

Biogeosciences Discuss., referee comment RC1  
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## Comment on bg-2022-117

Anonymous Referee #1

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Referee comment on "Nutrient release and flux dynamics of CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O in a coastal peatland driven by actively induced rewetting with brackish water from the Baltic Sea" by Daniel Lars Pönisch et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-117-RC1>, 2022

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Poenisch et al. investigate the impact of rewetting on nutrient and greenhouse gas exchange in a brackish coastal wetland with a history of agricultural use. They sampled surface water in the rewetted area, compare concentrations with available long-term monitoring in the adjacent bay and estimate lateral exchange rates based on a hypsometric curve. In addition, they measure CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O exchange in the peatland as well in the aquatic systems.

The authors capture an important transition phase, because they were able to sample prior, through and immediately after rewetting, an event with strong impact on biogeochemical cycling. Thus, the manuscript is well in the scope of Biogeosciences. However, I find especially the discussion somewhat descriptive and would like to see a more thorough statistical analysis. The authors have collected a lot of data across different environments and time periods, and sometimes that makes it hard to follow the main story

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.

- 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  $Q_{in}$  is a positive flux and  $Q_{out}$  negative (equations 2 and 3). However, in equations 4 and 5, this seems to have been flipped:  $Q_{in}$  is explicitly a negative flux, and presumably  $Q_{out}$  is positive, although that is not clearly defined. This reverse step seems unnecessary and potentially confusing to me.

- 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.

- 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.

- Peatland CO<sub>2</sub> 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<sub>2</sub> fluxes (more directly on the vegetation itself) is not analyzed or discussed at all. Are the CO<sub>2</sub> 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?

#### 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.

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

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.

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 iron sulfides). 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.

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

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

description.

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?

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.

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<sub>2</sub> fluxes). If the group intends to continue with these measurements on site, it is worthwhile to say that.