

Anonymous Referee #2, 02 Aug 2026:

Reviewers Comment (RC): The manuscript presents a multi-proxy dataset from a lagoon system in the western Canadian Arctic, combining 65 years of shoreline-change analysis, a matched land–lagoon–marine sediment geochemistry transect, mass accumulation rates from lagoon and marine short cores, and Landsat-derived turbidity and temperature patterns under contrasting wind regimes. I particularly like the gradient approach and think this represents a valuable dataset and a generally well-argued contribution that addresses an important gap in the Arctic land-to-ocean carbon cycle literature. The combination of multiple proxies and methods across the land–ocean continuum is a clear strength of the study.

My main concerns are less related to the measurements themselves, but rather to how strongly some of the framing and conclusions are stated compared to what the current dataset can actually support. In addition, some aspects of the terminology and methodological description would benefit from further clarification to improve transparency and reproducibility. I also think that there is a missed opportunity to strengthen the main conclusions through a more quantitative assessment (mass balance , mixing model). Overall, I think these issues are addressable through moderate revision and would substantially improve the robustness and impact of an already valuable study.

Authors response (AR): We thank the reviewer for the positive and constructive assessment of our manuscript and for highlighting the value of the multi-proxy approach across the land–lagoon–ocean continuum. We appreciate the concerns regarding the strength of some interpretations, methodological transparency, and the potential for further quantitative assessment. In response, we have revised the manuscript to better distinguish observations from interpretations, clarify the scope and limitations of our dataset, improve the methodological descriptions and terminology, and refine the framing of our main conclusions. We address the reviewer's specific comments and suggestions individually below.

Main comments

RC: **Land–lagoon–ocean gradient and transport pathway.** The land–lagoon–ocean gradient framing used throughout the manuscript appears to conflate a simple spatial gradient with an actual transport pathway. The manuscript treats the terrestrial sites, lagoon short cores and marine HB cores as representing a continuous land-to-ocean transfer pathway, and geochemical proxies are interpreted along this implied gradient (Figs. 4–6). However, the authors' own description of sediment transport pathways and potential OM sources in the study area and discussion suggests that the HB site may receive sediment and organic matter from several sources that are not directly connected to lagoon export. It therefore seems that the marine HB cores are not necessarily hydrodynamically downstream of the lagoon, but rather represent a nearby marine environment. This is an important distinction and not uncommon. Land–ocean transition zones are generally complex and dynamic systems. Spatial proximity between a lagoon/estuary/river and a coastal ocean site does not necessarily demonstrate connectivity. This point requires further discussion because the reported ~50% OM decline along the gradient is one of the main results, and because the stepwise comparison of terrestrial, lagoon and marine OC pools in Section 4.3 and Table 2 relies on this framing. But the observed land-to-ocean "gradient" could simply reflect differences in OC

inputs and transport/degradation histories between sites rather than progressive OC transformation during export. I would suggest either providing stronger evidence for hydrodynamic and sedimentological connectivity between the lagoon and HB site, or alternatively reframing the interpretation throughout as a comparison between distinct terrestrial, lagoon and regional marine end-members. The latter appears to be the more defensible interpretation based on the information currently provided.

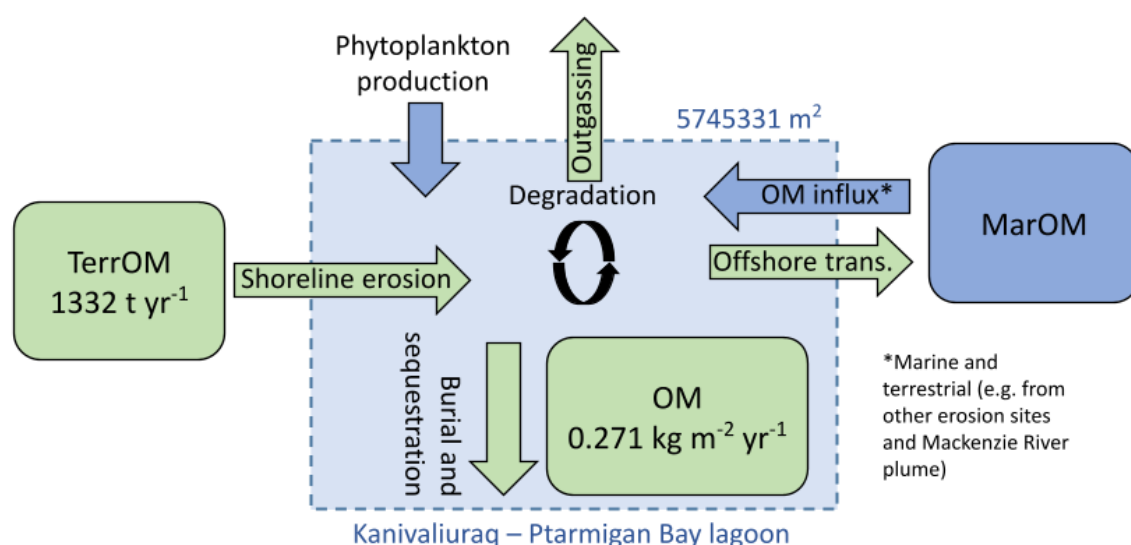
This issue also affects Fig. 5. The Jong et al. data points from Herschel are plotted together with the in-lagoon transect, which implies a common spatial axis or transport pathway. However, these represent an independent dataset, and the physical connection between this dataset and the sampled lagoon transect is currently not clear.

