Reply to Referee #2:

Manuscript overview

Th manuscript present a modelling study into the origin of marine nutrients in a semi-enclosed bay off the west coast of Japan. It tracks nutrients from both riverine sources and fresh ground water sources and quantifies their importance to the local nutrient budget and the local primary production.

Review overview

The manuscript is generally well written though the objectives and main results could be more clearly presented. The presented method is not new (as clearly stated), but is applied to an area where is has not been used before and to a marine nutrient input source (fresh ground water) that has not been considered before. As such I find that the presented work merits publication. The manuscript is accompanied by an extensive appendix which I have not considered. The presented results are very local and more effort could be made to derive conclusions for other areas. At the same time the authors could make their local results more clear (percentage contributions, area affected) in the abstract and conclusions. Validation of simulated results with observations from the same period would be desirable, as would be discussion on other nutrient input sources (atmospheric deposition, direct discharges, aquaculture). More detailed comments are provided below.

Thanks very much for your helpful comments and suggestions. In this study, we did not consider benthic phytoplankton, such as 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 may be higher. 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.

Regarding the validation of simulated results using observations from the same period. Thank you for your suggestion. However, due to the limited availability of observational data from the same period, it is challenging to perform a direct comparison. Regarding nutrient inputs in Toyama Bay, the main sources are rivers and groundwater from the landward side, and inputs from the Japan Sea on the ocean side. Contributions from other sources, such as atmospheric is minimal. Therefore, we did not include these in our model. We have addressed these issues in the revised manuscript.

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

Line 18: "narrow band of the coastline (< 3km)", this results is also mentioned in the conclusions but with less detail. As the (short) abstract should be a reflection of the main findings of the presented work, and the (longer) conclusions likewise, I would expect the abstract to have the same or less information as the conclusions section, not more.

Thank you for pointing this out. We have strengthened the description of the main findings in the conclusions section and modified the abstract in the revised manuscript.

Line 18: "middle and bottom layers", please specify these layers as this is very location specific.

Thank you for your comment. These layers refer to the areas located approximately 5 m below the sea surface. We also added this clarification in the revised manuscript.

Line 55-57: this technique was also applied in Northwest Atlantic areas, see Lenhart & Große (2018), OSPAR (2013) and Painting et al (2013). Please acknowledge this work as well.

Thank you for your suggestion. We have cited these papers in the revised manuscript to acknowledge their contributions.

Line 60: and hence eutrophication issues.

Thanks. We have added this presentation in the revised manuscript.

Line 61: I would prefer to see a more elaborate site description here. What does the seabed consist of mainly? What is the general circulation pattern, is there inflow of deep water from the Japan sea? Just to get a physical feel for the area.

The following two websites provide detailed information about the geology and deep water characteristics of Toyama Bay. We also provided an introduction to the physical field in Section 3.1 based on the model results.

https://gbank.gsj.jp/geonavi/geonavi.php#11,36.93757,137.25200

https://t-deepsea.jp/en/deepsea/about/

Toyama Bay stretches approximately 46 km from east to west and 74 km from south to north. It has an average water depth of about 550 meters, with a maximum depth reaching 1,114 meters. The seabed primarily consists of soft sediments, including mud and silt, particularly in the deeper regions. Shallower areas have sandy or gravelly substrates, often influenced by riverine inputs from nearby rivers such as the Jinzu, Sho, and Kurobe Rivers. The water in the bay is made of three different water masses: the coastal surface layer water (~50m) with low salinity due to the freshwater from the river and precipitation, the Tsushima Warm Current (TWC) water (~ 200m) with high salinity, and the deep water (Japan Sea Proper water). The surface circulation in the inner part of Toyama Bay follows a counterclockwise pattern along the coast, moving from west to east. This circulation is influenced by the eastward coastal branch of the TWC, which flows into the upper layer of the bay. The deep water primarily remains separated from the surface waters by the TWC water and does not mix easily with the shallower layers.

Line 65: "are greater is this area than in most areas worldwide", that does make this site a good choice, but also means that the found percentage contribution from fresh ground water to primary production is likely a maximum compared to other sites known to have fresh ground water inputs. I miss this comparative observation in the manuscript.

