Response to Reviewer 1 comments on manuscript egusphere-2026-582 titled "Different paths, same destination: similar functional outcomes in nitrogen cycling within artificialized coastal habitat".

RC1-1: The manuscript "Different paths, same destination: similar functional outcomes in nitrogen cycling within artificialized coastal habitat" by Pascal et al. investigates how man-made structures aimed at mitigating coastal erosion, such as breakwaters, shape the natural coastal ecosystem, particularly the benthic habitat and its nitrogen-cycle. Method wise, they look at porewater geochemistry, flux measurements, denitrification process rates and macrofauna at both impacted and non-impacted coastal sites. Interestingly, while there are clear differences in benthic biogeochemistry and fauna between the impacted and non-impacted sites, denitrification rates do not follow this pattern but show high and low rates at both impacted and non-impacted sites. This leads the authors to the conclusion that there is no impact of the breakwater structure on the benthic nitrogen-cycle. This is an interesting and important study, and I had pleasure reading it. Because of climate change, man-made structures to protect the coastline are of increasing societal and infrastructural importance, and it is nice to read that this does not necessarily come at the cost of ecosystem functioning. Particularly the nitrogen-cycle is important in a coastal setting, given that coastal eutrophication is pre-dominantly driven by nitrogen.

AC-R1-1: Dear Reviewer 1,
We thank you for your careful evaluation of our manuscript and for the supportive and constructive comments. We have carefully addressed all the concerns raised and responded to each comment below.
Please note that, as we have added new data to address both reviewers' comments, we have included Carole-Anne Guay in the author list. She collected the additional sediment samples and performed the solid-phase ascorbate-extractable iron analyses presented in the revised manuscript.

Specific comments:

RC1-2: However, I do have a couple of concerns, the major one being the lack of explanation for the variation in denitrification rates, from which then the conclusion was derived that there is no general/concise effect of the breakwater on the nitrogen-cycle (as the variation was in both impacted and non-impacted sites). The impacted sites had much higher organic matter and organic carbon content, different macrofauna, and a lower OPD / higher DOU than the non-impacted sites, while nitrate concentration in surface sediments were about similar between all sites. Nevertheless, given the higher organic carbon content, which was seemingly of labile composition given the higher DOU, it could have been expected that denitrification rates would have been higher than at the non-impacted sites. Instead, the highest rates were found at an impacted and a non-impacted site, and the main explanation offered is "functional plasticity"- which, I feel, is too vague and a bit unsatisfying (l. 436). I would like to encourage the authors to look more thoroughly into their biogeochemical data and use them for their conclusions. Referring to the title, this means to describe in more detail what the "different path" is. Overall, for a manuscript that focusses on the nitrogen-cycle, the nitrogen data are too little discussed, esp. the results of the denitrification rate measurements.

AC-R1-2: We thank the reviewer for the suggestion. We agree that "functional plasticity" was too vague. We have thoroughly revised the Discussion (Section 4.1 and 4.3) to integrate more biogeochemical results (porewater and new solid-phase reactive iron profiles to explicitly demonstrate how distinct mechanisms ("different paths") lead to similar denitrification rates at LBWS and VS. Also, we now compare our denitrification rates with the literature.
Here is how the distinct pathways are explained:

- Vegetated sediment (root-driven pathway):
  The porewater data at our vegetated reference site (VS) shows a distinct subsurface NO_3^- peak (Fig. 2c), which we interpret as a signature of coupled nitrification-denitrification fueled by cordgrass root oxygenation deep in the sediment. To strengthen this, we have added solid-phase ascorbate-extractable Iron profiles (reactive iron oxyhydroxides) to the supplementary material (Fig. SX). At VS, this reactive Fe(III) fraction exhibits a peak at 3-4 cm depth suggesting rhizosphere oxidation. This deep O_2 release drives the high denitrification rates and coupled nitrification-denitrification at VS despite lower surface OM content and (quasi) absence of bioturbation.
- Landward southern side of the breakwater (bioturbation-driven pathway):
  The porewater data at LBWS (Fig. 2c) shows a rapid accumulation of NH_4^+ and PO_4^{3-} from the surface to the deep layers of the sediment, indicating a strong reducing environment driven by the accumulation of fine, organic-rich particles (Table 1). In such condition, sedimentary nitrification should be limited. However, the high density of small bioturbators such as *H. divericolor* and oligochaete maintains bioturbation of the surface sediment layer. The bioirrigation supplies O_2 to sustain nitrification and its coupling

with denitrification as well as supplying NO3 to sustain high denitrification rates

LBWN shares the high organic matter characteristics of LBWS but lacks the high density of bioturbators. Consequently, the sediment remains reduced which can inhibit nitrification and result in low denitrification.

US represent a sandy mudflat with low OM and low macrofauna density, resulting in low denitrification rates due to organic carbon limitation.

The section 4.1 now reads as follow:

"4.1 Impact of breakwater construction on benthic habitats

Our study highlights marked alterations in surficial sediment characteristics, with impacted sites (i.e., LBWS and LBWN) accumulating finer and more organic-rich sediment compared to reference and intermediate sites. These observations are consistent with expectations for low-energy depositional environments landward of high-crested breakwaters (Da Silva et al., 2022; van Rijn, 2011; Wang et al., 2021). In contrast, sediments of the intermediate site (SBW) resembled reference sites (i.e., US and VS), suggesting minimal breakwater influence in this area. The consistent sedimentary $\delta^{13}C_{org}$ *signature among sites suggested a shared origin of organic carbon, likely a mix of terrestrial and marine inputs typical of intertidal zones. This homogeneity suggests that low-energy hydrodynamic conditions landward of the breakwater enhance deposition of fine, organic-rich particles without selective retention of organic matter from specific sources. Therefore, differences among sampled areas are driven by organic carbon quantity, not origin.*

