Revision of *Egusphere-2026-959*, point-to-point replies of changes made

Line numbers refer to the revised document with visible track-changes.

Replies to referee #1

Considering the very long time the Baltic Sea needs to recover, it would be an improvement of the manuscript if the authors could include a section giving their view on the suitability of sea-based methods (often also called ecological engineering) to improve the environmental status of the Baltic Sea or at least the coastal part of the Baltic Sea. Would, according to the authors, for example oxygenation (in one way or the other) faster lead to improvements than only reduction of external nutrient loads? Are there any other sea-based methods that the others favour or recommend? Considering that the authors' model results suggest so very strong persistence of the legacy OM and ammonium, one would expect that the authors add this section and respond to the questions I have raised. I am sure that most future readers of this manuscript have the same questions.

Response: We followed the reviewer's suggestion and added a section on artificial aeration (**chapter 5, lines 532 ff).** It reads: "*Following OM load reductions, NH_4^+ effluxes in our model decrease slightly faster and with a smaller flux magnitude under normoxic than under O_2-deficient conditions (Fig. 4b), which might raise the question whether artificial aeration methods could be a justifiable option to aid the ecosystem recovery. However, the effects of artificial aeration on benthic N-cycling in the coastal Baltic Sea are not straight forward and sediment-water NH_4^+ effluxes both did (site Länneröstasundet, Lehtoranta et al., 2022) and did not (site By Fjord, De Brabandere et al., 2015) significantly decrease under aeration, or even increased (site Sandöfjärden, Lehtoranta et al., 2022) due to aeration induced sediment warming and a return of anoxia despite aeration. Similar conflicting results were found also outside the coastal Baltic Sea (e.g. Harris et al., 2015, Testa et al., 2025), including increased air-water methane exchange under conditions of aeration (Lapham et al., 2022), which suggests that engineered aeration approaches often have unexpected and unintentional impacts on both nutrient cycling and greenhouse gas emissions. Given these variable responses, we do not consider artificial aeration to be a clear-cut solution to shorten the duration of NH_4^+ effluxes in the coastal Baltic Sea.*"

The model includes different levels of organic loading, but I did not notice whether you discussed variable C/N ratios of the deposited organic matter. Would doing that improve the model predictions?

Response: The model uses three organic matter (OM) pools: a reactive pool with a molar carbon/nitrogen (C/N) ratio of 6.6 representing marine-derived fresh OM (62% of deposited OM), a less reactive pool with a molar C/N ratio of 13 representing terrestrial-derived OM (15% of deposited OM), and a non-reactive pool that gets buried directly without degradation (23% of deposited OM), according to model calibration data from Jilbert et al. (2021). We added this information to the model-description (**lines 197 ff**). As different combinations of these OM-shares can change the amount of released ammonium, the magnitude of calculated ammonium pool and efflux may slightly differ if we would re-calibrate the model using geochemical profiles from other coastal areas. We further explore such effects under different boundary conditions, including different combinations of C/N ratios and different reactivities of OM loaded in sediments, in our sensitivity analysis (**chapter 4**, **lines 443 ff., figures 6, B2**).

Line (L) 20: "… *accumulated via OM burial and mineralization*…"; Please instead write "…accumulated via OM **deposition** and mineralization…". The term burial is often used to reflect the ultimate removal of a substance from further (re)cycling.

Response: We revised the sentence according to the reviewer's suggestion (**line 20**).

L 40 and elsewhere: "*...phosphate (PO_4^{3-})..*."; PO_4^{3-} is not the main species of dissolved inorganic P in seawater. Please instead use Dissolved Inorganic Phosphorous (DIP) – here and everywhere in the manuscript.

Response: We revised the terminology according to the reviewer's suggestion (**lines 54 ff**).

L 45-46: *…PO_4^{3-} due to its crucial role in the repeated occurrences of massive cyanobacteria blooms and metal recycling…*; Do you mean the role of metal recycling in the dynamics (retention and release) of PO_4^{3-} (or DIP)? If so, this is not what you wrote.

Response: We edited the sentence as following: "*Research on internal nutrient loading has often focused on DIP due to its crucial role in the repeated occurrences of massive cyanobacteria blooms and close coupling to metal recycling."* (**lines 58 ff**).