Thank you for your comments. The reported SGD flow rates in the eastern coastal area of the bay, observed as approximately 72–187 $cm\ day^{-1}$ (Zhang et al., 2005), are greater than the global average of 6.5 $cm\ day^{-1}$ (Santos et al., 2021). The dissolved inorganic nitrogen (DIN) and dissolved inorganic phosphorus (DIP) fluxes entering Toyama Bay via SGD, estimated using a box model, are 2.13 $mmol\ m^{-2}\ per\ day$ and 0.02 $mmol\ m^{-2}\ per\ day$, respectively (Hatta et al., 2005), which are comparable to the global averages of 4.06 $mmol\ m^{-2}\ per\ day$ and 0.06 $mmol\ m^{-2}\ per\ day$ (Santos et al., 2021). We have included a clarification of this comparative observation in the revised manuscript.

Line 114: "precipitation from the GVP-MSM was specified in the model", was atmospheric deposition of nutrients also considered in the model? If not, please state so clearly.

We did not consider atmospheric nutrient deposition in the model. Thank you for pointing this out, and we have made this clear in the revised manuscript.

Line 119: here the land-based inputs are specified as being the riverine contributions and the fresh ground water contributions. But what about other sources? The bay seems to be densely populated (Toyama city alone has a population of over 400,000) so I would expect significant direct discharges in the area (e.g. sewage discharges, industrial discharges). These are generally lower in nutrient loads than rivers but could be of the same order of magnitude as the ground water loads. Naturally these discharges would be at the surface, and thus mixed with the riverine signal, but some information on these sources would be beneficial. Is there any aquaculture activity in the bay that could add to the nutrient loads?

Thank you for your comments. Our riverine nutrient loads have accounted for some nutrient contributions from industrial and sewage discharges. We provided the necessary explanations regarding these sources in the revised manuscript. In addition, there is some aquaculture activity in Toyama Bay, but its scale and impact on nutrient loads appear limited. The region's nutrient dynamics are more significantly influenced by inputs from rivers and SGD, which contribute considerable amounts of nutrients from land to the bay.

Line 120: please specify what constitutes a first class and a second class river.

Thank you for highlighting this. Regarding these rivers, we included a table (Table S1) in the revised supplementary materials to list all the river names along with their discharge and nutrient loads.

Line 154: for me the model schematic belongs in the main text, not the supplementary materials. And does the biogeochemical model have a name?

Thank you for your feedback. We agree that the model schematic is an essential part of the explanation and should be included in the main text rather than the supplementary materials. We relocated it accordingly in the revised manuscript. Regarding the biogeochemical model, it does not have a specific name but is a customized NPZD-type model developed for this study.

Line 162: "The nutrient (DIN and DIP) loadings (Figure 2c)", Figure 2c only shows DIN, not DIP. I would prefer to see DIP also.

We provided a figure (Figure S1) showing the DIP loadings in the revised supplementary materials.

Table 1: the information is appreciated but for me this could be in the supplementary materials.

Thank you for your input. We agreed with your suggestion and moved this table to the revised supplementary materials as Table S2.

Line 184-186: am I correct in assuming that any initially present nutrients (pelagic, benthic) represent the residual term, and that these are replaced by the named nutrient sources over time as the residual goes to zero? Does this mean there is no long term storage of nutrients in the sea bed?

Thank you for your observation. Yes, the initially present nutrients can be considered as the residual term, which diminishes over time as they are replaced by the named nutrient sources. Long-term storage of nutrients in the sea bed was not considered in our model, as its contribution is minimal.

Line 211: why are the model results from 2015 and 2016 validated using observations gathered from 1934 to 2001? Surely more recent observations are available? Given the rise in global temperature across the 20th century which is continuing to accelerate in the 21st century I would expect a discord between historic observations and current simulations. Not to mention the population increase in the area over the observational time span and since. I think the authors are doing themselves and their model a disservice like this. It may be that more recent observations are not available, but this should be discussed in the main text: discrepancies between the model results (2016) and the observations (1935-2001) are not necessarily the models fault.

Thank you for raising this point. We agree with you that using observational data from a different time period may introduce discrepancies, given the potential changes in climate, human activities, and environmental conditions over time. Unfortunately, the availability of concurrent observational data is limited, which makes direct validation challenging.

To address this, we opted to use multi-year historical data as the best available reference for validation. We have now included a discussion in the revised manuscript to explain this limitation and to clarify that discrepancies between the model results and the historical observations are not necessarily due to model inaccuracies but may also reflect temporal changes in environmental and anthropogenic factors.