Geochemical measurements further support this pattern. Sediment cores from impacted sites showed up to 2.3 times higher diffusive O_2 *uptake (DOU) and shallower* O_2 *penetration depth (OPD) than reference sites (Fig. 2a and b), consistent with enhanced organic carbon mineralization driven by higher organic carbon content (Glud, 2008). These results align well with our porewater nutrient profiles extracted from another sediment core. The decrease in porewater* NO_3^- *concentration with depth in all sites indicates a consumption by the sediment (Fig. 2c), consistent with our measured* NO_3^- *benthic fluxes (Fig. 6). This consumption results from the denitrification process, which has been detected among all sites (Fig. 6). VS exhibited a subsurface* NO_3^- *peak, suggesting active nitrification consistent with rhizosphere oxygenation (Koop-Jakobsen et al., 2018), which may explain the relatively high proportion of coupled nitrification-denitrification detected in incubation experiment.*

This interpretation is further corroborated by the solid-phase ascorbate extractable iron (Fe$_{asc}$) profiles (Fig. 2d). At VS, Fe$_{asc}$ increase progressively from the surface to a peak at 3-4 cm depth, well below the NO_3^- maximum (Fig. 2c), suggesting subsurface oxidation consistent with rhizosphere oxygenation. The absence of NO_3^- peak co-located with the Fe$_{asc}$ one suggests a tight coupling between nitrification in the rhizosphere area and denitrification in adjacent anoxic sediment (Koop-Jakobsen and Giblin 2010, Zheng et al. 2016). The quicker NO_3^- consumption in the porewater of impacted sites in the upper four centimeters compared to six centimeters in reference sites suggests a more reduced condition in impacted sites. Similarly, deeper sediment layers in impacted sites exhibit greater increases in NH_4^+ and PO_4^{3-} compared to reference sites, indicating larger ammonification driven by their higher organic matter content, along with PO_4^{3-} release, typically associated with the reductive dissolution of iron-oxyhydroxides in anoxic sediment layers (Slomp, 2011). Consistently, Fe$_{asc}$ concentrations at impacted sites are highest at the sediment surface and decrease rapidly with depth (Fig. 2d). This indicates that aerobic processes, including nitrification, are restricted to the very surface of the sediment at these sites. Overall, these O_2 and nutrient profiles suggest more reduced conditions and greater nutrient accumulations in the sediment column in impacted sites compared to reference sites. Despite these changes in sedimentary conditions, denitrification rates were not substantially altered, suggesting that the capacity of these sediments to remove excess nitrogen remains resilient to these perturbations.”

The section 4.3 now reads as follow:

“4.3 Potential convergence in ecosystem functioning

Despite marked differences in sediment characteristics, macrobenthic community structure and trait expression between impacted and reference sites (i.e., LBWN vs. US and particularly LBWS vs. VS), total benthic fluxes and denitrification rates remained similar. These findings, derived from dark incubations (see section 2.10), suggest that distinct benthic environments can yield similar functional outcomes regarding oxygen uptake, nutrient cycling and notably denitrification rate. Our measured denitrification rates ranging from 6 to 75 µmol m-2 h-1 fall within the range of reported denitrification rates for temperate intertidal mudflats and salt marshes (Sousa et al. 2012, Koch et al. 1992, Poulin et al. 2007 Hinshaw et al. 2017). Coupled nitrification-denitrification (D_n) was the dominant N-removal pathway at impacted sites and VS, indicating that denitrification relies on locally produced NO_3^-.

Conversely, at US, denitrification was primarily driven by water-column nitrate. The high density of subsurface bioturbators at LBWS (Fig. 5c) likely enhances oxygen and nutrient exchange at the sediment-water interface. The sediment irrigation enhances aerobic mineralisation of the OM, stimulates nitrification fuelling coupled nitrification-denitrification, a pattern supported by our isotope pairing results (Fig. 6). In VS, although macrofauna is present, bioturbation is absent or constrained to the sediment surface (Fig. 5c). However, as evidenced by the deep subsurface Fe_{asc} *peak (Fig. 2d; Section 4.1), radial* O_2 *loss from S. alterniflora roots actively creates deep oxidized zones (REF, Koop-Jakobsen). This rhizosphere oxygenation sustains subsurface nitrification, which is tightly coupled to denitrification, thereby achieving high denitrification rates similar to those driven by bioturbation at LBWS. By contrast, LBWN showed a trait expression tendency toward US that could yield similar benthic fluxes. The low denitrification rates at US likely reflect the relatively low organic carbon availability compared to impacted sites, which limits local nitrification fuelling denitrification. Moreover, macrofauna at US is dominated by a few large burrowers (e.g., Mya sp.) whose local influence may not be captured at the core scale, leading to low benthic fluxes. Interestingly, despite being both impacted sites and having similar sediment characteristics, LBWN and LBWS exhibited different ecosystem functions based on total benthic fluxes and denitrification rates. This most likely results from the higher density of macrobenthic organisms at LBWS than at LBWN, highlighting the critical role of macrobenthic community activity in shaping biogeochemical processes (Snelgrove et al., 2018). In the absence of dense bioturbators, the reduced sediment at LBWN (Fig. 2d) prevents nitrification despite the high availability of organic carbon, effectively limiting denitrification. However, the factors driving the variation in macrobenthic community structure between these two areas remain elusive. The absence of a detectable breakwater effect on denitrification rates therefore does not reflect an absence of biogeochemical change, but rather a functional compensation mediated by distinct biological drivers at each site.*

From a coastal-management perspective, one of the main issues is whether areal flux rates measured in LBWS can offset, at the seascape level, the functional loss caused by salt marsh vegetation removal. LBWS area (~18,800 m^2*) is about three times larger than the vegetated area directly lost due to the breakwater implementation (~6,300* m^2*). Assuming that the measured fluxes capture the whole area heterogeneity, LBWS could compensate for these specific functional capacity losses of that part of the salt marsh. However, it is worth noting that this area is still more than three times smaller than the total vegetated area lost due to combined erosion* *and breakwater implementation (~62,700 m^2) so any functional compensation remains only partial at the scale of the whole seascape.”*

RC1-3: Furthermore, dissimilatory nitrate reduction to ammonium (DNRA) is not mentioned in the introduction but suddenly pops up in materials & methods (l. 192) and results, whereas anaerobic ammonium oxidation (anammox) is mentioned in the introduction, but not in the materials & methods and results; neither of them is mentioned in the discussion.

