Author's Response

Manuscript Title: Seasonal variations and controlling factors of nitrogen fluxes at the sediment-water interface in a semi-enclosed inland sea

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Dear Editor,

We sincerely thank you and the reviewers for the careful evaluation of our manuscript and for the constructive comments and suggestions. We greatly appreciate the time and effort devoted to improving our work.

We have carefully considered all comments and revised the manuscript accordingly. In particular, we have substantially revised the sections Introduction, Discussion and Conclusions to strengthen the scientific significance and mechanistic interpretation of the study. The revised manuscript now places greater emphasis on the processes controlling seasonal variations in sedimentary nitrogen cycling, including the distinct responses of different nitrogen pools and fluxes to organic matter input, temperature, dissolved oxygen, and internal biogeochemical transformations. We have also clarified the mechanisms underlying the observed decoupling between particulate and dissolved nitrogen fluxes and highlighted the buffering role of sediments in regulating nitrogen exchange at the sediment–water interface.

In addition, we have improved the presentation of the study motivation and broader implications of the work, expanded the discussion of model uncertainties and limitations, clarified the description of the study area and model framework, and revised the manuscript throughout to improve clarity, organization, and readability.

We believe that these revisions have substantially strengthened the manuscript and addressed the concerns raised during the review process. Detailed responses to all comments are provided below, together with descriptions of the corresponding revisions made in the manuscript.

We thank the editor and reviewers again for their valuable comments and consideration.

Sincerely,

Xinyu Guo

Reviewer 1:

We sincerely thank the reviewer for the thorough evaluation of our manuscript and for highlighting several important concerns regarding the clarity, completeness, and scientific contribution of the study. We carefully considered these comments and recognized that some key concepts, model assumptions, and methodological descriptions were insufficiently explained or given in the manuscript, which may have obscured the scientific rationale and significance of our work.

In the revised manuscript, we have substantially improved the presentation and interpretation of the study. Specifically, we clarified and expanded the descriptions of model variables, boundary conditions, and biogeochemical processes; strengthened the mechanistic explanations of sediment nitrogen cycling and benthic DIN fluxes; and revised several statements that previously lacked sufficient justification or supporting assumptions. We also reorganized parts of sections Introduction, Results, and Discussion to improve the logical structure and readability.

Regarding the novelty of the study, we admit that the original manuscript did not sufficiently emphasize the scientific issues addressed by the model analysis. We therefore revised sections Introduction and Discussion to more clearly highlight that the study focuses not only on quantifying seasonal sediment-water nitrogen exchanges in a muddy sediment, but also on identifying the relative roles of bottom-water temperature, organic matter deposition, bottom stress, and sediment biogeochemical processes in controlling the magnitude, direction, and seasonal variability of various nitrogen fluxes. We believe these revisions have significantly improved the clarity and scientific value of the manuscript.

Detailed responses to each comment are provided below.

Comment 1:

Line (L) 51-52: *"Furthermore, nitrous oxide, a byproduct of denitrification…"*

N_2O is also a product of nitrification.

Response 1:

Exactly, N_2O is not only a product of denitrification, it is also produced during nitrification and DNRA (Quick et al., 2019). Our expression here was inappropriate and has been corrected.

Quick, A. M., Reeder, W. J., Farrell, T. B., Tonina, D., Feris, K. P., and Benner, S. G.: Nitrous oxide from streams and rivers: A review of primary biogeochemical pathways and environmental variables, Earth-Sci. Rev., 191, 224-262, 10.1016/j.earscirev.2019.02.021, 2019.

Revision in manuscript 1:

Section 1, Page 3, Line 50 – 52

Comment 2:

L 56-59*: "Direct observation of particulate organic nitrogen (PON) flux at this interface poses challenges due to the influence of resuspension processes. Consequently, numerous studies focused on DIN flux and its seasonal variations primarily through in situ observations, incubation experiments, or estimations based on Fick's law…"*

What has problems of estimating PON deposition, due to resuspension, to do with the DIN flux and its seasonal variations? Logics, and/or explanations, are lacking here.

Response 2:

The expression presented here appears somewhat abrupt, primarily due to the absence of transitional explanation. We want to clarify that PON flux measurement techniques are confined to in situ observations, with relatively limited studies. In contrast, DIN flux can be obtained not only through in situ methods but also via laboratory experiments, leading to more related research. Consequently, DIN has become a central focus in investigations of nitrogen fluxes at the sediment-water interface. This clarification has been incorporated into the original manuscript.

Revision in manuscript 2:

Section 1, Page 3, Line 55 – 61

Comment 3:

L 61-62: *"…that DIN flux exhibits seasonal variations, with higher levels in summer*

and lower levels in winter, likely influenced by water temperature…"

What about influence of seasonally varying deposition of reactive organic matter?

Response 3:

We agree that the original text may have overemphasized temperature as the sole explanation. The two referenced studies (De Vittor et al., 2012; Niemistö et al., 2018) do not explicitly indicate that seasonal variation in organic matter deposition directly influence DIN flux. Rather, their findings primarily suggest that elevated temperatures during summer enhance biological activity, which subsequently increases ammonium production and thereby elevates DIN flux. Notably, some literature has documented the stimulatory effect of organic matter on DIN flux (Albert et al., 2021; Ratmaya et al., 2022). This is actually a key issue discussed in this study, specifically addressing whether organic matter deposition or temperature serves as the principal factor of seasonal variations in DIN flux. The original manuscript did not sufficiently emphasize this point. Accordingly, we have revised the text to strengthen this aspect of the discussion.

