 0.82 $g*m^{-2}*h^{-1}$ as mentioned in line 475. Thus, the pre-rewetting CO_2 emissions would be much higher without the consideration of photosynthesis and hence, the activity and presence of vascular plants. The lack of photosynthesis by the macroflora is also visible in the shrinking variability (at least the negative amplitude, which is the CO_2 uptake), from a range of -0.38 to 3.0 $g*m^{-2}*h^{-1}$ when looking at the opaque chamber fluxes only compared to pre-rewetting fluxes from all chambers, which have a range of: -3.3 to 3.0 $g*m^{-2}*h^{-1}$. Thus, in general, we overestimate the post-rewetting net fluxes since we do not include, likely minor, uptake by for instance algae floating in the water. Therefore, our estimate on how much the rewetting decreases overall GHG emissions is conservative which is the common approach in the literature on rewetting and its impact on GHG emissions.

Minor comments

> **Lines 389-341: It is worth mentioning that along the 15km distance between peatland and central bay station, some of the nitrogen will be transformed and lost to the atmosphere, so that this is a maximum estimate.**

Reply:
It is absolutely true that nitrogen undergoes transformations and might also be lost to the atmosphere along the way. We indirectly pointed this out in the method section 2.4.2, lines 234-236 and in the results section 3.2.2, lines 390-391 by calling it <total possible export>. However, we will add the following sentence in method section 2.4.2, line 236 to make this clearer:
"Due to transformations and potential losses along the way to the monitoring station, especially of the nitrogen species, the calculated total possible export is a maximum estimate."

> **Lines 504-511: This is a repetition of the results.**

Reply:
Thank you for pointing this out. We are going to remove lines 506-511. The transition will be changed to the following in line 506:
"[...] with those of the inner bay and of an unaffected monitoring station ("central bay"), showing generally higher mean concentrations. The remineralization of upper peat layers [...]"

> **Lines 522-533: I like the comparison to the river, but I think it would be important to discuss the different range of uncertainties for the two sources to the coastal ocean. I do not doubt that coastal peatlands are hot spots and relevant despite their small scale, but we still have real difficulties quantifying their lateral exchange.**

Reply:
It is right that the uncertainty range of our calculated exports is high and that the ranges of our values and the ones from the river we used for comparison are highly different. To address this important issue, we intend to include the following sentences in discussion section 4.1 "Nutrient dynamics and export" at the end of line 533:
"However, we also want to shortly address the reasons for the high uncertainty range of our calculated nutrient exports. Firstly, they derive from high fluctuating nutrient concentrations in the surface water within the seasons. This is also visible in the high standard deviations (Table 2). Therefore, the 95 % confidence level of the nutrient exports is high and reflects the natural dynamic. Secondly, we

conducted default error propagation during the export calculation which leads to even higher ranges on top of the high natural dynamic.

Compared to the Warnow river, it is noticeable that the range of uncertainties is highly different for the two sources. While our uncertainties are mostly higher and in the same order of magnitude compared to the means, the uncertainties of the river data are one order of magnitude lower. This is likely due to the different time scales of the two data sets. Our export data were generated by taking only the first post-rewetting year into account in which the system was still in a transition state and thus, showed very dynamic nutrient concentrations. The uncertainties of the river exports were generated by using 25 years of data, leading to lower uncertainties than using data from only one year. However, the uncertainty range of the river exports was calculated as standard deviation and not as standard error, as was done for the exports of our study site. Therefore, this has to be considered when their uncertainty ranges are compared directly."

Line 540: It is worth pointing out that the 'seafloor' includes the now wetted peatland. Anoxic decomposition processes, such as sulfate reduction will produce alkalinity, if the sulfide is removed from active cycling (e.g. via building ironsulfides). It is also worth separating 'primary production' in the different components of phytoplankton and vascular plants. The proportion and relevance of either contribution should change with the flooding.

Reply:

This comment shows your profound understanding of coastal wetland biogeochemistry. And yes, there is many interesting changes going on. Indeed, separating primary production in the components of phytoplankton and vascular plants would be helpful and likely change with the flooding. Unfortunately, our data set does not allow to distinguish between these contributions, as this was not within the scope of the work and therefore, we cannot make any estimates on the individual impacts and changes with the flooding. However, we will include the following change: "[...] or can be introduced by mineralization processes from the seafloor, which includes both, the seafloor in the inner bay and the flooded peatland."

