

Referee # 2

Our responses are in red in the text below.

The study assesses spatial variation in organic matter (OM) decomposition rates in peatland pools at a temperate peatland site. The authors also evaluated the influence of OM chemistry on decomposition rates of fresh litter in situ and on sediments ex situ. The authors' finding that decomposition rates vary spatially in peatland pools is highly relevant for assessments of peatland greenhouse gas (GHG) emissions and the paper will be of interest to the readership of Biogeosciences.

Thank you for the thoughtful and constructive review. Here we provide a point by point answer to the concerns you have raised.

The research approach and methods are well-designed overall, however the authors should justify the decision to oven-dry litter samples for the in situ decomposition experiment, and compare decomposition rates of litter to other studies in the discussion.

The litters were oven dried at 40°C to ensure that a uniform residual moisture content existed, particularly for *Sphagnum* which is difficult to dry uniformly and that desiccation would discourage *Sphagnum* from regrowing. We will specify this in the revised ms.

The comparison with decomposition rates of other litters and locations is valid as they are the same species and are treated in the same way. Regarding comparison with other studies, we will address this in the revised ms. We present below some comparisons as an answer to another question raised by the referee.

The threshold for statistical significance should be clarified in the methods.

The significance level we used is $P < 0.05$ (except in Table 4, where we used either $P < 0.05$ or $P < 0.1$ but this is specified). However, we refrained from using the terms 'significant' in most of the text (except in three occasions where P-values were > 0.2) and rather chose to give the reader every P -value and, ultimately, the opportunity to interpret the significance of each statistic. We therefore believe it is not necessary to clarify the significance level in the methods section.

The paper presents a comprehensive dataset and the interpretation and discussion of results needs to be re-examined and strengthened in some instances. A few issues stand out:

According to the introduction OM decomposition is generally faster in pools than in surrounding soils, but in this study k values tended to be much higher in the acrotelm compared to the pools

for *T. latifolia* and k values appear to be similar for pools and the acrotelm in the case of *S. capillifolium* (Table 1). This should be discussed.

Here, we are not totally sure what the referee refers to. In the introduction, we say that pools ‘generally have a negative C balance because of distinct environmental conditions (e.g. higher oxygen – O₂ – availability and warmer temperature) compared to the surrounding soils that lead to faster OM decomposition than production’. We do not imply that decomposition is faster in the pools than in the soil, but rather that decomposition occurs faster than accumulation. To avoid any confusion, we will rephrase to: “[...] that lead to rates of OM decomposition that are faster than rates of OM production decomposition than production”.

We also believe the differences in decomposition rates for *Typha* and *Sphagnum* in the acrotelm vs. in the pools reinforce our justification to use both an easily degradable (*Typha*) and a more recalcitrant substrate (*Sphagnum*) to distinguish the effects of environmental and chemical properties on litter decomposition rates.

While the authors propose that decomposition rate in pools depends primarily on environmental conditions (i.e. depth), there was no difference in decomposition rates of *S. capillifolium* among depths (Table 1). It seems there are dual influences of environment and litter composition on decomposition rates *in situ* that the authors should consider more carefully.

Yes, there are environmental and litter quality influences at play, and a core objective of our study was to disentangle to what extent each aspect plays a role. We found that where the litter decomposition rate is ‘relatively’ fast (*Typha*), environment does play a role in differentiating rates. Where it is ‘slow’ over the short period (27 months; i.e. *Sphagnum*) patterns are not clear though they may have appeared if mass loss was larger.

The slow rates of decomposition of *S. capillifolium* in the open-pools are similar to those found in the surface of bog and fen peatlands in temperate and boreal regions (e.g. Moore and Basiliko 2006; Moore et al. 2007) and there is little differentiation of rates within the pools. The faster rates of decomposition of *T. latifolia* at the surface of the open-pools (0.10 to 0.19) is similar to rates obtained in the surface of peatlands and in freshwater marshes, where k values range from 0.14 to 0.59 (e.g. Moore et al. 2007; Dong et al. 2024). The decrease in the k value with depth of emplacement in the open-pools is also consistent with studies that placed the *T. latifolia* leaves at depth in peatlands, resulting in a strong correlation between the k value and the period of saturation derived from the water table position, declining from k values of about 0.2 where never saturated to 0.05 when continuously saturated (Moore et al. 2007; Clarkson et al. 2014). This is consistent with the open-pool decline in k value from an average of 0.15 near the water surface to an average of 0.07 in the pool sediment, suggesting that anaerobic conditions are a major control on decomposition rates of fresh organic matter in these peatlands. We will add these numbers and references to the discussion in the revised ms to better support our hypotheses.

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* 32 issue 4. doi.org/10.1111/rec.14135.

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.

