Biogeosciences Discuss., 6, C1248–C1253, 2009
www.biogeosciences-discuss.net/6/C1248/2009/
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.

Answer to anonymous Referee #1

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.

We have added an additional section in the paper discussing potential future developments of the marine silicon cycle based on our results and those of earlier modeling studies, while specifically addressing the impacts on the coastal and open ocean (section 3.3 Impact of river inputs of silica on the coastal and open ocean).

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 more technical aspects of the models could be shortened.

We have modified the abstract to include several lines on the effects of changes in river inputs of silica on the marine silica cycle and the role of other nutrients. The lines added are:: "Our work demonstrates that the effects of changes in riverine dissolved silica on ocean biogeochemistry depend on the availability of the other nutrients such as nitrogen, silica and iron. The model results suggest that the effects of reduced silica inputs due to river damming are particularly pronounced in the Gulf of Bengal, Gulf of Mexico and the Amazon plume where they negatively affect opal production". We have kept the remaining text on the model comparison, however, given that, in our view, it is critical to the paper and presents important findings that are of interest to researchers in the field of nutrient biogeochemistry – both modelers and non-modellers.

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.

We have removed the expression "surprisingly similar" and have replaced it by "comparable" in the abstract. The key point we wish to make here is that two models with profound conceptual differences can still generate results that are very similar with respect to their response to the same perturbation. Given the uncertainties in possible scenarios for silica cycling in the ocean on a time scale of 150 kyrs, such a qualitatively similar result makes box models a very good alternative to general circulation models when assessing the average ocean response to change. We have addressed the second issue in a separate section 3.3. (see above).

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.

We have added a statement at the end of page 4465 to specifically describe observed trends in tropical regions: "In tropical regions, dissolved Si concentrations appear to be less affected by anthropogenic factors and climatic, geological and geomorphological factors likely are more important (Jennerjahn et al., 2006)." We also modified the text on the nutrient ratios to make clear that these relate mostly to the temperate zone.

Moreover, the use of and the inferences made from the Conley et al. (Conley et al.) paper are wrong. Conley et al. state that DSi was leached from cut vegetation in an experimental forest, not from increasing exposure to weathering.

We corrected this and modified the text to “Note that deforestation increases the continental input of silica to the ocean by increasing dissolved silicate losses from vegetation”.

Also, inferring from their single case study on a "general worldwide decline in riverine silica input" is unacceptable.

Conley et al. (Conley et al.) specifically discuss the global scale consequences of their findings for their single case study and we simply refer to their discussion of the topic. We agree with the referee, however, that some caution is required and we have therefore removed the sentence: ”However, this effect is not large enough to balance the general worldwide decline in riverine silica input (Conley et al., 2008).” We have replaced it by “However, it is uncertain what role deforestation plays in counteracting the worldwide decline in river silica input”.

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 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. (Laruelle et al.) or is there any global scale result/study you have in mind? If so, please cite it.

Our statement about "enhanced biogenic silica dissolution due to global warming" and its impact on coastal productivity is indeed a conclusion from Laruelle et al. 2009. This paper is now in press and available for download at the following address: http://www.agu.org/contents/journals/ViewPapersInPress.do?journalCode=GB. The decrease of the riverine DSi due to river damming described in the simulation is in agreement with the work of Humborg et al. (Humborg et al.).

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?

The simulations presented in Laruelle et al. 2009 suggest that an increase in temperature may enhance the terrestrial uptake of silica, leading to more production of phytoliths and other biogenic material ultimately entering ground waters and rivers. This enhanced coupling between the terrestrial and aquatic cycles leads to an increased availability of Si in the rivers compensating for the higher retention by dams. We reformulated the text to clarify this point: “Results of global scale box modelling of the silica cycle for the coming century, for example, indicate that enhanced biogenic silica dissolution due to global warming may enhance silica availability in aquatic systems. Ultimately this may allow coastal siliceous productivity to recover from the downward trend caused by river damming (Laruelle et al., 2009).”

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.

HAMOCC5 only computes the opal export production from POC export. It is therefore not possible to investigate silicification or any aspect of the living part of the marine silica cycle. We believe that discussion of this topic lies outside the scope of this paper, particularly since we also do not specifically address changes in the phytoplankton community composition.

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?

The coarser resolution in the tropics is due to the irregularity of the model grid. Note, however, that 390 km is the maximum size of grid cells in HAMOCC5 and this does not imply that the tropical regions all have this grid size. For example, the spatial resolution in known hotspots for silica inputs such as the mouth of the Amazon is of the order of 250km. The representation of the continental margins is also improved by the finer vertical resolution (40 layers). This is in line with grid sizes of other high resolution OGCMs and thus is not unusual.

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?

The purpose of our model comparison is precisely to point out the strengths and weaknesses of each particular modeling approach. While both the box model and HAMOCC5 include a detailed description of the coastal ocean, of these two, only the box model can be used for long term simulations due to the computational demands of HAMOCC5. By explicitly making these comparisons, any user can knowingly choose which model is most suited for his or her study and can better evaluate and understand the results obtained. Where both models are applicable, the results are very similar, giving confidence in the results and demonstrating that box models are valid research tools despite their simple structure.

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".