AC-R1-3: We apologize for the inconsistency. We added DNRA to the introduction alongside nitrogen removal processes. Which now reads as:

“*In this context, understanding nitrogen removal processes in salt marshes is crucial when assessing the impacts of shoreline hardening on coastal ecosystem functioning (Sousa et al., 2008; Valiela and Cole, 2002). Shoreline hardening can alter sediment characteristics, hydrodynamic conditions, and benthic communities (Gittman et al., 2016), all of which influencing nitrogen cycling processes (Albert et al., 2021; Stief, 2013). Two key microbial processes drive nitrogen removal from coastal systems. (i) Denitrification which involves a microbially-mediated oxidation of organic matter (OM) using nitrate (NO_3^-) as an electron acceptor under anaerobic conditions, resulting mainly in the production of dinitrogen gas (N_2^), and to a lower extent,but can also release potent greenhouse gases as nitrous oxide (N_2 O), a potent greenhouse gases, and together with carbon dioxide (CO_2^). (ii) Anammox (anaerobic ammonium oxidation), produces N_2^ through the oxidation of ammonium (NH_4^+) coupled to the reduction of nitrite (NO_2^-) although in many organic-rich coastal sediments its contribution can be smaller than that of heterotrophic denitrification Dalsgaard et al. 2005(Devol, 2015). In addition to denitrification and anammox, dissimilatory nitrate reduction to ammonium (DNRA) can also occur, competing for nitrate and retaining nitrogen within the system.*”

We also added a clarifying sentence in the methods section (2.7) confirming that both DNRA and anammox rates were negligible, justifying our focus on denitrification in the discussion.

“*The validity of the original isotope pairing technique (Nielsen, 1992) was tested by checking the independence of the estimated ^28 N_2 production and the amount of added ^15 N-NO_3^- (Risgaard-Petersen et al., 2003). No production of ^15 N-NH_4^+ was detected over the incubation periods, DNRA was thus considered to be* *negligible. Since no deviation from the original isotope pairing technique was detected, anammox was also considered negligible. Both processes are therefore not considered further in the manuscript and denitrification rates were computed on the six sediment cores following Nielsen 1992*"

RC1-4: Another issue that needs clarification is the sampling in two years: 2021 and 2022 (l. 125). However, it is not mentioned whether this means (i) that some sites were sampled in 2021 and others in 2022, or (ii) that all sites were sampled repeatedly over two years? Maybe I missed it, but I couldn't find this information. If the sites were sampled in different years, it would add potential interannual variability to the data and maybe that could be a reason for the variation in results (particularly the nitrogen results). Please clarify that point.

AC-R1-4: We apologize for the confusion. All benthic flux incubations, including isotope pairing experiments, porewater extraction, and macrofauna sampling were conducted during a single campaign in June 2021, ensuring no interannual variability in our main dataset. The June 2022 campaign was specifically dedicated to strengthening this dataset by performing O2 microprofiles at SBW (which was not possible in 2021 due to logistical constraints) and to increasing the replication of baseline sediment characteristics. Cross-validation between the two field campaigns confirmed that spatial gradients remained consistent across years, based on sediment characteristics and O2 microprofiles at VS and US (Fig 1 and 2 below), further supporting the temporal stability of our observations.

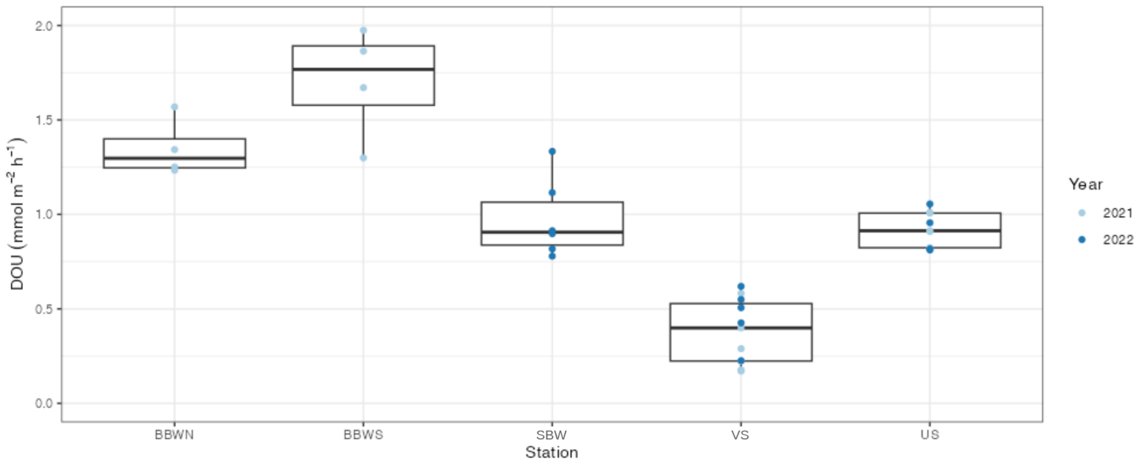

Figure 1: Computed diffusive oxygen uptake (DOU in mmol m^{-2} h^{-1}) at the different stations in 2021 and 2022.

