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Biogeosciences Discussions

Interactive comment on "Impact of dust deposition on Fe biogeochemistry at the Tropical Eastern North Atlantic Time-series Observatory site" *by* Y. Ye et al.

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General

We would like to thank both reviewers for the time and considerable effort they put into reviewing our manuscript. Although we do not agree with all comments by referee #1, we think that both reviewers comments were very helpful for improving the paper. Especially the review by referee #1 raises a number of points that criticize our model setup. We think that some central criticisms by the reviewer were justified and have adressed the issues raised:

- We ran a large number of additional model sensitivity runs varying the ecosystem model parameters, in order to check how strongly our model results are affected by these choices. We also compared our parameter settings to a number of other ecosystem models for the region. The outcome is that the model results are relatively insensitive to most of the parameters, except to the phytoplankton excretion rate.
- We checked our formulation of particle aggregation, described it much more in detail in the text, and performed a set of sensitivity runs with respect to aggregation rates.
- We changed our formulation of the temperature dependency of organic matter remineralization to bring fluxes at depth more into line with sediment trap estimates.
- We performed two more sensitivity studies with respect to the remineralization rate of weak organic iron-binding ligands, and to the re-dissolution of particle-adsorbed iron.
- We included many more data to compare our model results with (including an unpublished profile of dissolved iron from the TENATSO site, thanks to Micha Rijkenberg; NIWA, New Zealand), and eliminated direct comparisons with data sets that are from outside the eastern subtropical and tropical Atlantic.
- We checked again the sources for our parameter choices, and documented them better in the manuscript.
- We extended our modeled region to the upper 1000 m of the water column instead of only the upper 400 m, to be able to better constrain weak ligand dynamics. This also necessitated that we now use a much longer spinup-period for our model runs (25 years instead of 3 years).

In consequence, all model runs were redone and most numbers in the text have changed slightly. However, almost all of our qualitative conclusions are unchanged and we still stand to every result that was stated in the first manuscript version, except to those related to the composition of sinking particulates below the mixed layer. We have also tried to make the presentation of our results clearer than in the first manuscript.

We would like to state at this point that the main aim of our model at present is not to reproduce observations that were made at a specific time as closely as possible, but rather to contribute to process understanding. This is done by implementing in the model hypotheses (e.g. on the source or life-time of organic ligands) that have been put forward, and by checking whether the model outcome is compatible with the few available observations. As more data becomes available from the TENATSO site, we hope to progress to a less qualitative approach.

General comments

1.Please include all model description and sensitivity study descriptions in the methodology section. 2. Please be clear about when and how you are comparing to observations, and make these comparisons easier to follow. 3. Please be clear about what are new model elements and results in this paper, compared to previous papers.

The description of sensitivity studies was shifted into the model description section. We have added a number of new comparisons to data and tried to be more precise about what are observations and what is model output. We also have discussed the new aspects of the present model (ligand dynamics, particle aggregation) and the aims of the study more prominently in the introduction now.

Specific comments

Your abstract really only talks about the model, and perhaps that's all you can talk about. But if you can show that your model represents reality, then you can infer something about the real world. So showing more obs/model comparison is very important.

We have added more data from the literature, e.g. from other sediment traps in the region, and also an unpublished iron profile directly at TENATSO that was kindly made available to us by Micha Rijkenberg. In the comparisons, we have made it clearer which is model result and which is observations.

Also, please think about demonstrating more clearly that your hypotheses are the BEST hypotheses: e.g. other hypotheses contradict the few observations we have. This is not always argued very clearly in the text. I try to highlight below where I see obvious improvements can be made. sentence fragment: 'To provide a better understanding of this complex, several numerical models'

We have tried to demonstrate the validity of our hypotheses more clearly where appropriate. In many instances, however, it is not that our hypotheses contradict the commonly held beliefs, but only that no one has tested them in a model before. This relates e.g. to our discussion on the life-time of weak vs. strong ligands. Our findings here support a generally held belief that has e.g. been expressed in Hunter and Boyd (2007).