Moore, T. and N. Basiliko 2006. Decomposition. Pages 126-143 in R.K. Wieder & D.H. Vitt (eds.) *Boreal Peatland Ecosystems*, Ecological Studies Vol. 188, Springer-Verlag.

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. doi:10.1007/s10021-013-9726-4.

Regarding the influence of OM chemistry on decomposition of sediments ex situ statements in the abstract and discussion contradict each other. The abstract states that CO₂ production by sediments decreases with increasing OM humification and at line 440 it is stated that "CO₂ production was positively related to an increase in OM humification". The abstract states that CH₄ production decreases with increasing N:P but this result is not significant at $p < 0.05$ (Table 4), though there is a significant correlation between CH₄ and C content as well as Na content at $p < 0.05$ (see comment above about clarification of threshold for determination of statistical significance). This section of the discussion doesn't address the potential mechanisms explaining these correlations, though an explicit aim of the study is to "assess the role OM chemistry plays on decomposition rates for litter and sediments" and an implied aim is to increase knowledge of mechanisms controlling OM decomposition in peatland pools.

Thank you for pointing to what we stated at line 440. This is clearly a typo, as shown by the results in Table 4. We will correct this mistake in the revised ms.

We agree that there are inconsistencies between our discussion and how we present results in the abstract and how to identify potential drivers of decomposition processes in peatland pools. To address this, we believe we should better highlight what we identify as correlations in the discussion (plus limitations due to small sample sizes) and, more importantly, that we only make suggestions for what could be causes for the observed relations. We will then remove less clear relations from the abstract.

It is also true that one of our main objectives is not entirely met. We will address this in the revised ms and discuss why we did not meet this objective by emphasizing the potential mechanisms that could explain the correlations our results point to by citing relevant literature. For example:

- The importance of peat quality in controlling OM decomposition, as suggested by the negative correlation between CO₂ production and humification indices and previously observed by Estop-Aragonés et al. (2022).
- A possible P limitation in our systems (as suggested by the positive correlations between C emissions and P concentration in the material; Figure 6, Table 4) rather than N limitation as observed in permafrost settings (Schädel et al. 2014).

Estop-Aragonés, C., L. Heffernan, K.H. Knorr, D. Olefeldt 2022 Limited Potential for Mineralization of Permafrost Peatland Soil Carbon Following Thermokarst: Evidence From Anoxic Incubation and Priming Experiments *JGR Biogeosciences* 127: e2022JG006910.

Schädel, C., E.A.G. Schuur, R. Bracho, B. Elberling, C. Klobaich, H. Lee, Y. Luo, G.R. Shaver, M.R. Turetsky 2014. Circumpolar assessment of permafrost C quality and its vulnerability over time using long-term incubation data *Global Change Biology* 20: 641-642.

The abstract states that differences in fresh litter and pool sediment decomposability is a function of O₂ concentrations, light, and temperature, which all decrease with increasing depth. Variation in these environmental parameters with depth are not presented in the present study but it seems they were included in a previous paper (Arsenault et al. 2018). The relationship between decomposition rates and O₂, light, and temperature should be presented in the present study to substantiate this conclusion.

This is an interesting suggestion that could greatly improve the message we are sending in the paper. Unfortunately, we only measured such parameters on 6 occasions during the 27-months period of our field work (at t=0 and then at every litter retrieving time, to be precise). Hence, we do not have a large enough sample size on which to base correlations. We must then rely on other studies, one of which (Arsenault et al. 2018) was conducted at the same study site but on different pools. We will specify this in the discussion in the revised ms and remove the mention of these specific drivers in the abstract because we did not directly measure them..

It's also not clear how sediment decomposition rate can vary with depth since the sediments were collected from the bottoms of the pools. Perhaps the authors mean overall pool depth, but the ex situ decomposition rates were highest at the intermediate pool depth, not the shallowest pools. It's interesting that the total ex situ production of CO₂ corresponds to the sediments from the pools with the highest CO₂ fluxes in situ. The authors have focused on the influence of physical factors but it seems that chemistry is also important.

We indeed meant general pool depth, this will be clarified in the revised ms.

Our incubation study shows that decomposability (or decomposition potential) is indeed higher in the intermediate pool depth group (~1 m deep) than elsewhere, but our chemical analyses of individual sediment pool samples do not point to many correlations that could explain such results (see Table 4). We also believe that sediment chemistry is important in influencing sediment decomposition, but our experiment being the first to directly study decomposition processes in peatland pools, it is thus difficult to put forward hypotheses without falling into simple speculations.

This is why we call, at line 448 of the submitted ms, “for further studies on the specific mechanisms responsible for sediment degradation”. To avoid any misunderstanding, we will also emphasize on the hypothetical nature of the interpretation we make from the results presented in Figures 5 to 7 in the revised ms.