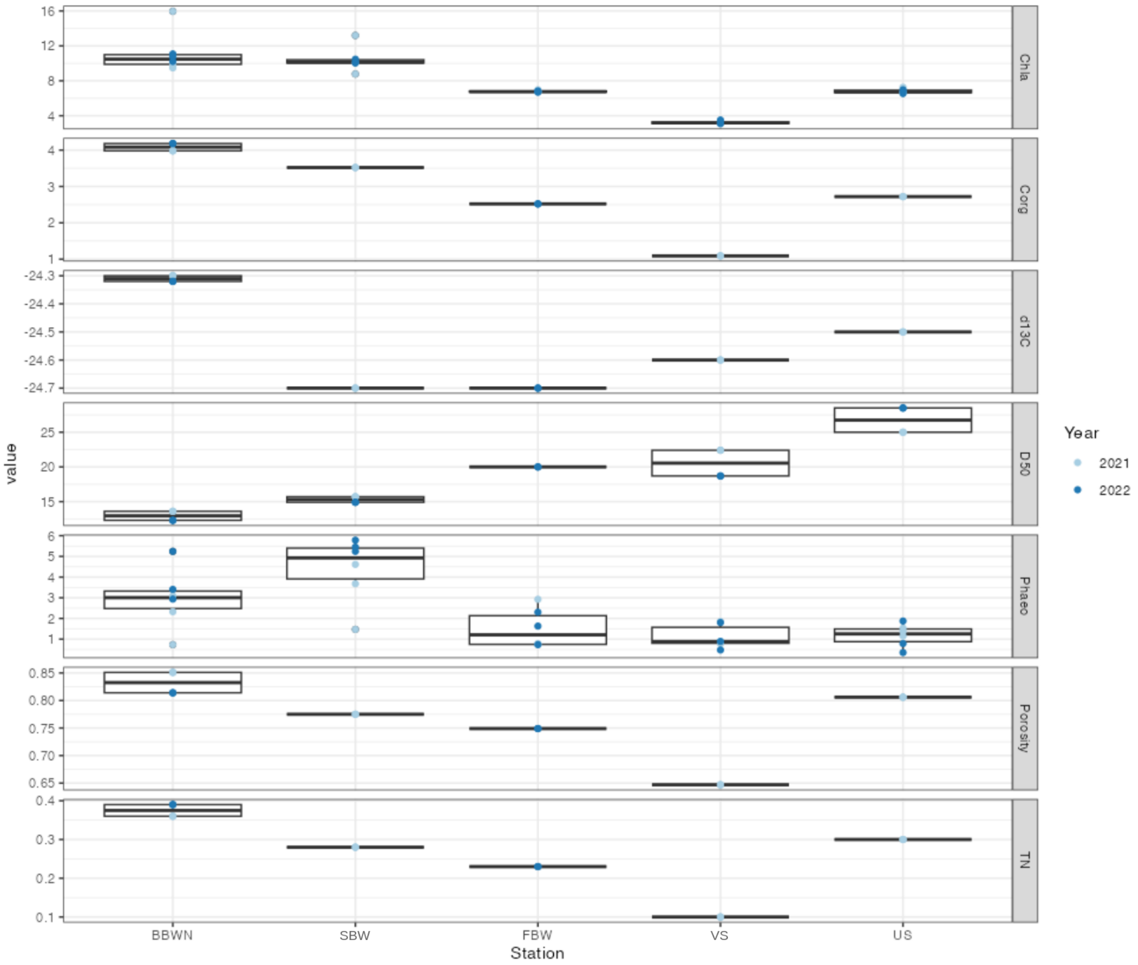

Figure 2: Sediment characteristics in 2021 and 2022. Median grain size (D_{50} in µm), porosity (volume ratio), chlorophyl a and phaeopigments (in µg g^{-1}), organic carbon and total nitrogen (C_{org} and TN in %) content along with $d^{13}C$ isotopic signature (in ‰)

RC1-5: L. 168: I would be cautious with the porewater data taken in 0.5 cm resolution; commonly the "catchment area" of rhizons is assumed to be 1 cm (Seeberg-Elverfeldt et al. (2005) Rhizon sampling of porewaters near the sediment water interface of aquatic systems. Limnol Oceanogr Methods 3: 361−371); hence, when sampled in smaller resolution you likely draw water from "wrong / overlapping layers".

AC-R1-5: We agree with the reviewer. This was, in fact, partly acknowledged in the methods section L168-169 of the original manuscript

"*To avoid significant overlap between successive samples, porewater extraction was limited to 8 mL*".

We have modified this sentence to make this limitation clearer and should read as follow:

"*To avoid significant overlap between successive samples, porewater extraction was limited to 8 mL, we cannot exclude that nutrient profiles in the uppermost cm of the sediment column are slightly smoothed due to limited smearing from adjacent layers.*"

RC1-6: L. 178: did you account for the different nutrient concentration of the replacement water in your flux calculation (given that during the flux incubation nutrients will either accumulate or deplete, different to the replacement water)?

AC-R1-6: We did not correct the flux calculations for the nutrient concentration of the replacement water because the dilution effect was negligible. Specifically, at each sampling point (1 h, 2 h, and 3 h), we collected less than 20 mL of water (12 mL for exetainers and 4.5 mL for nutrients), which represents less than 2% of the total overlying water volume in the incubator (>1.1 L). Cumulatively, the total volume replaced over the course of the entire incubation was around 5%. Because of this very low dilution ratio, any correction would have been close to the analytical uncertainty. We have added a brief sentence in the methods (Section 2.7) to clarify this.

"*Overall, cumulative volume of replacement waters corresponded to <5% of the overlying water and its dilution effect was thus considered as negligible.*"

RC1-7: L. 185: how did you decide for the pre-incubation time of 3 h, was it calculated? Given the OPD of max. 1.3 mm, the silty sediment with a porosity of 0.65-0.83 and a temperature of 9°C, you should have reached a 90% steady state denitrification within less than 15 min (see e.g. Dalsgaard et al 2000, NICE handbook).

AC-R1-7: The 3 h pre-incubation time was chosen to allow the 15N-NO3 tracer to penetrate deeper into the sediment and reach the active coupled nitrification-denitrification zone in vegetated (shallow rhizosphere) and bioturbated sediment. Assuming an apparent effective solute transport coefficient of 2–3 cm^2 d^{-1}, accounting for both molecular diffusion and enhanced transport due to bioturbation, the ^15N-NO3^- tracer was estimated to penetrate approximately 1 cm during this period. Now the corresponding sentence in the methods (Section 2.7) reads as follow:

"*A 3 h pre-incubation period of 3 hour was implemented to ensure sufficient diffusion of ^15 N-NO_3^- to the sediment nitrate reduction zone (Nielsen, 1992), including shallow rhizosphere and bioturbated layers.*"

RC1-8: L. 189: did the replacement water during the 15N-incubation also contain the tracer?

AC-R1-8: No, the replacement water did not contain the 15N-NO3 tracer. Based on the response to RC1-6 cumulative water replacement over the incubation period had a negligible (<3%) effect on the enrichment ratio well below the spatial variability of replicate sediment core.

RC1-9: L. 190: did you also take a sample for denitrification after mixing the porewater with the overlying water? Commonly, a lot of the produced 29N2 and 30N2 sits in the porewater, rather than diffuses out into the water phase; hence, if you do not sample the porewater, you likely miss a portion of 29N2 and 30N2.

