Reply to Editor:

Dear Menghong Dong and co-authors,

two expert reviewers have independently evaluated your manuscript. While both aknowledge the merit in your work, they raise important concern, which I would like to underscore and support herewith. Most importantly, the reviewers struggle with the presentation of your data and would like to see some baseline evaluation / groundtruthing of the applied model. Further criticism arose with respect to balancing the regional nature of your measurements with a wider extrapolation.

From your answers, I understand that you aim to follow the reviewers' suggestions for improvement and I would thus like to invite you to hand in a revised version of your MS.

With the best wishes, Helge

Dear Helge,

Thank you for your thoughtful feedback and for the opportunity to revise the manuscript. We appreciate the reviewers' efforts in evaluating our work and acknowledge the validity of the concerns raised.

Regarding the groundtruthing of the model, we have addressed model validation in the supplementary materials (Figure S3), which is mentioned before section 3.1. There are some discrepancies between model outputs and field observations, which likely result from mismatches in the temporal resolution of the observation data (snapshot) and model outputs (monthly mean) as well as their different time periods. However, the spatial distribution and seasonal patterns derived from both observation and modeling are consistent, indicating that the model reasonably captures the major features of ecosystem in the Toyama Bay.

Additional, we have strengthened the comparison of our results with those from other regions in the revised manuscript. In this study, we did not consider benthic phytoplankton, such as seagrass, on the seabed, and the distribution of submarine groundwater discharge (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 prepared a comprehensive revised version of the manuscript that incorporates these improvements and addresses all reviewer comments. We aim to resubmit the revised manuscript promptly and welcome further feedback.

Reply to Referee #1:

I struggled with the manuscript for many hours trying to figure out what the authors are saying, mainly because the presentation is very poor and very reader-unfriendly. My comments are divided into two categories:

We apologize for the poor presentation and greatly appreciate your helpful comments and suggestions. Our responses to the comments are as follows. The referee's comments are cited in italics.

Science:

1. I have followed Profs. Guo's and Zhang's work for years and have no doubt about Guo's modeling ability and the high quality data from Zhang's lab. The NPZD model, however, is a very simple one so ground-truthing is important. I failed to notice the comparison of field data and model results although clearly large discrepancies exist. It should also be said whether sea grasses or other plants on the bottom are important.

Thank you for your valuable feedback.

We have addressed model validation in the supplementary materials (Figure S3), which is mentioned before section 3.1. There are some discrepancies between model outputs and field observations, which likely result from mismatches in the temporal resolution of the observation data (snapshot) and model outputs (monthly mean) as well as their different time periods. However, the spatial distribution and seasonal patterns derived from both observation and modeling are consistent, indicating that the model reasonably captures the major features of ecosystem in the Toyama Bay.

Although seagrasses are present in Toyama Bay, they are not particularly significant in the eastern part of the bay, where submarine groundwater discharge predominantly occurs (Ministry of the Environment, 2008). Therefore, seagrasses and other benthic phytoplankton were not included in our model. We have clarified these aspects in the revised manuscript (on lines 181-182, and 237-240).

2. It is not clear why 70m was chosen as the lower limit of the SGD input. How the SGD discharge distributes within this 70m range was also not given.

The 70-meter depth limit for SGD input was determined based on prior observational studies in Toyama Bay, which indicate that groundwater discharge is primarily concentrated in the nearshore eastern regions of the bay (Hatta and Zhang, 2013). This area is characterized by the steep seabed and the localized nature of SGD, and therefore the zones deeper than 70 m have little groundwater discharge.

Additionally, because the spatial distribution of SGD discharge in this area is not distinctly defined, we assumed a uniform distribution of SGD discharge over all the grid points in this area. We have added this explanation on line 109 of the revised manuscript.

3. For coastal modeling work frequently moving boundary is applied. I assume that the land boundary is not moving in this case because the tidal range is very small in the Japan Sea. It should be stated so.

Thank you for your insightful comments. We agreed and included this clarification in the revised manuscript (on lines 145-146), stating that a moving boundary is not applied because of the small tidal range in the Japan Sea.

4. Should explain the huge residual in Fig. 10.

Thank you for pointing out this issue. We assumed that the initially present nutrients represent the residual term, which are gradually replaced by the modeled nutrient sources over time. If it eventually diminishes to zero, we can judge that the initially present nutrients have been fully replaced the specified external sources of nutrients. If not, we need to define the other external sources of nutrients and make it approach zero. We have added this explanation on line 333 of the revised manuscript.

5. Figures 8,9,11,12: not clear whether they are simply comparisons of SGD vs. rivers, or the ocean-side input is considered.

They are individual simulation results that do not consider ocean-side inputs. The simulations for SGD-derived nutrients only consider inputs from SGD, while those for riverine nutrients solely account for river inputs. We have added annotations to the figure captions for clarity.

