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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

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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 us know if we can be of further assistance with your assessment of this manuscript. Best Regards, Keith Moore

Reviewer # 1 Comments O. Aumont (Referee) Before beginning the review, I should mention that I have agreed on reviewing both manuscripts submitted by the authors to BGD. However, since these papers are not supposed to be part I and part II papers, they are meant to be independent. As such, I haven't read yet the second paper to avoid, at least for my first review, to have part I and part II reviews... My review is not anonymous as from the second review and from my english, I presume it will be pretty obvious to guess who I am (French, global modeler and pretty aware of the Aumont and Bopp (2006)paper). In this manuscript, the authors have constructed a global compilation of dissolved iron observations. This compilation is based on previous efforts achieved by several modelling groups (Greggetal.,2003; Parekhetal.,2004; AumontandBopp,2006) and has been augmented by about 30% relative to the version published by Parekh et al (2004).Several conclusions are drawn from its analysis. First, the distribution of iron is consistent with the conceptual colloidal pumping model of Honeyman and Santschi (1989). Second, two external sources of iron, i.e. dust deposition and mobilization from the sediments, are demonstrated to be critical. Finally, the authors deduce some constraints for the modelling of the iron cycle: basically, significant scavenging and/or coagulation occur for concentrations below 0.6nM(which is, to some extent, connected to the agreement with the colloidal pumping model). In the first part of this work, the authors redo and follow quite closely the seminal study by Johnson et al. (1997). However, the database is now much larger and as a consequence, some hypotheses can be now much better addressed. Furthermore, in a second part, the authors deduce some constraints for the modelling of the iron cycle by comparing the database to model outputs, which is quite new and interesting. Thus, I find this work quite interesting and definitely consider it worth to publish. However, I have some major concerns that should be addressed before, which will imply (to my opinion) major revisions. As I am not a native English speaker, I don't think I am qualified to judge of the quality of the writing.

Major concerns: My first major concern is a bout the whole discussion section. I find it quite weak and insufficient. Many arguments are highly speculative and do not appear to be really deduced from the database or the model output. For instance, in the second paragraph of the discussion, the authors present a description of the current knowledge on the iron cycle. It is not really a discussion and it is more or less already said in the introduction. I don't think there is anything new here. Furthermore, this is completely qualitative with no attempt to derive any time constants (for instance) from the observations, on the contrary to previous studies (e.g.,Brulandetal.,1994). Furthermore, the authors comment on the different forms of iron in the ocean by quoting the literature. The problem is that I don't understand the objectives of this discussion and how it is related to the database. The only connection I am able to find in the paper is that soluble iron may partly explain the nutrient like profile of iron. So what?! Finally it is postulated that scavenging should increase above 0.6 to 0.7nM as ligands become saturated with iron (basically, what was postulated already by Johnson et al.,(1997)). The only problem is that in many parts of the ocean, ligands concentrations have been observed to be much higher than 0.6 or 0.7nM (Boyeetal,2005; Cullenetal.,2006;...). Thus, the argument seems to be incorrect. Last point here, the authors claim that surface iron has a bimodal distribution. There is no figure nor statistical analysis to confirm that statement.

****** The discussion section has been edited to minimize repetition and to remove the discussion of soluble iron, and to focus more directly on the results (pages 19-21 of the revised ms). Time constants and residence time are addressed more directly with the model output in the companion paper. The text has been modified to clarify that scavenging increases at higher iron concentrations, but not necessarily tied to the 0.6 nM value. In addition, evidence for the elevated scavenging rates at high iron are noted in the comparison of dust inputs with observed iron concentrations (pages 16-17 of the revised ms). Both reviewers point out the lack of data supporting our statement regarding the bimodal distribution. The figure that illustrates this is actually in the companion paper. We have removed references to this bimodal distribution from this paper, and will address this issue in the companion paper.******

My second concern is about the seasonal cycle. The authors mention several times that iron exhibits a strong seasonal cycle in some regions. It would have been interesting to have an analysis (even basic) of this seasonal cycle, when enough data make it possible. It would add value to the study as it has never been done before on the global scale. It would also put some strength on the arguments on the seasonal cycle.

******As noted above, there simply isn't enough observational data available to credibly examine the seasonal timescale. In the entire N. Atlantic basin (one of the better sampled regions), 426 out of 592 observations are from the month of May. There are no observations during Feb, Nov, and Dec, and only six measurements in Jan, and only three in June and Sep. In the Southern Ocean there are no observations in the months May-September. Things are equally bleak seasonally in the North Pacific. Essentially no winter season is available at higher latitudes. We note this lack of winter season data in the revised ms (pages 12-13) and suggest that time series that provided seasonal coverage over multiple years would be extremely valuable. ******

Finally, the result section is not always easy to follow and some parts seem redundant. In the first part of this section, the authors analyze the surface iron distributions using full vertical profiles. Then, a potential relationship between the surface distribution and the magnitude of the dust deposition is looked for. Finally, the rest of the vertical profiles are examined. Not very fluid Ě Furthermore, the analysis of the spatial plots is very often a repetition of the analysis of Figure 5.

