

## 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.

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*The authors try to address an important issue in ocean biogeochemistry, namely, the importance of riverine inputs of dissolved silica (DSi) to the global biogeochemical cycles on the global scale. They address the issue by adding the riverine fluxes of nutrients to a global scale biogeochemical general circulation model (BGCM) and examining the difference between output results with or without the riverine fluxes. In view of the recent attention given to Si as an important bio-active element, this study is timely in providing insight into the complicated interplays among various limiting ingredients in the marine ecosystem that has proven critical in the global climate system. It is commendable that the authors limit their scope of the paper by focusing on Si, while their colossal model system comprises rather complicated biogeochemical components. However, in a few places, they dwell on subjects not entirely relevant to the focus of this paper without illuminating the connections between the theme and some of the topics addressed, making the paper somewhat loose. On the other hand, the authors fail to address some critical topics directly related to the theme of the study or their tool of exploration, namely, the numerical models. Because of the wealth of information generated by the models it is promising that the authors will make the paper more concise and rigorous, turning it into a valuable contribution to the understanding of the global biogeochemical cycles crucial to the earth system that is undergoing transformation due to human perturbations.*

We thank the reviewer for his thoughtful comments and suggestions, they will particularly help to improve the manuscript in the way asked, namely to make it more concise and rigorous. In the revised version, we will clarify the issues raised to the best of our abilities.

#### Specific comments

##### 1. Introduction

*1.1. Lines 22-26 p. 4920: The authors rightfully stress the significance of continental margins (CMs) in the ocean biogeochemical cycles, but the references referred to are almost all more than ten years old. It is recommended that the authors refer to more recent publications on the CMs as a biogeochemical agent in the earth system, such as Mackenzie et al. (2005) and Liu et al. (2010), or cite syntheses of more recent findings regarding CM biogeochemistry, such as: Jahnke (2010)) and Ducklow and McCallister (2005).*

More recent references were added in the introduction.

“Recent modelling studies successfully pointed out the sensitivity of shelf seas to anthropogenic perturbations of the riverine studies but unfortunately omitting riverine DSi inputs and focusing mostly on the carbon sink/source role of continental margins (Ducklow and McCallister, 2005; Liu et al., 2010; Mackenzie et al., 2005).”

1.2. Line 19 p. 4922: *Yool and Fasham (2001) mainly addressed the fate of carbon injected from the shelf edge to the deep ocean. They cited Longhurst et al. (1995) for the continental shelf production. It is perhaps more appropriate to cite Muller-Karger et al. (2005), who made their own assessment of the total primary production of global continental margins.*

The reference was replaced, and the following text was inserted:  
“8.99 Pg C y<sup>-1</sup> according to Muller-Karger et al. (2005)”

1.3. *It will benefit the readers, if the authors would provide a more complete description of the global Si cycle as the background information. Especially useful is how the riverine fluxes balance the removal of DSi as opal in the ocean. Recent review papers, such as Ragueneau et al (2010), may be mentioned as supplementary information source.*

A very short description of the silica cycle will be inserted in the introduction, since other reviewers expressly asked for a shortening of the manuscript.

## 2. Model and data base

2.1. Line 8 p. 4924, *Misleading statement: “In the surface layer, nitrate (NO<sub>3</sub>), is linked to phosphate (PO<sub>4</sub>) by the Redfield ratio and is computed simply following the phosphate dynamics, multiplied by a constant stoichiometric ratio, RN:P.” I suppose this means that nitrate uptake or regeneration follows that of phosphate, but it could be wrongfully interpreted as nitrate concentration or distribution follows that of phosphate. It is better to rephrase the sentence.*

Yes, the sentence was rephrased:

“In the surface layer, nitrate (NO<sub>3</sub>) dynamics (uptake and remineralisation) follows phosphate (PO<sub>4</sub>) dynamics corrected by the Redfield ratio (molar element ratio: R<sub>N:P</sub>).”