De Vittor, C., Faganeli, J., Emili, A., Covelli, S., Predonzani, S., and Acquavita, A.: Benthic fluxes of oxygen, carbon and nutrients in the Marano and Grado Lagoon (northern Adriatic Sea, Italy), Estuarine, Coastal and Shelf Science, 113, 57-70, 10.1016/j.ecss.2012.03.031, 2012.

Niemistö, J., Kononets, M., Ekeroth, N., Tallberg, P., Tengberg, A., and Hall, P. O. J.: Benthic fluxes of oxygen and inorganic nutrients in the archipelago of Gulf of Finland, Baltic Sea – Effects of sediment resuspension measured in situ, J. Sea Res., 135, 95-106, 10.1016/j.seares.2018.02.006, 2018.

Albert, S., Bonaglia, S., Stjärnkvist, N., Winder, M., Thamdrup, B., and Nascimento, F. J. A.: Influence of settling organic matter quantity and quality on benthic nitrogen cycling, Limnol. Oceanogr., 66, 1882-1895, 10.1002/lno.11730, 2021.

Ratmaya, W., Laverman, A. M., Rabouille, C., Akbarzadeh, Z., Andrieux-Loyer, F., Barillé, L., Barillé, A.-L., Le Merrer, Y., and Souchu, P.: Temporal and spatial variations in benthic nitrogen cycling in a temperate macro-tidal coastal ecosystem: Observation and modeling, Cont. Shelf Res., 235, 10.1016/j.csr.2022.104649, 2022.

Revision in manuscript 3:

Section 1, Page 3 – 4, Line 61 – 67

Comment 4:

L 68-69: *"Moreover, the direction of DIN flux varies across different regions, with the underlying mechanisms remaining unclear…"*

Are underlying mechanisms really remaining unclear*?* In what way are they unclear. Please clarify.

Response 4:

Here, our description was overly simplified, resulting in unclear expression. What we actually meant to convey is that the directions of ammonium and nitrate fluxes vary across different regions, along with the factors that influence them, and that there is no broadly agreed-upon understanding based on mechanistic analysis. We have added and highlighted this point in the revised version.

Revision in manuscript 4:

Section 1, Page 4, Line 67 – 74

Comment 5:

L 146. *"…ODU (the oxygen demand units)…"*

What does the oxygen demand units mean? Do you mean sediment oxygen uptake?

Response 5:

Thank you for pointing this out. Oxygen Demand Units (ODU) do not represent sediment oxygen uptake (SOU). Following Soetaert et al. (1996), ODU is a lumped variable that represents the concentration of reduced compounds generated during anoxic mineralization (e.g., reduced Fe, Mn, and sulfur species), expressed as oxygen-equivalent units. It quantifies the potential oxygen demand required for the re-oxidation of these reduced substances. We have added a clarification in the manuscript when ODU is first introduced.

Soetaert, K., Herman, P. M. J., and Middelburg, J. J.: A model of early diagenetic processes from the shelf to abyssal depths, Geochim. Cosmochim. Acta, 60, 1019-1040,

10.1016/0016-7037(96)00013-0, 1996.

Revision in manuscript 5:

Section 2.2, Page 8, Line 171 – 174

Comment 6:

L 158: *"…vertical diffusion caused by bioturbation…"*

Also by molecular diffusion as stated below.

Response 6:

Thank you for your addition. However, for solid materials, the diffusion does not involve molecular diffusion, so we prefer not to mention molecular diffusion here to prevent any confusion. We discussed this process in the next paragraph when introducing dissolved substances in pore water.

Comment 7:

L 163: Please explain what ODU oxidation and ODU burial mean.

Response 7:

Thank you for pointing it out. We have added the definition explanation in the manuscript.

Revision in manuscript 7:

Section 2.2, Page 9, Line 192 – 195

Comment 8:

L 166-168: *"The exchange fluxes of NH4+, NO2− , NO3− , DO and ODU at the interface are determined by the product of the diffusion coefficient and their concentration gradient between the fluff layer and the overlying water."*

No influence of bioturbation, which is mentioned above?

Response 8:

The diffusion coefficient mentioned here includes both bioturbation and molecular diffusion, not just the molecular diffusion coefficient alone. Additionally, a detailed formula is given in Equation 14 of Section 2.3. We have revised the original text to

eliminate any ambiguity.

Revision in manuscript 8:

Section 2.2, Page 9, Line 198 – 201

Comment 9:

L 173: *"…while the exchange fluxes of NO3− and DO there are set to zero."*

Why are they set to zero? Clarification is needed.

Response 9:

Thank you for pointing it out. Since dissolved oxygen and nitrate cannot exist in the deep anoxic sediments, we set the fluxes of both variables at the bottom boundary of the model to zero. We have added this explanation in the original text.