******We have re-arranged the paper to follow the more logical order suggested by the reviewer (dust - iron relationship is moved to pages 15-16, after the discussion of vertical profiles). There is some repetition (which we have tried to edit down) but the patterns repeat through the figures (with depth) and we feel this is worth noting.******

Specific comments: Page 1244, lines 20-21: in most current ecosystem models, this is not true anymore as the all include some simple (or less simple) description of the iron complexation with ligands which allows some scavenging below the ligands total

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concentration (Moore et al., 2004; 2006; Parekh et al., 2005; Aumont and Bopp, 2006).

******We have added a sentence noting than many models do now permit some scavenging of iron at these lower concentrations (revised ms page 4).******

Page 1246, lines 5-10: reversible adsorption is still subject to debate. Some observational studies suggest irreversible adsorption or very slow desorption at the molecular level (Quigley et al., 2001; Santschi et al., 2006;Ě).

******We note this uncertainty in the revised ms page 6. (We also return to this topic in some more detail in the companion paper).******

Methods section: Have you gridded the observations onto the BEC mesh? From page 1249, line 4, it seems to be the case. But it is never said. Furthermore, have you included the total dissolvable iron observations from the database? Examination of the database (both literature and iron values) suggests that they are not included (which is correct in my opinion) but it should be stated in the text.

******Observations have been averaged onto the BEC grid. This was noted only in the figure captions originally, now in the text of the revised ms on page 17. The reviewer is correct, we have included only dissolved iron measurements, not total dissolvable iron. This is noted in the revised ms page 8.******

Page 1252, lines 8-10: scavenging equations in BEC. The Chigh parameter has been drastically changed since Moore et al. (2004). In fact, I don't really understand that value (perhaps it is better explained in the other submitted manuscript). I assume the Dfe is in nM, then Fescave is extremely high (more than 1714/d). If it is in nM, then it is extremely low Ě Could you explain it a little more?

******There was a units problem here, that has been corrected. The value is the same as in Moore et al., (2004).******

Page 1253, 1st paragraph: Here are some statements on the seasonal cycle. But no figures to support them (see my general comments).

******These statements on the seasonal cycle were unclear in the original manuscript, as noted by both reviewers. We have clarified the text. One would expect a maximum surface concentration in the winter based on first principles (deeper mixing, less biological uptake, and vertical profiles that generally increase with depth in the upper ocean). Secondly, that this is the pattern seen in the model output at high latitudes, but that there is insufficient winter season observations to confirm this pattern (see revised ms pages 12-13).******

Page 1253, line 20 and Figures 2: It is hard to follow because the figures are quite difficult to read. A zoom over the top 100-500 meters of the ocean would help the reader.

******Zooming over the upper ocean actually didn't bring out this pattern any clearer. We have removed this statement concerning the Arabian Sea profiles from the text.******

Page 1254, line 8: definition of the HNLC regions. Your criteria to define the HNLC regions is incomplete as you only use the annual mean NO3 concentration. With this criteria, you take into account the North Atlantic Ocean, the Benguela upwelling, part of the Arabian Sea, the Mauritanian upwelling, Ěwhich are not exactly HNLC regions. You should use not only NO3 but also Chla to define the HNLC regions (obviously, High Nutrient but also Low Chlorophyll). In that case, the mean iron concentration in the HNLC regions will be certainly lower (by how much, I don't know).

******This was a good point, noted by both reviewers. We have tightened our definition for the HNLC regions to include both a "high nitrate" ($>$ 4.0 uM) and "low chlorophyll" (annual chl. $<$ 0.5 mg/m3) criteria. However, it made no difference in the mean HNLC chlorophyll concentrations as the additional areas excluded (i.e. Benguela) had few or no iron observations.******

Page 1254, line 26: reference to figure 3 is incorrect. Furthermore, the information could not be read from figure 2 (at least, I can't). See one of my previous specific comments.

******This line on the Arabian Sea has been removed from the paper.******

Page 1255, lines 1-22: This is basically a description of the dust deposition fields which is not the scope of this paper and has been done elsewhere in other specific studies. You should concentrate on the (quite weak) relationship between iron and dust deposition. Furthermore some statistical information would be nice.

******We have expanded our analysis and discussion of the relationship between iron and dust deposition significantly (revised ms pages 15-16, we also revised the figure (now Figure 4) by placing dust deposition on the x-axis and observed iron on the y-axis).******

Page 1256, line 28: relatively well correlated (figure 5b). It is quite subjective. I would not have said that. In fact, there is much less variability in the model than in the data. You are certainly not very happy with those results otherwise you wouldn't have done the second study (topic of the second manuscript).

******Both reviewers objected to this section and the phrasing "relatively well correlated", which we did not intend mean the model was doing a wonderful job. We have weakened this statement to say "modestly correlated" (revised ms page 17).******

Page 1257, lines 1-3: again the seasonal cycle. But you chose not to show the results which is, in my opinion, unfortunate. Last two paragraphs of page 1257: Many parts here are redundant with the analysis of figure 5. Discussion section: see my general concerns.

******The lines referred to on page 1257 referred to model output not the observations. These lines have been removed from the revised manuscript. The last two comments here have been addressed above.******

Interactive comment on Biogeosciences Discuss., 4, 1241, 2007.