AR: Thank you for this important comment. We agree that spatial proximity does not necessarily imply direct hydrodynamic or sedimentological connectivity between the lagoon and Herschel Basin (HB). Our data do not provide sufficient evidence to demonstrate that the HB sites represent the downstream endpoint of sediment and OM exported from the lagoon. We have therefore revised the manuscript to distinguish more clearly between the spatial land–lagoon transition sampled within the Kanivaliuraq system and the HB sites, which are now treated as a regional marine reference rather than as the endpoint of a continuous land–lagoon–ocean transport pathway.

Accordingly, we have revised the terminology and interpretation throughout the manuscript, including the discussion of the lateral changes in OM characteristics and the comparison of OC pools in Section 4.3. The observed differences between terrestrial, lagoon, and marine sediments are now interpreted as differences among environmental compartments that may reflect different OM sources, transport histories, degradation, and depositional conditions, rather than necessarily representing progressive transformation along a single transport pathway. We have also clarified in the description and interpretation of Fig. 5 that the Herschel Island data from Jong et al. represent an independent regional marine dataset and are not part of the sampled lagoon transect.

RC: **Lagoon carbon budget** The manuscript provides estimates of OC erosion from shorelines into the lagoon (Table 3) and OC burial rates from lagoon cores (Table 1), but then qualitatively states that "a major portion of OM is also quickly removed offshore". I think this represents a missed opportunity to provide a first-order lagoon carbon budget. Given the available erosion-derived OC fluxes (t yr^{-1}) and lagoon OC mass accumulation rates ($\text{kg m}^{-2} \text{yr}^{-1}$), a simple estimate of the retained fraction could be calculated by scaling OCMAR rates by lagoon area. Such an approach would obviously involve uncertainties and assumptions (which should be discussed), but even a first-order estimate would allow the authors to quantify the relative importance of burial versus export rather than only discussing this qualitatively. This would substantially strengthen the "bioreactor" concept by providing clearer constraints on inputs and outputs. Importantly, while being independent of the connectivity issue raised above regarding the marine cores. Also, a simple summary figure or box showing the conceptual OM budget (erosional input, burial, degradation and export, including uncertainties) would also help readers understand the overall mass balance that underpins the manuscript.

AR: Thank you for this helpful suggestion. We agree that a conceptual mass-balance framework helps to better illustrate the different OC inputs and pathways within the lagoon. We therefore added a summary figure showing the quantified erosion-derived terrestrial OC input and sedimentary OC accumulation together with additional, currently unconstrained pathways, including marine and fluvial inputs, primary production, degradation, outgassing, and offshore transport. We emphasize that this does not represent a closed carbon budget, as several of these fluxes cannot be quantified with the available data. Nevertheless, the comparison between erosion-derived OC supply and sedimentary OC accumulation provides a first-order constraint on the potential retention of terrestrial OC within the lagoon and explicitly illustrates the remaining uncertainties in its fate.



Caption: Conceptual OC budget for the Kanivaliuraq–Ptarmigan Bay lagoon. Quantified fluxes include erosion-derived terrestrial OC input and sedimentary OC accumulation, whereas marine and fluvial inputs, primary production, degradation, outgassing, and offshore transport remain unconstrained. The scheme therefore represents a first-order conceptual budget rather than a closed carbon mass balance.