Revision in manuscript 9:

Section 2.2, Page 9, Line 206 – 208

Comment 10:

L 231: *"…to be 50 times faster than that in the sediment below it…"*

How did you arrive at the factor of 50? Please explain.

Response 10:

Thank you for pointing this out. The factor of 50 was not directly adopted from the cited references. Instead, the references were cited to support the concept that the fluff layer is characterized by enhanced biological activity and rapid organic matter turnover relative to the underlying sediment. The specific enhancement factor was determined during model calibration to reproduce the observed PON inventory and DIN fluxes. We have revised the manuscript to clarify this point.

Revision in manuscript 10:

Section 2.3, Page 19, Line 277 – 281

Comment 11:

L 348-349: *"N-loss is produced through denitrification and anammox inside the sediment, which releases N2 and N2O to the atmosphere."*

Do you have observations confirming that the N2 and N2O really are released to the atmosphere? No transformation of N2O in the water column? No fixing of N2 in the water column by e.g. cyanobacteria?

Response 11:

Thank you for this comment. We agree that our original wording was imprecise. The model only simulates nitrogen transformations within the sediment and exchanges across the sediment-water interface, and does not explicitly represent subsequent processes in the water column or air-sea gas exchange. Therefore, the fate of N_2 and N_2O after leaving the sediment cannot be directly inferred from our simulations. We have revised the text accordingly by replacing "released to the atmosphere" with "released to the overlying water".

Revision in manuscript 11:

Section 3.2, Page 24, Line 405 – 406

Comment 12:

L 402-403: *"Sinking of PONbw serves as the source of PON in the sediment, controlled by the concentration of PONbw and bottom stress."*

The vertical particle flux of PON through the water column must be an important factor determining the PON concentration in the surface sediment. Furthermore, sinking of PONbw is not only controlled by the concentration of PONbw and bottom stress, but also of the sinking rate of the PON particles. The reasoning by the authors is obscure to me. Please clarify.

Response 12:

Thank you for this comment. We apologize for the lack of clarity in the original text. The sediment PON flux in this study is calculated using Eq. (13). When bottom stress is below the critical threshold, the flux represents deposition and is formulated as the product of near-bottom PON concentration, sinking velocity, and the bottom-stress-dependent coefficient. When bottom stress exceeds the critical threshold, the flux represents resuspension and is calculated as the product of the resuspension coefficient and the bottom-stress-dependent coefficient.

Regarding the role of water-column particle fluxes, we do not explicitly simulate PON transport within the water column. Instead, the near-bottom PON concentration is prescribed from observations collected close to the sediment-water interface. Therefore, the effects of particle production, aggregation, settling, and other water-column processes are implicitly reflected in the observed near-bottom PON concentrations used as model input.

Regarding the sinking velocity, we agree that it is an important parameter determining the magnitude of the depositional flux. However, in our model the sinking velocity is prescribed as a constant parameter and does not vary seasonally. Consequently, it may affect the absolute magnitude of the PON flux but does not contribute to its temporal variability. Seasonal variations in PON flux are therefore controlled primarily by variations in near-bottom PON concentration and bottom stress. This conclusion is also supported by the analysis presented in Section 4.2 and Fig. 7a. The sinking velocity used in this study was based on previous studies and its sensitivity is evaluated in Supplementary Fig. S3, which shows an approximately linear response of PON flux to this parameter.

Revision in manuscript 12:

Section 4.1, Page 26, Line 464 – 466

Comment 13:

L 405-406: *"...the extent of seasonal variation in PON inventory is relatively minor..."*
What is the magnitude of the variation of the vertical particle flux of PON through the water column? Would that not have an influence on the seasonal variation in PON inventory? Again, I have problems following your argumentation.

Response 13:

Thank you for this comment. Same as the previous reply, we do not explicitly simulate the vertical transport of PON through the water column and the effects of particle production, aggregation, settling, and other water-column processes are implicitly incorporated into the observed PON^{bw} values.

Therefore, our study focuses on the depositional flux reaching the sediment rather than

on the total vertical particle flux within the water column. Because the latter is not explicitly represented in the model, we cannot independently quantify its seasonal variability. However, its results should appear in the observed PON^{bw}.

Regarding the PON flux across the sediment-water interface, the daily sinking PON amount constitutes merely between one ten-thousandth and one thousandth of the PON inventory within the sediment. Consequently, the influx of PON into the sediment does not result in a substantial alteration of the sedimentary PON inventory.

Comment 14:

L 416-417: *"…only the bottom water temperature …as it affects the mineralization rate that produces NH4+ in the sediment…"*

I would be very surprised if not also the quality and the quantity of organic matter in the sediment (and their seasonal variation) would have a strong influence on the seasonal variations of NH4+ inventory. Please explain why this is not so in this study.

Response 14:

Thank you for this comment. We agree that, in principle, both the quantity and quality of sedimentary organic matter can influence NH_4^+ production through mineralization. In our model, mineralization is represented as the product of PON concentration, the mineralization rate constant, and a temperature-dependent factor (Table 1).

The key point is that the sedimentary PON inventory is several orders of magnitude larger than the daily amount of depositional PON flux. As a result, seasonal variations in PON input cause only very small perturbations to the total sedimentary PON inventory. In our simulations, the depositional PON flux per day is typically only about 10^{-3}–10^{-4} of the existing PON inventory, which is insufficient to substantially modify the amount of organic matter available for mineralization on seasonal timescales.