Line 222: it would help to have a visual overview of the main (horizontal) current patterns in the bay.

Thank you for your suggestion. The current field is shown in Figure S4 of the revised supplementary materials.

Line 234: month → months?

Thanks. We have made this change in the revised manuscript.

Figure 3: it would be good to know the temporal resolution of the observations being compared to the simulated monthly average. Some text on this should be included in the main text. A simulated monthly average is unlikely to validate well against observations if these amounted to a handful a month. See also Skogen et al (2021).

Thank you for your suggestion and the reference. Since the number of observations varies across locations, we have included a distribution figure (Figure S2) in the revised supplementary materials that shows the quantity of observation data available for each point.

Figure 4: please indicate the direction of the spatial axis. Distance from where? This figure could also be enlarged to show more detail in the euphotic zone.

Thank you for pointing this out. The direction of the spatial axis represents the transect along the S1 line from north to south, as shown in Figure 1b, with the distance from the northern end of the S1 line. In the revised manuscript, we have added this clarification to the figure caption. We have also enlarged the figure to show more detail in the euphotic zone.

Line 245: which are stratified in summer → with stratification in summer.

Thanks. We have made this change in the revised manuscript.

Line 271: yes, not surprising in an NPZD model. Is there any dynamic mortality for zooplankton included that could cause differences between zooplankton dynamics and that of their food source?

Thank you for your comments. In this study, the focus has been primarily on nutrient and phytoplankton dynamics, so we have not incorporated dynamic mortality for zooplankton. As a result, zooplankton dynamics closely follow the availability of their food sources (such as phytoplankton). However, incorporating dynamic mortality could provide more realism by introducing factors such as predation, starvation, and environmental stress, which might decouple zooplankton dynamics from their food sources. We would like to treat this modeling effort as our future works.

Figure 7: shows an autumn bloom that is not mentioned in the text anywhere. And the transect starts at the mouth of the Jinzu river, correct? Again, it is not mentioned what the distance from refers to.

Thank you for pointing this out. Regarding the autumn bloom, we updated the text to mention this event in the revised manuscript.

As for the transect, it is correct that it starts from the mouth of the Jinzu River. We have noted your suggestion and clarified the distance reference in the figure caption to make it more precise.

Line 283: as transect S2 seems to start at a river mouth it is not surprising that riverine nutrients account for more than 50% of nutrients close to shore. I would have expected it to be more. Is the rest input from the Japan Sea?

Yes. The rest is primarily from the Japan Sea (Figure S11).

Line 315: please provide the annual average percentages. ? And would it be possible to add a spatial plot with the average, annual, depth-integrated contributions from rivers and SGD to primary production in addition to figure 10?

Thank you for your suggestion. The annual-volume-averaged concentration of total phytoplankton was 0.456 mmolN m^{-3}, with phytoplankton supported by nutrients from river water, SGD, and the Japan Sea contributing 0.055 mmolN m^{-3} (12%), 0.018 mmolN m^{-3} (4%), and 0.383 mmolN m^{-3} (84%), respectively. We have provided these annual average percentages of nutrient contributions in the revised manuscript. Additionally, we included two spatial plots (Figure S12 and S14) that show the average, annual, depth-integrated contributions from rivers and submarine groundwater discharge (SGD) to primary production in the revised supplementary materials.

Discussion: in general I miss a discussion here about the transferability of these results to other sites. What about sites which have benthic phytoplankton or macrophytes on the seabed? Or larger rivers which reach more offshore areas? The limitations of the applied model (no benthic phytoplankton, no nutrient storage in the sea bed it seems) should be discussed in this light. An NPZD model is after all a relatively simple model. This also applies to the statement at the beginning which said this site *experiences high ground water discharges. Does that mean the found values (percentages contribution from rivers and SGD) can be seen as maximums with regard to other sites?*

Thank you for pointing out this important aspect.

We agree that the discussion could benefit from addressing the transferability of these results to other locations with differing characteristics, such as sites with benthic phytoplankton, macrophytes, or larger rivers that extend their influence farther offshore. We have included a discussion (on lines 471-475 of the revised manuscript) of these factors, emphasizing the limitations of our applied NPZD model, which does not currently include benthic phytoplankton in the seabed. These limitations could influence how results are interpreted in different contexts.