Lines 559-562: See above, possible influence of anoxic decomposition in the peat.

Reply:

We will add the following sentence in line 562:

"CT and AT values during this period [...] the recently inundated peat. Besides, local $CaCO_3$ weathering as well as local anoxic processes, such as sulfate reduction may have increased the A_T in the submerged soil and finally contributed to higher AT values compared to the inner bay."

Lines 579-581: To me this is an observation that is worthwhile to put in the site description.

Reply:

We think that this is a great idea and therefore intend to make the following changes: Line 579-581 will be removed and changes will begin in line 147:

"Therefore, minor changes in the water level lead [...] from 0.08 to 0.7 km^2 (Figure 3, Figure A2). The ditch system was only partly removed and hence, some deeper areas with water depths of up to 4 m remained. It is noteworthy that in the first months after rewetting, former grassland and ditch vegetation (Elymus repens L. (Gould) (Couch grass), Phragmites australis (Cav.) Trin. ex

Steud. (Common reed)) died almost completely and the cover of emergent macrophytes was then negligible."

Line 775: Which vegetation is supposed to expand under these hydrological conditions? If the authors have information on this, that would be helpful. Presumably the grass will die back but maybe Phragmites can withstand the water level height?

Reply:
Yes, the grass died back completely because of the permanent inundation. At first, Phragmites also died back in most spots. But over time, it grew back and expanded around the ditches where it was established already before the rewetting. We intend to include this information in section 2.1 "Study site" after the new sentences we wrote as a reply to the previous comment:
"[...] and the cover of emergent macrophytes was negligible. However, Phragmites was able to grow back during the growing season and expanded especially around the ditches."

Line 776: That may well have depended on the amount of soil moisture/position of water level in the drained peatland, on which there seems to be no information.

Reply:
It is right that the soil moisture and the position of the water level are influencing the amount of N_2O emissions from soils. In our study site, the water level was permanently below the soil surface prior to rewetting. Therefore, it seems very likely that the drained peatland was a source of N_2O, as was already shown for other similar sites (e.g. Martikainen et al., 1993; Regina et al., 1999; Goldberg et al., 2010). To make this clearer and give some more information about our study site, we intend to add in line 750:
"[...] grasslands, respectively (Augustin et al., 1998). N_2O fluxes in drained peatlands are due to a low water level which allows the penetration of oxygen into the peat to fuel N_2O producing processes (Martikainen et al., 1993; Regina et al., 1999). As the water level in our study site was permanently below the soil surface before rewetting, it is likely that it was a source of N_2O."

Line 779-782: These implications for future (or adjacent) land development are interesting. However, in my opinion a lot will depend on whether vascular vegetation is going to be established, otherwise I do not see the potential for increasing carbon storage (high positive CO_2 fluxes). If the group intends to continue with these measurements on site, it is worthwhile to say that.

Reply:
Of course, the potential for carbon storage depends on the development of vascular vegetation and its burial in anoxic sediments, but also on continued scientific research in the future. Therefore, we intend to do the following changes beginning in line 782:

"Nonetheless, because degraded peat is both nutrient [...] OM as was demonstrated by other studies. In addition, whether or not the area will act as a C sink in the future, depends on the success and speed of the establishment of vascular vegetation and its burial in the anoxic parts of the sediment.".

Furthermore, the investigations are continuing and hence, we will add a comment in line 788:
"The investigations addressed in this study will continue in the study site during the next years in the framework of the DFG funded graduate school Baltic TRANSCOAST."

Reviewer 2,

thank you very much for your feedback on our manuscript. Especially the comment on the GHG flux estimates is an important issue that we hopefully improved and clarified in the revised version. In order to respond to the comments in detail, we have reposted the comments (in bold) and placed our responses below them.

Pönisch et al investigated the nutrient and greenhouse gas response in a coastal peatland rewetted by the Baltic water. They suggest the rewetted peatland could be an overlooked nutrient source to coastal areas. The observations are interesting, and their conclusions could be supported by the data, but the potential of the data could be explored further.

