

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

K.-K. Liu (Referee)

kkliu@ncu.edu.tw

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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.

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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.

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).

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.

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

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riverine fluxes balance the removal of DSi as opal in the ocean. Recent review papers, such as Reganeau et al. (2010), may be mentioned as supplementary information source.

2. Model and data base

2.1. Line 8 p. 4924, Misleading statement: "In the surface layer, nitrate (NO₃), is linked to phosphate (PO₄) 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 re-phrase the sentence.

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.

2.3. Line 14 p. 4926, "The dissolution of opal depends on temperature and follows a constant dissolution rate, k_{opal}/DSi ." Is the temperature in Kelvin or Celsius? Change "follows a constant dissolution rate" to "the 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.

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

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.

3.2. Line 19 p. 4929: "Mapping the silicic acid concentration of the ocean surface

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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.

3.3. Line 7 p. 4930: "In the Eastern Gulf of Guinea, the largest river in Western Central Africa, the Congo River (21 Mt SiO₂ yr⁻¹ or 3.5 Tmol Si yr⁻¹)" "3.5 Tmol Si yr⁻¹" should be "0.35 Tmol Si yr⁻¹".

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 1.74×10^6 km² 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).

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⁻¹) much greater than the total riverine flux of DSi (6 Tmol y⁻¹). 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.

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

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oceanic uptake of atmospheric CO₂ in the plume (Körtzinger, 2010), which may be mentioned as a supporting evidence for the model results.

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 (Wells and Mayer, 1991; Poulton and Raiswell, 2002). 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 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.

5. Figures

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

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