'by choosing a slightly different turbulence parametrization, and a slightly different time/space discretization.': . It makes sense to do this, but please just describe better: either the basis of these changes (more mixing because we know there's more mixing based on study blah), and/or the exact values.

We now give more information on the physical model setup and a motivation for choosing a somewhat higher background turbulent kinetic energy.

1st paragraph, section 2.2: you describe 5 different iron species: please tell us, are these the standard species? Are these the ones measured in the field? Are these similar or dissimilar to obs?

We distinguish between size classes ('soluble', colloidal and particulate) which correspond to operationally defined (by filter cutoffs) classes measured in the field. Within 'soluble' iron we distinguish between organic and inorganic species and between redox species, which are all measurable, but not often measured all on one sample. We discuss the relation of our modeled species to the measured ones briefly now.

'Here we chose the rate for colloid redissolution from Rose and Waite (2003b) (kcd = 0.41 $d^(-1)$) and assumed a rate for re-dissolution of particulate iron to ensure that the flux from particulate to colloidal pool is in the same order of magnitude as colloidal aggregation (kpd=1.5 $d^(-1)$).' Is this a new value or a new process? Please tell us why are changing things.

The process itself is not new, see e.b. Bacon and Anderson (1982) for thorium. However, we are not aware of rate measurements for this process, so we performed a sensitivity study varying this parameter. This study and its results are now described in the manuscript.

'1% solubility (Johansen et al., 2000; Spokes and Jickells, 1996; Baker et al., 2006a,b).' 1% solubility is good for close to dust regions, as you are here: please indicate that you are reasonable for this reason.

A brief explanation for choosing 1% solubility is added in the paper. Moreover, we studied the sensitivity of our model results to variations in the combined parameter solubility $\times$ percentage iron content in dust and describe the outcome in the manuscript.

'Particles in our model are split into four classes by their composition and size: 1) small detritus; 2) fine terrigenous material deposited by Saharan dust events; 3) large, pure organic aggregates and organic material in mixed aggregates; and 4) terrigenous material in mixed aggregates. This classification is based on the size distribution of sinking particles at the TENATSO site (see Sect. 4.4 Removal of dissolved iron). Two different settling velocities are assumed for the small organic and inorganic particles 1) and 2), and the aggregates 3) + 4), respectively. Particle aggregation is described as coagulation between small particles. Parametrization and choosing of the rates and constants are explained in detail in Sect. 4.4.' why is the model description in a results

section? It makes much more sense to put it in the model description section.

We moved the description of size classes and coagulation rates into the model description section. The description also has become more detailed now.

'The modeled chlorophyll a concentration in surface waters is between 0.2 to 0.45 μg L^{-1}1 and consistent with the observations at the TENATSO site or during cruises past the Cape Verde Islands, which vary from 0.06 to 0.7 μg L^{-1}1 (Cruise data of POS 320/1, POS 332, Meteor 68/3, POS 348/2, Merian, 20 April 2008, L. Cotrim da Cunha, personal communication). Between March and November, a deep chlorophyll maximum with values around 0.4 μg L^{-1} develops at the depth of nutricline near 70m. Primary production in the model shows a strong daily, but only a weak seasonal variation, with an annual average of 660mgCm^{-2} day^{-1}. Primary production estimated from MODIS data, using the algorithm by Behrenfeld and Falkowski (1997) averages to 470mgCm^{-2} day^{-1} for the 1x1 square around the TENATSO station and the period from July 2002 to December 2007. Phytoplankton growth is limited by nitrogen rather than iron from surface to the depth of the deep chlorophyll maximum. The lowest value of the nitrogen limiting factor f_N is around 0.3 found in surface waters during phytoplankton blooms in summer and autumn.' Here you are showing that your biological model has some validity. It's probably worth a figure or two, to show what you are doing.

We now show two figures containing model chlorophyll together with regional observations, one converted from biomass in nitrogen units using a fixed C:Chl ratio, one using the empirical C:Chl relation by Cloern et al. (1995). We also added more estimates for primary production from the region.