- **For the nutrient part, the authors have attributed nutrient increase after rewetting to more mineralization. So what about the changes of N to P ratios before and after rewetting? Their ratios could point to some nutrient source changes via rewetting. Another question is organic carbon from the Baltic Sea, could it bring in OC that is mineralized in peatland or most of nutrients were derived from local organic carbon degradation. At least some cross plots of e.g. NH4 vs PO4 are needed to explore if there is any patterns or any dependency among variables.**

Reply:

In fact, changing N:P ratios can be used to identify changes of the nutrient source. However, we did not include these values because the nutrient concentrations alone, as shown in Figure 5, indicate that there was a strong shift towards higher N-nutrient concentrations and thus, higher N:P ratios in the inner bay shortly after rewetting.

Shortly before rewetting, in autumn 2019, the N:P ratio was ~30 in the inner bay, whereas it increased to ~350 in winter 2019/2020 after rewetting. To make the observed N:P ratio shift more prominent, we suggest to include the following sentence in results section 3.2.1 "Pre- and post-rewetting spatio-temporal dynamics and comparison with a nearby monitoring station" in line 356:

"[...] slightly higher post-rewetting (Figure 5). This increase of N-nutrients led to a drastic increase of the N:P ratio from ~30 in autumn 2019 before rewetting to ~350 shortly after rewetting in winter 2019."

We assume that this increase was due to the release of mainly N-nutrients out of the top soil and its lateral export into the inner bay, as was stated in line 520. This N excess fits well to the history of the study area that had been agriculturally used before rewetting. Additionally, leaching of e.g. ammonium induced by saline water can also contribute to the DIN pool in the surface water as was described by Rysgaard et al. (1999).

Concerning the organic C, we found higher DOC concentrations in the peatland than in the inner bay. We therefore assume that most OC derived from local OC degradation or DOC pools in the peat and subsequent soil leaching within the peatland. We can exclude DOC from the Baltic Sea as potential source since we measured much lower concentrations in the inner bay as has been stated in line 344.

It is of course right that cross plots are helpful to explore potential patterns among the nutrients, but we did not include these into the manuscript. Since we did not measure any process rates, we are not able to link any relationships to ongoing processes. However, we included some cross plots both for the peatland and the inner bay for all seasons within this reply. Based on these cross plots (will be included in the Appendix: "Appendix D: Nutrient cross plots") we intend to include the following sentences in results section 3.2.1 in line 367:

"[…] only during summer ($p < 0.05$). Additionally, some general correlations between some nutrient species were found (Figure D1). Both in the peatland and in the inner bay, especially correlations between NO_2^-/NH_4^+ and NO_3^-/NO_2^- were significant."

