

We thank Dr. Tagliabue for a thorough and thoughtful review. In response to many of his detailed comments, we have modified the manuscript. In addition, we address each of his comments below.

### **General Comments**

**What the model misses:** Dr. Tagliabue suggests providing the reasoning behind the use of more complex OBGCMs. We've added the following sentences to the beginning of Section 2 in accordance with his suggestion:

“These simplifications provide multiple performance advantages, including more transparent behaviour, simpler code manipulation, and a reduction in the number of parameters that must be prescribed. However, they also present some obvious disadvantages compared to more complicated models. Foremost among these is the lack of explicit organic particles, so that only inorganic components are subject to advection and mixing by the ocean circulation, which could bias results in frontal regions at high resolution. In addition, with only four tracers, we cannot resolve the rich behaviour of the nitrogen cycle with its interactions with iron (Moore & Doney, 2007), phosphorus (Tyrell, 1999) and oxygen (Schmittner et al., 2007). Nor can we explicitly resolve changes in phytoplankton types (Bopp et al., 2005), variable stoichiometry in phytoplankton (Klausmeier et al., 2004), or zooplankton dynamics. However, insofar as this model is able to simulate large-scale biogeochemical cycling with good skill, it suggests that additional complexity does not necessarily provide additional predictive ability, in terms of simulating the large-scale distributions of nutrients and chlorophyll.”

**Iron limitation in previous models:** Dr. Tagliabue points out that other current models also utilize multiple limitation mechanisms. However, in our reading of the supplementary material for Aumont and Bopp (GBC, 2006) it was not apparent that this generation of the PISCES model included multiple mechanisms for iron limitation—particularly, in modifying the coefficients  $a$  and  $q_{chl,max}$  (although the PISCES iron uptake rate depends on light, notably – an effect we have not explored with BLING). Similarly, regarding the BEC model, Moore et al., (2002) use a constant  $\Theta_{N\_max}$  and  $\alpha_{chl}$ . We were unable to find evidence that this was changed in Moore and Braucher (2008). We would be very happy to alter the manuscript if either of these assessments is in error.

**Model cost:** Dr. Tagliabue expresses some confusion regarding the cost of our ‘cheap’ model, due to insufficient detail in the original manuscript. The cost of BLING is, indeed, relatively inexpensive compared to a more complex ecosystem model running *within the same physical model*, due to the reduced number of tracers. However, the overall runtime still requires integration of the physical model itself. Essentially, the use of BLING only increases the total integration cost of our ocean model by ~10%, rather than the >50% typical of a complex OBGCM with >25 tracers. This does not help a great deal in terms of conducting long simulations, since the use of BLING provides only a 30% discount in total computational cost relative to a complex OBGCM. However, it is more relevant for determining whether or not to include biogeochemistry at all in general experiments, or for running multiple instances within the same physical model (an ability which is

currently under development). We have modified the text slightly to better convey this nuance.

**Length of simulation:** Dr. Tagliabue points out that our simulations are relatively short, in comparison to the hundreds or thousands of years often required for ESMs. This is correct, especially in that 100 years is of the same time as the iron equilibration time scale. We have now extended the simulation out to 800 years for the control and 400 years for the perturbations (tripling the total years run from 1200 to 3600) and find that the results for our metrics are essentially unchanged. For example, comparing the correlation coefficient for the time varying nutrient we find

At year 500: [0.74 0.85 0.85 0.90 0.94 0.93 0.93 0.93]

At year 800: [0.73 0.84 0.84 0.90 0.93 0.93 0.93 0.93]

We hope that this tripling of the number of years run addresses Dr. Tagliabue's concerns, as none of the results appear likely to change substantially with more integration. Table 2 has been updated to reflect the results of the longer simulations, as have the Figures.

As an aside, it is worth considering why we are able to get away with this. Part of why our model doesn't need to run for very long is that we are not looking at the carbon system, where the equilibration of the deep ocean can lead to trends over many years. An expanded version of BLING, including carbon, shows that the nutrient cycle approaches equilibrium in terms of the metrics discussed here (mostly surface-ocean) much more rapidly than does the carbon cycle. Another issue with regard to more complex models is that we don't have feedbacks between the oxygen and nitrogen cycles, which also introduces a long timescale into the system.

**Vertical structure:** This is a good point – changes in the vertical structure are, indeed, among the most interesting effects of iron limitation. We had been planning to avoid treating this issue here, but for completeness, have added a figure and some discussion about the vertical structure of production to the revised manuscript. There is also an interesting story regarding the interaction of iron with the deep chlorophyll maximum, which we are preparing as a separate paper of its own (we find four separate oceanic regimes, in terms of response).

