Biogeosciences Discuss., 7, C2572–C2576, 2010
www.biogeosciences-discuss.net/7/C2572/2010/
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
Received and published: 27 August 2010

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.

We thank the reviewer for his comments and suggestions, they will help to improve the manuscript. In the revised version, we will clarify the issues raised by the reviewer to the best of our abilities, notably on the reference years for the different nutrients. The introduction will be modified to emphasize that the use of preindustrial riverine inputs of DSi was considered as a first step in tackling the issue of perturbation of the riverine DSi inputs on a global scale.

Sensitivity of the shelf seas ecosystem and comparison with different model was described in another paper (Bernard et al., 2010). It should be noted however, that simulating coastal areas correctly is still very difficult (Allen et al., 2010). The major effort here was to get a spatialized picture of effect of riverine DSi inputs, which is new. Coupling of modern day (post industrial) and scenarios of climatic and anthropogenic perturbation of the riverine DSi fluxes is in progress (not available for the present study); it is now made possible since better data is constantly becoming available.

As this is a concentrated modeling effort, providing simulations with estimated postindustrial 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.

In preparation, the next step will be the implementation of Global NEWS databases of river nutrient fluxes based on Millennium Ecosystem Assessment scenarios as soon as made available. This work will lead to a new manuscript.

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.

The multi millennial residence time of N, P, Si and C allows us to run HAMOCC5 without riverine inputs on a short time (100 years) without causing any severe drop of the oceanic nutrient concentrations. Most of GCM used at the moment to study anthropogenic carbon uptake from the atmosphere do not include riverine nutrients.

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.

The authors consider important to state in the introduction how little we know about the fate of riverine DSi inputs on the shelf. The description of the effect of regionalised inputs of silica on the shelf's biogeochemistry aims at giving a simple description of DSi concentration and opal production supported by the rivers as a first step towards a better understanding of the shelf seas as land/ocean interface. As mentioned above data on DSi concentration and discharge are scarce, only recent models can provide post industrial riverine DSi inputs, allowing scenarios of future nutrient fluxes (Beusen et al., 2009). This will be the next step. The introduction will be clarified.

Next to encouraging the authors to provide post-industrial scenarios for all variables, The authors are fully aware of the interest of coupling present day riverine fluxes and post industrial scenarios. We are currently working on it and it will soon lead to a new manuscript. The general description of riverine fluxes (even pre industrial) constitutes for us the first step of this reflexion.

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?

(Same answer as for Referee #2) Unfortunately little information is available to document equations in this version of HAMOCC5, the version used here is an upgraded version of HAMOCC3.2 and has been continuously improved and calibrated. As all current GCMs and OGCMs, this is a heuristic model and many equations and parameters result from "educated guesses" without explicit references and backing up studies. The 0.5 DSi limitation slows down the DSi up take and depletion in the euphotic layer to better compare with observations. This model set up was used by Six and MaierReimer (1996), Wetzel (2004) and Wetzel et al. (2005).

Six and MaierReimer (1996) stated: "Despite the simplicity of our model the simulation reproduces a similar diatom pattern as found in a more complex diatom model representation from Aumont et al (2003) with only slightly higher relative diatom abundance in the equatorial Pacific in our model"

- 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.

Assuming constant fluxes all over the year is due to the lack of data. The effect of such an approximation has a different effect depending on the location of the riverine fluxes. For example we know that in Indonesia heavy rains during typhoon episodes provide most of the riverine inputs in a very short time (Meybeck et al., 2003). A solution could be to reconstruct a seasonal variation of the dSi inputs following available river runoff data. One bias would be that a realistic utilisation of the riverine DSi inputs relies on the accuracy of the delivery of the limiting nutrients (N,P,Fe). This reconstruction might result in introducing an even larger bias.

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

The detritus fraction that originates from the dead plankton and faecal pellets follows the Redfield ratio. DSi uptake and opal formation is related to the detritus fraction (det) via $R_{Si:P}$ in Eq.3

$$\mathrm{Pr}\,od_{opal} = \min\left(\frac{\Delta \det}{\Delta t} R_{Si:P} \frac{DSi}{\mathrm{K}_{\mathrm{Phy}}^{DSi} + DSi}, 0.5\,DSi\right)$$

The next sentence will be added to the manuscript:

"with $R_{Si:P}$ stoichiometric coefficient to relate P and Si according to the Redfield ratio."

- 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.

Coastal waters DSi is not only supplied by the Amazon River, a significant part is provided by the equatorial upwelling. This run aimed at showing which part of the opal production is supported by the Amazon River.

- 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?

Please, see answer below, to comment on photosynthesis in the Arctic Ocean).

- 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 aim of this paper is to provide estimates of the effect of riverine in puts of DSI on the biogeochemistry of the shelf seas and offer a chance to distinguish the relative support of rivers to opal production. The text might give the impression of a repetition since it deals with the same regions 3 times but it also allows a simple classification of the riverine inputs effect at the three important stages of the marine silica cycle. In the revised manuscript, we will do our utmost to reduce unnecessary repetitions and to shorten the discussion.

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.

The run without riverine nutrient inputs (100 yrs) was simply computed with the exact same forcing and river runoff but without any riverine nutrient inputs starting from the same spin up (3000 yrs) as the other scenario simulations. A 100 years run does not lead to overall new equilibra, but allows to track direct riverine influence.

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

Beusen et al. (2009) and Dürr et al. (2009) are both papers that describe the riverine fluxes to the coastal zone but not further out at sea than the river mouth, at the last measuring station, mostly located upstream of the marine (tidal) influence of estuaries or at the apex of deltas. Discussing the range of the riverine fluxes might sound redundant with these papers but it is impossible to discuss their effects on shelf seas biogeochemistry without providing to the reader their range.

- 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?

The sentence was rephrased.

“The silica delivery in these areas is dominated by direct riverine flux and delayed flux due to recycling in the sediments of the opal produced and initially supported by the riverine inputs”

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

We agree that it is an exaggeration to write that there is no photosynthesis in the Arctic Ocean. There is a weak primary production in the Arctic Ocean due to an overestimation by the model of the sea ice coverage. In summer, the Siberian coast is a large ice free zone of the Arctic; this allows utilisation of Siberian rivers inputs of DSi. Of course the sea ice cover benefits to the DSi concentration rise but the geography of the Arctic basin and its ventilation also benefits to comparatively high DSi concentration. The model results should however be considered carefully given the coarse quality of the sea ice cover representation in the model and the sensitivity of nutrients utilisation to sea ice cover, as shown in recent studies: sea ice retreat caused by climate change is expected to increase primary production by ~20% in the Beaufort Sea, affecting surface water nutrient concentration (Arrigo et al., 2008; Lavoie et al., 2010).
This is now included in the manuscript.

Finally, I would like to make some minor comments.
Page 4920, line 23. Full stop behind ocean.
Done

Page 4920, line 24. Put “:” behind “2001) “
Done

Page 4921, line 2-3 remove “as well as. . .”, redundant with line 1.
Done

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

Page 4923, line 26: unusual to refer to PhD thesis in this way
The reference was corrected.

Page 4927, line 14-22: can this be reduced?
Lines 14-22 were shortened.

Page 4929, line 1-4: you should not emphasize all you will do in the future
This section was added upon the request of a previous reviewer.

Page 4930: figure 3 is only mentioned after figure 4

Figures have been renumbered.

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