AC-R1-9: No, we did not collect sample for denitrification from the slurry after mixing. The denitrification samples were collected exclusively from the overlying water. Mixing the sediment at the end of the incubation would release the 29N2 and 30N2 that had accumulated in the porewater during the 3 h pre-incubation period. Since the initial samples were collected from the overlying water without mixing, we wanted to use the same undisturbed sampling approach at the end of the incubation. Using a final slurry sample would have introduced an inconsistency between the initial and final sampled pools. This could have led to an artificial overestimation of the denitrification rates by including the pre-incubation production not accounted for in the initial sampling. However, for DNRA, slurry samples were indeed collected at the end of the incubation to capture the total ammonium pool. Now the corresponding sentence in section 2.7 reads as follow to clarify this point:

"*At the end of the incubations, the remaining water was mixed with the top 5 cm of the sediment column. From the slurry, 12 mL of sample was transferred to an Exetainer to which 200 μL of 7 M ZnCl_2 were added for further ^15 N-NH_4^+ analysis to assess DNRA rates.*"

RC1-10: L. 190: did you treat the slurry sample with KCl to desorb the 15N-NH4 that might be attached to the grains? The particle-bound portion of NH4 can be significant, so if no KCl was applied or a correction factor used, DNRA rates can be potentially underestimated.

AC-R1-10: Yes, we treated the slurry samples collected for DNRA by adding 200 µL of 2 M KCl directly into the exetainers. This is now mentioned in the methods section 2.7

"*The water samples were then treated 200 µL of 2 M KCl to desorb any particle-bound NH_4^+ followed by the addition of alkaline hypobromite to oxidize NH_4^+ to N_2 prior to analysis using MIMS as described in Yin et al. (2014)*"

RC1-11: L. 200-201: checking 28N2 vs 15N-concentration relates to the potential occurrence of anammox, which should be mentioned here. Further, did you also check for the linear increase of D15 with 15N-concentration, needed to assure nitrate-limitation of sediment and functioning of the method / reliability of calculated rates (see Risgaard-Petersen et al. 2003, page 69)?

AC-R1-11: Yes, we verified the linear increase of D15 with 15N-concentration (Fig. 3) to ensure the reliability of the calculated rates, which confirms the sediment was nitrate-limited and the isotope pairing assumptions were met.

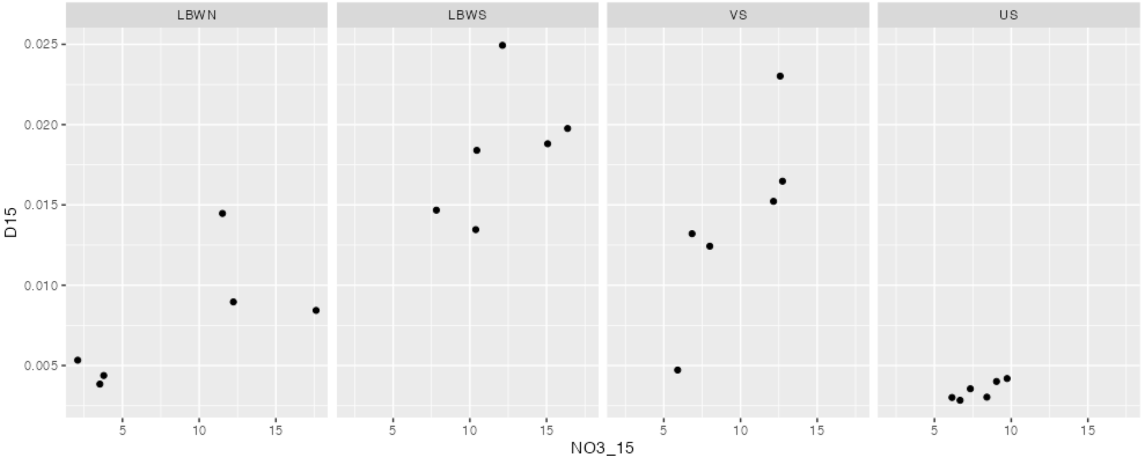

Figure 3: Relationship between denitrification based on ^{15}N-labelled nitrate (D15) and the concentration of ^{15}N-NO$_3^-$ added in the overlying water of the incubators across stations.

RC1-12: L. 423: I am skeptical of the argument that higher denitrification rates at LBWS than at LBWN are due to higher density of macrofauna at LBWS, as the reference site VS had similarly high denitrification rates as LBWS but also similarly low macrofauna as LBWN.

AC-R1-12: We agree that without distinguishing the underlying mechanisms, this comparison appears contradictory. Particularly for the US site for which mechanisms were not detailed. In addition to the changes made to section 4.3 detailed for **RC1-2**, we added a discussion about underlying mechanisms occurring at US.

"*By contrast, LBWN showed a trait expression tendency toward US that could yield similar benthic fluxes. The low denitrification rates at US likely reflect the relatively low organic carbon availability compared to impacted sites, which limits local nitrification fuelling denitrification. Moreover, macrofauna at It is worth noting that US is dominated by a few large burrowers (e.g., Mya sp.) whose local influence may not be captured at the core scale, leading to low benthic fluxes.*"

Technical comments:

RC1-13: L. 14: nitrogen removal is part of nitrogen-cycling, so maybe reformulate to something like "particularly nitrogen-cycling and its removal processes denitrification and anammox" or similar.

AC-R1-13: This has been reformulated as suggested

RC1-14: L. 19: maybe add some location to the reference sites, such as "reference sites outside the breakwater system".

AC-R1-14: This has been corrected as suggested

RC1-15: L. 20: "higher organic carbon content" compared to what? Please add.

AC-R1-15: We meant "compared to the other sites". This has been added

RC1-16: L. 23: "NO3, NH4" needs a prior abbreviation or write in full.

AC-R1-16: Abbreviations have been removed from the abstract and replaced by full word.

RC1-17: L. 29: "benefits" or maybe "services"?

AC-R1-17: This has been corrected as suggested

RC1-18: L. 36-38: could you add the difference between the two structures? They sound very similar, but I guess bulkheads might be typically a bit smaller (?).