**Parameter choice:** Of course, the results depend on the parameter choice, to some degree. We agree that an additional comment is warranted here, which we will include in the revised manuscript, clarifying that the relative effect of the Liebig term vs. the photosynthetic efficiency terms will depend on the relative choices of  $P_m^c$  vs. alpha and thetamax. However, given that our model is very simple, it really does not seem suitable for more than pointing out the general principles. More complex physiological models with multiple plankton types would be useful to evaluate the principles more quantitatively.

**Specific comments:**

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

Agreed. We have added text to clarify that we are referring to HNLC regions.

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

Units are now given here.

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

As discussed above, we were unable to find evidence that this is untrue, though we would be very happy to be corrected.

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

Agreed, we now clearly state that the model has 4 tracers (originally, we had planned to make oxygen optional).

**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.*

We have added a new paragraph here, as suggested, and detailed above.

**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 Huntsman (1997).*

No, we did not include the effect of increased Fe:P uptake under low light, as we considered it an unnecessary complication. We will consider it for future versions of the model. In the meantime, this has been clarified in the text.

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

We highlight the functionalities of alpha and theta as follows: "Note that we chose linear dependencies in order to minimize complexity, rather than based on first principles, and that both  $\alpha^{chl}$  and  $\theta_{max}^{Fe}$  are restricted to vary between prescribed maxima and minima."

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

We have altered the notation as suggested.

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

We have added the following: " $P^*$  is a scaling term determining the pivotal phytoplankton concentration at which a size class becomes the dominant food source."

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

Corrected.

**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.*

Indeed, this seems like a great approach, and we hope to consider its implementation in future versions of BLING.

**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?*

The sediment flux given on p7531 is, indeed, the flux used. We simply wished to highlight, later, the fact that we 'tuned' this parameter significantly. The text has been modified to clarify this.

**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  $F_{min}$ . As such, the net result of this parameterization is that there is a higher surface ocean loss rate for dFe when dFe concentrations are  $\gg F_{min}$ ? 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.*

We agree that the iron cycling is highly uncertain, and have added text in order to emphasize this fact. We would also point out that other trace elements known to limit growth are entirely absent from all biogeochemical models, despite the fact that they show different spatial patterns and are undoubtedly important (Zn, Co – e.g. see work by Saito). Improving these deficiencies will be a long, but important undertaking.

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

See above.

**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.*

The  $I_{mem}$  allows the model to be run with a diurnal cycle. However, the runs we show here do not make use of this fact, instead using an averaged daily insolation.

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

We chose to not use interactive chlorophyll here in order to maintain the same physical ocean circulation across all runs and improve comparability of the results. However, we have carried out these experiments and plan to present them in future, as part of the discussion of changes in the vertical dimension.

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

See comment above regarding simulation length. The experiments are detailed in Table 2.

**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  $PO_4$ .*

The fact that we call our macronutrient ' $PO_4$ ' doesn't really make it phosphate –instead, we would argue that it behaves something like a hybrid between nitrate and phosphate. This is because it limits growth, commonly the role of nitrate, but does not undergo denitrification and  $N_2$  fixation, like phosphate. We therefore feel that it remains more appropriate to compare the simulation to the average of nitrate and phosphate, adjusted by the redfield ratio. We have adjusted the text to make this reasoning clearer.

**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.*

As far as we can tell, the high correlation is driven by the fact that low values occur in the same places as observed, as well as high values.

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

The regression and correlation coefficients have been corrected (they were initially incorrect, due to a script error) to 0.67 and 0.70, respectively. Not so surprisingly high. They are both for the annual averages.

**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.*  
These correlations have been calculated, and are now given in the text: “Correlations of log(Fe) with the data compilation of Moore and Braucher (2008) give a value of 0.52 globally, 0.58 in the top 1000m and 0.60 in the top 100m..”

**P7536, line 25:** *How is this ‘time varying’ correlation calculated?*

These are the correlations with the zonally-averaged, monthly climatologies of productivity and export.

**P7538, lines 20-22,** *is this PO<sub>4</sub> 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.*

As stated in the figure caption, the comparison was made to the average macronutrient. There is no clear seasonal change in the degree of departure from observations, except at very high latitudes where the wintertime (July) concentrations are much higher than observed. However, given the confounding influences of circulation errors, we’re not sure what this means about the biogeochemistry.

**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)?*

Perhaps ‘paradox’ was the wrong word. We have changed this to read,  
“This leads to the counterintuitive result that when iron is most scarce, its effect on photosynthetic efficiency has the least impact on biogeochemistry.”

Note that this is the effect of the photosynthetic efficiency only. The effect of the overall growth limitation, through the decrease in the maximum light-saturated growth rate, is large where iron is strongly limiting.

**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.*  
In the model, the Bering Sea provides the best example. There, it can be seen to have an effect on the light limitation, but the effect on total uptake-weighted growth rates remains small due to the low overall  $P_m^C$ .

**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'.*  
Addressed above.