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Biogeosciences Discussions

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

Anonymous Referee #1

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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 phytoplankton 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 phy-

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toplankton (there were at least two presentations at the last ASLO meeting in Santa Fe discussing this issue), and 2) Fe:C ratios in field populations of phytoplankton may vary substantially within the same species. Is there any field evidence supporting the authors' claim on p. 6 (line 7) that 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.

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 mentioned in the introduction that offshore advection of iron in the Gulf of Alaska is important. What about the advection of iron from those river plumes? What about the mid-depth high-salinity water flowing out from 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 transport from that Mediterranean water mass would affect iron levels more than atmospheric deposition or a sedimentary source.

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 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

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and transported to the Antarctic by the NADW (Abouchami and Goldsmith, GCA 59 (9) 1809-1820, 1995; Sanudo-Wilhelmy and Flegal. G3, 4 (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 upper layers in 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.

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 tropics and subtropics 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 between 0.8 and 5 nM 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 phosphorous (p. 5), but no results are presented.

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