

Interactive comment on “Iron profiles and speciation of the upper water column at the Bermuda Atlantic time-series Study site: a model based sensitivity study” by L. Weber et al.

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General comments

We thank the referees for their constructive remarks, which greatly helped to improve our manuscript. We followed most of their suggestions for minor changes and further made numerous small alterations in the text based on the two reviews.

The two most important changes in the manuscript are:

- Referee #1 criticised that we did not include iron limitation of phytoplankton growth

in our model. We now redid all model runs with a model version that includes limitation of phytoplankton growth by iron. Details can be found below and in the the revised manuscript.

- Following a remark by referee #2 on the residence time of iron that suggested a comparison of particle concentrations with Deuser et al. (1990), we discovered that a unit conversion error had sneaked into our 1-dimensional model code (albeit NOT into the 0-dimensional version presented in Weber et al (2005)). This error caused inorganic particle concentrations to be a factor of 1000 too high. We regret having submitted a manuscript with a still faulty model. The error is corrected now, and the particle concentrations are in excellent agreement with Deuser et al. (1990), as they had been in Weber et al. 2005.

These two changes forced us to rerun all model experiments again and redo all analyses and figures. Luckily, these changes do not change our conclusions qualitatively, although there are of course quantitative changes. The largest changes are in: a) the estimate of the scavenging residence time, which now agrees better with conventional estimates (Section 7), and b) the magnitude of the change in colloid aggregation rate between the 0-dimensional model and the 1-dimensional model, that is required in order to bring the 1-dimensional model close to observations and to produce reasonable deep iron concentration profiles (Section 5). In the old model version, the colloid aggregation rate had to be reduced by a factor of 1000 respective to the value used in Weber et al. 2005. In the revised model version, the reduction has to be only by a factor of about 10.

The changes in model results are mostly minor, although the modeled inorganic particle concentration decreased by a factor of 1000 between the model versions. This can be explained by the fact that only the sum of biogenic and inorganic particle concentrations enter the model equations (in the expressions for scavenging and colloid aggregation). This sum on average does not change by a factor of 1000, but only by

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a factor of roughly 4, because biogenic particles dominate the abiotic at the BATS site (both in reality and in the new model runs).

We have included two new references: Schartau and Oschlies (2003) and Droop (1983). In addition we have replaced Table 5 by a Figure (Fig. 8).

In the following we list our response to the specific comments of referee # 2 in detail:

Reply to the specific comments by referee # 2

1. The authors need to be careful to emphasise that the model results are not real data (e.g. p840 line 11 and elsewhere).

We agree with the reviewer that this model is intended to help understanding the key processes of the iron cycle and their sensitivities rather than as a predictive general model of the iron cycle that gives numerically accurate results. The following sentence in the introduction should make that clear (p826, line 27f) "...The model is primarily a tool to help in understanding the key processes of the iron cycle and their sensitivities to uncertainties in our present descriptions of these processes rather than as a numerically accurate reproduction of reality...". At p840 line 11, we added "In the model, changes in iron input due to increased dust deposition are buffered by ..."

2. Detailed comments P827: You may need to define colloids as you use the term.

We added the following sentence here: "...defined here by filter cutoffs 0.02-0.4 μm (Wu et al. 2001)"

3. The assumption in the model of excess ligand, governs all subsequent results and

in some ways represents the most intriguing issue in oceanic iron cycling.

We agree with the reviewer that the unknown origin and fate of organic iron-binding ligands is probably one of the most intriguing issues in understanding the global cycling of iron. In principle we could have tested several hypotheses on ligand origin in our model by making assumptions of the generation (e.g. by phytoplankton under iron stress or by cell lysis) and destruction (e.g. by photochemical reactions or bacterial breakdown) of the ligand. However, this would have created even more dependence of the model results on unknown parameters. We have therefore decided not to model total ligand concentrations explicitly but use the observational evidence from the BATS station (and many other open-ocean stations) that ligands concentrations are in excess of the dissolved iron pool. Our model results only depend very weakly on the exact value of this excess, as long as it is in the observed range of values in the open ocean. We have added an explanatory sentence in the model description.

4. The model emphasises the potential importance of photoreduction reactions but it is not clear to me which rate is used for this process in which parts of modelling work and why. I am also unclear what if anything is assumed about the photochemistry of colloidal iron.

