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Reply on RC2

Daniel L. Pönisch et al.

Author comment on "Nutrient release and flux dynamics of CO_2, CH_4, and N_2O in a coastal peatland driven by actively induced rewetting with brackish water from the Baltic Sea" by Daniel L. Pönisch et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-117-AC2, 2022

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.

- **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 Figure 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)

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

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2022-117/bg-2022-117-AC2-supplement.pdf