Second 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

Already given in the first review round.

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.

Recommendation

Minor revision

Detailed Comments

1. Line 19: "*and used by the phytoplankton* for *growth*"
2. Line 129: the term "first class rivers" is still not explained in the text.
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.

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.
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.
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?
7. Line 295: "*changes in detritus* (derived from phytoplankton and zooplankton) *coincide with*"
8. Line 399: I don't see a detritus flux to the sediments in Figure 13, I assume you mean Figure 14.
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.
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.
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.
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 sphere. 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.