

**Review of manuscript “Effects of nitrogen and phosphorus amendments on CO<sub>2</sub> and CH<sub>4</sub> production rates in peat soils of Scotty Creek, Northwest Territories, Canada: exploratory incubation results highlight a potential impact of wildfires and permafrost thaw on peatland carbon exchanges” by Eunji Byun et al.**

I enjoyed reading the manuscript and I found the study well designed and the manuscript overall written well and definitely worthy consideration for publication. It still, nevertheless, requires some careful revision and (sometimes) restructuring, to better lead the readers through to complex methodology and comprehensive (hence, also confusing) results and especially discussion. I recommend the manuscript for publication, after a further revision, and I encourage the authors to complete the great task they started so well. Please see my more detailed comments below:

**Title:** I find is somewhat long, even though I understand it was corrected based on previous comments. Nevertheless, it is strikingly informative, so I suppose it is mostly for the editor and the authors to decide if to leave it so long or maybe cut the last part following the colon.

**Abstract:** overall well written, well summarises the project and presents its aims. The only sentence I would recommend to reconsider is the last one – all our studies are not final, and more research can always help, but I doubt this is the only strong conclusion you can make out of this experiment. I can see that in the previous round of reviews you received some harsh criticism, which I found unmerited. Your findings have an important value and should have a corresponding concluding sentence in the end of the abstract.

**Introduction:** overall good, I only have one comment related to Table 1 and the style of presenting the references

Line 45. I would contest the need of the Table 1 to prove your point here, and I would recommend moving the table to supplementary materials. While the table shows important examples of nutrient enrichment after wildfires, it is slightly out of scope for your study’s objectives, and I doubt you must have it there, in the main text, to justify your experiment. You are doing an excellent justification with your introduction text already. I would rather focus on more clear presentation of the references for the first sentences of this paragraph.

This comment is applicable for the entire text – the reader is often left with lengthy stretches of text without references, followed with a bunch of references in one place (e.g., Emelko et al., 2016; Van Beest et al., 2019; Emmerton et al., 2020; Orlova et al., 2020). In such cases, readers might be left confounded – do all these mentioned studies prove one statement (as it is in line 50; in which case, it is not necessary to show that many references, 1-3 is usually enough) or all 3 to 5 statements before that (as in lines 45, 80, etc... in which case, it would be beneficial to attribute the references to their corresponding statements).

**Methods.** It was detailed enough and mostly easy to follow. Adding a graphic summary or a table pointing to each step done before and during the experiment would greatly help readers, but I would not insist on adding one, it is just an idea.

**Section 2.1.** I recommend adding some essential information that would help readers to make better interpretations of the results. Firstly, you mention that the study site is within discontinuous permafrost area. It would be important to indicate whether the two peatlands you study are affected by permafrost, and most importantly, whether the coring sites are underlain by permafrost. Secondly, you sampled in October, so it would be worth mentioning if the peat samples were already frozen when you sampled, and if they remained frozen during the travel to Waterloo.

**Section 2.2. Line 121.** Was the bulk density used anywhere in the study? Judging from the method you used to estimate it, it might not be the best variable to present. As I did not find any further mentions of density in the manuscript, I would recommend removing it from the methods as well – it slightly reduces the quality of the study, when mentioned like that. But if you used density somewhere else, then keep it.

**Results and Discussion.** I would recommend a detailed revision of this part. You have a good study design and diverse data to present. The latter often causes difficulties, when writing a clear discussion, and this was the main issue I identified while reading your manuscript. If it helps, you may consider separating results from discussion; otherwise, please carefully revise each section, and make sure you do not limit yourself to commenting on each particular detail of presented results but also expand into a larger context. In many cases, a presentation of what is normally expected for different variables would make the discussion clearer (not everyone reading the article will be as knowledgeable in all the domains you touch). Overall, I missed a discussion on the meaning of your findings – what did we learn from this, where do we go here? May we expect similar wildfire effects in other subarctic wetlands: what will be the effect of permafrost disappearance, or increasing peatland ground temperatures? Please, do not get discouraged by the previous reviewer's comments about your experiment being unrepresentative. While it is admirable that you carefully acknowledge the limitations of the study, we often must work with what we have, and careful interpretations (based on similar studies) are acceptable. P.S., fens and bogs are highly representative for boreal regions and comparing these two is a significant contribution to the scientific knowledge, even if you work with only one particular study site. Boreal/subarctic regions are strikingly similar across North America and Eurasia.

I am aware that you have submitted for a short communication, so my preceding comments might overcharge the paper. In that case I would strongly recommend revising which results you want to present (and discuss) in detail, and which could be left for a supplementary information.

**Section 3.1.** I have missed a broader discussion in this part (or did you mean to have this section as strictly results only?). You mentioned that fen peat had higher initial microbial biomass, but you may go further than that. What other differences in peat properties (decomposition level, density, organic matter and water quantities, ect) or the context of the peatlands itself? Maybe permafrost has some impact, maybe climatic conditions? You present a lot of important and interesting information in this section, but you do not comment more on it. Maybe more perspective globally or within the subarctic peatlands? Why would temperature and nutrient additions/limitations affect CO<sub>2</sub> and CH<sub>4</sub> production differently?

Overall, I recommend considering connecting this section with 3.2, in which you present same information, only different units. But the missing discussion questions remain.

**Section 3.3. Lines 283-285.** I am not sure I follow your interpretation here. CO<sub>2</sub> and CH<sub>4</sub> are, in large, produced by different mechanisms and different microorganisms. Combining the production of both

these gases does not necessarily better explain the nutrient effects, but maybe a better explanation of what you meant could suffice. Nevertheless, I would recommend discussing each gas separately, taking into account different needs of methanogens and other heterotrophic microbes; also, please consider a potential effect of gas consumption by other microorganisms (e.g., methanotrophs) or chemical reactions.

**Line 294** “It is difficult to imagine that the observed CO<sub>2</sub> and CH<sub>4</sub> production throughout the incubation period could have occurred without microbes in fen soil samples”. I understand you added this sentence following comments from previous reviewers, but I find it slightly excessive, and I would recommend removing it. The remaining explanation, as to why you do not present the results from fen, is sufficient. Alternatively, I would recommend reconsidering whether you need to present the microbial biomass changes at all (maybe move Table 3 into supplementary material?). The experiment results are informative and interesting without this part; moreover, it becomes confusing since you cannot compare fen with bog, as you do in other instances.

**Line 313.** By “the use of solid organic particles”, do you mean the microbial use, or the continuous leaching of DOC from the particles you have. I recommend considering (and clearly naming) both options.