In contrast, the temperature-dependent factor varies strongly and nonlinearly throughout the year, producing much larger seasonal changes in mineralization rates. Consequently, seasonal NH_4^+ production is controlled primarily by temperature-driven changes in mineralization activity rather than by the changes in sedimentary PON inventory. We have revised the manuscript to clarify this mechanism.

Revision in manuscript 14:

Section 4.1, Page 28, Line 506 – 511

Comment 15:

L 455-456: *"The DO flux is primarily controlled by bottom water temperature (Figure 8e). Temperature regulates molecular diffusion and thus governs the exchange of DO across the sediment–water interface."*

Is the DO flux really not at all influenced by the oxygen demand in the sediment, i.e. by the reactivity of organic matter undergoing oxidation in the sediment?

Response 15:

Thank you for this comment. We agree that the original text did not sufficiently explain the role of sediment oxygen demand in controlling the DO flux.

In the model, DO entering the sediment is primarily consumed by oxic mineralization and, to a lesser extent, by nitrification. Therefore, the seasonal variability of DO flux is not only determined by molecular diffusion across the sediment–water interface but also by the intensity of oxygen-consuming reactions within the sediment. Bottom water temperature affects both processes. In addition to regulating molecular diffusion, temperature strongly controls the rates of mineralization and nitrification, which together account for most of the sediment oxygen demand. Consequently, temperature influences DO flux through both physical transport and biogeochemical consumption pathways. Bottom-water DO concentration also contributes to the seasonal variability of DO flux by modifying the concentration gradient across the sediment–water interface. We have revised the manuscript to clarify these mechanisms.

Revision in manuscript 15:

Section 4.2, Page 31, Line 572 – 580

Comment 16:

L 469-470: Please explain what you mean with *reflective boundary conditions*.

Response 16:

Thank you for pointing this out. We agree that the term "reflective boundary conditions"

was inappropriate and may have caused confusion. Our intention was not to refer to a specific numerical boundary condition, but rather to approaches that estimate sediment DIN fluxes directly from PON deposition or near-bottom PON concentrations without explicitly representing sediment biogeochemical processes and their temperature dependence. To avoid ambiguity, we have revised the text and removed the term "reflective boundary conditions."

Revision in manuscript 16:

Section 4.2, Page 30 – 31, Line 568 – 571

Comment 17:

L 490-492: *"NH4+ is released from the sediment to the bottom water in our study area. Such upward flux has been commonly observed in other coastal regions, including Northern Adriatic Sea (De Vittor et al., 2012), Vilaine Bay (Ratmaya et al., 2022) and Eastern Taiwan (Zhou et al., 2017)."*

A benthic efflux of NH4+ has been observed numerous times in a wide variety of coastal and continental margin environments world-wide. This is a very common observation.

Response 17:

Thank you for this comment. We agree that an upward benthic NH_4^+ flux is a very common feature of coastal sediments and that simply listing other examples does not provide sufficient scientific context.

Our intention was to emphasize that the persistent upward NH_4^+ flux in Harima Nada is associated with the large concentration gradient between sediment porewater and bottom water. In our study area, NH_4^+ is continuously produced through organic matter mineralization within the sediment, while NH_4^+ concentrations in the bottom water remain comparatively low. Consequently, sediment NH_4^+ concentrations are consistently higher than those in the overlying water, resulting in a sustained upward diffusive flux.

This contrasts with some environments where downward NH_4^+ fluxes have been reported. In such systems, elevated bottom-water NH_4^+ concentrations and/or weak

sedimentary NH_4^+ production can reduce or reverse the concentration gradient across the sediment–water interface. We have revised the discussion accordingly to focus on the mechanisms controlling the flux direction rather than simply citing other observations.

Revision in manuscript 17:

Section 4.3, Page 35, Line 613 – 624

Comment 18:

L 520-522: *"…we found that the flux of PON settling from the bottom water to the sediment is the main source of nitrogen in the sediment…"*

Is this not a quite trivial finding? What else would it be? DIN uptake by the sediment? Benthic (dark) N2 fixation? By the way, was benthic N2 fixation measured or modelled?

Response 18:

Thank you for this comment. We agree that, when stated in a general way, the sentence may appear trivial. Our intention was not to suggest that PON deposition is universally the dominant nitrogen source to sediments, but rather to highlight the relative importance of different nitrogen fluxes across the sediment–water interface in the studied system.

In Harima Nada, both PON deposition and NO_3^- uptake contribute nitrogen to the sediment. However, the magnitude of the downward PON flux is substantially larger than that of NO_3^- uptake, making PON deposition the dominant pathway of nitrogen input across the sediment–water interface. More importantly, we aimed to emphasize that the seasonal variability of this dominant nitrogen input is controlled primarily by bottom-water PON concentration and bottom stress. To avoid the impression of a general statement, we have revised the conclusion accordingly.

Benthic N_2 fixation was neither measured nor included in the model and therefore was not considered in the sediment nitrogen budget.