Presentation:

1. It is not even clear how much SGD contributes to the study area. It is said at several places that the SGD contributes slightly more nutrients than all rivers combined to the study area. Yet, line 169 says that ...the SGD mean value of DIN loading of 26.7 g/s ...is approximately 20% of total riverine nutrient loading. Only two lines above it is said that the SGD nutrient loading is the same as rivers.

We are sorry for the misleading description. The value of 20% is for the total riverine nutrient loading into Toyama Bay. “The same as rivers” is for the nearby rivers close to the SGD sites, which represent only a part of rivers along the coast of Toyama Bay. In revision, we will delete the sentence with “is approximately 20% of total riverine nutrient loading”.

2. Figure 10. There are many symbols to use. It is not possible to differentiate red circles from pink ones.

Thank you for pointing this out. We have revised this figure (now labeled as Figure 11 in the revised manuscript) to improve clarity.

3. Line 433, Should use positive tone. Instead of saying that "we paid little attention to..." should say that seasonal and short term variations will be the focus of future work.

Thank you for your suggestions. We have made this change on lines 487-488 of the revised manuscript.

4. It would help the readers if the currents are plotted when horizontal distributions of parameters are provided.

Thanks. We have included a figure showing the currents in the supplementary materials (Figure S4).

Minor points:

1. Line 37 quotes Santos et al., 2021 as summarizing the SGD-related nutrient inputs. Wilson et al. (L&O Letters, 9,4,411,2024) gave a much more comprehensive summary.

Thank you for your suggestion. We have revised the text (on lines 41-42 of the revised manuscript) to incorporate the more comprehensive summary provided by this literature to ensure the introduction is thorough and up to date.

2. Figure 1, state that the depth contours are in "m", and put "A, B, C, D" at the proper places.

Thanks. We have revised this figure following your suggestions.

3. Figures 4,7 and 8. The X-axis is distance, but from where?

Thank you for pointing this out. This was our oversight. The distance on the x-axis is measured from the shore and we have added the relevant explanation to the figure captions.

4. Figure 6 is about phytoplankton but the color bar gives N.

To maintain consistency, we used the raw model output data (in units of mmol N m^{-3}) to present the phytoplankton concentration (now labeled as Figure 7 in the revised manuscript).

5. Figures 7, 8, what is the unit for the color bar?

The unit for the color bar in Figure 7 (now labeled as Figure 8 in the revised manuscript) is mmol N m^{-3}, while Figure 8 (now labeled as Figure 9 in the revised manuscript) does not have a unit as it represents proportional values. We have provided additional clarification in the figure captions.

6. Figure 13, areas 1,2,3,4 should be A, B, C, D.

To differentiate from the labels (A), (B), (C), and (D) used in Figure 1b, we used 1, 2, 3, and 4 here to represent the four different depth areas within the blue line region near the SGD outlet locations shown in Figure 1c. This figure is now labeled as Figure 14 in the revised manuscript.

7. Line 586, "T"oyama "B"ay; should state (in Japanese).

Thank you for pointing this out. We have adjusted the capitalization of the initial letters on line 662 of the revised manuscript, and confirm that this document is written in English, not Japanese.

Reference

Hatta, M. and Zhang, J.: Temporal changes and impacts of submarine fresh groundwater discharge to the coastal environment: A decadal case study in Toyama Bay, Japan, J. Geophys. Res. Ocean., 118, 2610–2622, https://doi.org/10.1002/jgrc.20184, 2013.

Ministry of the Environment: The 7th National Survey on the Natural Environment: Report on Shallow Marine Ecosystems Survey (Seaweed Bed Survey), https://doi.org/https://www.biodic.go.jp/reports2/6th/6_moba19/6_moba19.pdf, 2008.

Reply to Referee #2:

Manuscript overview

The 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 (on lines 482-486). 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 on lines 59-60 of the revised manuscript to acknowledge their contributions.

Line 60: and hence eutrophication issues.

Thanks. We have added this presentation on line 64 of 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 (on lines 71-77).

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 on line 179 of 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 (on lines 180-181). 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 (Figure 3). 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 (on lines 228-229 of the revised manuscript) 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 (on lines 237-241 of the revised manuscript).

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 on line 255 of 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 (now labeled as Figure 5). 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 on lines 267-268 of 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 (on lines 286-287).

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 (now labeled as Figure 8 in the revised manuscript) 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 (on lines 346-348). 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 482-486 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 on line 369 of the revised manuscript.

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

Thank you for the suggestion. We have revised this word on line 394 of the revised manuscript.

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 (now labeled as Figure 13 in the revised manuscript) 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 (on lines 452-455 of the revised manuscript) 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 on line 457 of 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 458-460) 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 on line 462 of 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 on lines 474-475 of 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 (on lines 475-476 of the revised manuscript). We also included the relative contributions (4%) and affected areas (~3 km from the shore) of the SGD-derived nutrients in the revised manuscript (on lines 459, 469-471).

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.