2.2. Line 8 p. 4925, *“The predation of zooplankton on the phytoplankton”: There are many occasions of usage of predation like this in the paper. It is suggested to use grazing instead of predation.*

The replacements were made.

2.3. Line 14 p. 4926, *“The dissolution of opal depends on temperature and follows a constant dissolution rate,  $k_d$  (opal;DSi).” Is the temperature in Kelvin or Celsius? Change “follows a constant dissolution rate” to “the rate constant”.*

We rephrased as follows:

”With T: temperature (°C)”

”The dissolution of opal depends on temperature and rate constant,”

2.4. *It would be a more complete study, if the numerical experiment includes the run with riverine Si but without other riverine nutrients.*

A run with only riverine DSi nutrients would show higher DSi concentrations due to lower utilisation by siliceous plankton but is not realistic since the observation and prediction show decreasing trends of Si:P and Si:N

*2.5. It deserves an explanation why the model riverine nutrients include carbon, which is not exactly a nutrient element.*

Global NEWS includes particulate and dissolved organic forms of Nitrogen and Phosphorous, these elements are measured separately, while in the HAMOCC model code, they form a single entity: the detritus (particulate organic matter). The particulate and dissolved organic N and P had to be merged into detritus and DOM. In the code the detritus and DOM is formed by N, P and also C, linked by the Redfield ratio. If one of the organic elements is in excess regard to the Redfield ratio, the excess is added under its inorganic form. That's why C is not further discussed in the manuscript but was worth being mentioned.

This will be inserted in the manuscript.

*3. Global results, silicic acid concentration*

*3.1. Line 18 p. 4929: Heading: It is not clear why the authors choose this heading. As far as I can see, a simple "Results" should do. The opal fluxes are also presented in addition to distribution of silicic acid.*

This was a mistake and has been corrected.

*3.2. Line 19 p. 4929: "Mapping the silicic acid concentration of the ocean surface shows four major hotspots (Fig. 2d) when comparing the run with riverine silicon loads (Fig. 2b) to the run without (Fig. 2a): the Arctic Ocean, the Eastern Gulf of Guinea (coast of Cameroon, Gabon, Congo and Angola), the Amazon plume and South-Eastern Asia (Fig. 2d)." As I can see, there is a 5th hotspot in the eastern Equatorial Pacific, where there is no significant riverine input of DSi. It warrants some explanation.*

The Central and South American Pacific coast count among the hyperactive zones of riverine DSi inputs to the ocean (small basins but high fluxes). But given the size of the area that responds to riverine DSi inputs (not more than a few model grid cells), we chose not to discuss it in the manuscript. The increase of DSi concentration in the eastern Equatorial Pacific probably also results from recycling of DSi. 100 years run allows a little DSi enrichment of the Pacific waters up welled in that region. DSi concentration also benefits from iron in the Pacific Ocean which amplifies the effect of recycling.

*3.3. Line 7 p. 4930: "In the Eastern Gulf of Guinea, the largest river in Western Central Africa, the Congo River (21 Mt SiO<sub>2</sub> yr<sup>-1</sup> or 3.5 Tmol Si yr<sup>-1</sup>)" "3.5 Tmol Si yr<sup>-1</sup>" should be "0.35 Tmol Si yr<sup>-1</sup>".*

Thanks, this will be corrected in the revised manuscript.

*3.4. Lines 17-19 p. 4930: "The time series in Fig. 4 show the annual cycle of nutrients, photosynthesis, opal and calcium carbonate production on an integrated ocean surface of*

*1.74×10<sup>6</sup> km<sup>2</sup> adjacent to the Amazon mouth.” It may provide important insight into the model’s performance as well as the marine Si cycle in this hotspot region by comparing the modeled Si budget with that based on box model (DeMaster and Aller, 2001).*

The following sentence was inserted in the discussion:

“This is in agreement with DeMaster and Aller (2001), who estimated that nearly all of the riverine silicate inputs was transported offshore as either silicate or as biogenic silica.”

