**Biogeosciences
Discussions**

Interactive comment on "Sedimentary and mineral dust sources of dissolved iron to the World Ocean" *by* J. K. Moore and O. Braucher

J. K. Moore and O. Braucher

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Dear Editors of Biogeosciences, We have revised our manuscript taking into account the comments and suggestions of both reviewers. The reviewers comments and our detailed responses (marked by ******) are given below. Please let us know if we can be of further assistance with your assessment of this manuscript. Best Regards, Keith Moore

Review 2 by Olivier Aumont

Review of the manuscript entitled Sedimentary and mineral dust sources of dissolved iron to the World Ocean by Moore and Braucher.

Before beginning the review, I should mention that I have also made the review of the other manuscript submitted to BGD on the global iron database. As I mentioned my

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name in this previous review and as it will be pretty obvious from my comments, my review will not be anonymous. To be honest, I should also mention that I had a few mail exchanges with Keith Moore when he had discovered the bugs in their model code. However, I do not think it had any impact on the resubmitted version of the manuscript.

In this manuscript, the authors evaluate the potential impact of the sedimentary source of iron on the iron distribution and on the primary productivity in the ocean. They used an improved description of the sedimentary source to prescribe sediment mobilization in a coarse-resolution model. The model results suggest that this source is almost as important as dust deposition and should be considered when modeling the iron cycle (and marine primary production). As an other objective, they also evaluate the role of the iron loss presses in the water column and found a significant sink (either scavenging, coagulation,) in the water column improves significantly the simulated iron distribution, especially in the subsurface.

I find this manuscript to be much more interesting than their first one on the iron database. It is well written (even if as a non-english speaker, I am not exactly the best person to judge that), clear and quite convincing. So I strongly support its publication once my concerns are properly addressed.

General concerns: There should not be any surprise here for the authors as these concerns were already mentioned in the mail exchanges we had in June.

1. The model includes a parameterization of desorption with a rate which seems to me quite strong (about 0.06/d). In fact, reversible adsorption is still a matter of debate. Some observational studies suggest irreversible adsorption, at least at the molecular level (Quigley et al., 2001; Santschi et al., 2006). It would be nice to have a discussion on that topic.

****** Our value is on the low end of the range applied by Parekh et al. (2004). However, we acknowledge that there is no data and large uncerainties concerning this

process. We have expanded the discussion of this topic in the methods section noting this uncertainty. We also return to this point in the discussion, noting that for many purposes inclusion of desorption is not necessary. One sensitivity study includes no desorption of iron, and a second reduced our base desorption by 50%. ******

2. Parameterization of the sediment source (+other parameterizations). In their study, Aumont and Bopp (2006) followed a pretty similar approach to describe sediment mobilization. In fact, many of the improvements presented here were already and previously introduced in their model: ligand dynamics, scavenged iron is added to the iron particulate pool (and thus is subject to dissolution/remineralization in the water column), part of the dust iron is dissolved below the surface with a length scale of 25000m). However, Aumont and Bopp did not discuss on these processes and on their impacts on the iron cycle and marine biological activity. Thus, I think that this study by Aumont and Bopp (2006) should be mentioned in the model description section of the manuscript.

****** We are grateful to the reviewer for this point. We were unaware of the similarities in the approach of Aumont and Bopp (2006) at the time of submission of this work. Several references to this paper have been added to the methods and introduction sections to address this deficiency. ******

More specific comments:

Page 1284: The optimum Fe/C ratios for diatoms and nonphytoplankton seem tome quite low (6 umol/molC). In the North Pacific and North Atlantic, field studies have shown much higher ratios (up to 15 umolFe/mol C). And of course, lab studies have revealed much higher ratios (luxury uptake which may rarely occur in the open ocean).