Revision in manuscript 18:

Section 5, Page 37, Line 688 – 690

Comment 19:

L 534-536: *"Denitrification is the main process of the nitrogen removal and contributes to the seasonal variation of N-loss. However, the overall nitrogen removal amount is relatively low, possibly due to the low NO3− concentration in the bottom water caused by the oligotrophic environment of Harima Nada."*

What about the importance of coupled nitrification-denitrification in these sediments? Did you consider this process?

Response 19:

Thank you for this comment. Coupled nitrification-denitrification is included in our model because both nitrification and denitrification are explicitly simulated and dynamically linked through the sediment NO_3^- pool. NO_3^- produced by nitrification can subsequently be consumed by denitrification, and therefore the model accounts for internally regenerated NO_3^- as well as NO_3^- supplied from the overlying water.

Our intention was not to imply that denitrification depends solely on bottom-water NO_3^-. Rather, the model results suggest that nitrogen removal is limited by the overall availability of NO_3^- within the sediment system. Under the oligotrophic conditions of Harima Nada, sedimentary NO_3^- concentrations remain relatively low despite internal production through nitrification, and external NO_3^- supplied from the overlying water frequently contributes to maintaining the sediment NO_3^- pool. Because denitrification is the dominant nitrogen removal pathway in the model, the limited availability of NO_3^- restricts denitrification and results in a relatively low overall nitrogen removal rate. We have revised the manuscript to clarify this point.

Revision in manuscript 19:

Section 5, Page 38, Line 705 – 711

Reviewer 2:

We would like to thank the reviewer for the positive evaluation of our work and for the valuable suggestions for improving the manuscript. We appreciate the reviewer's careful reading and insightful comments, which helped us clarify several aspects of the study and improve the presentation of the results. All comments have been carefully addressed, and the corresponding revisions are described below.

Comment 1：

According to my view, the lack of verification of some important parameters including PON (PN) accumulation rate and benthic fluxes should be included into Discussion using the text in authors responses.

Response 1:

We thank the reviewer for this important comment. We agree that the lack of direct observations for PN accumulation rates and several benthic nitrogen fluxes (NH_4^+, NO_x^-, and nitrogen removal fluxes) introduces uncertainty to the model evaluation. Following the reviewer's suggestion, we have added a paragraph in the section Discussion explicitly addressing this limitation.

Although direct measurements of these fluxes are unavailable for the study area, the model was evaluated against available observations of sediment PN, NH_4^+, and NO_x^- concentration profiles, as well as seasonal variations in sediment-water DIN fluxes. These variables represent both the internal nitrogen distributions and the integrated exchange across the sediment-water interface. The overall agreement between simulations and observations partly supports the model's performance. Nevertheless, we acknowledge that the absence of direct measurements of PN accumulation rates and individual benthic nitrogen fluxes prevents a more rigorous validation of some modeled processes. This uncertainty and the need for future field observations have now been discussed in the revised manuscript.

Revision in manuscript 1:

Section 4.3, Page 37, Line 675 – 678

Comment 2：

Grain size distribution and benthos (seems important) in the study area should be described in the section Study area.

Response 2:

Thank you for this helpful suggestion. Additional information on sediment characteristics and benthic communities has been added to the section Study Area. Specifically, we further described the predominance of muddy sediments in Harima Nada and added a brief description of macrobenthic communities reported for Harima Nada. We also clarified that benthic organisms could influence sediment biogeochemical processes through bioturbation and organic matter processing, which is represented in our model by the bioturbation diffusion coefficient.

Revision in manuscript 2:

Section 2.1, Page 6 –7, Line 145 –151

Comment 3：

Regarding PON (better PN since the organic form is not determined), in the marine chemistry PON is used in the seawater column (suspended particulate matter) while in sediments it is Norg or Ntot.

Response 3:

Thank you for this valuable comment. We agree that the terminology used for sedimentary organic nitrogen varies among disciplines and that Norg or Ntot are frequently used in sediment studies. In the present study, however, we retain the term particulate organic nitrogen (PON) for two reasons.

First, PON is an explicit state variable in the sediment diagenetic model framework adopted here, where organic nitrogen is represented as particulate organic matter undergoing deposition, burial, and mineralization. Second, the sediment measurements used for model validation were obtained from the solid phase after removal of porewater, and therefore represent particulate nitrogen rather than dissolved nitrogen species.

To avoid ambiguity, we have added a clarification when PON is first introduced in the model descriptions, explicitly stating that PON refers to particulate organic nitrogen in the sediment solid phase throughout this study.

Revision in manuscript 3:

Section 2.2, Page 8, Line 170 –171

Reviewer 3:

We sincerely thank the reviewer for the careful and constructive review of our manuscript. The comments not only helped us improve the clarity and transparency of the model description, but also encouraged us to strengthen the broader scientific interpretation of our results. In response, we have revised the manuscript extensively, including clarifications of the model formulation and boundary conditions, improvements to figures and terminology, and a substantial reorganization and expansion of the Discussion and Conclusions. These revisions have allowed us to better highlight the mechanisms controlling sedimentary nitrogen cycling, the role of resuspension, the limitations of the modeling framework, and the broader implications of our findings for semi-enclosed coastal seas. We are grateful for the reviewer's thoughtful suggestions and believe that they have significantly improved the quality and impact of the manuscript. Detailed responses to each comment are provided below.