Figure 1: There are more published papers on NH4 effluxes under varying bottom water oxygen conditions in the Baltic Sea than those cited in Figure 1. Since this manuscript focuses on the Baltic Sea, I suggest that the authors are more comprehensive in this regard and cite more of these papers.

Response: We searched for published benthic ammonium flux data under varying bottom water oxygen conditions using web-of-knowledge and the search criteria "benthic ammonium flux" and "Baltic Sea". Subsequently, we selected the coastal studies manually by going through the results, so as not to miss studies that are coastal but might use another wording, e.g. fjord or estuary. Only studies from the coastal Baltic Sea including Kattegat (coastal zone defined as 1 nautical mile from land, following the European Water Framework Directive) that were derived from actual flux incubations, using sediment cores or benthic chambers, and provided ammonium flux results under varying oxygen conditions were included. While there is a large pool of studies conducted in the open waters of the Baltic Sea, studies from the coastal zone are much fewer and only include the ones we show in Figure 1. We repeated our literature search in early summer 2026 to include new papers that were published recently but did not find any new studies that match above criteria. If the reviewer has specific papers in mind that we missed, we are happy to include them in our compilation.

L 77-78: "*This is not only a larger pool of bioavailable N than the pelagic pool of dissolved inorganic nitrogen (DIN) in the coastal Baltic Sea…";* Please explain how you define the **coastal** Baltic Sea, preferably the first time you mention the coastal Baltic Sea.

Response: We defined the coastal area of the Baltic Sea according to the definition of "coast" given by the European Water Framework Directive; the direct wording of the implementation strategy is *"'Coastal water' means surface water on the landward side of a line, every point of which is at a distance of one nautical mile on the seaward side from the nearest point of the baseline from which the breadth of territorial waters is measured, extending where appropriate up to the outer limit of transitional waters"* (European Communities: Common implementation strategy for the water framework directive (2000/60/EC), guidance document no 5, transitional and coastal waters – typology, reference conditions and classification systems, working group 2.4 – COAST, ISBN 92-894-5125-4, 2003).

We added the following information to the text at the first mentioning of 'coast': *"...coastal Baltic Sea (defined here as the coastal areas of the Baltic Sea extending one nautical mile seaward from territorial water baselines, following the Water Framework Directive; European Communities, 2003)"*. (**lines 63 ff**)

L 111: "*…and thus available…*"; Should be: …and **is** thus available…

Response: We edited the sentence according to the reviewer's suggestion (**line 131**).

L 124-125: "*Only a small part of water column NO3- is assumed to diffuse back into the sediments, as coastal water turbulence is stronger than diffusion."* I do not think that this is correct. The nitrate formed during nitrification in the oxygenated bottom water may very well be taken up by the reduced sediment surface and consumed by either denitrification or DNRA – regardless of turbulence. Please rewrite or at least explain yourself in greater detail.

Response: We agree with the reviewer that nitrate can diffuse back into the sediment. However, being subject to a dynamic coastal system (including vertical water mixing by wind, waves, upwelling and density-differences), it can also be displaced to other water layers and subsequently be taken up in primary production. Thus, it cannot be assumed that all nitrate that was formed from benthic ammonium release will stay in the bottom water and diffuse back into the sediment. We reformulated the sentence as following: *"Some of this water column NO_3^- can be expected to diffuse back into the sediment where it can be removed in denitrification or transformed to NH_4^+ in dissimilatory nitrate reduction to ammonium (DNRA); however, being subject to a dynamic coastal system, the additional N might also be quickly exported to other water layers and, for instance, taken up in primary production."* (**lines 145 ff**)

L 125-127: "*In the absence of hydrogen sulfide (H2S), this NO3- can be reduced to N2, while in the presence of H2S dissimilatory nitrate reduction to ammonium (DNRA) dominates, further enriching the NH4+ pool";* Please be more specific and correct: Denitrification **can** proceed with H2S as electron donor. Also, please provide references stating that DNRA dominates (over denitrification) in the presence of H2S.