Furthermore, geographical conditions can also have an influence on the utilization of nutrient sources originating from SGD. For example, in Toyama Bay, due to its substantial variations in water depth, SGD-derived nutrients in deeper areas are less likely to be utilized, especially in the absence of benthic phytoplankton. Consequently, it is difficult to consider these contributions as maximum values applicable to other locations.

Thank you for your valuable suggestion; we have incorporated this discussion in the revised manuscript.

Line 340: Case 2 than Case 1 → in Case 2 with respect to Case 1.

Thanks. We made this change in the revised manuscript.

Line 365: shouldn't the term "photosynthesis" be replaced by "uptake"?

Thank you for the suggestion. We have revised this word.

Figure 12: the standard figure sub-numbering is left to right, then down, not down and then right. Figure 12e should read Case 2.

Thank you for pointing this out. We have revised this figure following your comments.

Figure 14: great figure, and I would prefer to see the equivalent graph for riverine sources in the main text.

Thank you for the suggestion. We have moved the equivalent graph for riverine sources to the revised manuscript, now labeled as Figure 15.

Line 408: please make the conclusions section self-explanatory by avoiding acronyms like SGD without explanation.

Thank you for the comments. We have revised the conclusions section to ensure it is self-explanatory and clear for all readers.

Line 412-413: this result mentioned in the conclusions is only supported by evidence in the supplementary materials. I would argue that any conclusion presented here must be supported by material included in the main text.

Thank you for your comment. We agree that the conclusions should be clearly supported by evidence presented in the main text, rather than relying solely on supplementary materials. We have revised this part to ensure that all statements are directly supported by the main text of the manuscript.

Line 415: please define "close to river mouth areas"

Thank you for your comment. "Close to river mouth areas" refers to regions within approximately 20 km from the mouth of a river. We also included this definition in the revised manuscript for clarity.

Line 417: please specify the contribution of SGD nutrients in percentage to the total, this is the main objective of the manuscript.

Thank you for your suggestion. This has been stated in the revised manuscript (on lines 449-450) to ensure it aligns with the focus of the study.

Line 419: I would say it was based on simulations with and without the buoyancy effect, as it is in essence the same model.

Thank you for highlighting this. We agree with your expression and have made this revision in the revised manuscript.

Line 426: "the shallow water depth allows for inclusion in the photic zone and thus use by phytoplankton"

Thank you for your suggestion, which indeed clarifies the expression. We have made this modification in the revised manuscript.

Line 427: please rephrase to make it clear you are still talking about the SDG-derived nutrients, what distance from the coast you are referring to and why it would be difficult for phytoplankton to use them. Surely you are referring to the lack of dispersal of these nutrients to offshore euphotic areas? That does not make it difficult for plankton to use them, it simply means they have no access to them. Given the objective of this manuscript, listing the relative contributions here (in %) and the area affected (in km) by them should be a priority.

Thank you for your thoughtful suggestion. We agree with your points and have revised the manuscript following your suggestions. We clarified that the challenge is not the phytoplankton's ability to use the nutrients but rather the nutrients' lack of dispersal to offshore euphotic zones. We also included the relative contributions (4%) and affected areas (~3 km from the shore) of the SGD-derived nutrients in the revised manuscript.

References

Lenhart, H. J., & Große, F. (2018). Assessing the effects of WFD nutrient reductions within an OSPAR frame using trans-boundary nutrient modeling. Frontiers in Marine Science, 5, 447.

OSPAR (2013) Distance to target modelling assessment, report 2013-599, ISBN 978-1-909159-32-7, https://www.ospar.org/documents?v=7319

Painting, S. J., Van der Molen, J., Parker, E. R., Coughlan, C., Birchenough, S., Bolam, S., Aldridge, J.N., Forster, R.M. & Greenwood, N. (2013). Development of indicators of ecosystem functioning in a temperate shelf sea: a combined fieldwork and modelling approach. Biogeochemistry, 113(1), 237-257.

Skogen, M.D., Ji, R., Akimova, A., Daewel, U., Hansen, C., Hjollo, S.S., van Leeuwen, S.M., Maar, M., Macias, D., Mousing, E.A., Almroth-Rosell, E., Sailley, S.F., Spence, M.A., Troost, T., van de Wolfshaar, K. (2021) Disclosing the truth: are models better than observations?, Marine Ecology Progress Series, DOI: 10.3354/meps13574

Thank you for sharing these references.