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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.

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This is an interesting study and I have no major comments except for two: the authors combine pre-industrial (or is it pre-dam) river DSi export with current N, P and C export, current climate. To show the importance of silicon in ocean biogeochemistry this may be all-right. However, some discussion could be added on the fact that all nutrients have changed. DSi has probably decreased, and N and P have increased, but with large variability of the changes in molar C:N:P:Si ratios. In addition, constant fluxes were used, which is another simplification. So the role of Si may be rather hypothetical.

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. The coupling of the riverine nutrient at that stage is the best we could at the time of the simulations. New data bases of scenarios of river nutrient are being developed at the present time. GlobalNEWS simulations based on Millennium Ecosystem Assessment scenarios now become available, and will be included in future model versions. This article aims at giving the best possible picture of the effect of riverine nutrients at present time, while being spatially explicit (in contrast to most earlier studies that use one average river input value for the whole global coast).
These 100 years simulations shouldn't be seen as correct real-world representations, but experiments that allow tracking direct riverine influence. This will be incorporated in the revised manuscript. At the time of the model simulation, the COSCAT DSi database was the only one to cover the global coastline.

The authors discuss the recycling of opal in comparison with N and P. Does this mean that opal recycling is slower than than of N and P, or N and P in organic matter? I wonder what this all means, since according to Treguer silicon is recycled many times before it is deposited on the ocean floor. How do the model results compare with this, and how is this in comparison with N and P?

The fast redissolution of opal allows Si to be recycled many times in the euphotic layer before its export to the sediment. The dissolution rate of opal is not the only parameter. The high density of siliceous shells favours a fast sinking speed. Bernard et al. (2010) compare the effects of riverine DSi perturbation in 3 different models, one results of that paper was that HAMOCC5 is not the best suitable model to investigate Si fluxes at the ocean floor. The computation cost of the Biogeochemical General Circulation model doesn't allow us to run the spin up of the sediment compartment until equilibrium.

Minor comments and questions are listed below:
1) The term opal needs to be defined.

"(biogenic silica)" was inserted at the first appearance of opal in the text.

2) When mentioning the Redfield ratios, please mention it is molar ratios.

The sentence "(molar element ratio)**"** was inserted at the first appearance of "Redfield ratio".

3) Page 4921, line 1-3: there seems to be a repetition here.

The repetition was removed:
"The high level of production is supported by complex dynamics including interplay of riverine supply of nutrients with coastal processes such as tidal currents or upwelling."

4) Page 4922, line 24: I guess fisheries also has a major impact on jellyfish biomass (see the Purcell et al reference).
We agree: Purcell et al. (2007) discussed the effect of fisheries combined with eutrophication. Fisheries affect the predator chain and favour jellyfish biomass by reducing the grazing pressure. Eutrophication benefits Jellyfish by increasing small-zooplankton abundance, turbidity and hypoxia, all conditions that may favour jellyfish over fish (Purcell et al., 2007).
This will be stated in the revised manuscript.

5) In the methods section the model is well described. However, the some parts need further explanation of at least a reference to support the choices made. For example, page 4926, line 1: why is 0.5 DSI the upper limit? Also equestions 4, 5 and 6 need more explanation and references.

(Same answer as for Referee #1) 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"

Explanation for equations 4, 5 and 6 were added:
"The calcium carbonate production is regulated by the DSi availability, and follows the reverse path of opal production in Eq. (3). For that reason it is computed using the same

constant as for opal production corrected by the Redfield ratio. In presence of abundant DSi, the model produces opal, while lower DSi concentrations benefit to calcium carbonate production.
Opal concentration results from the equilibrium between opal production and its remineralisation. Symmetrically, DSi concentration results from the equilibrium between DSi uptake by opal production and DSi release by opal remineralisation."

6) The reference Kroeze and Seitzinger was based on the knowledge then available. Since then other projections have been made which could be more realistic, especially for the period 1990 (the base year of Kroeze) till 2005 (for which we have measurements). For example, the work of Seitzinger et al in GBC (2010) gives a more recent projection which is much lower.

Reference to Seitzinger et al (2010) was included as well as the following text:
"All over the world, all nutrients are expected to be impacted; a model based study suggests a 30 % increase in riverine DIN export between 1970 and 2000 (Seitzinger et al., 2010), the same study for the period 2000-2030 predicts changes varying from an 18% increase to a general decrease for scenarios focusing on ecosystem management and water quality policy on a global scale."

7) In the discussion section some statements are repeated, for example page 4935 line 9-11 is also on page 4937;
The section on p. 4935 describes past changes, while the page 4937 discusses the expected perturbations of the future riverine fluxes. The two paragraphs have been merged, and the text has been modified to avoid the impression of being repetitive.

page 4937 lines 14-18 also seem to have the same statement twice.
The duplicates were removed (page 4937 lines 14-18).

Page 4933, line 23 is also on page 4935. My suggestion is to shorten the discussion section somewhat, by avoiding duplications and by combining some paragraphs.

We were unable to identify a repetition here. However, in the revised manuscript we have somewhat shortened the discussion as suggested.

8) Page 4933, line 24: river inputs of silica do have a stronger effect: stronger than what ? I do understand what is mean, but the sentence needs rephrasing.

The sentence was rephrased:
"The effect of riverine inputs of silica is often amplified by the absence of alternative supply of DSi; these areas encounter no major input from upwelling"

References:

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