Comment 1:

I still do not understand why the carbon mineralization kinetic formulations in Table 1 are divided by the SUM_Lim term, if kinetic limitations are already included in the rate expressions. This was not addressed in the first round of reviews. Furthermore, in the footnote of Table 1, "lim" is described as equal to the sum of the O2 limitation term plus the product of al the other electron acceptor inhibition terms multiplied by x:y. These terms have different units and therefore cannot mathematically be added together. The "lim" expression and application needs to be corrected and clearly explained.

Response 1:

We thank the reviewer for raising this point. The mineralization formulations, including the normalization by $\sum \mathrm{lim}$, were adopted from Soetaert et al. (1996). In this formulation, the limitation functions are not intended to impose additional kinetic constraints on the total mineralization rate. Instead, they are used to partition organic matter mineralization among the competing respiratory pathways (oxic respiration, denitrification, and anoxic mineralization). The normalization by $\sum \mathrm{lim}$ serves as a

rescaling procedure that ensures the combined mineralization rate does not exceed the prescribed first-order degradation rate.

We agree that the previous footnote in Table 1 was not sufficiently clear and may have caused confusion regarding the definition of $\sum \mathrm{lim}$ and the notation x:y (the molar C:N ratio). To address this issue, we have revised the manuscript and footnote to explicitly state that $\sum \mathrm{lim}$ is the sum of the dimensionless limitation functions used to normalize the competing mineralization pathways following Soetaert et al. (1996). The description of the C:N ratio has also been separated and clarified.

Soetaert, K., Herman, P. M. J., and Middelburg, J. J.: A model of early diagenetic processes from the shelf to abyssal depths, Geochim. Cosmochim. Acta, 60, 1019-1040, 10.1016/0016-7037(96)00013-0, 1996.

Revision in manuscript 1:

Section 2.3, Page 10 – 11, Line 240 –244

Section 2.3, Page 13, Line 260 –263

Comment 2：

Temperature corrections are made to the carbon mineralization reactions but, puzzlingly, not the other redox reactions. This needs some careful justification.

Response 2:

We thank the reviewer for this important comment. We agree that many redox reactions involved in sediment nitrogen cycling are temperature dependent. In the present model, however, temperature correction was applied only to organic matter mineralization. This choice was made because organic matter mineralization is the primary source of reactive nitrogen and reduced substances in the sediment and therefore exerts a major control on the subsequent nitrification, denitrification, and other redox processes.

In addition, temperature affects the model through temperature-dependent molecular diffusion coefficients. As a result, the rates of other nitrogen transformation processes are influenced indirectly through coupled biogeochemical interactions and changes in substrate availability. We acknowledge that explicit temperature corrections could also

be applied to individual redox reactions, but this would introduce additional parameters that are difficult to constrain with the available observations.

Since the objective of this study is to investigate the mechanisms controlling the seasonal variability of sediment nitrogen cycling and benthic nitrogen fluxes, we adopted this simplified formulation to demonstrate the dominant effects of temperature. At the same time, we also want to limit parameter uncertainty as little as possible. To clarify this modeling choice, we have added an explanation in the revised manuscript.

Revision in manuscript 2:

Section 2.3, Page 11, Line 244 –247

Comment 3：

The alpha, beta, gamma, and delta leakage terms that appear in Table 1 and 2 need to be explicitly explained in the model description as they are crucial for the net benthic fluxes of N species.

Response 3:

We thank the reviewer for this comment. The definitions of γ, α, β, and δ were already provided in Tables 1 and 2 following the parameterization of Akbarzadeh et al. (2018). However, we agree that their functional role within the nitrogen cycling network was not sufficiently explained in the model description. These coefficients were introduced to represent pathway partitioning and intermediate-product leakage during nitrate reduction, thereby allowing explicit simulation of nitrite and N_2O production. To improve clarity, we have added a brief explanation of their role in the model description while retaining their detailed definitions in Tables 1 and 2.

Revision in manuscript 3:

Section 2.3, Page 11, Line 248 –250

Comment 4：

L242: Is D_sur is not clearly defined. In Eq. 14, the molecular diffusion coefficient, DM, already appears, so what does Dsur represent? This equation should include the

bioturbation mixing coefficient if it assumed that porewater diffusion is a result of faunal mixing and molecular diffusion.

Response 4:

We thank the reviewer for pointing out the ambiguity in the definition of D_{sur}. In Eq. (14), D_M^s represents the surface molecular diffusion coefficient, while D_{sur} was introduced as an additional effective mixing coefficient to account for non-molecular transport processes occurring near the sediment-water interface. These processes may include bioturbation as well as physical disturbances such as resuspension-induced mixing. Therefore, the effective transport coefficient is represented as the sum of molecular diffusion and additional mixing processes.

We agree that the original term "surface diffusion coefficient" was not sufficiently clear. To avoid confusion, we have revised the manuscript to explicitly define this parameter as an effective surface mixing coefficient and clarified its role in Eq. (14).

Revision in manuscript 4:

Section 2.3, Page 19, Line 290 –292

Comment 5:

L327: Why is bioirrigation excluded from the calculation of the surface solute fluxes? This omission does not seem warranted since irrigation will contribute to the total flux, which is the key metric that should be reported.