****** The Fe/C ratios used in the model for the diatoms and smaller phytoplankton with a minimum of 2.5 μmol/mol and a maximum of 6 μmol/mol are on the low end of recent field observations, including some not even yet published as noted by the reviewer. Fully exploring the sensitivity and impacts of variations in the Fe/C ratios is beyond the scope of this paper (it would be a paper in itself, and in fact will be the focus of future

work). We have added a paragraph in the discussion section that addresses the impact of model assumptions on our derived scavenging rates (including Fe/C ratios). ******

Page 1289-1290: The units are wrong or not mentioned for Febase and Chigh. For instance , since Febase is multiply by a mass flux to give a rate, it cannot be equal to a rate.

****** Units for Febase have been corrected. Chigh is a unitless coefficient (noted in the paper). ******

Page 1294: I would have liked to see that figure 4 in the database paper!

****** We debated this but wanted to include the distributions from the improved model in this plot. ******

Page 1300, lines 5-10: The authors suggest that the underestimation of iron concentrations in the deep North Pacific can be explained by a too low solubility for mineral dust at the surface. I am not really convinced by this argument. Increasing the solubility at the surface would certainly increase the surface iron concentrations, and thus the scavenging and the Fe/C ratios of phytoplankton leading to a higher export below the surface. However, matching the deep concentrations would require to increase quite substantially the input of iron if I read correctly fig 10 and if I follow the authors arguments. In fact, observed iron levels in the North Pacific are similar to those in the North Atlantic whereas in the model they are underestimated by a factor of 2 or even 3 relative to the observations and to the levels simulated in the North Atlantic. The problem is that at the surface, simulated iron concentrations are already similar to the observations and cannot be raised significantly. Furthermore, increasing significantly surface iron concentrations would probably suppress iron limitation (which is not what is wished I suspect).

****** These are valid points. The underestimate of dust input is noted only as one possibility. Deficiencies in the scavenging and underestimation of the margin source,

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or some combination of these factors are other possibilities. Increasing dust inputs at the surface would not necessarily raise surface iron concentrations, if for example phytoplankton Fe/C ratios increased, biological production and export increased, etc The model underestimates the Pacific deep observations by less than a factor of two (Table 1). However, the deep North Pacific still is a problem area for the model despite some improvements here. Getting the deep ocean fields correct requires more observations in the many data sparse regions, and will likely be a problem for biogeochemical models for some time. ******

Page 1301, lines 1-5: the arguments on dust solubility are quite confusing in this manuscript. They mentioned in the previous page and at page 1300 that the solubility is probably underestimated in the model and here, they state that it is overestimated over high dust regions. The authors probably (my interpretation) want to say that over low and moderate dust regions, iron solubility is underestimated and over high dust regions, it is overestimated. Thus, it should be made clearer in the manuscript. However, I agree with the authors that solubility is a key parameter and observations suggest that it is far from being constant as assumed in models.

****** The reviewer is correct in what we were trying to say in these passages. The text has been revised for clarity. ******

Page 1304: This is not so sure that 0.6nM is really the threshold above which scavenging should increase strongly. For instance, observations suggest that, except in the Southern Ocean (where iron levels are the lowest in the deep ocean), ligands concentrations are higher than 1 nM in general. Furthermore, Parekh et al. (2004, 2005) achieved reasonable simulated iron fields with a 1 nM uniform ligands concentration (if I remember correctly the exact value). Thus, I would not state too firmly that scavenging should increase strongly above 0.6-0.7 nM.

****** This is a valid point. In the revised discussion section we note the following... There is a similar high-end peak in the observations in the deep ocean (Figure 4C),

suggesting a common controlling process, most likely increased scavenging losses as iron exceeds ˜0.6-0.7 nM, as suggested by Johnson et al. (1997a). Scavenging losses must increase at higher iron concentrations to match the observed distributions. However, it is not strictly tied to a concentration of 0.6 nM. Scavenging likely increases rapidly as iron concentrations exceed ˜0.5-1.5 nM depending on local ligand concentrations and dynamics. ...

The model simulations assume a uniform 1 nM with moderately strong binding affinity. In reality, the ligands and binging strength likely varies in space and time. ******

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