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

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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 mentioned that I have also made the review of the other manuscript submitted to BGD on the global iron database. As I mentioned my 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 mentioned that I had a few mail exchanges with Keith Moore when he 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

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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 ocean 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 processes in the water column and found that 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 of that), clear and quite convincing. So I strongly support its publication once my concerns are properly addressed.

General concerns:

Their 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 dˆ(-1)). 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.

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.

More specific comments:

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

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 ...

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

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)

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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.

Page 1304: This is not so sure that 0.6 nM 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 wouldn't state too firmly that scavenging should increase strongly above 0.6-0.7 nM.

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