Reply to Editor:

Public justification (visible to the public if the article is accepted and published):

Dear Menghong Dong,

The expert reviewers have assessed your revised manuscript. While they consider your work suitable for publication, they have highlighted a few outstanding issues. In particular, some revision points raised during the initial review—which you had agreed to address—are still unresolved.

One reviewer has provided a detailed report, which I fully support. I would therefore like to ask you to carefully address these comments before I can proceed with the acceptance of your manuscript.

Looking forward to your revised submission. Best wishes, Helge

Dear Dr. Helge,

Thank you for your thoughtful feedback and for giving us the opportunity to revise the manuscript. We sincerely appreciate the reviewers' time and effort in evaluating our work and providing valuable insights.

We acknowledge that some revision points from the initial review still require further clarification, and we have carefully addressed all outstanding issues as highlighted by the reviewer. We have prepared a revised manuscript that incorporates these improvements.

Thank you again for your guidance throughout this reviewing process.

Reply to Referee:

Review overview

Following the first review round the authors made changes to their manuscript in line with the reviewers comments, and have substantially increased the supplementary materials. However, I still miss topics already indicated in the first review round, such as the local scale of the results, the fact that this location has high groundwater discharge and thus that the presented results can serve as a upper limit of the effect of SGD nutrients in a coastal bay area. The difference between the observational period (1931-2001) and the simulated period (2015-2016) is mentioned now, but not further explained. What was the temperature increase in Japanese waters during this period? How did nutrient inputs and population size change? In their reply the authors state "We have strengthened the comparison of our results with those from other regions in the revised manuscript. Additionally, we also enhanced the presentation of our findings in the abstract and conclusions.", but I don't see this in the new manuscript much. I also do not see why the authors are so keen on studying this subject, as they do not mention any eutrophication issues in the area or fisheries/aquaculture decline. They use existing work to quantify the reach of this nutrient source, which I find a worthwhile exercise, but more information on why this is important for the region is not given. Is the Toyama Bay area economy focussed on the bay much and thus dependent on its primary production? In short, I still miss some context here. The authors have replied to my comments, but not all of their reply has made it into the manuscript, leaving potential readers with the same questions. This applies to the site description in their reply, the comparison to other areas (the authors include more of a comparison but not the observation that therefore their work can be seen to provide a maximum for SGD nutrient influence) and the zooplankton mortality.

In other replies the authors have failed to substantiate their new text, e.g. in the claim that atmospheric deposition in the area is small compared to other nutrient inputs. They also now mention sewage and industrial loads, but state that these have been added to the riverine loads without further specification (direct discharges are discharges directly into marine waters, so downstream of the last tidal gauge point in a river or directly from the coast). If they know these additional sources, how much do they contribute to the riverine load and where is the reference for them?

I do appreciate the new percentage information, the shifting of figures between the supplementary materials and the main text, and the new figure on the surface current patterns, and think this manuscript is nearly there. Some more points are listed below.

Thanks very much for your helpful comments and suggestions. We acknowledge that our revisions in the first round may not have sufficiently addressed the comparison of our results with other regions. Since there are no existing studies quantifying the contribution of submarine groundwater discharge (SGD) derived nutrients in other areas, as you pointed out, under similar conditions, the presented results can serve as an upper limit of the effect of SGD nutrients in a coastal bay area. However, we did not consider benthic phytoplankton or seagrass on the seabed, and the distribution of SGD varies with changes in water depth. Given the complexity of environmental conditions in different marine regions where benthic phytoplankton or macrophytes are important, or where SGD is located at shallower depths, the contribution of nutrients derived from SGD to phytoplankton growth could be higher. Indeed, in some regions, SGD-derived nutrients can even contribute to eutrophication (Luijendijk et al., 2020). For example, in shallow coastal bays such as Liaodong Bay (Luo et al., 2023) and Zhenzhu Bay (Xu et al., 2024), where the average water depth is less than 30 meters, SGD-derived nutrients may exert a more significant impact and can also lead to eutrophication. Additionally, in areas where seagrass meadows or benthic microalgae are present, SGD can strongly influence the biotic characteristics of seagrass beds (Kantún-Manzano et al., 2018). We have refined our discussion in the revised manuscript (lines 466-476) to reflect these considerations.

