Referee #1

Our responses are in red in the text below.

General Comments:

The paper outlines an interesting study of peatland pools in Canada. They combine a litterbag decomposition experiment with lab-based incubations to understand the drivers of OM decomposition and GHG emissions from peat sediments. Peatland pools are under-studied – this paper is a significant contribution to research on these interesting environments. The justification for the research is clear, and the methods are well described. The results are sometimes a bit unclear, but there are some really interesting results hidden in there. The implications for C emissions and climate need to be more explicit.

Thank you for the thoughtful and constructive review. Below we provide a point by point answer to the concerns raised by the Reviewer, including better expliciting the implications for C emissions and climate.

Abstract:

As it is a complex experiment, there is a lot of information in the abstract, which makes it a bit more difficult to read. However, it generally outlines the research well.

The abstract is indeed dense because our article combines two complementary experiments. We nonetheless agree that it can be difficult to read and will be restructuring the abstract to separate the results of both components of the study more clearly.

Introduction:

The introduction outlines existing research briefly and succinctly. There are a few papers on temperate peatland pools that missing from this summary of past research (relating to pools in the UK Forsinard Flows, and small ditches in Sweden) that could be included to support biogeochemistry findings (especially re: depth, DOC and dissolved CO_2 in pools).

We are aware that several studies conducted in the UK and Sweden have looked at peatland water bodies. However, we omitted to refer to most of them because they were conducted in different environmental settings (e.g. disturbed or restored peatlands, blanket bogs) than our undisturbed raised bog. Previous studies have shown that differences in environmental conditions influence the biogeochemistry of peatland pools (especially C cycling) and thus the decomposition processes occurring within (e.g. Chapman et al. 2022; Arsenault et al. 2023). We therefore focused the introduction on peatland pools of similar context.

We will address this comparison issue in the introduction to better contextualize our research settings.

Chapman, P. J., C.S. Moody, T.E. Turner, R. McKenzie, K.J. Dinsmore, A.J. Baird, M.F. Billett, R. Andersen, F. Leith and J. Holden 2022. Carbon concentrations in natural and restoration pools in blanket peatlands. *Hydrological Processes* 36:e14520.

Arsenault, J., J. Talbot, L.E. Brown, M. Helbig, J. Holden, J. Hoyos-Santillan, E. Jolin, R. Mackenzie, K. Martinez-Cruz, A. Sepulveda-Jauregui and J.F. Lapierre 2023. Climate-driven spatial and temporal patterns in peatland pool biogeochemistry. *Global Change Biology* 29: 4056-4068.

Methods:

Figure 1 is good, it's a clear and concise explanation of the experimental design.

Thank you.

Typha latifolia isn't found at the site, why did you choose it for the litter bags? Was it because it isn't found there naturally? It would be good to include a justification for this.

Typha latifolia was chosen for several reasons, that we will synthesize in the methodology section of the paper:

It can be cut into consistent, standard lengths and breadths and it is easy to remove material brought into the litterbags during extraction (it can be tedious on softer materials).

It decomposes moderately quickly (for wetland vegetation) so we were assured of there being a reasonable mass loss during the duration, *cf* Sphagnum which we knew would decompose slowly and may show no significant differences with emplacement location.

One author has used *Typha latifolia* from the same source in a variety of wetland locations as a 'standard' litter, providing an assessment of the 'quality' of the environment for decomposition. Thus, we can compare the 'decomposability potential of the environment' against, for example, Canadian bogs and fens, New Zealand peatlands and wetlands in Ontario and Manitoba:

Moore, T.R., J.L. Bubier and L.A. Bledzki 2007. Litter decomposition in temperate peatlands: the effect of substrate and site. *Ecosystems* 10: 949-963.

Clarkson, B.R., T.R. Moore, N.B. Fitzgerald, D. Thornburrow, C.H. Watts and S. Miller 2014. Water table regime regulates litter decomposition in restiad peatlands, New Zealand. *Ecosystems* 17: 317-326.

Dong, D., P. Badiou, T.R. Moore, C. von Sperber 2024. Litter decomposition and nutrient dynamics of four macrophytes in intact and restored freshwater marshes of Canada. *Restoration Ecology*.

Do the measurements are line 104-105 mean that you did a site survey of 158 of the 600 pools?