RC: **Degradation versus mixing.** The authors appropriately acknowledge that $\delta^{13}\text{C}$ enrichment may reflect either degradation or increased mixing with marine OM, and that OC loading is affected by mineral dilution effects. This discussion is useful. However, would a simple end-member mixing calculation help quantify how much of the observed lateral OC decrease could be explained by conservative mixing alone, compared to actual degradation? Such an analysis would provide additional support for the degradation interpretation and would make the "reactor" framing more robust.

AR: We thank the reviewer for this helpful suggestion. We agree that an end-member mixing approach could, in principle, help distinguish conservative mixing from OM degradation. However, the coastal system in our study area is strongly influenced by terrestrial OM inputs, and the available data do not allow us to define sufficiently constrained and spatially representative terrestrial and marine end members for a robust quantitative mixing model. In particular, terrestrial material may originate from multiple coastal and fluvial sources, while a

distinct marine OM signal becomes more apparent farther offshore and with increasing distance from the influence of the Mackenzie River (Naidu et al., 2000; Martens et al., 2001) (now included in the manuscript, see full sentence below). We therefore refrain from quantitatively partitioning the observed lateral OC decrease between conservative mixing and degradation. Instead, we have revised the Discussion to more explicitly acknowledge this limitation and emphasize that the observed decrease in OC loading and enrichment in $\delta^{13}\text{C}$ are consistent with OM degradation, but cannot uniquely distinguish degradation from changes in OM source and mixing. In detail, we added “The dominance of terrestrial OM is typical for coastal and estuarine regions of the Arctic with marine signatures only becoming apparent tens of kilometres offshore (e.g., Naidu et al., 2000; Martens et al., 2021). Although an end-member mixing approach could potentially separate conservative mixing from OM degradation, the strong terrestrial influence and the lack of well-constrained terrestrial and marine end members preclude a robust quantitative partitioning in our study. Consequently, the lateral decrease in OC and enrichment in $\delta^{13}\text{C}$ are interpreted as being consistent with degradation, while contributions from source mixing cannot be excluded.” to the discussion.

RC: OC pool comparison (Tab2) should more explicitly state considered depths/timescales and consider the limitations of comparing different depth/time scales. OC contents in sediments don't scale linearly with depths and similar depths scales can represent largely different timescales. The authors acknowledge that other studies used different depth intervals (3 m vs 30 cm here), yet still directly compare absolute values in the text (larger than OC pools observed in other regions). Normalize pools to a common depth or show OC density profiles instead of integrated pools. In addition, the conclusion that the lagoon holds two times more OC than the marine OC pool is based on a fixed 0–30 cm depth interval. Given that MAR can differ substantially between the two core locations the top 30 cm likely represents very different time intervals. A depth-fixed comparison would then also reflect differences in burial history rather than in sequestration capacity. Consider either normalizing to a common time window using the age models already available, or explicitly discussing this limitations. Finally, OC fluxes through a 30 cm depths horizon are not burial fluxes in the traditional sense (ie transition of C from the surface to the geological C cycle, see Bradley et al. <https://www.nature.com/articles/s41467-022-35112-9>). Explicitly state what you mean by “burial” here (ie sequestration beyond the 30cm depths horizon) and also by “longterm” (ie on which timescale is C sequestered between the 0-30cm depths horizon, assuming that DIC released within the first 10s of centimeters of sediment can still interact with the surface C cycle on policy relevant timescales).

AR: Thank you for this important comment. We agree that comparison of OC pools integrated over a fixed 0–30 cm depth interval requires careful interpretation because this interval may represent different depositional timescales at the lagoon and marine sites due to differences in sediment accumulation rates. We have therefore revised the manuscript to explicitly state this limitation and no longer interpret the higher 0–30 cm OC pool in the lagoon as direct evidence for a greater sequestration capacity. In addition, following the reviewer's previous comment, the Herschel Basin site is now treated as a regional marine reference rather than as the endpoint of a continuous lagoon-to-marine transport pathway.

We have also clarified our use of the term “burial.” Following Bradley et al. (2022), we distinguish sedimentary OC accumulation within the studied surface sediment interval from longer-term burial beyond the actively cycling surface sediment layer. We acknowledge that

OC within the upper sediment column can remain subject to degradation, resuspension, and bioturbation and therefore should not automatically be considered permanently sequestered. We have revised the terminology throughout the manuscript accordingly and specify the depth intervals and timescales represented by our OC pool and accumulation estimates.