Regarding the discrepancies between our simulated results and the observational data, we acknowledge that we did not provide sufficient explanation of the contributing factors. Based on previous observational records, the long-term temperature increase in the western Japan Sea over the past 100 years has been estimated at approximately +1.51°C (JMA, 2024). This warming trend is not uniform throughout the years, and is more in winter and spring temperatures than in summer and autumn (JMA, 2024). Additionally, while the population around Toyama Bay has increased over the decades (Aoki, 2021), the implementation of new wastewater treatment systems has resulted in a 50% reduction in riverine nutrient inputs despite relatively stable river discharge (Katazaki and Zhang, 2021a). Furthermore, climate change-induced shifts from snowfall to rainfall have altered the SGD recharge patterns and chemical composition, leading to lower nutrient concentrations in SGD (Katazaki and Zhang, 2021b). Both the long-term warming trend and the reduction in nutrient inputs contribute to the discrepancies between our model results and the historical observational data. We have mentioned these factors more explicitly in the revised manuscript (lines 251-258) to improve the discussion of model validation.

We appreciate your feedback on the need for additional context regarding the significance of this study for the region. Toyama Bay is known for its high biological productivity and unique oceanographic features, which support commercially important fisheries. While no severe eutrophication issues have been reported (Tsujimoto, 2012), nutrient dynamics play a crucial role in shaping primary production, which directly impacts the ecosystem and fisheries in the bay (NPEC, 2010). Given the recent reduction in nutrient inputs from riverine sources due to improved wastewater treatment (Katazaki and Zhang, 2021a) and the decrease in nutrient concentrations in SGD caused by climate change-induced shifts from snowfall to rainfall in midlatitude Japan (Katazaki and Zhang, 2021b), understanding the role of SGD as a nutrient source is essential for predicting future changes in primary production. Although the Toyama Bay economy is not heavily dependent on the primary production in the bay, fisheries remain an important sector (Bank of Japan Toyama Local Office, 2021). Gaining insight into nutrient supply mechanisms will help assess the potential long-term implications for coastal productivity and resource management. We have enhanced the manuscript by providing more context on the regional importance of SGD-derived nutrients (lines 78-85).

Regarding the exclusion of atmospheric deposition of nutrients, previous studies (Itahashi et al., 2021) estimate that the atmospheric deposition flux of dissolved inorganic nitrogen (DIN) in the study area is approximately 1.2 g/s. This value is significantly smaller than the nutrient fluxes from both SGD and riverine inputs, which is reason why atmospheric deposition was not considered in our model. We have clarified this point in the revised manuscript (lines 190-191).

The riverine loads of nutrients used in our model were from a calculation that estimated the output of nutrients from surface land by including sewage and industrial treatments (Mutsuura et al., 2023).

In this calculation, Mursuura et al. (2023) did not separated the contribution of “direct discharges” from those in the rivers. Therefore, we consider that all the nutrients from surface land were given as riverine loads of nutrients. However, such treatments do not affect the distribution of nutrients in the bay because the nutrients of “direct discharges” flows into the bay along the coast and the riverine nutrients also distribute along the coast. In the realistic bay, these two sources of nutrients should move together.

Our responses to the more detailed comments are as follows. The referee’s comments are cited in italics.

Recommendation

Minor revision

Thank you for the positive evaluation.

Detailed Comments

1. *Line 19: “and used by the phytoplankton for growth”*

   Thanks. We have revised this expression on line 19 of the revised manuscript.

2. *Line 129: the term “first class rivers” is still not explained in the text.*

   Thank you for pointing this out. That was our mistake. In Japan, rivers are classified into two main categories based on their significance, scale, and management structure. First class rivers are important for national land conservation and the economy, managed by the national government or prefectural governors under national supervision. Second class rivers are significant for regional public interests and are managed by prefectural governments. We have added this clarification on lines 136-139 of the revised manuscript.

3. *Lines 167-169: I assume the applied boundary conditions were taking from the larger model for the times specified in the manuscript, and did not consist of climatological values based on the monthly values from the larger model. Is this correct? May be better to state this explicitly.*

   Thank you for your comment. We realize that our explanation was not clear in the manuscript. The larger model we used is a climatological model, so the boundary conditions applied in our study were also based on climatological data rather than time-specific values. We have clarified this point explicitly in the revised manuscript (line 178) to avoid any confusion.

4. *Line 179: how small exactly is the atmospheric deposition? Please provide input (estimates) and their references. Otherwise readers cannot judge whether these depositional values were indeed negligible.*

Thank you for your comment. Based on estimates from previous studies (Itahashi et al., 2021), the atmospheric deposition flux of dissolved inorganic nitrogen (DIN) into the study area is approximately 1.2 g/s. This value was derived using reported atmospheric deposition rates in the Japan Sea region and scaled to the surface area of Toyama Bay. We have included the relevant references and detailed calculation in the revised manuscript (lines 190-191) to support our assumption that atmospheric deposition is negligible compared to other nutrient source.