#### 4. Discussion

*4.1. Lines 20-23 p. 4932: “Rivers are the most significant input of silicic acid for the global ocean, thus also in our model and are assumed to balance the burial/redissolution of opal in the sediment.” It is confusing to the readers that Table 3 shows the global flux of opal deposition (32-34 Tmol y<sup>-1</sup>) much greater than the total riverine flux of DSi (6 Tmol y<sup>-1</sup>). The modeled opal deposition fluxes are also much greater than the previously estimated opal burial rate (e.g., DeMaster, 2002). The apparent imbalance needs be addressed.*

The computing cost of simulation does not allow a complete spin up of the sediment compartment. This was discussed in Bernard et al. (2010) as one of the weaknesses of high resolution GCM simulations in comparison with box models studies. The burial is therefore not further discussed in this paper. The solution would be to use acceleration methods, to run several years of the sediment compartment per each simulated year of the all GCM.

This will be added to the manuscript with a reference to Bernard et al. (2010).

*4.2. Lines 27-29 p. 4934: “As shown in Figs. 2 and 5, the north-westward Amazon plume input is so powerful that it exports its load up to a few hundreds of kilometres from the coast, supporting opal production far outside the continental shelf.” The far reaching biogeochemical impact of the Amazon plume is echoed by the observed oceanic uptake of atmospheric CO<sub>2</sub> in the plume (Körtzinger, 2010), which may be mentioned as a supporting evidence for the model results.*

We will add this as suggested.

*4.3. Lines 20-23 p. 4935: “The complex pattern of iron during its transfer to the ocean does not allow a direct flux from the rivers (Poulton and Raiswell, 2002; Wells and Mayer, 1991). Iron aggregates and settles in the estuary when freshwater and sea waters mixes (Poulton and Raiswell, 2002). A large part remains trapped in the estuary.” It is not clear why the authors discuss iron limitation in this paper focused on Si. I do not mean iron is not important. In fact, iron is so important and deserves a separate treatment on its own right. If the iron issue is to be addressed here, its connection to the Si cycle needs to be highlighted and it should be treated with rigor.*

The iron issue is mentioned here as a weakness in this study. The authors are fully aware that not including iron among the other riverine nutrients artificially creates a situation of iron limitation that might reduce DSi utilisation on the shelf and lead to an overestimation of its export to the open ocean.

*The assumptions made in this study do not take into consideration of more recent studies on iron cycling in the ocean. For instance, Chase et al. (2007) demonstrate that riverine iron from the northwestern US is not trapped in the estuary and promotes primary productivity. It*

*is especially problematic to apply the same assumptions to the southeast Asian margins, where the numerous mountainous rivers have rather short water residence time in estuaries. It is doubtful that the riverine iron would be effectively trapped.*

The authors chose not to include iron in order to avoid an unrealistic iron fertilisation of the coastal waters with the present day knowledge on the estuarine iron dynamics.

The following sentence was added to the manuscript:

“However recent studies show that most of the riverine iron inputs from the north-western US is not trapped in the estuary and supports coastal waters primary productivity (Chase et al., 2007). The transfer riverine iron might also be affected by the regional climates. Indeed, monsoonal climate would logically benefit to a fast transfer of iron to the coastal seas rather than to favour estuarine retention. “

## 5. Figures

5.1. Figure 3 Left, annually integrated photosynthesis (expressed in  $\text{mmol P m}^{-2} \text{ yr}^{-1}$ ): As shown in the figure, the global mean photosynthesis rate is about  $5\text{-}10 \text{ mmol P m}^{-2} \text{ yr}^{-1}$ , which, according to the C:P ratio of 122 assumed in the paper, corresponds to  $0.6\text{-}1.2 \text{ mol C m}^{-2} \text{ yr}^{-1}$  or  $7\text{-}14 \text{ g C m}^{-2} \text{ yr}^{-1}$ , which is much too small.

There was a unit conversion mistake that has been corrected.

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