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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

Anonymous Referee #1

The main objective of the manuscript by Moore and Braucher is to use an improved version of the Biogeochemical Elemental Cycling ocean model to address the relative contributions of dust deposition and sedimentary sources in driving biological activity and observed dissolved iron distributions in the World Ocean. The manuscript is well-written and relevant for the oceanographic community. Although I believe that the manuscript should be published, I think that there are some inconsistencies that I would like the authors to address, and a few areas that could benefit from some additional discussion.

Methods: How sensitive is the model to variable Fe:C ratios? I think that this question is relevant because: 1) it is clear now that Fe:C ratios measured under laboratory conditions do not reflect ratios measured in field populations of phytoplankton (there were at least two presentations at the last ASLO meeting in Santa Fe discussing this issue), and 2) Fe:C ratios in phytoplankton depend on ambient iron concentrations? Although it is possible that some of these questions were already addressed in a previous article, I feel that this manuscript should be self-explanatory.

****** 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). ******

Observed dissolved iron distributions in the world ocean: I was surprised by the fact that the model (based on atmospheric deposition and benthic remobilization) tends to overestimate the “observed” surface layer dissolved iron concentrations (Table 1 and Figure 3), despite the fact that the other important sources were not included in the model. For example, because the contributions of the Amazon, Orinoco, Niger and Congo rivers were not considered in the model , I would expect that the predicted dissolved iron concentrations in the Atlantic Ocean would be lower than the observed values, but that was not the case. Does this suggest that the input of iron from these major rivers is not important? The authors mention in the introduction that offshore advection of iron in the Gulf of Alaska is important. What about the advection

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of iron from those river plumes? What about the mid-depth high-salinity water flowing out of the Mediterranean? I don’t remember if the iron concentrations in those waters are high or low, but that water mass is detectable in large regions of the Atlantic Ocean. I would expect that advective from the Mediterranean water mass would affect iron levels more than atmospheric deposition or a sedimentary source.

****** The reviewer raises a valid point concerning the potential riverine source of dissolved iron. We note this potential source in the revised manuscript in the introduction and discussion sections. Rivers with large estuaries likely lose much of their dissolved iron before reaching the ocean, but large rivers that discharge to the shelf may carry significant iron. Very little data is available at this point. The ocean model we use here does not include the Mediterranean outflow, thus this cannot be addressed. ******

It is hard for me to believe that a sedimentary source can affect iron levels in the upper layers of the open ocean. While I acknowledge that the sediments are an important source of iron in shallow water environments, most of the iron from sedimentary sources in well-stratified open ocean basins must be transported away from the basin along the advective flows of the deep ocean circulation. Consistent with that hypothesis, stable lead isotopic composition measured in surface waters and manganese nodules from the Southern Ocean reflect a Saharan source deposited in the central Atlantic ocean and transported to the Antarctic by the NADW (Abouchami and Goldsmith, GCA 59 (9) 1809-1820, 1995; Sanudo-Wilhelmy and Flegal. G3, (7) 1063, 2003). I would expect that the same would happen to any iron being remobilized from bottom sediments. Similarly, I would expect that iron levels in the eastern boundaries of the world ocean are more dependent on upwelling intensity that transport mid deep waters to the surface than on benthic remobilization alone. Again, I would expect that the model would also underestimate iron concentrations in surface waters in those regions as well.

****** As the concentrations differ by often a couple of orders of magnitude, only a small fraction of the coastal iron has to reach the open ocean to have a significant effect. We

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agree the importance of the coastal source in this study is somewhat surprising. In the discussion we note ways that the coarse resolution of the model could be either underestimating or overestimating the flux from the boundaries to the open ocean. In the Atlantic we find that inputs are dominated strongly by Saharan dust sources in agreement with the point made above. It is only in other basins where dust inputs are lower that the margin sediment source becomes important. The coarse resolution model does not capture the EBC upwelling very well, and we have not included benthic remobilization explicitly. This would increase simulated iron concentrations. These high iron values are sensitive to our scavenging parameterization at high iron which keeps model values generally $<$ ~2-3 nM. In fact, values greater than 10 nM are sometimes seen in coastal waters. Given the coarse resolution of the model, we are trying only to capture the high coastal/shelf values to first order. ******

Impact on biological activity: I am not sure that the results of the new BEC simulation presented in Table 3 suggesting that most of the diazotrophs in the tropic and sub-tropics are iron-limited are consistent with the relatively high levels of dissolved iron presented in Figures 5 and 8. Do the authors believe that dissolved iron levels be-tween 0.8 and 5nM are limiting in the Atlantic ocean? So they have phosphorus data? Could this be a limiting nutrient? In the methods, the authors mentioned that the BEC model includes phosphorus (p. 5), but no results are presented.

****** The reviewer raises a good point that was unclear in our original manuscript. In fact the diazotrophs are not iron-limited in these high iron areas (but are P-limited instead). Most of the response seen in Table 3 is due to changing N fixation rates in the Pacific and parts of the South Atlantic and South Indian oceans where iron is limiting growth. We have clarified this in the revised manuscript. ******

Interactive comment on Biogeosciences Discuss., 4, 1279, 2007.