Response*:* We agree with the reviewer that denitrification can proceed in the presence of H_2S, albeit at often much lower rates than in the absence of H_2S (e.g. Jäntti et al. 2021), assuming that the denitrifiers are not chemolithotrophic, which in coastal, organic-rich sediments they commonly are not. DNRA can proceed at much higher rates than denitrification when H_2S is present (as long as NO_3^- is available), thus it dominates N-turnover under those conditions (Jäntti et al. 2021).

We reformulated the sentence and added a reference as following: "*If hydrogen sulfide (H_2S) builds up in the surface sediment, DNRA commonly dominates over denitrification (Jäntti et al., 2021), further enriching the NH_4^+ pool*." (**lines 148 ff**)

L 281-287: In sediments with high organic loading, can an abundance of sulphide explain low rates of nitrification, and hence high ammonium effluxes, even under normoxic bottom water conditions?

Response: Our model includes relevant sulfide processes; sulfide can be either consumed by reduced iron or oxidized by oxygen. The latter can lead to low rates of nitrification, but the degree of this impact is largely dependent on the availability of oxygen and iron oxide. Please note that all of these processes are tightly linked to the amount of organic matter available for degradation, and our model outputs are the integrated net effects of all of these processes on ammonium efflux. Further, please note that due to a small error in the model code (please see explanation further below), we re-run the model and updated its calibration using data published in Jilbert et al (2021), which changed the results. Oxic and anoxic ammonium effluxes now follow a more common pattern, with low fluxes under normoxic conditions (both under high and low organic loading).

L 335-336: "*For instance, a higher salinity is often found in open coastal systems*;…" Is it? If so, why? And higher than where? I do not understand what you mean. A clarification is absolutely necessary.

Response: With "open coastal systems" we meant coastal systems with free water exchange, which are mainly at an oceanic coast and thus high saline, as opposed to a semi-enclosed systems with limited water exchange such as the Baltic coastal zone. However, this section has been re-written and the term "open coastal systems" does not occur anymore. (**lines 455 ff**).

Replies to referee #2 (Mark McCarthy)

Line 41: Is 'consumption' the best word choice here for NH4, assuming that the authors are referring to nitrification? Perhaps 'transformation' would be better, since 'consumption' might imply assimilation into biomass, rather than transformation to NO2/NO3?

Response: We agree with the reviewer that "consumption of ammonium" might imply assimilation into biomass. We changed the word to "turnover", which is close to the reviewer´s suggestion of "transformation" (**line 54**).

Line 47: "This is a critical oversight…" --- here and throughout the manuscript (e.g., Lines 77, 95, 138, 249, 257, 273, 281, 303, 351…I've likely missed others), I suggest specifying what 'This' refers to. In this case (line 77), perhaps something like "This omission is…".

Response: We revised the use of "this" as suggested by the reviewer, see e.g. **lines 61, 95, 113, 163.**

Lines 93-95: Consider changing 'most' to something like 'a substantial proportion' or 'much'. I think that there are enough exceptions to this pattern to avoid using 'most' here.

Response: We edited the text according to the reviewer's suggestion (**line 111**).

Lines 96-97: Consider revising to something like: "Microphytobenthos, if present, can intercept and assimilate nutrients effluxing from sediments."

Response: We edited the sentence following the reviewer's suggestion (**lines 114 ff**).

Line 98: Consider a slight qualification to: "…leaving only residual fluxes originating from, for example, faunal activity", since there are likely additional explanations for any 'residual' flux.

Response: We edited the text according to the reviewer's suggestion (**line 117 ff**).

Figure 3 caption: I suggest including a citation for the statement about the effects of H2S on the balance between denitrification and DNRA. For example, Murphy et al. 2020 (Env Microbiol 22(6):2124-2139), but there are others.

Response: We added Jäntti et al. (2021) to the caption of figure 3 as reference for the effect of hydrogen sulfide on the balance between denitrification and DNRA (**line 149**).

Line 242: "…regenerated from the in sediment…" --- needs editing.

Response: We edited the sentence as following: "*This legacy effect is caused by NH_4^+ being continuously regenerated from the OM that accumulate in the sediment over subsequent years of strong eutrophication*." (**line 325 ff**).