AC-R1-18: We have added more details to better characterize both structures. Bulkheads are primarily designed to retain soil in low- to moderate-energy environments, whereas seawalls are built to reflect and/or dissipate wave energy along higher-energy shorelines. The corresponding sentences in the introduction now read as:

“*Bulkheads are vertical walls typically deployed in environments with low to moderate hydrodynamic energy, built directly on the shoreline to retain soil and prevent land loss due to wave action. Seawalls are robust, vertical or sloped structures designed to protect the shoreline in high-energy environment by reflecting and/or dissipating wave energy and preventing coastal flooding and erosion.*”

RC1-19: L. 39: “they” don’t refer to the waves (as I guess you wanted to) but rather the structures; please edit.

AC-R1-19: “they” has been replaced by “the waves”

RC1-20: L. 51: coastal eutrophication is predominantly driven by nitrogen-enrichment, which I would put here into focus to also justify why you specifically look at the nitrogen-cycle (rather than the phosphorous-cycle); this also makes your next sentence “understanding nitrogen removal is crucial..” a bit smoother.

AC-R1-20: “phosphorus” has been removed

RC1-21: L. 57: “microbially-mediated” is redundant as you already said that it is a microbial process.

AC-R1-21: This has been removed

RC1-22: L. 59: this is a matter of taste, but in the context of denitrification I would argue that the release of N2O is more important that that of CO2; N2O has a higher warming potential than CO2 (due to a longer lifetime in the atmosphere) and denitrification can be a significant source of it under certain circumstances; whereas a portion of CO2 is always emitted and kind of part of the process, as it relates to OC oxidation.

AC-R1-22: This sentence has been modified to reflect the higher warming potential of N2O compared to CO2 and should now read as:

“*(i) Denitrification which involves a oxidation of organic matter (OM) using nitrate ($\mathrm{NO_3^-}$) as an electron acceptor under anaerobic conditions, resulting mainly in the production of dinitrogen gas ($\mathrm{N_2}$), but can also release as nitrous oxide ($\mathrm{N_2O}$), a potent greenhouse gas, together with carbon dioxide ($\mathrm{CO_2}$).*”

RC1-23: L. 59 ff: anammox is often not too common in coastal systems, which can be organic-rich, thus putting (autotrophic) anammox at a disadvantage compared to (heterotrophic) denitrification, see e.g. Dalsgaard et al. 2005. However, it of course can appear. Maybe reformulate, e.g. either talk about “sediment systems” (rather than “coastal systems”, line 56) or add some info to anammox.

AC-R1-23: We agree with the reviewer comment and have modified the sentence accordingly.

“*Two key microbial processes drive nitrogen removal from soft-sediment systems. (i) Denitrification which involves a oxidation of organic matter (OM) using nitrate (NO_3^-) as an electron acceptor under anaerobic conditions, resulting mainly in the production of dinitrogen gas (N_2^), but can also release as nitrous oxide (N_2 O), a potent greenhouse gas, together with carbon dioxide (CO_2^). (ii) Anammox (anaerobic ammonium oxidation), produces N_2^ through the oxidation of ammonium (NH_4^+) coupled to the reduction of nitrite (NO_2^-) although in many organic-rich coastal sediments its contribution can be smaller than that of heterotrophic denitrification (Dalsgaard et al. 2005, Devol, 2015).*”

RC1-24: L. 71: please add a “that” after “showed”.

AC-R1-24: This has been done

RC1-25: L. 71 ff: do you mean here that the areal nitrogen removal rates would decrease (due to a shrinkage of the area) while the process rates itself would stay constant? Please reformulate for clarity.

AC-R1-25: Yes, O'Meara et al. (2015) showed that, at sites with hardened shorelines, nitrogen removal process rates remained comparable to those at sites without hardened shorelines. However, the primary effect was a reduction in salt marsh area due to coastal squeeze, which ultimately decreases areal nitrogen removal capacity. The sentence has been modified to clarify this point and should read as follows:

“*Previous research on bulkhead implementation found no direct effects on nitrogen removal rates per unit area, but showed that these structures can reduce the overall nitrogen removal capacity of the ecosystem by decreasing the total area of salt marshes through coastal squeezing, rather than impacting nitrogen removal process rates (O’Meara et al., 2015).*”

RC1-26: L. 73: could this be simplified to “when built with sufficient vertical distance between…”

AC-R1-26: We agree this sentence was too complex and we thank the reviewer for their suggestion. We have modified the sentence as suggested.

RC1-27: L. 93: “adaptation” or “structure”?

AC-R1-72: The original sentence “… provides a representative example of such adaptation” has been replaced by: *“… provides a representative example of coastal hardening.*”

RC1-28: L. 93 ff: this sentence seems a bit out of place; is this not exactly what you first want to find out?

AC-R1-28: This sentence has been removed

RC1-29: Fig. 1: maybe a slightly stupid question: is all the green in Figure 1C the saltmarsh?

AC-R1-29: We agree the Fig. 1 did not allow to clearly identify the saltmarsh. This figure has been modified as well as its caption to make it clearer.

