We thank Dr. Aumont for a very insightful review that complements the suggestions provided by the first review. Again, we have altered the manuscript to add further information, in accordance with his comments, and provide additional response to the comments here.

General comment 1: *Disentangling the impact of iron limitation on iron concentration from the direct impact on growth.*

Dr. Aumont has a point in that there are essentially two intertwined avenues of iron limitation – through the direct impact on local growth rate, and the knock-on effect through modification of the iron cycle. The experimental design he suggests, using the same climatological iron field for all experiments, would certainly provide additional information by isolating the first avenue. However, one of the purposes of this study was to approximate the behaviour of models that do not include one or more of the iron limitation effects – and these models would naturally include the resulting modification of the simulated iron fields. Therefore, we feel that simulations including prognostic iron redistributions are the most relevant for the task laid out here.

Nonetheless, we completely agree that it is helpful to see the changes in the iron simulation across the experiments – we had initially omitted this figure, in the interests of brevity, but have included it in the revised manuscript.

We note also that Dr. Aumont's concern that dissolved Fe concentrations could become negative is unnecessary, since equation 4 ensures that the uptake of Fe approaches zero as the concentration of Fe approaches zero.

General comment 2: As stated in the response to Dr. Tagliabue, we have added a figure and text to the revision. Please see the response to Dr. Tagliabue for more details.

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.*
Actually, in Geider et al., (1997) this coefficient can be interpreted in a number of different ways. It is introduced as a coefficient governing the ratio between chlorophyll synthesis and carbon uptake which Geider et al. take to be proportional to the ratio between the realized growth rate (limited by light and nutrients together) and light-limited growth (i.e. the light-harvesting capacity). At this point in the derivation one can distinguish between nutrient-limited growth and chlorophyll synthesis- which is what we do. Solving the resulting equation for θ and letting Irr go to zero the result is $\theta = \theta_{max}$ (though note that nutrients need not be replete to get this result, they simply must be much less limiting than light). We have clarified this distinction in the revision.

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).*

We agree that further analysis of the model is warranted, before using it with a daily cycle. In particular, the smoothing timescale of biomass should be investigated. However, although we agree the paper by Flynn and Fasham is interesting, it is primarily concerned with resolving the N cycle and food web dynamics, neither of which are resolved in the model presented here. As a result, we continue to hope that BLING will be useful for diurnal simulations.

Page 7523, line 1-13: *Formulation is equivalent to Monod with altered constant.*
True. We point this out in the revised manuscript.

Page 7531, lines 10-15: *The sediment flux of iron is small here.*
We did not mean to indicate that the flux was further reduced, beyond the ~10 Gmol Fe/year stated – that is, in fact, the approximate flux used. It is roughly one third of the Moore and Braucher (2008) flux, commensurate with the smaller dust input that we use. The use of these relatively small fluxes means that we use a weaker scavenging rate constant, in order to balance the input fluxes. Therefore, our iron cycle has a longer residence time than does the Moore and Braucher model, leading to a greater decoupling between input regions and iron concentrations. This makes the model less dependent on inaccuracies in the source input distribution, although it reduces the dynamic range of iron concentrations.

Page 7531, equation 14: *This equation looks like it is 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.*
We cannot disagree with the apparent provenance of this equation. We have added text to the manuscript, in order to show the reasoning behind the equation, as well as to emphasize the uncertainty in the parameterization. This now reads,

"The latter term in eq. 14 reduces photodissociation when iron concentrations Fe approach Fe_{min}, an effect similar to the formation of siderophores (strong ligands) by microbes under iron stress (Trick et al. 1983, Granger and Price, 1999). This results in a more rapid removal of iron from the surface ocean when iron concentrations are significantly in excess of Fe_{min}, improving the simulation, a result that Moore and Braucher (2008) achieve instead by increasing the scavenging rate constant by roughly a factor of 6 in the upper ocean. We emphasize that this is an *ad hoc* approach, as currently required by the incomplete understanding of environmental controls on the speciation of iron, and hope that it will be improved in future generations of the model as more information becomes available."

Page 7532, eq 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?*
Again, this is an incomplete solution to a difficult problem. We have changed the text to read,

"Because the underlying processes are poorly understood, we use a globally uniform rate constant, and increase the order of the iron concentration dependence to 1.5 in order to represent the nonlinear increase of colloid formation where iron concentrations are high. In practical terms, the latter acts to prevent dissolved iron concentrations from growing very large in regions with high inputs, such as the beneath the Saharan dust plume. Again, improving these parameterizations is a clear target for future work."

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?*
The correlation coefficient given was, indeed, really high. Unfortunately, it turns out this was due to a scripting error in the order of operations. This has been corrected, to give a much less surprising correlation of 0.70 for the log mean chlorophyll concentration, and a regression coefficient of 0.67. We apologize for the prior error.

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)?*
We have calculated the correlations of log(Fe) with the data of Moore and Braucher. They give a value of 0.52 globally, 0.58 in the top 1000m and 0.60 in the top 100m.

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.*
As explained in the response to Dr. Tagliabue, the tracer is simply the averaged NO_3 and PO_4 concentration after adjustment for the Redfield ratio of 16. We continue to feel that this is the most appropriate comparison, given the nature of our 'PO_4' tracer is intermediate between real-world nitrate and phosphate. The text has been changed to read

"Note that, although we refer to our limiting macronutrient as 'PO_4' (since we do not model denitrification and nitrogen fixation) NO_3 tends to limit growth in the ocean, and therefore our 'PO_4' is actually more like NO_3 in this respect. We therefore compare our modeled PO_4 to an "average macronutrient", for which NO_3 concentrations are scaled by the Redfield N:P ratio and averaged with PO_4, i.e. $(PO_4 + NO_3 / 16) / 2$."

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.*
Before responding to this comment we would have agreed with it- after all one expects low seasonal cycling within the gyre where nutrients are low and higher seasonal cycling in the subpolar gyres where it is high. But in fact there's a fair degree of variation of nutrients within high nutrient regions which isn't well-correlated to nutrient concentration For example Taro Takahashi has pointed out that nutrient uptake rarely exceeds 1 micromole/kg over the course of a season and seasonal cycling is relatively weak in some

high-nutrient upwelling zones. As a result the correlation for surface nitrate in the 2001 WOA with its standard deviation is only 0.50. For phosphate the correlation is a bit higher, 0.58, comparable with what we see here. So in fact, the mean concentration explains *less than half* of the observed variance. Now of course some of this is likely sampling error since the WOA01 is extrapolated- but we are ourselves surprised by how poor the correlation is- and how relatively well the model therefore does. Our results actually suggest that a major part of this is the impact of iron, ALLMEAN shows a much higher correlation between nutrients and their standard deviation (0.71) than does ALLVAR (0.44). Results are similar if one looks at the range.

Page 7536, line 14: *very similar seems a little bit too strong.*
We have removed the 'very'.

Page 7536, line 18: *The low DefFe in the equatorial Pacific is located south of the equator, not right at the Equator according to what I can see on the figure.*
We have changed this to the 'most of the tropical Pacific'.

Figure 6: The equation number has been corrected.