5. *Lines 237-242: the authors have added text here to explain the discrepancies between their simulated results (2015-2016) and the observational data (1931-2001), as suggested. But I would still like to see a bit more meat to this bone here. What do observational records show as the T increase in western Japan waters over the total period (1931-2016)? If there was an increase in T then how was this distributed over the year? Surely there are references for this? There is an 1.75 deg.C biased in the T validation. As in general the sea water temperature is easy to get right I strongly suspect the time difference between the obs and the model data, but the authors need to substantiate this.*

Thank you for your comment. We acknowledge that additional discussion is needed to better explain the temperature discrepancy between our simulated results and the observational data.

Based on previous observational records (JMA, 2024), the long-term temperature increase in the western Japan Sea over the past 100 years has been estimated at approximately +1.51°C. This warming trend is not uniform throughout the year and is larger in winter and spring than in summer and autumn (JMA, 2024). This warming trend may partially account for the 1.75°C bias from model validation. We appreciate your suggestion and have improved this section accordingly in the revised manuscript (lines 251-258).

6. *Line 268-272: the authors should specify the different water masses here or refer to a paper that does. What are the S, T characteristics of these different water masses?*

Thank you for your suggestion. We have included the reference (Hatta et al., 2005) in the revised manuscript (line 291), which specifies the different water masses.

The coastal surface water mass is characterized by low salinity and is influenced by freshwater discharge from rivers into the bay. The Tsushima Warm Current water mass is a warm, saline water mass originating from the Tsushima Current, typically found in the upper layers. In contrast, the Japan Sea Proper water is a cold, high-salinity water mass that dominates the deep layers of the Japan Sea.

7. *Line 295: "changes in detritus (derived from phytoplankton and zooplankton) coincide with"*

Thanks. We have made this change on line 315 of the revised manuscript.

8. *Line 399: I don't see a detritus flux to the sediments in Figure 13, I assume you mean Figure 14.*

Thank you for pointing this out. Yes, this was our mistake. The correct reference should be Figure 14, and we have corrected this in the revised manuscript.

9. *Line 428: I would say there is a difference in the contribution of SGD derived nutrients compared to rivers, and a difference in where in the water column they contribute. But surely their impact on phytoplankton growth is the same as it is for the river-derived nutrients.*

Thank you for your comment. We agree with your statement and have revised the manuscript (lines 447-449) to clarify this point more explicitly.

10. *Line 455: the fact that changes in zooplankton and detritus follow the changes in phytoplankton is not a conclusion from the work presented, but a given when a simple model like an NPZD is used, as is the case here.*

Thank you for your suggestion. Indeed, this correlated change is most likely determined by the characteristics of the NPZD model. We have removed this sentence in the revised manuscript.

11. *Line 483: seagrasses are macroalgae and phytoplankton are microalgae. The authors should not refer to seaweed as phytoplankton. And the mention here of limitations to the study is inappropriate: this belongs in the discussion, not the conclusions.*

Thank you for pointing this out. We acknowledge the mistake in referring to seaweed as phytoplankton and have corrected this in the revised manuscript. Additionally, we also agree that the mention of study limitations should be placed in the discussion section rather than in the conclusions. We have made the necessary adjustments accordingly in the revised manuscript (lines 466-476).

12. *Lines 487-489: I disagree. In this manuscript the authors have quantified the contribution of SGD derived nutrients to phytoplankton growth in the bay as 4% of the total. They have also defined the spatial and temporal extent of this contribution, which is limited to the coastal area and occurs in places at depth. Given the fact that it will be difficult to target this nutrient source with management action (in case of eutrophication issues) I would suggest that future work focusses on other issues. For me, the main contribution of this work is the quantification of the SGD nutrient influence to primary production, and its spatial influence sp here. The authors show this is limited in this area, despite the SGD load contribution being large compared to those in other locations. For me, this work is important in that it allows for researchers to disregard this nutrient source (in locations with little or no knowledge of them) as even here, where the source is relatively large, the influence is limited.*

Thank you for your suggestion. Our original intention was to emphasize that future research should focus on the short-term variations of riverine inputs, as they have a greater impact. However, since the main focus of this manuscript is on the influence of SGD-derived nutrients, we have removed this part from the text. As you pointed out, considering that SGD-derived nutrient is difficult to be managed and has a relatively minor impact, future work should indeed focus on other issues.