Response 5:

We thank the reviewer for this comment. Bioirrigation was not omitted from the model calculations. Instead, it was treated separately from the reported sediment-water fluxes because it was implemented as a distributed source–sink term within the sediment column rather than as an explicit boundary flux at the sediment-water interface. Therefore, the fluxes reported in Section 3.2 represent interface exchange driven by concentration gradients and mixing processes.

The contribution of bioirrigation was quantified separately in the nitrogen budget analysis (Section 3.3), and its seasonal variations were further presented in Fig. S5 and

Text S3. These results show that bioirrigation is substantially smaller than the reported sediment-water fluxes, with negligible influence on the conclusions regarding benthic-pelagic nitrogen exchange.

To clarify this distinction, we have added an explanation in Section 3.2 describing why bioirrigation is not included in the sediment-water interface fluxes and guiding readers to the corresponding budget analysis.

Revision in manuscript 5:

Section 3.2, Page 23, Line 379 –381

Comment 6：

The discussion sections 4.1 and 4.2 (based on the sensitivity analysis) are essentially an extension of the results, and contain no references to previous work. Comparison with previous studies is made in Section 4.3, but this is rather cursory. The manuscript lacks a broader discussion on e.g. the limitations of the approach taken, Implications and future directions. As a result, the manuscript reads more like a field report. This is major drawback of the study since the reader is left with little impression of how the findings fit into the large body of work on N cycling in other inland coastal seas, especially with regard to resuspension that seems to be a novel aspect of the model study.

Response 6:

We thank the reviewer for this insightful comment. We agree that the original section Discussion focused primarily on interpreting the model results and did not sufficiently place the findings within the broader context of sedimentary nitrogen cycling studies. In response, we have substantially revised Sections 4.1–4.3 and the Conclusions to strengthen the scientific interpretation, broaden the comparison with previous studies, and explicitly discuss the limitations, implications, and future applications of the modeling framework.

First, Sections 4.1 and 4.2 have been extensively revised to move beyond a description of the sensitivity analysis results. The discussion now focuses on the underlying mechanisms controlling nitrogen fluxes and nitrogen removal, emphasizing how

organic matter supply, oxygen availability, bottom-water nutrient concentrations, and resuspension jointly regulate sedimentary nitrogen cycling. Particular attention has been given to explaining why seasonal variations in DIN fluxes are largely decoupled from seasonal variations in PON deposition despite the latter being the dominant nitrogen input pathway.

Second, Section 4.3 has been completely reorganized. Rather than simply comparing flux magnitudes among regions, we developed a common mechanistic framework for interpreting benthic nitrogen fluxes across coastal environments. Within this framework, NH_4^+ flux is controlled primarily by the balance between sedimentary NH_4^+ production and bottom-water NH_4^+ concentrations, while NO_3^- flux is by the balance among nitrification, denitrification, and external NO_3^- supply, and N-loss is by the availability of reactive nitrogen, particularly NO_3^- to support denitrification. The simulated fluxes in Harima Nada were then compared with observations from a wide range of coastal systems to evaluate the generality of these controls. This substantially strengthens the connection between our findings and previous studies of sedimentary nitrogen cycling.

Third, a new synthesis section was added at the end of Section 4.3 to highlight the broader implications of the study. The revised discussion emphasizes that, despite large differences in environmental settings and flux magnitudes among coastal systems, a common set of mechanisms can explain much of the observed variability in benthic nitrogen fluxes. We believe this addition significantly improves the general relevance of the study beyond Harima Nada.

Fourth, we have expanded both the Discussion and Conclusions to explicitly address the limitations of the modeling approach and future research directions. The revised manuscript now discusses uncertainties associated with the one-dimensional framework, the lack of direct observations of individual benthic nitrogen fluxes, and the applicability of the model to highly permeable sandy sediments where porewater advection may become important. We also outline future applications of the model for investigating the impacts of long-term environmental changes, including eutrophication, de-eutrophication, and climate-driven warming, on sediment nitrogen cycling.

Finally, we agree that sediment resuspension is one of the distinctive aspects of the present modeling framework. In the revised manuscript, we have clarified its role in regulating PON deposition by controlling the balance between particle settling and resuspension. The discussion now highlights that resuspension is an important factor governing the magnitude and seasonal variability of PON fluxes to the sediment. At the same time, the revised analyses show that the seasonal variability of downstream nitrogen fluxes is controlled more strongly by temperature-dependent biogeochemical processes and bottom-water nutrient conditions than by seasonal variations in PON deposition itself. We therefore believe that the revised manuscript provides a more comprehensive and process-based interpretation of how physical and biogeochemical factors interact to regulate sedimentary nitrogen cycling in semi-enclosed coastal seas.

Revision in manuscript 6:

Section 4, 5

Comment 7：

Line 49: “Research”, not “researches”.

Response 7:

Thank you for pointing this out. The word has been corrected to "research".

Revision in manuscript 7:

Section 1, Page 3, Line 48

Comment 8：

Line 59: Fick’s First Law

Response 8:

We agree and have changed "Fick's law" to "Fick's First Law" for clarity.

