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Discussions

Interactive comment on "Regional impacts of iron-light colimitation in a global biogeochemical model" *by* E. D. Galbraith et al.

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Galbraith et al use a very simple biogeochemical model to analyse the impact of iron limitation on phytoplankton growth rate. More specifically, they include iron limitation at three different levels: (1) the maximum photosynthetic rate, (2) the α P-I slope and (3) the maximum chlorophyll-to-carbon ratio. Using this model, they carefully study the impact of the lack of iron on the growth rate and on the light-harvesting efficiency (both are of course related but the second aspect is more specific than the first one). In particular, they justify partly their study by the fact that the role iron played on the light harvesting system is often ignored in previously published biogeochemical models. In a first part, the authors describe their model which is a very simple model based on four explicit modeled tracers. They also introduce their iron limitation parameteriza-

tions. Then, they briefly compare their model output to some observations. Finally, they perform sensitivity runs to examine the impact of the three different components on which iron limitation plays a role. Their main conclusion (according to me) is that the impact of iron on the maximum photosynthetic rate dominates over most of the ocean and overwhelms the potential decrease in the efficiency of the light harvesting system, especially in the Southern Ocean. As such, biogeochemical models that ignore changes in the photosynthetic efficiency due to iron should not make a huge error (except perhaps in some parts of the Northern high latitudes).

This paper is very clearly written and presents a novel contribution to the understanding of the role of iron in controlling phytoplankton growth. As such, I strongly support its publication in Biogeosciences. However, before publication, I would like my comments to be adressed by the authors. Furthermore, the review by Alessandro was published by the time I wrote mine. So I had the opportunity to read it. I agree with most of his comments. Thus, I generally do not repeat these comments here and consequently, I strongly encourage the authors to carefully address all of his comments.

1 General comments

I have two general comments:

1.1 Deconstructing the response of phytoplankton

First, I find the approach really convincing and nicely presented. However, I have one serious concern on the method used to deconstruct the impact of iron. Basically, the authors perform different simulations differing by the number of processes on which iron acts. But, in this case, the iron distribution should change significantly. And thus, when comparing the different simulations, the authors compare not only the impact of one parameterization on phytoplankton growth rate but also the effect of a different iron distribution. This is not a problem for the AllMean experiment but for the other runs, it could be a problem. This makes the analysis much more tricky I believe. Furthermore, these could lead to numerical issues because when iron limitation is not accounted for (AllMean) or when it only alters θ or α, iron concentrations could drop below zero. However, when looking at figure 12, the linear sum of the different simulations does not lead to a fundamentally distinct solution from the full run (AllVar). I don't really understand this as it would mean that changes in iron distribution, in maximum photosynthetic rate and in photosynthetic efficiency combine to each other in almost a linear manner. Or, it would mean that only one process is important. In this case, one could suspect photosynthetic rate but this is obviously not the case in the tropical regions and in the North Pacific, as also stated by the authors (see Figs 9-10).

In fact a simple way to really deconstruct the signal would have been to read the iron distribution produced by the AllVar run in all experiments. In that case, the iron distributions is invariant and only phytoplankton growth rate changes. Of course, these experiments would have missed part of the answer but this would have brought additional and simpler information. Anyhow, I don't ask the authors to redo these experiments but rather to comment on my concerns and to clarify the text.

I am not sure to be perfectly clear here. To summarize my comments, I would say that it is difficult to catch here how much of the differences is due to the direct effect of iron on phytoplankton growth and how much is due to a different iron distribution due to a different uptake rate. A way to adress this is to perform the kind of simulations I suggest above. Another (complementary ?) way is to discuss on the differences in the iron distributions produced by the different experiments. Anyhow, it would be nice to have that information (sensitivity of the iron distribution to the parameterizations).

1.2 The vertical distribution

My second concern is about the same as one of Alessandro's concerns. So I won't really detail this here. The interplay between iron limitation and light limitation acts not only on the horizontal scale but also on the vertical scale (this is obvious of course). Yet, the authors only insist on the horizontal (and temporal) scale. I would have liked to see a few words on what happens on the vertical scale.

2 Specific comments

Page 7521, lines 5-9: The definition of θ_{max} is not very clear. From what I understand of Geider's paper, this is the maximum chlorophyll to carbon ratio when light is extremely limiting and when nutrients are not limiting. If one uses this definition, the impact of iron on this parameter should not be included. Otherwise, this is legitimate. The author should be clearer on this.

Page 7523, lines 25-29: I am not so sure that the model is adapted for diurnal simulations. See for instance the paper by Flynn and Fasham (2003).

Page 7525, line 1 to 13: The formulation proposed by the authors feels like it is different from Monod. But, in fact, it is exactly equivalent to the Monod Formalism as it is in Balanced growth. Thus, in other words, using this formulation or the classical Fe/(K'+Fe) with properly defined K' ($K' = K_{Fe} * K_{Fe:P}/(K_{Fe:P} + (Fe:P)_0)$) is exactly equal in this context.

Page 7525, lines 17: Fasham et al. (2006) have used a different formulation for the variation of this parameter. I just mention this for information.

Page 7531, lines 10-15: The sediment efflux is very small here relative to other estimates or studies. For instance, in Elrod et al (2004), this flux is about 100 Gmol

Fe/year. In Moore and Braucher (2008), it is 32 Gmol Fe/year. Yet, the authors even mention later (section 2.5) that this efflux has been further reduced. Could the authors be more precise on this source ?

Page 7531, equation 14: This equation looks like coming out of a hat. No problem with this as I understand why the authors chose it but some more explanations would be appreciated. See also Alessandro's comment.

Page 7532, equation 16: I understand the rationale behind this parameterization which is perfectly legitimate. However, I don't really understand the parameterization, especially the power 1.5. Could the authors describe this in more details ?

Page 7535, line 25: The correlation coefficient is really high, which is excellent. Could it be possible to see a plot of modelled chlorophyll vs. observed chlorophyll ?

Page 7536, line 1-20: Would it be possible to have a more quantitative idea of the agreement between observed and modeled iron distributions as for chlorophyll and PO4 or as in Moore and Braucher (2008) ?

Page 7535, lines 10-14: Like Alessandro, I do understand why the authors don't compare their macronutrient directly to NO3. Furthermore, I also understand why they don't compare it to PO4 because the comparison would be poor in the subtropical gyres (especially probably in the Pacific). However, their observed tracer looks a little bit magical. It needs more explanation.

Page 7535, lines 17-21: If the correlation between mean observed and modelled macronutrient distributions is good, the correlation between the standard deviations will be most probably good because mean concentration and standard deviations are most of the time highly correlated.

Page 7536, line 14: very similar seems a little bit too strong.

Page 7536, line 18: The low Def_{Fe} in the equatorial Pacific is located south of the equator, not right at the Equator according to what I can see on the figure.

Section 3: See my first general comment

Figure 6: The equation number is not correct. Furthermore, figures 8, 9, 10, and 12 are really really small.

Interactive comment on Biogeosciences Discuss., 6, 7517, 2009.