Response to referee comments to *Egusphere-2026-959*; referee #2 (Mark McCarthy)

Hellemann & Sun et al. report results from a 1-dimensional reactive transport model (RTM) for coastal areas of the Baltic Sea aimed at understanding internal ammonium (NH4) loading from sediments under different organic matter loading scenarios. The topic of internal nitrogen loading in aquatic systems, especially in non-oceanic systems, is indeed very much under-studied and under-appreciated, so the study is timely and novel. Overall, the study represents an important contribution to our understanding of internal nitrogen dynamics and the magnitude of nutrient legacies accumulated in coastal sediments. Unfortunately, the study also shows that mitigating these legacies may take centuries of reducing external loading, even after a return to pre-industrial loading levels. I fear that the response of resource managers/politicians, etc., will be to throw up their hands and say that any economic costs of reducing external loads are not worth the benefits, since those potential benefits (which are not even certain to occur) likely won't manifest until after the effects of climate change are more clear (e.g., AMOC collapse, melting polar ice caps, sea level rise, etc.), global geopolitics are likely much different, who knows what global populations might be, etc. However, humanity needs to realize that past economic growth and industrialization came at a huge cost, and perhaps there are lessons to be learned for future generations.

Overall, my comments are relatively minor and should be easily implemented, and I support publishing the manuscript after completing some minor revisions.

Response: We thank the reviewer for his thorough evaluation of our work and his positive opinion of its importance to be published.

However, we need to admit that during the revision of our manuscript, we identified an error in our model-code, specifically in using downcore porosity to rectify solid-to-porewater reaction rates and sedimentation rates of sediment solids and porewater. After fixing the error, we carefully re-calibrated and re-run the model. The main conclusions remain similar and the major changes expected in the revised text will be the magnitude of pools and efflux, while other interpretations and conclusions still stand as they are. We are truly sorry for this mistake and apologize for these late changes. The details of the corrected model will be described in the supplementary material during the revision, and all figures will be updated.

SPECIFIC COMMENTS

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. The suggested expression "transformation of ammonium" is correct from the viewpoint of the nitrogen-cycle, however, it does not completely capture the biogeochemical viewpoint of ammonium availability in the top sediment. There, due to nitrification activity (transforming ammonium to nitrate), the ammonium availability is regulated and leads under oxic conditions to a depletion of ammonium. Hence, we will change "consumption" to "depletion" and hope that the reviewer agrees with this expression.

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 will revise the use of "this" as suggested by the reviewer.

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 will edit the text according to the reviewer's suggestion.

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

Response: We will edit the text according to the reviewer's suggestion.

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 will edit the text according to the reviewer's suggestion.

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 agree with the reviewer and will add a reference to the figure caption regarding the effect of hydrogen sulfide on the balance between denitrification and DNRA.

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

Response: We will edit the sentence as follows: "Such legacy effects are caused in the model by NH_4^+ being continuously regenerated from the OM that accumulated in the sediment over subsequent years."

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 this statement needs editing. Nitrification is not shutting down as such but rather becomes overwhelmed / unable to keep up with the amount of available ammonium. We will revise the text accordingly.

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: Despite the potential underestimation of the anoxic ammonium fluxes in our model, the subsequent timeline for a return to pre-industrial conditions is not overestimated, as it is driven by the benthic ammonium pool and its internal turnover, rather than the periodic outburst of extremely high ammonium.

Regarding the effects of climate change, we consider a climate-driven increase in bottom water temperature in the model (see lines 205-212), 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 (see, e.g. Helcom & Baltic Eart 2021), we cannot give a very 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: We will soften the language following the suggestion of the reviewer.

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

Response: We will edit the text according to the reviewer's suggestion.

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 will soften the language of the paragraph as suggested by the reviewer.