Here, we write: "The steady state budgets for Si in all three models show general similarities (Fig. 1), in particular with respect to rates of sediment burial in the coastal zone and benthic recycling." In figure 1, it can be seen that the burial fluxes of Si in the coastal zone of the box model and HAMOCC5 are 7.7 and 6.6 Tmol/y, respectively. Benthic effluxes in the coastal zone for the box model and HAMOCC5 are 5 and 5.4 Tmol/yr, respectively. Benthic effluxes in the deep sea for these models are 19.9 and 18.7 Tmol/yr, respectively. All these values are very similar. Hamocc2 does not have a coastal zone and thus naturally cannot be included in the comparison for the coastal zone but the burial in hamocc2 at a value of 6.1 Tmol/yr compares well to the total burial in the box model of 6.8 Tmol/yr. The different burial in HAMOCC5 is explained later on in the text in the same section and is due to the fact that HAMOCC5 cannot be run to steady state because of computational costs. Thus, the value is too high. Because of this and other, generally more minor differences, we continued the text with "There are also significant differences, for example, …." and explain all these differences in detail in the remainder of the text.

To avoid misunderstanding, we have now marked the burial flux for HAMOCC5 in figure 1 with an asterisk and we have added the following explanation in the caption: "*Given that HAMOCC5 cannot be run to steady state because of computational costs, the calculated burial flux for this model overestimates the actual burial flux.". We have also changed the first sentences to: "The steady state budgets for Si in all three models show general similarities but also some major differences (Fig. 1). Examples of similarities are the rates of sediment burial and benthic recycling in the coastal zone in HAMOCC5 and the box model and the total ocean burial in the box model and HAMOCC2. Differences are observed in the process rates in the euphotic zone (0- 100 m) and intermediate waters (100-1000 m). In addition, the burial in the open ocean in HAMOCC5 is much higher than in the other models.

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.

We have modified the text to clarify what we mean by "box". We also explain that a faster or slower response is reflected in the slope of the lines in figure 2. The changes made are:
"As a consequence, the box representing the coastal zone in the box model responds to each perturbation much faster. This is reflected in the steeper slope of the lines for the box model in figure 2. "

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?

Figure 2 shows that the initial rates of export production of opal in HAMOCC2 and the box model indeed differ by almost a factor of 2. This is simply the starting point for each model scenario as described in detail in the model description section and as shown in Figure 1. If these two different models are fed with the same input of Si, in the long run the results indeed become rather similar – although there are still some discrepancies which we discuss separately. We already highlight the similarities in the text in the same section: "Overall, both models present similar qualitative responses to major long term variations in silica inputs from the rivers." since this is an important finding.

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.

This value has been corrected in the text.

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?

We assume the reviewer is referring to page 4478, since damming is not mentioned on page 4477. We certainly do not claim that damming has no effect on the marine Si cycle. We simply make clear that on short time scales of a century, the cycling of Si in the global ocean is not affected. This is not in contradiction with the fact that continental margins are important repositories of silica but just means that the concentration of silica in the open ocean is only slightly affected by river damming on short time scales. This is dicussed further in the conclusions where we point out that effects of changes in river inputs are particularly important in determining coastal zone processes. We have also added text discussing this in the new section 3.3.

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?

Given that bSiO2 is a solid and DSi is a solute, the effect of a change in water residence time on the retention of both compounds is not the same. This is because damming leads to a stronger trapping of particulates than solutes and, as a consequence, the concentration of $bSiO_2$ will drop substantially faster than that of dSi, even if their cycles are intimately linked. We have now rewritten this sentence to make this clear.

"In rivers, lakes and artificial reservoirs, damming leads to a stronger trapping of $bSiO_2$ than of dSi, despite the fact that loads of both components are affected by the increase in water residence time and the strong link between the cycles of bSiO2 and dSi."

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 discuss. This will have implications for the carbon pumps.

At the scale of the whole global ocean, we do not expect significant changes in siliceous primary production on short time scale other than regionally. This being due, in part, to the residence time of silica in the open ocean (~15kyrs) and the fact that a large fraction of the primary production on continental margins is sustained by upwelling processes. This conclusion is indeed of importance and is now further discussed in the text of section 3.3.

Moreover, similar observations have been made by several authors (Rabouille et al., 2001; Ver, 1998; Mackenzie et al., 1993) for other nutrients and we added some of these references in our manuscript.

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

This has been corrected.

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?

We refer to our responses above. We briefly repeat our answers here again: (1) the assumptions made in building the various models are different, as explained in detail in the text. This leads to a factor 2 difference in opal export production between hamocc2 and the box model. The response of both models to a perturbation is the same, however. This demonstrates that a difference in starting point does not preclude a similar response to a perturbation. (2) The short term effects on the open ocean are nearly negligible given the long residence time of Si in the ocean. For the coastal zone, it is a different matter, but here the effect of a change in Si depends on the limiting nutrient. We have added text explaining this in section 3.3.

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.

HAMOCC5 allows a full assessment of the role of Si relative to the other nutrients (N,P). This is why we felt that the original title was justified. However, given that the main focus indeed lies on silica, and to ensure that there is no misunderstanding about our goals, we have changed the title to: "Impact of changes in river fluxes of silica on the global marine silicon cycle: a model comparison"

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

See answer to earlier comment.

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.

The figures and font sizes have been increased.

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