Revision in manuscript 8:

Section 1, Page 3, Line 60

Comment 9：

L93: What is to be understood by “typical semi-enclosed inland sea”? What makes Harima Nada a typical example?

Response 9:

We appreciate this comment. We have clarified why Harima Nada can be considered representative of semi-enclosed inland seas by explicitly referring to its restricted water exchange, pronounced seasonal stratification, and active sediment-water nutrient cycling. These characteristics are now briefly introduced in the revised text, while a more detailed description is provided in the Study Area section.

Revision in manuscript 9:

Section 1, Page 5, Line 106 –109

Comment 10：

L116: DO is reported in mg/L. Please change to mmol/m3 to be in line with the rest of the manuscript.

Response 10:

We agree. The unit of DO has been changed from mg L^{-1} to mmol m^{-3} throughout the manuscript.

Revision in manuscript 10:

Section 2.1, Page 6, Line 134 –135

Comment 11：

L125: A previous study…

Response 11:

Thank you for pointing this out. The text has been revised as suggested.

Revision in manuscript 11:

Section 2.1, Page 6, Line 143

Comment 12：

L126: Delete “As moving to the sea bottom”

Response 12:

We agree and have removed this phrase.

Comment 13:

Fig. 2: (i) Why in the caption are NH4, NO2 and NO3 in boldface, and in other figures also. (ii) I think O2 and DOU boxes should be in dashed lines. (iii) I don't understand the right-side panel of the figure. This needs to be explained in the caption.

Response 13:

We thank the reviewer for these helpful comments on Figure 2.

(i) The bold appearance of NH_4^+, NO_2^- and NO_3^- was unintentional and resulted from figure formatting. The figure has been revised, and all chemical variables are now displayed in a consistent font style.

(ii) We agree that O_2 and ODU are dissolved-state variables and should be represented consistently. Their boxes have been changed to dashed outlines.

(iii) We agree that the right-hand panel was insufficiently explained. Additional description has been added to the figure caption. Specifically, the panel illustrates the conceptual division of each sediment layer into a solid phase (solid-line trapezoid, containing PON) and a dissolved phase (dashed-line trapezoid, containing dissolved constituents). The inverted geometry of the dissolved-phase trapezoid represents the higher porosity and water content in surface sediments. The uppermost layer is treated as a fluff layer.

Revision in manuscript 13:

Section 2.2, Page 8, Line 177 –184

Comment 14:

L171: "product", not "production"

Response 14:

Thank you for pointing out this error. The term has been corrected to "product".

Revision in manuscript 14:

Section 2.2, Page 9, Line 204

Comment 15：

L173: Do you mean that the lower boundary condition for NO3 and DO are set to zero gradient (Neumann) conditions? In general, the boundary conditions are introduced randomly throughout the model description. Due to complexity of the model, I strongly suggest separate sections in the model description for the “initial conditions” and “boundary conditions”. It seems to me that some species have a fixed concentration at the lower boundary (ODU) or a constant (non-zero) flux (NH4, NO2). Please clarify/justify.

Response 15:

We thank the reviewer for this comment. The lower boundary fluxes of NO_3^- and DO were set to zero. Because they are assumed absent in the underlying deep anoxic sediments, this treatment is mathematically equivalent to a zero-gradient (Neumann) boundary condition. For NH_4^+, NO_2^- and ODU, the lower-boundary exchange fluxes are calculated from the concentration gradient between the deepest model layer and observations from the underlying pore water.

Regarding the organization of the model description, the specific initial conditions and boundary-condition datasets are already described in Section 2.4 (Model Configuration), while Sections 2.2 and 2.3 focus on the conceptual framework and governing equations. To improve readability, we have added text in Section 2.4 explicitly indicating that all initial and boundary conditions are summarized there.

Revision in manuscript 15:

Section 2.4, Page 20, Line 304 –305

Comment 16：

L355 and Fig 5. The text states that DIN is released from the sediment, but in fig. 5 the fluxes have the same sign as the PON and DO flux. According to model, DO and PON fluxes are downwards and therefore positive, whereas upward fluxes are defined

negative.

Response 16:

We thank the reviewer for identifying this error. The sign convention was incorrectly plotted in Figure 5. In our study, upward fluxes are defined as positive and downward fluxes as negative. Therefore, the downward fluxes of PON and DO should be negative rather than positive as originally shown. This correction affects only the presentation of the flux directions and does not alter the calculated flux magnitudes or the conclusions of the study. We have also clarified the sign convention in the figure caption to avoid future confusion.

Revision in manuscript 16:

Section 3.2, Page 24, Line 398 404

Comment 17：

Section 3.3 and Fig. 6. Since mmol/m2/d are used up this point, the N budget should take the same units to make comparisons easier for the reader.

Response 17:

We appreciate your suggestion. To improve consistency throughout the manuscript and facilitate comparison with the interface fluxes presented in the preceding sections, we have revised the units used in Section 3.3 and Figure 6. All nitrogen budgets are now expressed as mmol N m^{-2} d^{-1} rather than g N m^{-2} yr^{-1}. These values represent annual mean daily rates derived from the corresponding annual budgets. The manuscript and figure have been revised accordingly.

Revision in manuscript 17:

Section 3.3