RC: **Critically discuss limitations/representativeness.** I really like the multi-proxy, multi-method gradient approach and support the general discussion, but think some conclusions and the manuscript's language sometimes read more confidently than the data justifies. For instance, the central 50% decline in OC along the land–lagoon–ocean gradient rests on two shallow short cores plus 11 surface-transect samples along a single 2 km transect, plus one lagoon depocenter core (PB) and one marine core (HB), which does not necessarily lie along a source to sink transport pathway. This is a very nice but still has its limitations in spatial/temporal resolution, connectivity and is not necessarily representative for a wider area. Please explicitly discuss potential limitations critically and tone down generalizing claims accordingly, or, alternatively, clearly justify why the data supports the strong claim. Also, section 4.5 carefully notes that different lagoon types need to be defined before generalizing but the abstract's framing ("Arctic lagoons... act as dynamic reactors,") reads more generally than a single semi-open, spit-protected, glaciated-terrain site can strictly support. Consider flagging the single-site caveat slightly earlier and more prominently.

AR: We appreciate the reviewer's comments and agree that the spatial and temporal coverage of our dataset requires careful consideration when interpreting and generalizing the observed patterns. We have addressed this issue at several points throughout the revised manuscript. First, we now explicitly identify the study as a detailed investigation of a single lagoon system and acknowledge its spatial limitations in the Introduction and Conclusion. Second, we have revised the interpretation of the land–lagoon–marine comparison and now treat Herschel Basin as a regional marine reference rather than as the downstream endpoint of a continuous transport pathway. Consequently, the observed differences in OC are no longer interpreted as necessarily representing progressive transformation along a single source-to-sink pathway. We have also clarified the limitations in distinguishing degradation from source mixing and offshore transport.

Finally, we have toned down broader statements in the Abstract and Discussion to distinguish observations specific to the Kanivaliuraq–Ptarmigan Bay lagoon from their potential implications for Arctic lagoon systems more generally. These revisions more explicitly acknowledge the spatial resolution, connectivity, and representativeness of our dataset while retaining the value of the integrated multi-proxy approach.

RC: **Minor comments**

RC: Spelling "Qikiqtaruk" or "Qikiqatruk" please correct throughout.

AR: Changed accordingly

RC: "Environment Canada Canada" duplicated word.

AR: Changed accordingly

RC: "CF model" and "CFCS model" are used interchangeably. Pick one term and use it consistently (see comment above).

AR: Changed accordingly. We use CGCS model throughout the manuscript.

RC: "long-term" be more precise. Over what timescale?

AR: We added [...] on geological time scales to make the point clearer.

RC: The abstract states erosion rates increased "from 0.6 m yr⁻¹...to 0.9...to 3.3 m yr⁻¹," but Table A2 gives slightly different "mean erosion rate" values (-0.7, -0.9, -3.7) compare to the the mean shoreline change rate values (-0.6, -0.9, -3.3) used elsewhere. Please clarify in the text which metric is being reported where.

AR: Thank you for pointing out this inconsistency in terminology. The values reported in the Abstract and main text (-0.6, -0.9, and -3.3 m yr⁻¹) represent mean shoreline change rates calculated including all transects, whereas the mean erosion rates reported in Table A2 (-0.7, -0.9, and -3.7 m yr⁻¹) are calculated only from transects exhibiting shoreline retreat. We have revised the terminology and the numbers in the Abstract and main text to consistently refer to the former as mean shoreline change/retreat rates and have added a clarification to Table A2 distinguishing the two metrics.

RC: Table 3: the annual area loss for 2011–2018 (26,976 m² yr⁻¹) appears inconsistent by an order of magnitude with the other two periods (3,926 and 4,088 m² yr⁻¹) relative to the "eroded area" column. A sentence explaining this would help.

AR: Thank you for pointing this out. The apparent discrepancy results from the different lengths of the observation periods. The "eroded area" represents the total area lost during each period, whereas the annual area loss is normalized by the duration of the respective period. Thus, although the total eroded area during 2011–2018 (188,832 m²) is comparable to that during 1972–2011 (159,431 m²), it occurred over only 7 years rather than 39 years, resulting in a substantially higher annual area loss of 26,976 m² yr⁻¹. We have added a sentence to clarify this in the manuscript. ("The high annual area loss during 2011–2018 reflects the short seven-year observation period, during which an eroded area comparable to that of the preceding 39-year period accumulated.")

RC: The Discussion (4.4–4.5) about the regional extent of lagoons along the Beaufort coast is useful context but reads somewhat like a literature review. Consider tightening or moving some of this to the Introduction, keeping the Discussion focused on interpreting your own data.

AR: We deleted some of the text or merged it with text in the introduction.