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Biogeosciences
Discussions

Interactive comment on “Observations of dissolved iron concentrations in the World Ocean: implications and constraints for ocean biogeochemical models” *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. We are grateful for the time and thought put into the reviews and feel the paper has been significantly improved. The reviewers comments and our detailed responses (marked by ******) are given below. Please let me know if I can be of further assistance with your assessment of this manuscript. Best Regards, Keith Moore

Referee # 2 Anonymous Received and published: 8 June 2007

Review of the paper 'Observations of dissolved iron concentrations in the world ocean: implications and constraints for ocean biogeochemical models'. J.K. Moore and O. Braucher. The authors used a new compilation of dissolved iron data to provide general patterns on the distributions of DFe, insights into the processes that control the DFe distributions, and constraints for biogeochemical models. I find the idea very interesting but I am disappointed by the paper and have major concerns with different issues of the work. 1) Analysis of the new data base: At different places in the text the authors make comments on the distributions of DFe that are not supported by the presentation of observations or by a rigorous analysis of observations. In the abstract and a the beginning of the discussion (page 1258), the authors claim that the surface iron distribution exhibits a strong bimodal distribution with peaks at 0.1-0.2 and 0.6-0.8 nM. There is neither a plot nor a statistical analysis to support this assessment (maybe this comes from the fig 4 in the companion paper?).

******Discussion of bimodal distribution has been removed (see response to Reviewer #1).******

Page 1254: the authors write "away from the high deposition regions, particularly at higher latitude, winter often has maximum surface iron concentrations due to deep mixing or weakening biological uptake" is this idea coming from the data set (if yes show which data) or is it coming from the model? Or, is it just an idea that makes sense but without any data to support it?

******This was a good point. We have clarified this comment to indicate that is based both on first principles and our model output (see response to Reviewer #1).******

Page 1254: they calculate mean values of the iron concentration for different sub-systems like the "high deposition regions" but I was not able to find the criterion used to define these regions. What is the sensitivity of the calculated mean of the DFe concentration to change in this criterion? The criterion used to define the HNLC system is the annual surface concentration of nitrate exceeding 1.0 uM (why 1 uM? And not

5 or 10 uM). This criterion is likely not correct because in addition to High Nitrate concentrations, HNLC are also characterized by Low Chlorophyll. Here also, what is the sensitivity of the calculated mean DFe concentration to change in this criterion?

******The high deposition regions are defined on page 13 of the revised ms as the North Indian and North Atlantic basins. As most of the data in these regions is from areas impacted by the large dust plumes, a more narrowly focused “high deposition” region within these basins, does not change mean iron concentrations much. It also seems arbitrary as to where one would draw this line. The HNLC region has been better defined in the revised ms (see response to Reviewer #1).******

Page 1256: “if all the concentrations are included () mean concentrations are higher in the North Pacific (0.87+/-0.37 nM) than in the north Atlantic (0.76 +/- 0.31 nM) “ I doubt that this will be confirmed by an appropriate statistical test.

******If we assume normal distribution, the values are significantly different at the 95% confidence level, we note this in the revised ms (page 15).******

I find it interesting to try to extract from the new large DFe data base some general patterns of the DFe distributions in different regions, which can help to understand the processes that control the distributions, but this must be done on the basis of clear and rigorous definitions of the regions. Comments on possible differences or similarities must be supported by statistical analysis.

******There is really only enough data for statistical analysis in the Southern Ocean, North Pacific, and North Atlantic. We have added error bars to the mean profile data in Figure 3, and modified the paper discussion accordingly (pages 14-15). We have also clarified and improved the definitions for “high deposition” and HNLC regions (see response to Reviewer #1).******

2) Comparison of the model with data. Based on a log log regression with a correlation coefficient of 0.73, the authors say (page 1256) that “There is broad agreement

between the model and the observation". I am not fully convinced with this kind of argument and I find this conclusion not very useful. It would be more interesting for example to compare the mean vertical profiles in the different basins presented in figure 4 (standard deviation are missing) with the mean profiles provided by the model. This was done only for the deep waters of the Southern Ocean and North Pacific (and the conclusion was that the model only weakly captures the strong contrast between deeper iron concentrations in the North Pacific and the Southern Ocean).

******We have weakened this statement somewhat to say there is "rough agreement" (page 17 revised ms, and comment above). A more detailed comparison between model output and the observations is not the goal of this paper, but is a key focus of the companion paper with the improved model. Tables there indicate the model and observation means for different basins over several depth ranges, and compare binned iron distributions for the model used here, the improved version of the model, and the observations.******

The seasonal variation of the vertical distribution of DFe has been studied at few sites in the ocean (Sedwick et al., GBC 2005 Boyle et al., GGA 2005). This gives the opportunity to compare on a seasonal timescale the outputs of the model with real observations at these sites. It is also possible to compare the annual mean of real data and model outputs. There is very likely much more to learn from the careful analysis of the differences between the model and the data than from a broad "agreement between data and model" or from figures 7 and 8. To some extent this strategy was applied by the authors (p. 1259 line 21), and has led to the companion paper. More should be done in this direction.

******See comments above on the seasonal cycle and model-data comparison. Annual means of the current model, the improved model, and the observations at basin scales are addressed in the companion paper.******

3) Description of iron cycling in the model. (section 2.2)

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Section 2.2 is based on the paper by Moore et al. (2004) with focus on the parameterization of the scavenging of Fe. This part is really obscure. It is not clear if the equations and numbers are coming from the paper (Moore et al. 2004) or if they are recent modifications of the model. Where are the number (Febase = 0.01369 day-1, MaxPe=0.05476 day-1 Chigh=4286) coming from ? (by the way two significant digits should be enough) LowFe=0.5 nM and highFe=0.6 nM. Why? What does the last sentence mean? Page1252 "Moore et al 2006 suggested some modifications to the original parameter values for the BEC (similar values are used here see Moore and Doney 2007)." Which modifications? Which parameters?

******These numbers are from Moore et al. (2004), we have clarified this in the text. Moore and Doney 2007 included some mostly minor parameter changes summarized in their Table S1. We refer explicitly to this table in the revised ms on page 10. It does not merit reproducing the table here.******

My overall recommendation is that the objective of the paper is good but that more rigorous work is needed in analyzing the new data base and in comparing the observations with the outputs of the model. In its present form the paper is also too long and needs to be more focused on the discussion of the results (for example statistical analysis) rather than reviewing the results from the literature. The examination of the differences between the observations and the model has led to a modification of the particle scavenging parameterization presented in the companion paper. I am wondering if two papers are really needed. The work reported in the manuscript reviewed here can very likely be included in the companion paper. Doing so would help to focus the presentation of the work discussed here and would provide the reader with the whole story presented in a logical manner: analysis of the data base, comparison with the model, implication for the modification of the model and consequences for the relative contribution of the sedimentary and mineral dust sources of DFe to the world ocean. Such a paper would certainly have a broader impact.

******As noted above the discussion section has been edited to focus more narrowly on the paper results, and to exclude literature discussion that is largely present in the introduction section. We considered one long paper originally, but the length of the resulting two manuscripts and the separate topics and foci, really point to the need for two papers.******

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