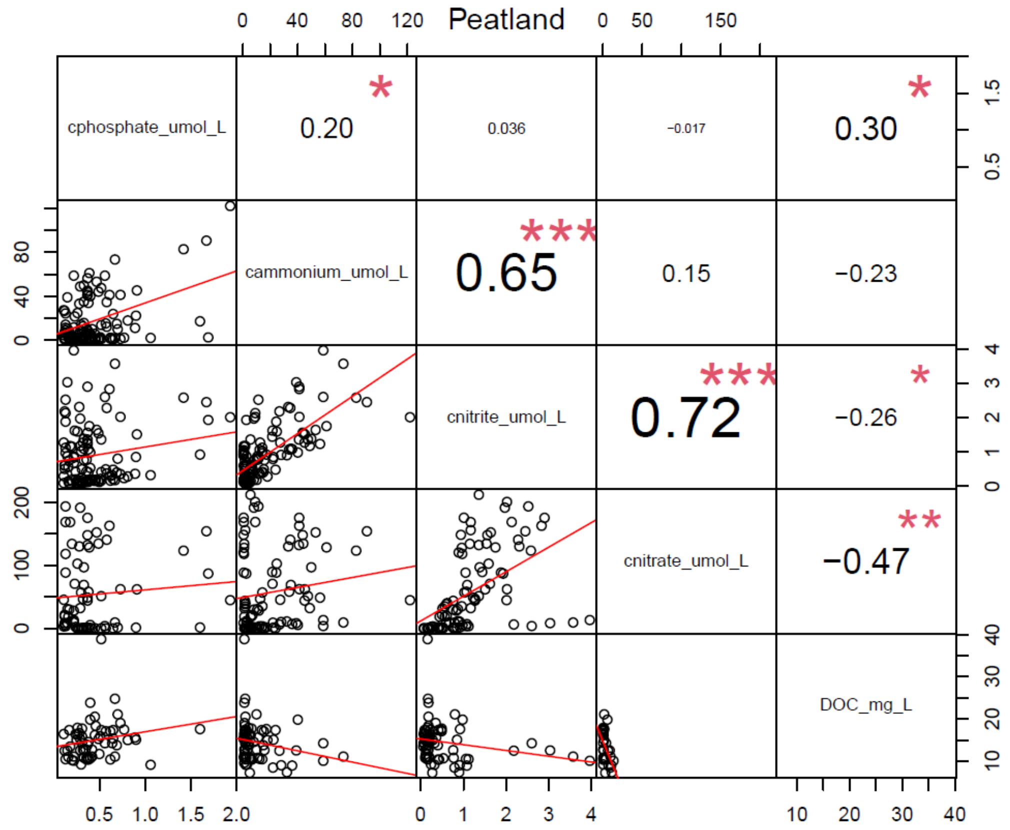
We also want to mention these correlations in the discussion section 4.2.3 "N_2O" and therefore intend to change and add in line 733:

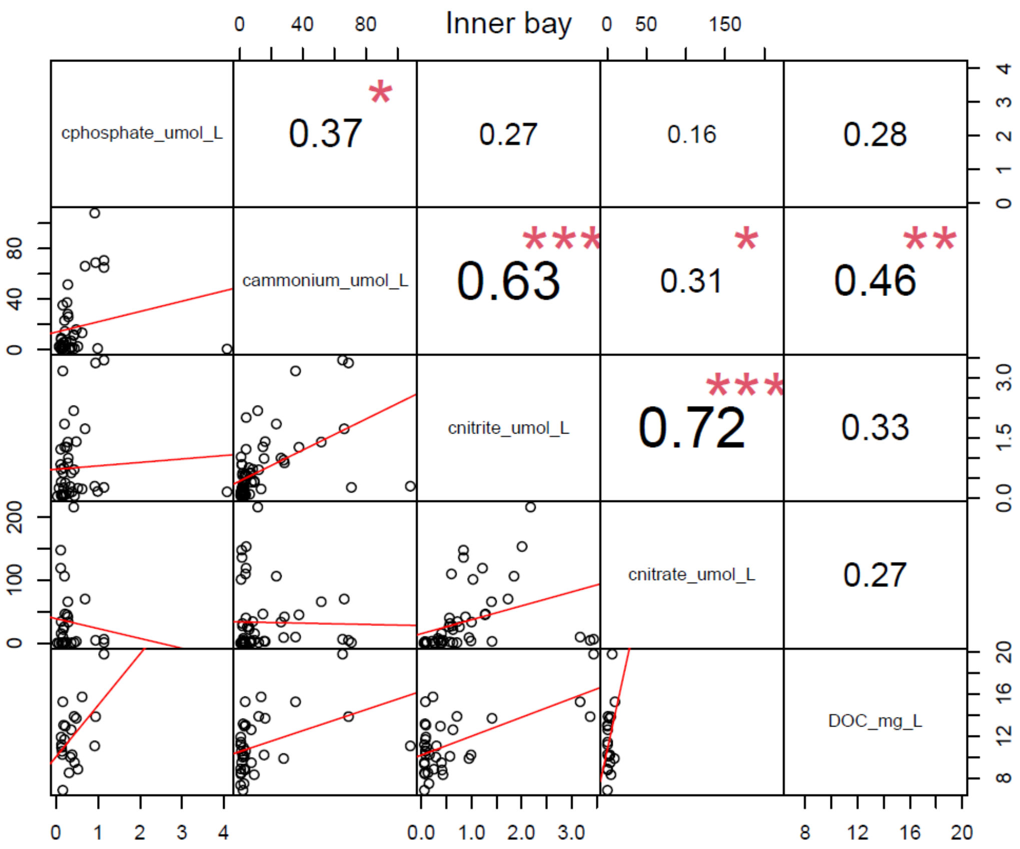
"[…] were measured one week after rewetting and a significant positive correlation between these two variables was found in winter. Additionally, some general correlations of NO_2^-/NH_4^+ and NO_3^-/NO_2^- were found in the peatland and in the inner bay. These results suggested that N_2O was […]"

