Review of Galbraith et al., "Regional Impacts of iron-light colimitation in a global ocean biogeochemical model"

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General Evaluation

Galbraith et al. present the description of a new model of ocean biogeochemistry that considers relatively few state variables and resolves a variety of other important tracers and processes in an implicit fashion. This model (BLING) is then used to examine the role of iron (Fe) limitation on three aspects of phytoplankton growth and physiology, namely the maximum light-saturated growth rate, the carbon to chlorophyll ratio (θ) and the initial slope of the photosynthesis versus irradiance curve (α). The authors come to the conclusion that in cold Fe limited waters (such as the Southern Ocean) the effect of Fe on the maximum light-saturated growth rate is predominant. The BLING model is a novel approach in the context of the more typically used ocean general circulation and biogeochemistry models (OGCBMs) and therefore provides an alternative and complementary tool to examine ocean biogeochemistry. The manuscript begins with a description of the BLING model and then discusses the model evaluation and its application to examine a specific question (impact of Fe limitation on phytoplankton growth and physiology). As such, this manuscript is a very welcome contribution and certainly merits publication in Biogeosciences. However, prior to its acceptance/publication I would like to see a few (minor) revisions to the manuscript. These are detailed in the following section.

General Comments

BLING is presented as an intermediate between complex OGCBMs and those that rest upon more simple parameterizations. However, as the manuscript currently reads, the reader has no reason to understand why complex OGCBMs with the explicit resolution of multiple tracers exist. Galbraith et al. need to include a brief discussion of what their model 'misses'. Why should anyone bother to include these additional tracers? Can you discuss under what conditions and to answer what questions BLING is as appropriate as a more complex OGCBM and alternatively, for which applications BLING may not be as appropriate, or even more appropriate? This could be included at the end of section 2 ('Model Description').

On page 7521 lines 24-27, the authors give the impression that no existing OGCBM includes the effect of Fe limitation on growth, θ and α. This is not true. For example, the current version of the PISCES model includes Fe limitation in all three of these processes (although it does not get a great deal of discussion). The BEC model of Keith Moore might also include these processes. Nevertheless, it is indeed true that no OGCBM has examined the individual and interactive role of these processes, which is this main focus of the science part of this manuscript.

I have some questions regarding the length of the simulations. BLING is presented as a 'cheap' tracer-lite biogeochemical model that is run in a relatively low-resolution physical framework (3° longtitude x 0.5 to 3° latitude). This should provide BLING with the scope to do much longer integrations than state-of-the-art OGCBMs. However, it appears that the model was only spun up for 400 years and then experiments of 100 years were conducted? Typical OGCBM experiments are spun up for 1000s of years and experimental runs are of the order of 500 years (see: Tagliabue et al., 2009a; Aumont and Bopp, 2006). Can you really state the model is at equilibrium after 400 years? What about the deep north Pacific? This might impact the correlation with data, since there is the potential that the model has changed little relative to its initial conditions in some places in the deep ocean. When I first read the BLING description I thought this might be an application that could 'hold its own' against complex OGCBM for historical simulations, but would also be very suitable for numerous long simulations that might be outside the scope of more complex OGCBMs.

Regarding the actual scientific results of the study, I have one issue: What about the vertical? This paper is dealing with the impact of light and subsurface changes in physiology/production could be easily transmitted to the surface. Moreover, accounting for these subsurface processes could prove to be an important part of understanding the surface response (Tagliabue et al., 2009b). The authors should at least look into this aspect and, if warranted, some station plots or zonal means could be presented to show how each of the Fe dependency terms act in a vertical context. Overall, the conclusions of the more scientific part of the study are interesting and important. I would like to see a little more discussion on how such features might be observed. In addition, given the computation cheapness of BLING, I think a few sensitivity experiments are necessary in order to provide some bounds to the conclusions. For example, how much do you have to change the parameter choices to modify your conclusions? Or are they relatively robust given reasonable variation in parameters?

Specific Comments

P7519, line 1: There are plenty of non-coastal environments where macronutrients are depleted and Fe remains in 'excess'.

P7519, line 25: For completeness please give the units of P^C. I assume it is s^{-1}.

P7521, lines 24-27: see above comments on moderating the strength of this statement

P7521, line 25: The model either has oxygen or it doesn't! Be consistent; say the model has 4 tracers.

P7523, lines 25-28: This is the ideal place for mentioning some drawbacks of the model and outlining some applications that it is suitable/less suitable for.

P7525, line 5: State if there is any light dependency of the Fe:P ratio – this is also evident from the experiments of Sunda and Hunstman (1997).

Equations 6-7: For non-modelers it might be useful to state that there is a maximum 'scope for change' for θ and α that is moderated by Fe limitation.