'Also, is the model N limited? Is that consistent with obs?

Yes, phytoplankton growth in the model is mainly limited by nitrogen which is consistent with observations. We added a reference for N limitation in this region.

What is equation 2? Is this a model equation or observed equation? If it is a model equation, please put it in the model description. Please be sure to define all the terms in the equation, and tell us why you are showing the equation. I'm a bit lost here.

This is a model equation for N uptake rate. We moved it into the model description and the terms are clarified there.

'The modeled DFe concentration ranges in the same order as observations (Fig. 3).' the results section. Is some of this scatter due to seasonal variability? It would seem better to show us seasonal variability of both models and obs, or is it not variable? This, especially, would seem to be an important figure to compare in detail to the very limited observations.

We now describe the modeled DFe seasonality. On average there is an annual cycle with two maxima during the year, one in winter, the other in late summer. However, there is also considerable interannual variation and also some short-term variability due to dust deposition events. This makes a direct comparison between the observed and modeled variability (e.g. in a seasonal plot) difficult; especially since we are presently still modeling the 1990s (due to forcing data availability), while the observations are from after 2000. This will change very soon, we have just obtained forcing data until 2007.

Section 4.2: if CDOM is important, could you make sure you indicate whether your CDOM amounts match obs?

CDOM is not a variable that is explicitly modeled, so we cannot compare. CDOM enters mainly in the rate of superoxide generation, for which we use an open-ocean value out of the range found by Micinski et al. (1993). This is consistent with CDOM concentrations from the region (Siegel et al., 2002).

'We considered the role of copper in the same way like Weber et al. (2005).' Replace like with as.

Changed as suggested.

Section 4.2: Does the inclusion of Cu improve your model? Or do you not know because you have not enough obs? Please indicate in the text what the conclusions are from these sensitivity tests.

The conclusion from Weber et al. (2005) was that considering superoxide dismutation by Cu in the model changes the amplitude of Fe daily cycle. Copper concentration in the model was taken from observations (Van Der Loeff et al., 1997), but we have no local information on its organic complexation yet, so we are forced to make reasonable assumptions here (Moffett, 1995). We do not yet know whether the redox cycle amplitude is consistent with observations yet, but there is work underway in the group of P. Croot in Kiel, so we will soon know. The only comparison that we can make is to the hydrogen peroxide inventories measured by Steigenberger and Croot (2008) and these compare favorably.

'Our modeled concentration of total strong ligands (Fig. 7) is close to the measured 20 data by Rue and Bruland (1995).' Please put their data on the plot.

We reworded the whole paragraph and replaced direct comparison with the data by Rue and Bruland (1995) with other more regional data, which are now shown on the plot, cited and referred to.

'Contrary to the observations, modeled weak ligands decrease exponentially with depth below its maximum at 90m and reach a relatively low concentration below 300m (0.4 nmol L^{-1}).' Which obs? Please indicate on plot, if possible. The readers need to know also how many/few obs we are talking about, and where they are from.

We listed the observations here and show a new figure of modeled ligand profile compared to these observations.

'For further sensitivity studies of our model we have therefore introduced a restoring of the concentration of total weak ligands towards 2 nmol L^{-1}, a commonly observed

value in the deep ocean, throughout the water column with a rate of 0.1 d^{-1}, in order not to affect iron speciation and losses by too little complexation. This restoring is weak enough so that loss processes near the surface (biological uptake and photochemical decay) still lead to the observed vertical gradient of total weak ligand concentration there.' What is the result of these sensitivity studies? Usually sensitivity studies are described in the model methodology section, and the results are described in the results section.

We put the description of temperature dependency of ligand decomposition into the model description and the sensitivity studies for Q10 into the result/discussion section. The restoring of weak ligands is described in the model description and discussed in the result/discussion section as result from those sensitivity studies.

Most of section 4.4.1 should be in the methods section: only the results should be in the results section.

We moved a large part of section 4.4.1 into the model description, making a new subsection on origin and fate of organic ligands.

Figure 15 should be referenced after Figures 12-14.