These correlations give a hint towards nitrification, as was stated in line 734, but we did not want to explore this further in this study as the field work was not designed to address this question.

Figure D1: Cross plot correlations of the measured nutrient concentrations for the peatland and the inner bay.

(please see the supplement for D1)





- **For greenhouse gas part, vegetation could play an important role in regulating GHG flux. Did the chamber measurements cover some typical communities? This is worth mentioning if there are any patterns and variations associated with GHG fluxes. And also dead vascular plant can still affect GHG emissions through their hollow stem, thus this has to be considered as possible factors driving seasonal variations. But the authors have missed all this.**

Reply:

It is right that the vegetation can play an important role in regulating GHG fluxes and the variability before and after rewetting. Before rewetting, chamber measurements covered the respiration rates of the grassland communities as net fluxes and hence, included all C exchanged by the plant communities. This indirectly accounts for the patterns and variations of all communities and dead vascular plant material. After rewetting, the individual contribution of vascular plant vegetation and dead hollow stems was not in the scope of this study, but we assume a negligible influence due to negligible stands of macrophytes (see line 579). Consequently, we attribute the primary production mainly to the water column. To make that more visible, we have already suggested an adaptation based on a comment of reviewer 1 and that means we will add the following:
“[…] It is noteworthy that in the first months after rewetting, former grassland and ditch vegetation (Elymus repens L. (Gould) (Couch grass), Phragmites australis (Cav.) Trin. ex Steud. (Common reed)) died almost completely and the cover of emergent macrophytes was then negligible.”
Consequently, we cannot provide a detailed analysis of the species and the corresponding contribution, since this was not in the scope of the study, because we were more interested in the general effects of the rewetting. However, it is likely that these stands will expand in the future and contribute stronger to GHG dynamics. Therefore, they should be considered in upcoming investigations. We also state this now in reaction to a comment of reviewer 1.

- **Another issue is flux estimates. I understand the authors have tried both the chamber measurements and the Wanninkhof equation to estimate the flux, however, both of them could generate large uncertainties. Especially for the Wanninkhof equation, it was developed for air-sea exchange in open ocean and it is barely valid when wind speed is below 4 m/s. In peatland, it does not make much sense to compare these two methods. But the large variation range and their influence on evaluating peatland as GHG source have to be further discussed.**

Reply:

The comment in particular on the caveats of using an open ocean wind speed based ASE for the Drammendorf site is fully justified. We were aware of the issue when processing the data. In this study, one focus was on GHG response to rewetting with brackish water. In order to obtain high spatial as well as temporal resolution measurements, we used two different methods with their different technical advantages and disadvantages to capture this rewetting process.

After rewetting, we evaluated the comparability of the two methods by comparing the results of a representative station (BTD7) for each post-rewetting season and we did not find significant differences between the fluxes derived with the different methods (Figure 9). Although both methods showed high variability, especially in summer and autumn, the comparison showed reasonable agreement. However, in order to obtain a trend of the GHG response to rewetting, we decided to pool the data from both methods after rewetting to obtain substantial benefits on the coverage and to get a comprehensible story, while we have the limitations in mind. It was actually the fact that both methods were so nicely in agreement at the site covered by both methods that encouraged this approach. Please note that this comparison of station BTD7 will be moved to the Appendix due to reviewer 1's suggestion.