Yes. Since 2015, we indeed have measured 158 of the ~600 pools found at the site. Temporal surveys showed little changes in water-level (only several centimeters over the course of a growing season; Arsenault et al., 2019) and satellite photos show no discernible changes in pool extent over time. We are then confident that both depth and area did not change much since the first pools were measured. To avoid any further question on the number of pools we surveyed, we will specify this in the methodology section of the paper.

Arsenault, J., J. Talbot, T.R. Moore, M.P. Beauvais, J. Franssen and N.T. Roulet 2019. The spatial heterogeneity of vegetation, hydrology and water chemistry in a peatland with open-water pools. *Ecosystems* 22: 1352-1367.

Why were the litterbags collected multiple times during the experiment? The paper focusses in those collected at the end of the experiment – did the ones collected earlier not show any interesting results?

The litterbags were collected several times to provide an indication of the times during the year when decomposition occurs and to provide data to estimate the exponential decay parameters. We therefore used data (litter weight loss after X months) from each of the five times we collected litterbags. We will specify this in the last paragraph of section 2.2.

At line 130, do you mean ‘for up to 27 months’ here? Or were molecular composition analyses only done on litter from the final sampling occasion?

As stated, only the initial samples and those exposed for 27 months were analyzed. We proceeded that way because we were mostly interested in how litter changed after three summers in the pools rather than the temporal evolution of litter chemistry. The in-between samples (4, 11, 16 and 23 months after installation) were used to better estimate mass loss parameters, but not for quality changes. We will rewrite parts of the last paragraph of section 2.2 to avoid any confusion.

What does the ‘t’ stand for at line 172?

At line 172, ‘t’ stands for ‘tons’. We will specify it in the text.

Results:

3.1.1. Figure 2 and Table 1 show similar results – does Table 1 add anything to the story that isn't covered by Figure 2? The values for intercept, %MR and r2 aren't discussed in the text, maybe they could go in SI?

We have also pondered whether we kept it in the main text or SI, and decided that although Table 1 would probably fit well in Supplementary information, we believe it provides the background to the rather simple Figure 2, and the variability as shown by the standard error, which would complicate Figure 2.

3.1.2. At line 255, you say there is a distinctive pattern, and then say there is no detectable change – and then say there was 106% and 98% (which I would say is a change, even if it's a small one). This gives mixed signals.

There is a distinctive change in the overall litter chemistry, but we feel that the range of C concentration at the end of the decomposition period was between 98 and 106% of that initial concentration is not worthy of comment, compared to the changes in N and P. We will clarify this in the revised version of the paper.

The results in this section are very interesting.

Thank you.

Line 267: 'regardless of the pool' – does this mean pool depth, or pool number/location?

We meant 'pool number/location'. To avoid any confusion, we will rephrase to : "In all six pools, sSpearman correlations showed that C/N ($\rho = 0.50$, $P = 0.002$) and N/P ($\rho = -0.59$, $P < 0.001$) ratios in both *T. latifolia* and *S. capillifolium* respectively increased and decreased with depth of incubation."

The caption of Figure 3 was difficult to interpret, where you refer to pool depth category, and then have 'regardless of depth of incubation'. I see why you have used that wording, but is there a way to make it clearer which 'depth' is which?

We will rephrase in the revised ms to clarify what we mean, by changing "regardless of depth of incubation" to "regardless of position of incubation within the pools"

In Figure 3, can you include the number of samples that each box represents? (e.g. n=2)

We will include the number of samples in the revised ms.

Line 280: HI – can you remind what the different HI ratios mean here?

We will do as suggested.

When you say 'increased over time', do you mean between initial and final weight, or for all litterbag retrievals between the beginning and end?

We mean differences between the initial litter chemistry and that of the material after 27 months as we did not analyze the chemistry of the litter at the other retrieving times. This should be clearer after clarifying the earlier comment on the confusion among incubation times.

3.1.3. Are the results in Figure 4A the same as those in Figure 2? The k values at different depths? Figure 4 shows a lot of information very clearly.

Thank you. Yes, the k values are indeed the same for both figures, but the messages are different: in Figure 2 we wanted to clearly differentiate decomposition rates among pools and incubation positions while in Figure 4A, we wanted to show the statistical relationship between incubation depth and k values. We therefore believe both figures are relevant and should be kept.

