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Biogeosciences Discussions

Interactive comment on "Contribution of riverine nutrients to the silicon biogeochemistry of the global ocean – a model study" *by* C. Y. Bernard et al.

Anonymous Referee #1

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This paper aims to demonstrate the importance of riverine input of DSi to the global biogeochemical cycles by adding the riverine fluxes of nutrients to a global scale biogeochemical general circulation model. Especially, the focus is on the export of the nutrients to the open ocean and how long these nutrients are available to support the plankton production before being exported out of the system.

The integration with the general circulation model is the most important emphasis of the paper, as the riverine influx used in this study is very similar or even extracted from the same database as recent papers by Beusen et al. (2009) and Dürr et al. (2009).

In the introduction the authors correctly stress that our perception of the functioning

of the biogeochemical cycles in the global ocean requires a far better understanding of continental margins processes. Especially benthic/pelagic coupling, tidal currents, coastal upwelling and wind forcing are crucial in this context.

I think this study does a good job a producing a first crude understanding of how riverine influxes are redistributed over coastal zones and the ocean. The modeling efforts are based on simulations with two numerical models. The paper produces interesting modeling results that are worth publication. It is however, as the authors also indicate in their conclusion, only a first step to a full integration of nutrient cycling, including continental shelf seas and land/ocean coupling, in a whole Earth system context. I cannot but feel that the main conclusions and results of the manuscript do not live up to the expectations created in the introduction: it is not really news that silica inputs from land to ocean are highly heterogeneous, and the identification of riverine hotspots for DSi delivery is not new either.

Particularly, I also feel that the authors could have done a bigger effort to try and include the sensitivity of their results to human activity in their analysis. Several factors in the manuscript approach currently prevent this. For example, the authors use DSi riverine fluxes from pre-industrial periods, as well as pre-industrial CO2 concentrations in the atmosphere. On the other hand, riverine C, N and P concentrations are set to the reference year 1995. This pre-industrial setting for some variables with post-industrial settings for other variables reduces the practical applicability to "our real world" of the analysis, especially as continental margin biogeochemical cycling is heavily modified by human intervention in the silicon cycle through e.g. dams and land use changes. I felt a bit disappointed to only find reference to this problem in the discussion. It might have been better to provide the readership with a set of different boundary conditions. As this is a concentrated modeling effort, providing simulations with estimated post-industrial fluxes of Si, and using e.g. different scenarios for N and P input, might prove useful for predicting how expected reductions in N, P as a result of improved water purification, and decreased Si fluxes will potentially impact on ocean and coastal

productivity.

I also do not entirely understand how the authors ran a global circulation biogeochemical model without input from rivers (page 4929 and beyond, also figure 2). As the authors correctly state later, riverine input is necessary to balance the burial of nutrients to the deep ocean. Do the authors mean that a fixed input to the ocean was used in these runs without riverine inputs (not spatially heterogenized)? Otherwise, I do not understand how this could have been a realistic model run, and what is the use of such a run.

My main concern with this manuscript is that the authors create huge expectations in the introduction, but the conclusions do no live up to these expectations. This, in my opinion, should not prevent its eventual publication. However, the authors should emphasize in their introduction that the main novelty in this paper lies in that it is the first to combine both a riverine input and global circulation model for estimating influence of riverine fluxes on global Si cycling. However, several factors (including the lack of post-industrial Si concentrations mentioned above) impede its practical applicability to really highlight the importance of of continental margin dynamics for ocean and coastal phytoplankton production. Next to encouraging the authors to provide post-industrial scenarios for all variables, I would also like to see further clarification of a few other issues:

- why is opal production limited to 0.5DSi? On which study or value is this based?

- what is the effect of assuming constant fluxes all over the year, neglecting seasonality. The biogeochemical cycling in continental margins is highly seasonal, as well as nutrient inputs from the continents.

- Page 4929, line 6: how is silicon uptake related to the other nutrients?

- I do not see any particular reason for performing a run for the amazon without Si (page 4930, line 20 and beyond). It is obvious this limits opal production.

- Page 4931: line 19 says NO PHOTOSYNTHESIS occurs in the Arctic Ocean. The next sentence then emphasizes that photosynthesis is not responding to... How can photosynthesis respond when there is NO photosynthesis?

- Overall, I feel the discussion on the hot-spots in the results section can be seriously reduced (page 4930-4931). There is particularly large overlap with recent papers on riverine export (Beusen et al. 2009; Dürr et al. 2009). I also think that the discussion points from both previous points are quite obvious and not worth particular emphasis. The authors need however to especially specify how they have performed the run "without river inputs" (see earlier), as their whole hot-spot analysis is dependant on it.

- Discussion, page 4933, 11-23, again redundant with these previous papers

- I do not understand the sentence in line 28, page 4933. Why would it not compensate. There is no upwelling, but why could it not compensate if there was?

- Discussion, line 6 and beyond, page 4934. Is this not mainly because of the absence of photosynthesis in the arctic ocean?

Finally, I would like to make some minor comments.

Page 4920, line 23. Full stop behind ocean.

Page 4920, line 24. Put ":" behind "2001) "

Page 4921, line 2-3 remove "as well as...", redundant with line 1.

Page 4921: bring line 22-24 forward to after line 9

Page 4923, line 26: unusual to refer to PhD thesis in this way Page 4927, line 14-22: can this be reduced?

Page 4929, line 1-4: you should not emphasize all you will do in the future

Page 4930: figure 3 is only mentioned after figure 4

References

Beusen, A. H. W., Dekkers, A. L. M., Bouwman, A. F., Ludwig, W., and Harrison, J.: Estimation of global river transport of sediments and associated particulate C, N, and P, Global Biogeochem. Cy., 19, 17 pp., 2005.

Durr, H. H., Meybeck, M., Hartmann, J., Laruelle, G. G., and Roubeix, V.: Global spatial distribution of natural riverine silica inputs to the coastal zone, Biogeosciences Discuss., 6, 1345–1401, doi:10.5194/bgd-6-1345-2009, 2009.

Interactive comment on Biogeosciences Discuss., 7, 4919, 2010.