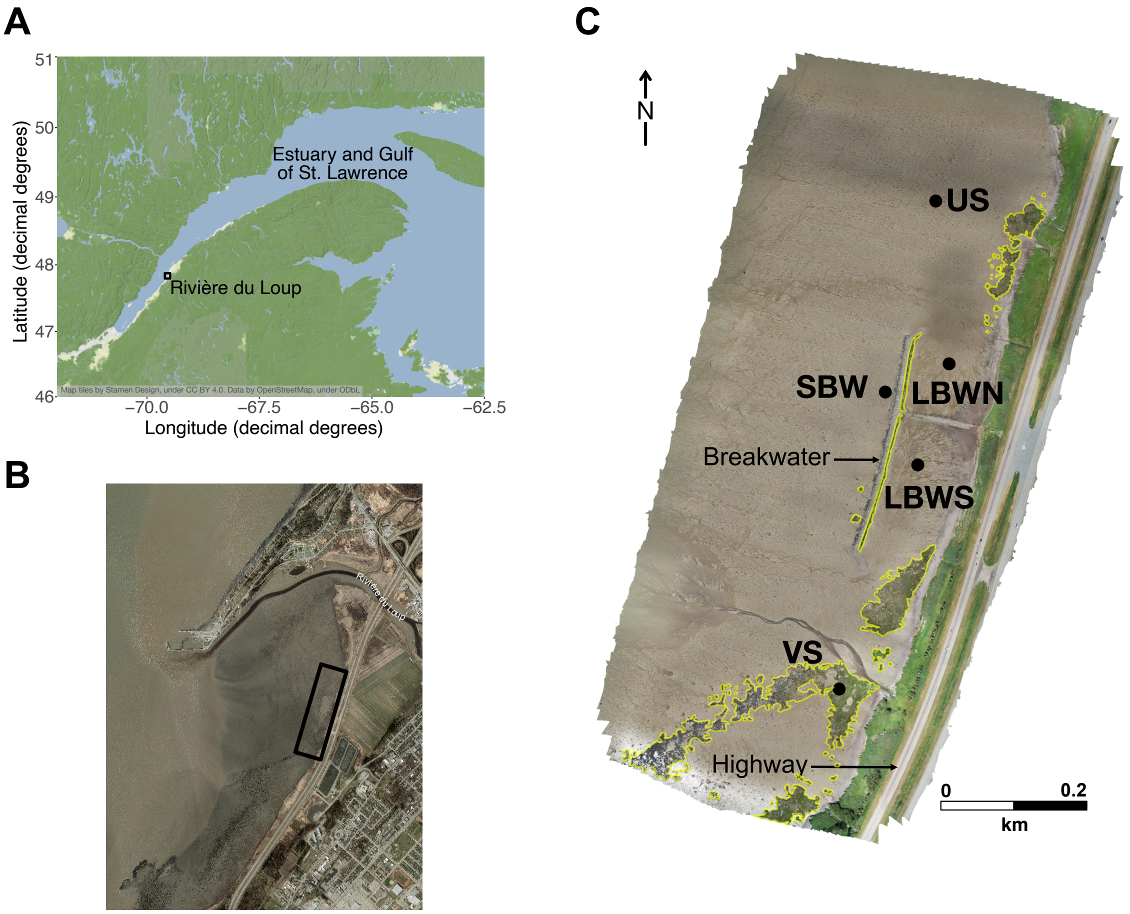

Figure 1*: Study site. (a) Location of the study area (open square) in the St. Lawrence system, (b) location of the study site (black rectangle) in the bay of Rivière-du-Loup and (c) aerial view of the sampling location around the breakwater (from Zouaghi et al., 2024) with saltmarsh areas highlited by yellow contours. Reference sites: Vegetated Sediment (VS), and Unvegetated Sediment (US); Impacted sites: Southern area landward of the breakwater (LBWS), Northern area landward of the breakwater (LBWN); and Intermediate site: Seaward of the breakwater (SBW). (a) © Stamen Design (CC BY 4.0) / © Stadia Maps; © OpenStreetMap contributors (ODbL), (b) © Gouvernement du Québec (CC BY 4.0; MNRF 2025).*

RC1-30: L. 102: what is the trophic status of the bay, resp. what is the nutrient load of the river / does it carry a significant nutrient load into the bay? This might not be essential for your study, but it gives some background understanding, especially as you mention coastal eutrophication in your introduction.

AC-R1-30: The river delivers a low to moderate nutrient load under mean conditions and is unlikely to drive eutrophication in the bay. Nevertheless, episodic high-discharge events, typically during spring freshet, may represent significant short-term nutrient inputs. Mean annual concentration have been added to the text to provide background context:

"*The Rivière-du-Loup embayment receives freshwater inputs from a river located in its northern part, with a mean annual discharge of 14.7 m3 s-1 (from 3.4 to 141.1 m3 s-1) and mean annual concentrations of 2.7, 13.7 and 0.5 µmol L-1 for NH_4^+, NO_3^- and PO_4^(3-), respectively.*"

RC1-31: L. 118-119: maybe one time "intertidal" is enough.

AC-R1-31: We agree and have removed the multiple "intertidal" words.

RC1-32: L. 129: the sediment cores were 15 cm long but the acrylic liners were 30 cm long- was the leftover 15 cm then overlaying water or air? Please clarify.

AC-R1-32: Sediment collection was performed at low tide, therefore, the liners were filled with 15 cm of sediment and the remaining headspace was air. The sediment surface was then capped with Styrofoam® before adding overlying water collected separately from the bay. The sentence has been modified and should read as follow:

"At each of the five sites, eight large sediment cores (10 cm diameter, ~ 15 cm of sediment and ~ 15 cm of air) were manually collected at low tide using transparent acrylic liners (30 cm long), with compaction < 0.5 cm. The sediment surface was immediately capped with Styrofoam® to prevent disturbance during transport."

RC1-33: L. 141 ff: you say that sediment characteristics were analyzed from the small cores taken at each location, but in the following sentences you specify to "analyzed in one sediment core"- this is a bit confusing (also, which core was chosen?), but as you have a separate chapter dealing with your replicates, maybe you could reformulate the sentence here a bit and refer to the other chapter?

AC-R1-33: This paragraph has been rewritten to make it clearer and now reads as follows:

"2.4 Characterization of the sediment

Sediment characteristics were analyzed using samples from the top layer (0 - 0.5 cm) of the five small sediment cores collected at each location. One core was dedicated to measuring sediment grain size (D50), porosity (ϕ), and organic carbon (Corg) and total nitrogen (TN) contents, as well as Corg natural isotope composition (δ^13 C_org). From this core, D50 was assessed by laser diffraction (Malvern Instruments©, 2 µm detection limit) after OM digestion with hydrogen peroxide and porosity was determined by measuring water loss after freeze-drying, corrected for sea salt content, using a dry sediment density of 2.65 g cm-3. Prior the Corg and δ^13 C_org analyses, inorganic carbon was removed by fuming of sediments in a closed container in the presence of an open beaker of concentrated HCl. Elemental characteristics of the OM, (i.e., Corg, TN) of the sediment were determined using an elemental analyzer (Carlo Erba© NC2500). The δ^13 C_org was then measured using a Micromass Isoprime 100 isotope ratio mass spectrometer coupled to an Elementar Vario MicroCube elemental analyzer. Carbon isotope ratios are reported using the delta notation, relative to the Vienna Pee Dee Belemnite standard. The remaining sediment from this first core, along with the other four cores, was used to determine the OM content via loss on ignition (LOI, n=5), as the weight loss after combustion at 550 ˚C during 4 h. Due to storage failure, LOI was not assessed at US.”