3.1.4. The second sentence in this section is hard to understand. Can you re-word it?

We will rephrase to: "Pools located in the treed area generally had higher DOC concentration (P = 0.007), higher SUVA (P = 0.044) and lower pH (P = 0.002) than pools of the open area (Table 2), although tests showed little evidence of differences in pool depth between both areas (Kruskal-Wallis; P = 0.967)" in the revised ms to clarify what we mean.

3.2.1. The sentence at line 320 makes it sound like the P concentrations were higher in the pool rather than the sediments.

This sentence is indeed confusing. We will rephrase in the revised ms to clarify that we are specifically talking about sediment chemistry, not pool water.

3.2.2. No attempt to explain the results or PCA here.

We will describe and explain more thoroughly the results presented in this section, especially those shown by the PCA, in the revised ms.

The lowest value of CH_4 production in Table 3 is -0.6, but in the sentence at line 323, you write the range as between '-0.03 and 123 ug CH_4'

Indeed. This is a typo that will be corrected in the revised ms.

3.3. You write that 'spatial patterns emerged' but then end this section saying 'there seemed to be little relationship between CO_2 and CH_4 production rates….' – so what are the spatial patterns?

We meant that pools of different morphology and location cluster differently when comparing CH_4/CO_2 production from decomposing sediments and k values of decomposing fresh litter (Fig. 7). But when comparing all pairs of variables (k vs CO_2 and k vs CH_4, regardless of pool morphology and location), there was indeed little correlation between k and GHG production. We agree that this paragraph is confusing and we will rephrase in the revised ms to clarify what we mean.

Figure 5 caption – '…of the six studied pools (G1 to G5)…' what about pool G6? Also please check you are referring to the correct graphs/gases in the text (especially at line 331). I don't think you refer to Figure 5A in the results text at all.

There is indeed a typo in the Figure 5 caption that will be corrected in the revised ms. We will also carefully check that we refer to the correct figures and results in the text.

Discussion:

4.1. The sentence at line 407 needs a bit more explaining please.

We agree with this suggestion and will develop our hypothesis that "the effects pools have on the C balance of the peatlands in which they develop may then be larger than currently expected" in the revised ms.

This is also a great opportunity to explicitly discuss the implications our findings have on our comprehension of pools' role in peatland C emissions and how climate change may affect those, as suggested by the referee in his general comment.

4.2. This section is clear and concise.

Thank you.

4.3. The header of this section sounds the same as section 4.1.

We respectfully disagree with this comment and do not think that the headers sound the same. We first (4.1) discuss controls of fresh litter decomposition, then (4.2) we discuss controls on pool sediment decomposition, and finally (4.3) we discuss what may drive decomposition processes in peatland pools, based on what was presented in 4.1 and 4.2.

In section 4.1. you state that O_2 concentration, light and temperature are drivers of litter decomposition in peat pools, whereas in this section you talk about P content of the sediment.

In section 4.1, we indeed discuss the influence of dissolved oxygen, light and temperature on fresh litter decomposition processes and, in Section 4.3, we point to potential chemical and physical drivers of both fresh litter and sediments decomposition in peatland pools.

The result at line 465 is very interesting and wasn't mentioned earlier in the study (or wasn't highlighted as much as it could be).

Thank you for this comment. We will highlight this finding in the revised ms.

Supplementary information:

Tables S1-S4 could be condensed into one table, just showing p values and post-hoc test results for each test? I don't think that knowing the degrees of freedom, sum of squares and mean square of each test adds much.

Figure S1 repeats the C/N and N/P ratio graphs that are in Figure 3 – probably unnecessary.

We respectfully disagree with the referee's comment on the content of Supplementary information. While the tables and Figure S1 may not add much to the paper itself, we believe that some readers may find those interesting like we would. Given that they are in SI, we prefer to keep that as is.

Text edits with line numbers:

Line 15: The sentence is long and contains a lot of information, could it be rewritten as two sentences to make it clearer?

To answer a previous comment made by this referee about the abstract, we will restructure the abstract to separate the results of both components of the study more clearly. This way, long sentences will be shorter.

Line 71: hence and hence and hence – too many hences.

There were indeed several 'hences'. We will be rephrasing these sentences.

Line 224: '… was more degraded than…'

We will change 'degradable' to 'degraded'.