The main advantage of pooling the flux data from both methods is to create a more representative post-rewetting data set by augmenting the spatially limited chamber flux measurements with the data set derived from surface water measurements (Wanninkhof/k-model), which had far higher spatial resolution (Figure 2). Since we expected a large spatial heterogeneity typical for shallow coastal regions, which we can show in Figure 8, we believe that the data of the chamber measurements should be expanded with the higher spatial resolution measurements based on the discrete water samples. Because chamber measurements are much more challenging than discrete water sampling, we were not able to carry out the chamber measurements with high spatial resolution in this logistically very demanding environment. A combination of both methods is therefore a possible way to obtain robust spatial resolution and was recommended in another study by Lundevall-Zara et al. (2021) with the limitation of high uncertainties and variability.

The Wanninkhof equation is controversial when applied to peatlands. However, we considered it suitable because, to our knowledge, there is no adapted equation for calculating peatland fluxes. Furthermore, field conditions probably had a greater influence than the choice of the k-model. For instance, water column mixing vastly contributes to the flux estimate of k-model approaches (Erkkilä et al., 2018) which was pronounced in our study site and likely resulted in direct sediment (degraded peat soil)-water interactions due to the very shallow water in the peatland. Moreover, besides the wind, the water-side convection at a seasonal scale is a relevant parameter in controlling air-sea gas exchange as was shown for marginal seas and coastal areas (Gutierrez-Loza et al., 2021). Therefore, the GHG fluxes from this study are hardly suitable for upscaling and have to be supported by e.g. eddy covariance measurements in the future (e.g. Erkkilä et al., 2018). To make that more visible, we plan to add the following statement in line 778:

"It is worth mentioning that due to the large variability and the pooling of chamber-based measurements with k model data, the GHG fluxes after rewetting are hardly suitable for upscaling and thus, the raw data should be used."

To discuss variability, we calculated GHG fluxes after rewetting by considering three different measurement methods: (1) chamber at transect, (2) k model at transect, and (3) k model within the area (see table below, right side). (In the manuscript, we showed the average of these three ways of measurements, to keep it more comprehensible). The table shows mean values from summer and autumn each for pre- and post-rewetting conditions. It is obvious that the post-CO_2 fluxes show high variability for both methods but comparable mean values. Variability is likely due to heterogeneity among transect stations as well as area stations. After flooding, the transect stations represent a water level gradient where some stations can fall dry and others are permanently flooded. To make that feature clearer, we will add the following in line 198:

"After rewetting, the transect formed a gradient of stations along varying ground elevations that fell dry depending on the water level and stations that were permanently flooded. [Atmospheric GHG fluxes were ...]".

For CH_4, we observed a high variability within the chamber measurements and higher values compared to the k-model. This is probably due to the influence of bubble-mediated transport, as described in line 711, and may have been also due to very shallow stations (several cm water depth).

Table for the Supplement: Calculated GHG fluxes after rewetting by considering three different measurement methods: (1) chamber at transect, (2) k model at transect, and (3) k model within the area.

(please see the supplement for the table)

		Pre-rewetting flux		Post-rewetting flux	
	location	method	GHG flux	method	GHG flux
CO_2 ($g\ m^{-2}\ h^{-1}$)	formerly dry	chamber - transect:	0.29 ± 0.82	chamber - transect:	0.20 ± 0.26
				k model - transect	0.47 ± 0.36
				k model - area:	0.28 ± 0.26
	ditch	chamber - transect:	0.28 ± 0.13	chamber - transect:	0.26 ± 0.23
				k model - transect:	0.48 ± 0.34
CH_4 ($mg\ m^{-2}\ h^{-1}$)	formerly dry	chamber - transect:	0.13 ± 1.01	chamber - transect:	2.33 ± 9.70
				k model - transect:	0.65 ± 0.40
				k model - area:	1.00 ± 0.70
	ditch	chamber - transect:	11.37 ± 37.54	chamber - transect:	11.33 ± 30.87
				k model - transect:	0.64 ± 0.33

Additional changes not related to the reviewers comment:

- Line 224: adding two sentences - "All nutrient concentrations below detection limit were not considered for further evaluation but can be found in the published data set. Therefore, nutrient concentrations of our study site and of the monitoring station are partly overestimated."
- Line 469: mistake in Figure 9 - "sampling season" will change into "sampling method"
- Addition of a person in the acknowledgments who helped with the statistics