Review of “Enhanced CO2 Emissions Driven by Flooding in a Simulation of Palsa Degradation”

General comments:

The study presented here provides meaningful insights in the role of water and peat quality upon permafrost thaw, which is I highly relevant and recent research topic. BG seems to be an appropriate journal for the publication of this study.

Mesocosm incubations are a robust and established method, although the simulation of permafrost thaw via partial freeing and thawing is technically demanding and therefore only few studies exist, which the authors also correctly emphasize. The study is therefore addressing a recent and relevant research gap. The authors describe their experiments clear and concise, although open questions regarding the choice of water level, experimental time frame and data filtering remain, which are more specifically addressed in the specific comments. Briefly, the assumption of a certain water level that stays constant over the chosen time period is realistic but just one of many scenarios. Therefore, the manuscript would profit from a better justification of why these parameters were chosen the way they are. An important issue is the filtering of data: The authors state that due to saturation of the sensor, no CO2 values greater than 5.000 ppm could be accurately measured and were filtered out. However, the presentation of the results does not allow to understand the extent of this filtering and how it affects the overall results and statistics. Furthermore, the manuscript lacks any hypotheses and only a general aim is stated, which should be clarified. The conclusion section mentions an initial hypothesis which is contradicted by the results but there is no such hypothesis stated in the introduction. Generally, the objectives are rather short and it seems like this study was performed more explorative rather than having clear expectations or hypotheses. The actual difference between “abrupt” and “gradual” thaw needs also more explanation, since it is not very clear how fast abrupt thawing is defined in this study.

Overall, the paper needs some clarification and justifications but seems to be suitable for publication after addressing the concerns mentioned above and in the specific comments. The title and abstract are appropriate and the overall language and presentation are well chosen. The authors take the recent literature into account and summarize it sufficiently to understand the research gaps and limitations of the methodology.

Specific comments:

L 29f: Please specify the kinds of changes (e.g. how will the vegetation change, will it become wetter or drier, etc.)

L 42ff: What is meant by “C production”? Shouldn’t it be gas production? Also, consider changing “C decomposition” to “OM decomposition”

L 100: What was sampled in October? Or was it just an exploratory visit in order to map vegetation and active layer depth?

L 110: Please specify how the corer was modified.

L 131ff: How fast was that abrupt thaw?

L 173ff: Fluxes will be underestimated when all flux data > 5000 ppm CO_2 is filtered out. With this in mind, results can still be interpreted in some way but it would be helpful to have information about the timing of these extreme values. Where they equally distributed throughout the experiment or did this problem occur only during a specific time frame? This information could be included in a graphic like figure 5 or figure S14 – S18

L 240ff: That was already explained in section 2.5.1.

L 249ff: Did you check beforehand whether the criteria for the tests (normality, homogeneity in variance, etc.) were fulfilled?

L 347f: Can you provide a rough estimate of how much higher your emissions are compared to other studies?

L 361: It would be good to have the measured and typical pH values stated here.

L 364: Did you also measure DOC after your incubation experiment? It would be interesting to see some kind of mass balance of OC over the incubation to get an idea of decomposition pathways, e.g. to see how much solid OC and DOC are transferred to gases and vice-versa.

L 381: Reads like there is a big bias caused by the dimension of the samples? Would a real-world scenario then maybe never reach anaerobic conditions because the soil dries or refreezes before?

L 391ff: I agree with the general concept of carbon release upon Fe(III) reduction and that this mechanisms can (partly) explain the results found here. However, I question that palsas are always Fe-rich. The cited work was a case-study and it would be good to see some kind of comparison of the both sites in terms of palsa formation, underlaying geology, etc. The addition of water indeed hampers oxygen availability but before Fe is reduced, other TEAs (NO_3, MnO_2) are used, which also needs some time. Since this study did not find anaerobic conditions immediately, couldn't it be that Fe is still not reduced?

L 484: How realistic is this abrupt thaw scenario? Since it was not stated how long it takes under that scenario, it is hard to estimate whether this is just a theoretical scenario or realistic in permafrost regions.

Technical corrections:

L 1: Remove comma

L 44: Is Baysinger still in prep? This study is cited quite often here, which is a bit unfortune when it is still in preparation .

L 247: Parenthesis before the phrase is not necessary

L. 256: R Core team not in literature list

L 260: Check the wording. "Deepest value" is slightly confusing since it also seems to be the lowest value? Do you mean the sample at the bottom of the core?

L 478: Missing space between "C" and "transport"