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Biogeosciences Discussions

Interactive comment on "Impact of changes in river nutrient fluxes on the global marine silicon cycle: a model comparison" *by* C. Y. Bernard et al.

Anonymous Referee #1

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General:

This paper aims at assessing the sensitivity of the marine silicon cycle to varying river inputs of silica (and other nutrients) by comparing model results. It is a topic of prime interest since it turned out that not only growing riverine inputs of nitrogen and phosphorus affect the biogeochemistry and ecology of the ocean. Silicon also plays an important role, because a change in the nutrient mix (including silicon) may affect the abundance and community composition of primary producers which, in turn, affect the biogeochemical cycling of elements. Prominent examples where such changes occurred are the Mississippi – Gulf of Mexico and the Danube – NW Black Sea regions.

As such the paper is well structured and reads fluently. However, I have some reservations about the overall scope and result of this paper. The title suggests two things: a contribution to the discussion on the impact of river nutrients on the marine silicon cycle and a comparison of three models. While clearly matching the second part, the paper fails to demonstrate new contributions to the discussion on the marine silicon cycle, to my opinion. As I am not a modeller I cannot say too much on the models themselves, but I find it generally a good idea to compare the performance and results of models, because models gain more and more importance in research. However, in the current version this manuscript would rather make a contribution in a more specialized journal (modelling) than a contribution to the silicon discussion in Biogeosciences. Many aspects of the natural and anthropogenically altered river fluxes of silicon are covered by at least the three other papers of the lead author and his co-authors cited in the references. Using this knowledge and then taking a model to predict future changes would be the logical step to continue this discussion. To my opinion, this paper has the ingredients, but the story is not yet there. A sometimes lengthy and tedious discussion of specific model aspects/results obstructs the view on the possible future changes of the silicon cycle.

In the current version I cannot recommend the manuscript for publication in Biogeosciences, but a substantial revision focussing on future developments of the marine silicon cycle would make it a valuable contribution to the scientific literature worthwhile being published in Biogeosciences.

Detailed comments:

Abstract:

The first part of the abstract introduces the "silicon part" while the second part is the "model part". In the light of global change discussions, here in particular the role of changes in river nutrient inputs, it would be rather important also to mention the role of other nutrients and the consequences for the biogeochemistry and ecology of the (coastal and open) ocean. Of course, it will need a bit more space, but the the more technical aspects of the models could be shortened. When you mention that model results are "surprisingly similar", the reader would expect an explanation for that. What is so surprising? However, somewhat slightly contradictory to the "surprisingly similar" results is the statement that the box model "shows a delayed response to the imposed perturbations". The response time to perturbations is a very important aspect with regard to the overall topic of this paper (and the current research as such). So, I am wondering about the final conclusion that "box models provide a good alternative when studying...". This seems counterintuitive. What I am really missing here are some conclusions with respect to the first part of the title. So, what will be the impact of changing river fluxes on the global marine silicon cycle in 150 years, in 150,000 years derived from your modelling efforts? Such a conclusion will make the ms relevant for publication in Biogeosciences, not the one presently given.

Introduction:

The introduction to the silica cycle looks nice, but has a major drawback in the section where it reports on human activities (paragraph end of p. 4465, beginning of p. 4466). This section suffers from the lack of information on the tropics. Major part of the Si input into the ocean occurs there and the natural vs. anthropogenic controls of inputs there are different from the higher latitudes. For example, the "observed decreases" in Si:N and Si:P stem mostly from regions like the Mississippi – Gulf of Mexico and Danube – NW Black Sea or even higher latitudes. Have a look at other chapters of the SCOPE 66 book you cited, particularly those on the tropical rivers by Subramanian et al. and Jennerjahn et al.

Moreover, the use of and the inferences made from the Conley et al. (2008) paper are wrong. Conley et al. state that DSi was leached from cut vegetation in an experimental forest, not from increasing exposure to weathering. Also, inferring from their single case study on a "general worldwide decline in riverine silica input" is unacceptable.

In the following paragraph on p. 4466 you state "enhanced biogenic silica dissolution due to global warming may ultimately allow coastal siliceous productivity to recover

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from the downward trend caused by river damming". First, how do you know that coastal siliceous productivity is decreasing on a global scale? Is that also a result of the model/paper by Laruelle et al. (2009) or is there any global scale result/study you have in mind? If so, please cite it. And second, how can siliceous productivity recover only from enhanced biogenic silica dissolution without adding Si? Isn't it so that the enhanced dissolution of biogenic silica simply means to recycle what previously has been produced by siliceous producers? But without adding "external" Si you can only increase the speed of recycling, the "internal" cycling, or am I wrong?