Changed as suggested. Several schematic figures have been taken out and replaced by figures showing data-model comparisons and model sensitivity results.

Maybe you should make a table of the sensitivity studies, so that it is clear what you have looked at in more detail?

We now show tables for the more extensive sensitivity studies. For some smaller sensitivity studies consisting just in one to three additional model runs, we did not to save some space.

'Despite the simplicity of the NPZD-type ecosystem model, observed chlorophyll a concentration and seasonality of primary production at the TENATSO site are well 20 reproduced.' If this is in the conclusions, you definitely need to show it in the paper.

We do now, hopefully convincingly.

'This double role of dust deposition should be taken into account in investigating the impact of varying dust deposition on Fe speciation and biogeochemistry.' Is this the first paper to argue this?

No, this sentence does not tell the readers a new finding from our our modeling. Our modeling here confirms what others have argued before. To our knowledge, the first model to account for particles as iron scavengers was the one used in the PhD Thesis of Andy Ridgwell (Glacial-interglacial perturbations in the global carbon cycle, PhD thesis, 134 pp., Univ. of East Anglia at Norwich, UK, March 2001). We reworded the sentence.

In the conclusions, please be sure to differentiate new results from this study, from previously found results.

We rewrote the conclusions.

References

Bacon, M. P. and Anderson, R. F.: Distribution of thorium isotopes between dissolved and particulate forms in the deep sea, J. Geophys. Res., 87, –, http://dx.doi.org/10.1029/JC087iC03p02045, 1982.

Cloern, J. E., Grenz, C., and Vidergar-Lucas, L.: An empirical model of the phytoplankton chlorophyll : carbon ratio-the conversion factor between productivity and growth rate, Limnology and Oceanography, 40(7), 1313–1321, 1995.

Hunter, K. A. and Boyd, P. W.: Iron-binding ligands and their role in the ocean biogeochemistry of iron, Environ. Chem., 4, 221–232, http://dx.doi.org/10.1071/EN07012, 2007.

Micinski, E., Ball, L. A., and Zafiriou, O. C.: Photochemical Oxygen Activation: Superoxide Radical Detection and Production Rates in the Eastern Caribbean, J. Geophys. Res., 98, –, doi:10.1029/92JC02766, http://dx.doi.org/10.1029/92JC02766, 1993.

Moffett, J. W.: Temporal and spatial variability of copper complexation by strong chelators in the Sargasso Sea, Deep Sea Research Part I: Oceanographic Research Papers, 42, 1273–1295, http://www.sciencedirect.com/science/article/B6VGB-3YS8ND3-19/2/277111ce50d20b88267dd1716ff6db94, 1995.

Rue, E. and Bruland, K.: Complexation of iron(III) by natural organic ligands in the Central North Pacific as determined by a new competitive ligand equilibration / adsorptive cathodic stripping voltammetric method, Marine Chemistry, 50, 117–138, 1995.

Siegel, D. A., Maritorena, S., Nelson, N. B., Hansell, D. A., and Lorenzi-Kayser, M.: Global distribution and dynamics of colored dissolved and detrital organic materials, Journal of Geophysical Research, 107, 3228, doi:10.1029/2001JC000965., 2002.

Steigenberger, S. and Croot, P.: Identifying the processes controlling the distribution of H_2O_2 in surface waters along a meridional transect in the eastern Atlantic, Geophysical Research Letters, 35, L03 616, doi:10.1029/2007GL032555, 2008.

Van Der Loeff, M. R., Helmers, E., and Kattner, G.: Continuous transects of cadmium, copper, and aluminium in surface waters of the Atlantic Ocean, 50[deg]N to 50[deg]S: correspondence and contrast with nutrient-like behaviour, Geochimica et Cosmochimica Acta, 61, 47–61, 1997.

Weber, L., Völker, C., Schartau, M., and Wolf-Gladrow, D.: Modeling the speciation and biogeochemistry of iron at the Bermuda Atlantic Time-series Study site, Global Biogeochemical Cycles, 19, GB1019, doi:{10.1029/2004GB002340}, 2005.

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