Lines 281-283: Does nitrification lose its function, or is it functioning at its maximum (for whatever reason) under NH4-replete conditions? Denitrification efficiency (proportion of external N load removed via denitrification) often decreases with higher external N loads (e.g., Gardner & McCarthy 2009; Biogeochem 95:185-198), but denitrification is still occurring, presumably at or near its maximum under the conditions. So, it hasn't lost its function, but its impact on the system might be less. Perhaps some clarification is needed to distinguish whether nitrification is shutting down (losing its function) or if it is functioning at a maximum level for the conditions, but perhaps unable to keep up with OM remineralization, as implied in the text.

Response: We agree with the reviewer that nitrification is not shutting down as such but rather becomes overwhelmed / unable to keep up with the amount of available ammonium. Please note that due to a small error in the model code (please see explanation further below), we re-run the model and updated its calibration, which changed the results. This text part is not relevant any longer as now oxic and anoxic NH_4^+ fluxes follow a more common pattern, with low oxic fluxes.

Lines 302-304: If the modeled anoxic NH4 efflux is lower than it can be in nature, then would the timeline for a return to pre-industrial conditions potentially be over-estimated? The effects of climate change were not pursued in this study, but I wonder about the possible effects of sea level rise (e.g., Kapsi et al., 2023; J Mar Sci Eng 11(8):1514). Would sea level rise perhaps improve redox conditions in coastal areas of the Baltic Sea (or maybe even the opposite), and if so, would the timeline for return to pre-industrial conditions perhaps be over-estimated? (I'm fishing for hope here!).

Response: Please note that due to a small error in the model code (please see explanation further below), we re-run the model and updated its calibration using data published in Jilbert et al (2021), which changed the results. The main difference to the previous results is a smaller legacy effect in the NH_4^+ effluxes and thus a shorter duration until return to pre-eutrophication conditions can be reached, if strict reductions of external OM loading are implemented. This result will hopefully restore the hope of the reviewer that ecosystem recovery of strongly eutrophied systems is indeed possible in realistic time-frames. The underestimation of the anoxic ammonium fluxes in our model, however, remains as it originates from the resolution of the model. The RTM runs on an annual resolution and thus cannot include individual seasonal or extreme events, which are, however, included in the field data. The difference between oxic and anoxic conditions in returning to pre-eutrophication conditions is approximately 30 years, which would likely be in a similar range if our anoxic fluxes were not slightly underestimated.

Regarding the effects of climate change, we consider a climate-driven increase in bottom water temperature in the model (see **lines 250 ff**), whereas assessing more complex ecosystem effects of climate change on our modeling outcome is beyond the scope of our study. Sea level rise, as asked for by the reviewer, is commonly considered to increase nutrient leakage from land to sea, which can stimulate coastal primary production and increase the potential for organic matter loading to the sediment. In such case, our estimated timeline for recovery would be underestimated. However, as sea level rise does not occur in isolation but in a complex web of effects (e.g. Helcom & Baltic Eart 2021), we cannot give a certain answer without further modelling.

Line 345: Consider softening the language a bit here. For example, I'm not sure that you've 'demonstrated' anything here by using a model…demonstration probably requires empirical studies, direct flux measurements, etc. I suggest editing to: "Modeling results suggest that the eutrophication-derived OM pool in the sediments of the coastal Baltic Sea is likely the key factor that continuously maintains…".

Response: This text part does not exist any longer in the updated manuscript version.

Line 349: "…continuously new deposited OM…" --- Consider editing to "…continuously-deposited, new OM…"

Response: This text part does not exist any longer in the updated manuscript version.