Model description and comparison:

2.2 HAMOCC2 Paragraph end of p. 4469/beginning of p. 4470: Considering the uptake/redissolution of silica, does the model account for changes in silicification of diatom shells in in areas under anthropogenically altered nutrient inputs? For example, in the Gulf of Mexico it was observed that changes in the nutrient mix (Si:N) do not necessarily immediately lead to changes in the phytoplankton community composition, but a first response could be a shift from heavily silicified diatoms to lughtly silicified diatoms (e.g. see review paper by Rabalais et al., (2000), Chapter 10, p. 241-268, in "Estuarine Science", editor John Hobbie, Island Press). I suppose this could be a quantitatively important factor. I do not expect that the model can account for that, but it would be worthwhile discussing this aspect.

2.3 HAMOCC5 2nd para, p. 4471: You state "the resulting resolution is 29 km in the Arctic to about 390 km in the tropics". Why is the resolution that coarse in the tropics? I think this could be an important factor of uncertainty if the resolution is that coarse in the regions where you have the highest inputs of silica into the ocean. Wouldn't it lead to an underestimate of the silica input?

2.4 Model comparison 1st para p. 4473: You state "...only the box model allows the assessment of the effects of coastal zone processes on the long-term silica cycle" and later state that "silica burial in shelf sediments has been underestimated so far". With regard to these two statements, how much sense then does it make to compare these models? Additionally, in the first sentence of the 2nd para on p. 4472 you mention that "all three models show general similarities in particular with respect to rates of sediment burial in the coastal zone and benthic recycling". When I look at the model results, there is a factor of 4 (!) between the box model and HAMOCC5 (burial). So, it seems that the similarities of the models cannot be more than "very, very general".

3 Model scenarios and results 1st para, p. 4476: You state "As a consequence, this box responds to each perturbation much faster". What is meant, which "box"? If I look at the model results in figs. 2 a and b I cannot see a faster or slower response of one of the models. In fact, there seems to be a difference in the slope of in/decrease. Another intriguing thing is that you almost end up at the same result after 150 kyr, but you start at a 100 % difference! Didn't you say that "all models were fed with similar inputs of DSi" (p. 4473)? Why do you have a 100 % difference then in the initial values in the beginning of the model runs? Did I miss something?

p. 4477, line 14: The export production in the box model is definitely not 500 Tmol yr-1 after 50 kyr; it looks like a bit more than 200 Tmol yr-1.

p. 4477, line 14 – end of page: So, does that mean that damming has almost no effect on the marine Si cycle? How does this comply with the statement that continental margins are important repositories of silica?

p. 4479, lines 14-15: What do you mean by this statement? The Si retention is a result of the conversion of DSi into biogenic silica. So, how can you say "...leading to more efficient retention of bSiO2 than that of DSi."? They are directly linked. How can they be decoupled?

p. 4480, lines 1-2: So, do we currently have sufficient Si in the ocean to maintain opal production (and gross primary production)? Does that mean that the scenarios of anthropogenic changes given in the beginning are only regional phenomena which are simply insignificant for the global marine Si cycle? This is an interesting aspect to

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discuss. This will have implications for the carbon pumps.

p. 4480, line 14: in the western N-Atlantic, not eastern!

4 Conclusions

p. 4481, lines 19-21: Yes, but they start at a 100 % difference. Does that mean that a 100 % change of Si input into the ocean does not change anything on the long-term? So, are anthropogenic changes of the nutrient inputs into the ocean insignificant for the marine Si cycle?

p. 4482, lines 11-12: So, shall we conclude that your approach is not suitable to "show the sensitivity of the marine silica cycle to anthropogenic perturbations of Si:N and Si:P"? When reading this I get the impression that your approach will not be able to assess the "impact of changes in river nutrient fluxes on the global marine silicon cycle", the title of this paper. I don't think that this is what you intended, but at least I get the impression from the current line of reasoning.

p. 4482, lines 19-23: I find the coarse resolution in the tropics (390 km) rather critical.

Figures

In general, the fonts are numbers are too small to read easily in the figures.

Figure 4 should be enlarged. It is not very comfortable, if you need a magnifying glass to have a look on a global picture.

Interactive comment on Biogeosciences Discuss., 6, 4463, 2009.