Review of the manuscript *Effects of submarine groundwater on nutrient concentration and primary production in a deep bay of the Japan Sea*, by M. Dong, X. Guo, T. Matsuura, T. Tebakari and J. Zhang, submitted to **Biogeosciences**

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.

Recommendation

Minor revision

Detailed Comments

1. 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.
2. Line 18: "*middle and bottom layers*", please specify these layers as this is very location specific.
3. 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.
4. Line 60: and hence eutrophication issues.
5. 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.
6. 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.
7. 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.

8. 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?
9. Line 120: please specify what constitutes a first class and a second class river.
10. Line 154: for me the model schematic belongs in the main text, not the supplementary materials. And does the biogeochemical model have a name?
11. Line 162: "*The nutrient (DIN and DIP) loadings (Figure 2c)*", Figure 2c only shows DIN, not DIP. I would prefer to see DIP also.
12. Table 1: the information is appreciated but for me this could be in the supplementary materials.
13. 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?
14. 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.
15. Line 222: it would help to have a visual overview of the main (horizontal) current patterns in the bay.
16. Line 234: month → months?
17. 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).
18. 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.
19. Line 245: which are stratified in summer → with stratification in summer.
20. 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?
21. 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.

22. 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?
23. 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?
24. 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?
25. Line 340: Case 2 than Case 1 → in Case 2 with respect to Case 1.
26. Line 365: shouldn't the term "*photosynthesis*" be replaced by "uptake"?
27. Figure 12: the standard figure sub-numbering is left to right, then down, not down and then right. Figure 12e should read Case 2.
28. Figure 14: great figure, and I would prefer to see the equivalent graph for riverine sources in the main text.
29. Line 408: please make the conclusions section self-explanatory by avoiding acronyms like SGD without explanation.
30. 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.
31. Line 415: please define "close to river mouth areas"
32. Line 417: please specify the contribution of SGD nutrients in percentage to the total, this is the main objective of the manuscript.
33. Line 419: I would say it was based on simulations with and without the buoyancy effect, as it is in essence the same model.
34. Line 426: "*the shallow water depth allows for inclusion in the photic zone and thus use by phytoplankton* "
35. 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.

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