Line 372: I'm not sure that I fully agree with this statement. Sure, we anticipate that distance from OM/nutrient source (e.g., tributary discharge) is a factor in how severe the impacts might be (in terms of your modeling results), but without empirical data/direct measurements of fluxes across the sediment-water interface along some OM/nutrient gradient, then it seems too big of a leap to suggest only localized effects. The model also does not consider internal N loading from the water column, which may be much larger than from sediments, or even larger than external N loads (e.g., Lake Erie;

Hoffman et al. 2022; L&O 67:2028-2041). A few sentences prior, the coastal current is mentioned as a conduit for nutrients and OM to be deposited in the open Baltic Sea, which might argue against the localized effects speculation? Basically, I don't think that the speculation is incorrect, but I'm not convinced that the evidence from this study is comprehensive enough to fully support the speculation. I think the text here could be rewritten in a softer way, which still allows you to make the point about possible localization of effects, but doesn't sound more definitive than can be justified (e.g., "…the impact of internal N loading, from sediments represented in our modeling study, on the coastal ecosystem may be localized, as despite…").

Response: We softened the language of the paragraph as suggested by the reviewer (**lines 525 ff**).

Replies to both referees and the associated editor

During the revision of our manuscript, we identified an error in our model-code of downcore porosity-related corrections for porewater and solid burial rates, which directly affected our calculations of the NH_4^+ pool and efflux. We corrected the error, and subsequently re-calibrated and re-run the model. The main changes to the model are:

- The use of a measured porosity profile from our model-site *Storfjärden* (Jilbert et al. 2021) adjusted to account for downcore sediment compaction and burial rates (previously, we used a constant porosity value).
- An improved solid-to-liquid ratio of downcore porewater fractions.
- A re-calibration of the model using (i) field-measured porewater values from Jilbert et al. 2021 and (ii) OM loading values that can create the eutrophication signal that we see in the *Storfjärden* sediment, i.e. OM loading is prescribed based on BALTSEM model output, but the magnitude is increased by 3-4 times, in order to fit the downcore OM profiles observed at *Storfjärden*, which we early on decided to use as a representative example site of an OM-rich accumulation bottom. The calibration details are given in **Appendix A** of the manuscript.

After the re-calibration, the model was run with OM values derived from the BALTSEM model as before (two loading scenarios), which makes it less *Storfjärden*-specific but broadly applicable to generally muddy, OM-rich sediments of the coastal Baltic.

The main new model results are:

- A much more pronounced differentiation between oxic and anoxic NH_4^+ effluxes (Fig. 4 in the manuscript), in line with field observations throughout the coastal Baltic (Fig. 1 in the manuscript)
- A strong eutrophication legacy effect in the sediment NH_4^+ pool, that leads to a long-lasting legacy effect in the sediment-water NH_4^+ effluxes, which, however, has only a small effect size. The magnitude of the legacy NH_4^+ effluxes can however increase under increased OM loading as well as anoxic conditions. Overall, sediment-water NH_4^+ effluxes respond more closely to the external OM loading than to the sediment NH_4^+ pool, which enables ecosystem recovery in realistic time-frames, if external OM loading is strictly reduced (and bottom waters stay oxic and sediments stay bioturbated).

We sincerely apologize for the overhaul of the results, but the corrections of the model code improve the robustness of the RTM and in turn bring more confidence in our findings. Afterall, the new results are not too far off from the previous manuscript version, except that the modeled NH_4^+ effluxes are now more realistic, and the legacy effect is milder. We further included more parameters in our sensitivity analysis, such as presence/absence of bioturbation and C/N ratio of OM, to widen the applicability of our results. We updated all model-related figures, appendixes, and ZENODO data files.

Replies to associate editor (Perran Cook)

I have now read the manuscript and have focused a little more on the model. I would like to have seen to more information on the calibration and validation of the model. I looked at the supplementary material on zenodo, but this did not really help my questions.

Response: We added a chapter on calibration and validation of the model to the **Appendix A (Fig. A1-A3, lines 622 ff)**. The model is calibrated and validated with field-data from the muddy accumulation site *Storfjärden*, located at the coast of the north-western Gulf of Finland. *Storfjärden* is an observation-rich site that is commonly regarded as representative for an OM-rich, muddy accumulation site in the coastal Baltic Sea. The specific field-data for calibration and validation, such as sediment and OM characteristics and porewater constituents, are taken from Jilbert et al. (2021).

Of key concern the model seems to produce similar NH4+ sediment effluxes with anoxic and oxic conditions. This does not make sense to me and disagrees with the data in figure 1, which shows a much larger effect of O2 (almost a factor of 10 going from 0-100%) and seems to suggest the magnitude of the modelled fluxes is much lower than real data. This point is critical because it is central to the finding that sediment ammonium pools (and hence effluxes) have a very long legacy effect.