Equation 10: if $\lambda^T = \lambda e^{kT}$, then λe^{kT} can be replaced by λ^T for a simpler equation (see also equations 11 and 12). Additionally, it would be nice to know what λ represents. How is it different from λ^T, which is described?

Equation 11: P* needs to be discussed. What does this parameter represent?

P7531, line 1: Tagliabue and Arrigo 2005 should be Tagliabue and Arrigo 2006.

P7531, paragraph beginning line 7: the authors may be interested to know that a recent paper (Tagliabue et al., 2009b) reports how the first order impact of complex Fe speciation can be included in a global OGCBM. They approach they use fits well with the BLING philosophy.

P7531, line 15: It is stated later that the sediment flux was reduced. Don't give unnecessary information - provide the sedimentary flux that was used. In addition, you state that this flux is linked to export production, how?

P7531, line 20 and around: The nuances of these parameterizations are not explicit. As I understand it, KFeL is reduced in surface waters, as a function of light, which means more inorganic Fe (Fe') is present, which should mean a greater loss of dFe by scavenging. However, within the time step of the model, might not the Fe(II) produced by photoreduction be recomplexed by the ligand (Barbeaus work shows that some ligands retain their complexing capacity after photoreduction, whilst some do not). In the real ocean, the ligand concentrations are also higher in the surface ocean, which should increase the net complexation of Fe. The impact of the production of siderophores on KFeL is present when Fe approaches Fe_{min}. As such, the net result of this parameterization is that there is a higher surface ocean loss rate for dFe when dFe concentrations are >> Fe_{min}? The mechanism invoked is photodissociation of ligands, except when the dFe concentrations are low enough to induce siderophore production. I assume this increased loss of dFe in high Fe waters is necessary for the model to match the observations. This rationale would suggest higher concentrations of ligands should be present where phytoplankton are Fe limited (for example in the Southern Ocean). I am not sure that the ligand data support this. I am not saying this parameterization is incorrect, but I feel it should be noted that there is more uncertainty on these processes that might be assumed from their description.

P7533, lines 11-12: just give the right sedimentary Fe flux initially and then this phrase is redundant.

P7533, line 27: You state here that there is no diurnal cycle, but previously you state that phytoplankton adapt to the light level over the past day (I_{mem}). Please address the inconsistency.

P7534, lines 1-3: Why not use the model derived chlorophyll for some consistency?

P7535, lines 1-7: See previous comments on the length of simulations. A table detailing the experiments would also be nice.

P7535, lines 13-14: I understand why you don't compare to NO_3, but why this complicated 'average macronutrient'? If the model simulates PO_4, then compare to PO_4.

P7535, lines 14-17. Could this high correlation coefficient not be weighted by a good fit at the high PO_4 concentrations? One could argue that getting PO_4 right matters more at the low concentration end of the concentration scale. Please comment on this.

P7535, lines 22-27: State if this is the correlation between monthly chl from seawifs and BLING or the annual mean.

P7535, line 28 and onwards: Simulated dFe should be compared statistically to the database of Moore and Braucher (2008). You can extract the modeled Fe at the same latitude, longitude, depth and month as the observations. We have the tools to be much more quantitative in how modeled dFe is evaluated against observations. This is a much more persuasive way of showing that BLING can reproduce the observed dFe than comparisons 'by eye', particularly since the whole paper is about Fe! The BLING correlations can then be compared to those reported for other OGCBMs. If you retain the discussion of the A16N track, then please provide the geographic location for this.

P7536, line 25: How is this 'time varying' correlation calculated?

P7538, lines 20-22, is this PO_4 or your 'average macronutrient'. Is the reduction due to greater uptake? In what season is this departure from observations occurring? This would help diagnose whether it is due to uptake by the biota or some sinking/remineralization aspect.

P7540, lines 27-29: Why is this a paradox? In addition, see my General Comments on the vertical aspect of these questions. The authors correctly note the importance of horizontal transport in understanding their results, but I found the lack of depth dependant discussion to be disappointing. Given the attenuation of light with depth (this is why I feel model chlorophyll should be used for the attenuation coefficient), one would imagine this would impact upon the processes of interest and could play some role in understanding the response. Also, how do your results regarding the dominant processes in the cold Southern Ocean square with the observations described in the introduction (P7521, lines 12-22)?

P7541, lines 18-20: What about coastal waters where waters are cold, but Fe is relatively high? One example would be the Ross Sea on the Antarctic continental shelf.

P7542, lines 5-7: See my previous comments on discussing what the advantages/disadvantages of BLING are, relative to OGCBMs. The time scale of simulations presented here are much shorter than complex OGCBMs, even if BLING can be run ‘cheaply’.

References Cited:

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