RC1-34: L. 144: could you add a small description after LOI like you did for porosity, i.e. “weight loss after combustion”?

AC-R1-34: This has been added (see text in **AC-R1-33**)

RC1-35: L. 148: do you mean “natural” rather than “stable”? While it’s both, it’s commonly called natural δ13C abundance.

AC-R1-35: “stable” has been replaced by “natural”

RC1-36: L. 162: please add the meaning of the variables.

AC-R1-36: The meaning of the variable in Eq. 1 has been added:
“*where ϕ is the sediment porosity, D0 the diffusion coefficient (m2 h-1) and δC/δz is the concentration gradient (mmol m-3 m-1)*”

RC1-37: L. 233: I feel this chapter is not needed but the given information could be integrated in the previous chapters, such as line 237 ff would fit better after l. 190.

Also, the information from l. 239 should come earlier, as the result (no DNRA) was already given in l. 202.

AC-R1-37: This paragraph was originally isolated to consolidate all analytical details within a single section. However, we agree that this may have appeared redundant with information provided earlier in the text. Following the reviewer's suggestion, we have moved the relevant analytical details to their respective sections where the corresponding experiments are described and removed the "Sample analyses" section accordingly.

RC1-38: L. 241: hypobromite *iodine*?

AC-R1-38: Yes, this has been added

RC1-39: L. 286 ff: was the difference statistically significant?

AC-R1-39: This point required clarification. We did not report formal statistical results for sediment characteristics in the manuscript because replication levels were uneven among variables and stations, as explained in in the methods (Section 2.10). However, we did performe statistical analyses where replication allowed. For LOI, replication was sufficient ($n = 5$) to support a significant difference between the impacted sites (LBWN and LBWS) and the reference stations (SBW and VS) with $p < 0.01$. For D50, the low replication level ($n = 2$) limited the statistical power to reliably assess pairwise differences although the main test showed the presence of significant differences between stations ($p < 0.05$). Therefore, for consistency across sediment characteristics, we chose not to report these statistical results in the manuscript and instead used descriptive wording such as “showed” or “tended to”.

RC1-40: L. 288: “TS” probably means “TN”

AC-R1-40: Yes, this has been corrected

RC1-41: L. 301 ff: three times “notable / notably” in three consecutive sentences is a bit too much.

AC-R1-41: We agree and this has been correct

RC1-42: L. 365: what “remains limited”: the effect of artificial structures on ecosystems or the amount of studies dealing with that? Please reformulate.

AC-R1-42: We agree the original phrasing was ambiguous. The sentence has been reformulated to clarify that it is the scientific knowledge on this topic that is currently limited, not the effect itself. "Remains limited" has been replaced by "remains poorly documented" to remove this ambiguity.

RC1-43: L. 383: Please reformulate this sentence, as coupled nitrification-denitrification cannot have been enhanced by rhizone oxidation, as you used different cores for rate measurements and porewater sampling.

AC-R1-43: The reviewer is correct that porewater profiles and denitrification rate measurements were obtained from different set of cores, preventing a direct mechanistic link. We have reformulated the sentence to present the geochemical profiles (NO3 and Fe_asc) as independent corroborating evidence consistent with rhizosphere-driven coupled nitrification-denitrification, rather than as a direct explanation for the incubation results. The causal language has been replaced with an interpretive one. This part now reads as follow:

"*This consumption results from the denitrification process, which has been detected at all sites (Fig. 6). VS exhibited a subsurface NO_3^- peak, suggesting active nitrification consistent with rhizosphere oxygenation (Koop-Jakobsen et al., 2018), this pattern is consistent with the relatively high proportion of coupled nitrification-denitrification detected in incubation experiment. This interpretation is further corroborated by the solid-phase ascorbate extractable iron (Feasc) profiles (Fig. 2d). At VS, Feasc increase progressively from the surface to a peak at 3-4 cm depth, well below the NO_3^- maximum (Fig. 2c), suggesting subsurface oxidation consistent with rhizosphere oxygenation. The absence of NO_3^- peak co-located with the Feasc peak suggests a tight coupling between nitrification in the rhizosphere area and denitrification in adjacent anoxic sediment (Koop-Jakobsen and Giblin 2010, Zheng et al. 2016).*"

RC1-44: L. 387: the impacted sites had also higher organic matter, so it makes sense that they have higher NH4 accumulations in porewaters.

AC-R1-44: We agree with the reviewer. The text has been amended to explicitly acknowledge that the higher NH4 accumulation observed in porewater at impacted sites is consistent with their higher organic matter content, which fuels greater ammonification. The sentence now reads:

“*Similarly, deeper sediment layers in impacted sites exhibit greater increases in NH_4^+ and PO_4^{3-} compared to reference sites, indicating larger ammonification driven by their higher organic matter content, along with PO_4^{3-} release, typically associated with the reductive dissolution of iron-oxyhydroxides in anoxic sediment layers (Slomp, 2011). Consistently, Feasc concentrations at impacted sites are highest at the sediment surface and decrease rapidly with depth (Fig. 2d). This indicates that aerobic processes, including nitrification, are restricted to the very surface of the sediment at these sites.*”

RC1-45: L. 390 ff: I slightly disagree with the argument / conclusion here, as earlier you suggested that nitrogen-cycling was not largely affected by the change in sediment biogeochemistry.

AC-R1-45: We agree with the reviewer. The original conclusion was inconsistent with our results, as denitrification rates were not substantially altered at impacted sites despite changes in sediment biogeochemistry. We have amended the text to reflect this, replacing the speculative statement about potential implications for nitrogen removal capacity with a conclusion grounded in our findings. The text now reads as follow:

“*Overall, these O_2 and nutrient profiles suggest more reduced conditions and greater nutrient accumulations in the sediment column in impacted sites compared to reference sites. Despite these changes in sedimentary conditions, denitrification rates were not substantially altered, suggesting that the capacity of these sediments to remove excess nitrogen remains resilient to these perturbations.*”