Response: After fixing the error in our model-code, the modelled NH_4^+ effluxes are very distinct between oxic and anoxic conditions (see **Fig. 4, line 272**), in line with **Fig. 1 (line 72**). The magnitude of the modelled oxic NH_4^+ effluxes is in the same range as the field-observations from the coastal Baltic Sea (**Fig. 1**, **line 72**). The magnitude of the modelled anoxic NH_4^+ effluxes is smaller than what can be observed in the field (**Fig. 1**, **line 72**), which is a result of the resolution of our model: the RTM runs on an annual basis and thus does not capture seasonal fluctuations and anoxic extreme events that can drastically increase the NH_4^+ efflux. Such events are generally challenging to incorporate in a model, as they originate from individual combinations of OM loading and loading history, anoxia duration, temperature, and more. Without these extreme values adding to the model´s annual average, the modelled anoxic NH_4^+ effluxes are lower than they can periodically be in nature. This underrepresentation of seasonal extreme values in our model affects only the anoxic and hypoxic, but not the normoxic fluxes, and it likely does not greatly affect the observed legacy period in the NH_4^+ effluxes, which shows a clear difference between oxic and anoxic conditions. We discuss this issue in the manuscript (**lines 340 ff**).

I would like to see some information on how well the model reproduces ammonium effluxes in comparison to data at representative sites set up with relevant carbon input rates and water column O2.

Response: As described above, the model now reproduces NH_4^+ effluxes, that (i) under oxic conditions are in the correct range of field-observations throughout the coastal Baltic Sea (including observations from Denmark, Sweden, and Finland; **Fig. 1, line 72**), and (ii) under O_2-deficient conditions are lower than they can periodically be in nature, due to the annual resolution of the model. Modelled oxic and anoxic fluxes are, however, distinctly different, which is in agreement with field-data (**Fig. 1, line 72**) and the common understanding of increased NH_4^+ efflux under anoxic conditions (**Fig. 3, line 134**).

I would also like to see how the model reproduces key aspects of the nitrogen cycle including NO3- efflux and denitrification at different oxygen concentrations and how this broadly matches with literature data.

Response: Please find below the downcore NO_3^- concentration and denitrification rate (Fig. a) and the NO_3^- flux (Fig. b) from our model, using the calibration results based on Jilbert et al. (2021; see **Appendix A** in the manuscript). Downcore

denitrification starts just below the NO_3^- depth in the top surface sediment, once the oxic surface layer, where NO_3^- is present, is depleted. Denitrification rates reach zero, once NO_3^- is depleted (Fig. a). Modeled porewater NO_3^- concentrations are similar to measured porewater NO_3^- concentrations from a site in the vicinity of the model site *Storfjärden* (~2 µM in 0-1 cm surface sediment in summer months, site *Långholmsbranten*, Hellemann unpublished). The modeled NO_3^- fluxes are negative (Fig. b), indicating an inflow of NO_3^- from the bottom water into the sediment and a net consumption of NO_3^- in the sediment (denitrification). This is similar to field data from the modelling site *Storfjärden*, where NO_3^- fluxes were measured over the course of 1 year and ranged from inflow (i.e. negative flux) to maximal 0.05 mmol m^{-2} d^{-1} outflow, depending of course on the bottom- and porewater NO_3^- concentration at the time of sampling (seasonal differences, Kauppi et al. 2017). Our model's bottom water NO_3^- concentration is 7 µM, which is in the range of an annual average at the modelling site (Hellemann et al. 2020).