The rates of photo-reduction (including colloidal iron) are taken from literature: k_{ph1} (photo-reduction of FeCol): Wells and Mayer 1991, where we need to apologise here for a typing error in Table 2. The actual value for k_{ph1} of Run A is 0.432. We corrected that. k_{ph2} (photo-reduction of FeL): Emmenegger et al. 2001; k_{ph3} (photo-reduction of Fe(III)':Johnson et al. 1994; k_{ph4} (photo-reduction of Fep): Johnson et al. 1994. All photochemical rates are assumed to be proportional to the instantaneous irradiance (Weber et al 2005). The photochemical reactions are always included in the study and not limited to special parts of the modelling work.

5. *The N component of the model. It has historically been difficult to produce a balanced N cycle at the BATS site (or elsewhere). I note both Jenkins and Williams have separately suggested that lateral N transport is important, it is not clear to me if this is also the case for Fe.*

The 1-dimensional model of the nitrogen cycle presented here represents the nitrogen balance at BATS about as well as other 1-dimensional NPZD models after data-assimilation, i.e. it gives reasonable values for vertical export, primary production and chlorophyll, but also shows some typical deficiencies, such as too high DIN concentrations in the summer mixed layer. Whether neglecting lateral transport is indeed the most important deficiency, as Williams and Jenkins have suggested, and not other shortcomings, such as overly simple description of the ecosystem, could be debated. The role of lateral advection for the Fe balance at BATS is difficult to judge, given the still sparse observations. But there is reason to believe that it is probably less important than for the N balance: While for the N balance, lateral advection must balance any small difference between vertical mixing and export of organic matter out of the euphotic zone (except for N fixation; see next answer) for Fe, additional sources and sinks (input from dust and the loss by scavenging onto sinking particles) come into play and can compensate for imbalances. We see our modelling effort as a contribution to understanding the balance between these fluxes; if a balance cannot be reached without resulting in model results conflicting with observations we would see this as possible indication of lateral transport.

6. *I note also the N₂ fixation is ignored here although it may well be important to the N budget and also be limited by Fe supply.*

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The importance of nitrogen fixation at the BATS site is still critically discussed. There is no doubt about significant nitrogen fixation happening near Bermuda. However, Watson 2001 suggests that N₂-fixers do not contribute much to the production at BATS, given that the nutrient requirements of phytoplankton for N and P are in closely the same ratio as they are found in deep water. Observations made by Karen Orcutt also indicate that it provides only a small fraction (about 2-3%) to total primary production during the summer period. Often the absence of nitrogen fixation in some models has been considered as an explanation for the discrepancy between observed and modeled primary production rates. According to a recent assessment of a nitrogen based ecosystem model, the discrepancy was shown to be systematic, such that it clearly pointed towards the inappropriate application of a constant Redfield C:N ratio for phytoplankton nutrient acquisition (Schartau and Oschlies, 2003).

7. P829 line 21 is sinking organic matter only regenerated in the lowest box, as implied,? that seems unlikely, there is regeneration at all depths.

This is a misunderstanding. The model considers regeneration at all depth indeed. The paragraph here refers only to the boundary conditions. To avoid further misunderstandings, we rephrased the sentence as follows: "Sinking biogenic matter remineralises throughout the water column. The remainder that reaches the lowermost model box is instantaneously remineralised there."

8. P830 DIN at BATS in summer is $\ll 1\mu\text{M}$ not sure if this is important but getting $N < 1\mu\text{M}$ is not a particularly rigorous test. This may also be true of the chlorophyll data on p831.

This is of course right. We changed the contour intervals in our plots in such a way

that the low concentrations in summer can be better compared between model and data. We rephrased the comparison in the text somewhat but the conclusions remain unchanged.

9. *P831 Excluding DOC and its sinking will underestimate export I think, which is not what is implied here.*

We agree completely with the reviewer and that is exactly what we tried to verbalise here (P831, line 11ff). We rephrased the sentence to make it clearer: “Both the assumption of a constant C:N ration of 6.625 in organic matter and the lack of a dissolved organic carbon pool might underestimate the export production of the model.”

