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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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General comments:

Bernard et al. aim at evaluating the importance of riverine inputs of dissolved Si to the global marine biogeochemical cycle of Si. Taking into account the very few studies addressing the impact of riverine nutrients on marine biogeochemical cycles at the global scale, their study is most relevant and well suited for publication in Biogeosciences. I have however identified a number of weaknesses in the manuscript which I would like to see addressed. I therefore recommend the publication with major revisions.

The use of recent global databases is a strong asset of this work. I wonder how the

'estuarine filter' is dealt with. We know that a poorly quantified fraction of the nutrients delivered by rivers gets trapped in the land-ocean transition zone. I understood that the authors apply a 100% transfer of river derived nutrients to the marine domain. Is that exact? In that case, did you consider sensitivity studies by varying the transfer rate?

In the introduction, it is stated that "global biogeochemical models partly fail to integrate processes taking place on continental margins": could you please be more specific. What processes are typical to continental margins and to what extend are they represented by the particular model you are using?

The model description is in part difficult to follow. What does it mean that "dissolution of opal is temperature dependant with a constant ratio"? Do you mean a dissolution rate constant?

Is the dissolution of opal and CaCO3 in the water column independent of the local saturation state?

How many detrital pools are distinguished by the model? POM, DOM, Bsi, CaCO3?

Not all the terms of equations (1) to (6) are identified. Of course the reader might guess what they are, but the authors should provide the information in any case. Are the equations correct or is the presentation lacking consistency? For instance, the text states that (p. 1097) "siliceous fraction of detritus is computed as: eq. (3)". I gather that eq. (3) describes the production per unit time of siliceous detritus or opal. It is a rate, rather than a tracer concentration or weight fraction. The expression includes a Michaelis-Menten. There is a typo in the equation. Please check all the equations accordingly.

Global results, silicic acid concentration:

Please include a table summarizing the model Si budget (with and without including river fluxes) and comparing it to independent data based estimates. How was riverine nutrient input handled prior to explicitly adding river fluxes?

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Interactive Comment

How do your results compare to Da Cunha et al. (2007)?

Discussion

Pelagic-benthic coupling: The authors mention the close pelagic-benthic coupling typical for shelf sea environments. On page 1105, they state that 'remobilisation of opal shelf sediments would not compensate for a large decrease of the riverine silica input'. Please give evidence to back-up this statement. I remember studies evaluating the input of DSi to shelf systems from sediments as being an important contribution to regional Si budgets. Since the model has a sediment compartment, I suggest that the authors evaluate DSi fluxes across the sediment-water interface. Modelled fluxes could also be compared to observations (Orfois data base).

Talking about the model sediment, I have a technical question: How did you spin-up the sediment? The duration of spin-up quoted in the manuscript (p. 1099) is to short to allow a steady-state to be reached between riverine input of DSi and sediment burial. Did you start from a given sediment composition and if that is the case, how was it obtained?

Iron: This section is quite confusing. On p. 1106 (bottom), I read that ' Iron was not originally part of the land to ocean fluxes ..." and "Not shown in this paper, the dissolved iron availability showed to interact ...". I wonder whether Fe is NOW part of land to ocean fluxes and how this was parameterized? Please show model results, if you decide to discuss them. On p. 1107 the reader gets informed that "the model fails to reproduce the iron recycling from shelf's sediment". Is Fe geochemistry part of the sediment model? I gather from the model description that this is not the case. The reason for the unexpected Fe limitation in the coastal zone is identified by Bernard et al. It is a model bias and as such has to be addressed. Please rewrite this section and clarify which processes are taken into account by the model and which aren't. Next discuss the implications to model results.

Concluding statements on p. 1108. The two paragraphs appear as final statements.

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They need to be streamlined. At present, it takes some effort to identify the main message.

Structure of the manuscript: the overall flow of the text could be improved by paying attention to its structure. Some ideas are repeated several times and certain sections lack 'glue'.

Miscellaneous:

1. please carefully check the spelling, as well as the grammar;

2. introduce the abbreviations early in the text and be consistent throughout the manuscript with respect of their use (ex. Dsi or dSi for dissolved Si);

3. avoid repetition of statements

4. I suggest that you extend your literature review to include observational studies dealing with the Si budget of shelf seas.

I did not check the reference list a this stage.

Reviewer: M. Gehlen

Interactive comment on Biogeosciences Discuss., 6, 1091, 2009.