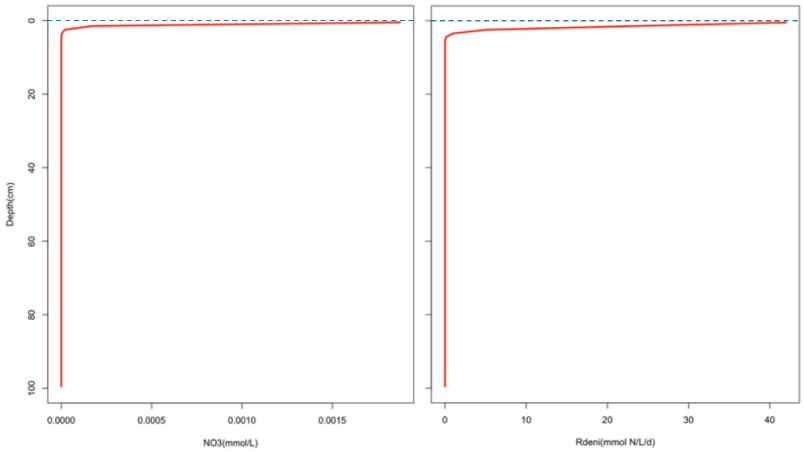


Fig. a: Downcore NO_3^- concentration (left) and denitrification rate (right) along the RTM's sediment model domain (1-m domain core). Broken line gives the sediment surface (0 cm depth).

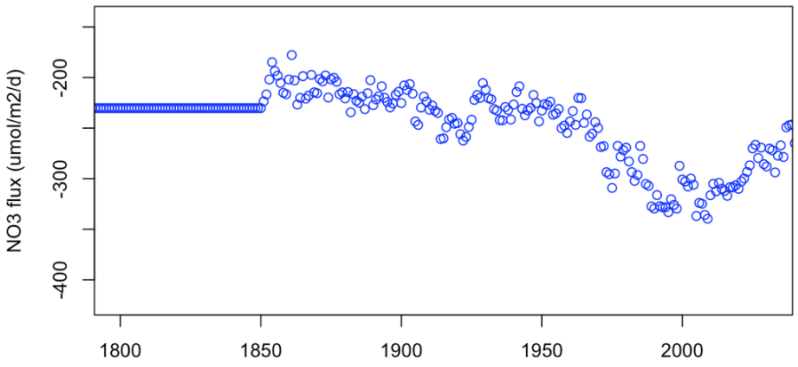


Fig. b: Sediment-water NO_3^- flux from the RTM over time.

Hellemann, D., Tallberg, P., Aalto, S., Bartoli, M., and Hietanen, S.: Seasonal cycle of benthic denitrification and DNRA in the aphotic coastal zone, northern Baltic Sea, Mar. Ecol. Prog. Ser. 637, 15-28, 2020.

Kauppi, L., Norkko, J., Ikonen, J., and Norkko, A: Seasonal variability in ecosystem functions: quantifying the contribution of invasive species to nutrient cycling in coastal ecosystems, Mar. Ecol. Prog. Ser. 572, 193–207, 2017.

Furthermore, I disagree with your rebuttal of reviewer 1s request to consider the importance of artificial oxygenation. Given that you explicitly simulate high O2 scenarios, I suggest there should be some synthesis of these findings with studies and opinion pieces on artificial oxygenation.

Response: We agree with the editor and added a section on artificial aeration **(chapter 5, line 532).** It reads: *Following OM load reductions, NH_4^+ effluxes in our model decrease slightly faster and with a smaller flux magnitude under normoxic than under O_2-deficient conditions (Fig. 4b), which might raise the question whether artificial aeration methods could be a justifiable option to aid the ecosystem recovery. However, the effects of artificial aeration on benthic N-cycling in the coastal Baltic Sea are not straight forward and sediment-water NH_4^+ effluxes both did (site Länneröstasundet, Lehtoranta et al., 2022) and did not (site By Fjord, De Brabandere et al., 2015) significantly decrease under aeration, or even increased (site Sandöfjärden, Lehtoranta et al., 2022) due to aeration induced sediment warming and a return of anoxia despite aeration. Similar conflicting results were found also outside the coastal Baltic Sea (e.g. Harris et al., 2015, Testa et al., 2025), including increased air-water methane exchange under conditions of aeration (Lapham et al., 2022), which suggests that engineered aeration approaches often have unexpected and unintentional impacts on both nutrient cycling and greenhouse gas emissions. Given these variable responses, we do not consider artificial aeration to be a clear-cut solution to shorten the duration of NH_4^+ effluxes in the coastal Baltic Sea.*