10. *P832 Given the model has several classes of Fe, the authors need to be very clear about what they mean when they say 8220;modelled iron concentrations8221; (line 2) is this dFE or dFe+colloidal etc. and elsewhere.*

“Modelled iron concentrations” refers here to dissolved iron (dFe), where dFe refers to 0.4 μm -filtered samples (see Introduction, p826, line 10). Therefore dFe includes the iron forms FeII', FeIII', organically complexed iron and colloidal iron. This was not very clear expressed by us here. We defined dFe in Section 2 (first paragraph) now, and made it clearer on p832 as well.

11. *P833 and later. Residence times as calculated from field data usually refer to residence times with respect to the overall loss from the euphotic zone. Again the authors need to be specific about the residence time they calculate with respect to what processes (p830and 840). The model results can also be compared to estimates of iron residence times provided the model results are averaged over an appropriately*

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long enough time scale.

The residence time as used here is the residence time with respect to abiotic loss processes to the particulate phase (aggregation and scavenging), i.e. it does not include other loss processes, such as biological uptake and vertical mixing. It is strongly time-dependent with large values in winter, when particles are scarce, and lower values in summer. Directly averaging the residence time has no physical meaning because the average over a product/ratio is not equal to the product/ratio of the averages. Averaging the fluxes and concentrationE s over time, and then calculating the residence time, we obtain values at depth on the order of 100 years, in the range of data-based estimates.

12. P833 line 24 'dissociation' of what? I found other parts of this section rather confusing.

We meant the dissociation of organic complexes, which is clarified in the text now.

13. P834 line 11 I suspect its surface area not number of particles that matter.

We agree that surface area would give a more reliable measure of the scavenging potential for iron than particulate matter, but we do not have much information available on surface area. In effect we are assuming that the particle size spectrum is invariant over time, which would make the area proportional to the mass. This is clearly an approximation. We mention that briefly here now.

14. P839 line 17 is it 0.6nM?

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The reviewer is probably alluding here to the Johnson et al. (1997) paper 'what controls dissolved iron concentrations in the worlds oceans', where it was stated that the concentration of iron in the deep ocean tends to be more or less uniformly 0.6 nM. Many subsequent model studies (e.g. Archer and Johnson) have used this as a yardstick to measure the performance of models and sometimes enforced this concentration by scavenging iron only above a solubility limit near 0.6 nM. In our model, the concentrations of colloidal and ligand-bound iron are restored in the lowermost grid point to the values observed by Wu et al. Besides the lowermost grid point, concentrations are not prescribed in any way but simply evolve according to their own dynamics. Especially the model does not use a prescribed solubility of 0.6 nM.

15. Residence times of 30,000 days seem unreasonably long to me (depending on the definition used as noted earlier) I suspect the particle concentration field may be the problem and I realise I don't know how this is generated in the model. Certainly in summer dust deposition increases particle concentrations but the effect is modest. Deuser and coworkers reported data on the particulate matter at this site which can be used to test this (Mar Chem 29, 203 1990).

The residence time has changed quite a bit after correcting an unit conversion error in the model. Our following comments refer to the model results without this error. The model distinguishes between two particle classes, biogenic (phytoplankton, zooplankton and detritus) and aeolian (dust). Dust deposition in the model is from the global model dust deposition fields by Mahowald et al. (2003). We assume that detritus and dust sink at the same speed (and probably associated in aggregates). This is of course a simplification, but any more detailed modelling would require information on the particle size spectrum in dust deposition. The modeled concentration of particulate organic matter, averaged over time and the upper 200 m is 24 mg/m³, compared to 33

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(60% of 55) mg/m³ in Deuser et al. (1990). Our average abiotic particle concentrations (which only represent the aeolian fraction, not other inorganic particles, such as diatom frustules and coccoliths) are 0.13 mg/m³, consistent with Deuser and coworkers, who state that less than 5% of the total 55 mg/m³ suspended particulate matter are clays. Concerning the quoted values of the residence time: it is important to note that firstly this is the residence time with respect to inorganic adsorption onto particles only, i.e. it does not include losses through biological uptake or vertical mixing. Secondly, this residence time varies strongly over time, and the quoted number is the maximum value reached at a time when there are almost no particles in the water to which iron could adsorb. Residence time is much lower during other times of the year. We have clarified this in the text.

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