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Comment on bg-2021-65

Anonymous Referee #2

Referee comment on "Effects of peatland management on aquatic carbon concentrations and fluxes" by Amy E. Pickard et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-65-RC2, 2021

Report to MS bg-2021-65:

Effects of Peatland Management on Aquatic Carbon Concentrations and Fluxes by Amy Elizabeth Pickard, Marcella Branagan, Mike Billett, Roxane Andersen, and Kerry Jane Dinsmore

The manuscript of Pickard et al. focuses on the fluvial carbon export from disturbed and restored wetlands. As this carbon flux is a substantial factor in the carbon balance, they monitored dissolved organic and inorganic carbon as well as CO_2 and CH_4 in peatland draining streams over a two year period. The manuscript is well written and the generated data set is worth to be published. However, the presented study shows some major limitations, which has been addressed before in a pre-review and were not considered in a minor manuscript revision by the authors beforehand. Major concerns still are: 1) unclear way of calculation of carbon fluxes on all six monitoring sites, when only three were equipped with pressure transducer for water level recording; 2) reliability and clarity of the DIC and CO_2 data 3) lack of data discussion and scientific hypotheses. These major points will be further addressed below. Especially point 2) raises my concerns and needs to be edited properly.

1) The description of p8 L151-156 is still not entirely clear to me. Do you conducted discharge measuring by dilution experiments on all six sites and correlated it to the three stage measurements? This will give you an error in the flux and FWM concentration data,

which should be addressed somewhere.

2) I can just repeat former questions and comments. If I understand correctly, DIC has been measured along with the DOC concentrations on filtered samples, which were stored up to 4 weeks! During this handling and storage, a lot happens to DIC, which is in equilibrium with CO_2 in the atmosphere. Changes in pH, outgassing of CO_2 and production in a non-sterile sample is most likely. I do not think that this kind of data meets scientific quality standards. Coming to measured CO_2 concentrations, a dependency of CO_2 speciation on pH in water samples is completely neglected. You state that you measured pH at each sampling. Why don't you use this information? Even more unclear to me is why you separate these two parameters, as normally CO_2 (as calculated by Henrys Law) in solution is a major part of DIC under low pH (as this is probably the case in these catchments). Having pH and CO_2 concentrations you can also calculate the entire DIC in the water sample. This might be more correct than the DIC measurements or could be used for validation. Additionally, in the results part these two measured parameters - CO_2 and DIC - were summed up in the carbon export calculations. This is simply not correct as you double the CO_2 contribution then. In the end, I don't see any sense in comparing CO_2 concentrations at different sites when the pH is not considered.

3) There are some shortcomings in the scientific significance. The study presents a good data set, but is mainly descriptive and clear conclusions or benefits from the study are not well stressed. No hypotheses raised. The relevance of different carbon species is not explained in the Introduction. Therefore, the research question how different carbon species vary and why it is important to measure them is not introduced. Moreover, the importance of DOC is highlighted before.

The discussion needs improvement and mainly cites literature from the UK. Maybe it would be helpful to additionally compare the study to international studies on rather natural sites, where more literature can be found? Another helpful publication might be:

Swenson et al. 2019: Carbon balance of a restored and cutover raised bog: implications for restoration and comparison to global trends, Biogeosciences 16 (3), p 713–731 DOI: 10.5194/bg-16-713-2019

At last I would like to read some statements why it is reasonable to compare different carbon concentrations? As you cannot draw conclusions on the carbon balance or losses from the peatland it is probably because of water quality issues? What conclusions can be drawn from it?

Some specific comments:

Fig 2. Tab. 2 and Fig 3 show all more or less the same data. Maybe you can reduce redundancy. The same goes for Table 5 and Figure 6.

P7 L 135-136: I am confused by the phrase "…affected by artificial drainage **alone**,..." I am no native speaker. Does this mean that that the non-drained catchments formerly has been drained and are additionally affected by other disturbances? Maybe you can clarify/rephrase this.

P12, L214ff: As CO_2 concentrations are greatly dependent on pH this should be considered here. Moreover, it would be nice to state if the water is supersaturated and outgassing prevails?

P 13 L233: link to "Figure 5" seems to be wrong

P 19 L302ff: "… with the second most important export component DIC followed by CO_2" This makes no sense.

P23 L414: see comment above