Reply to Dr. Alessandro Tagliabue (Referee #2):

Thank you for your comprehensive review and thoughtful comments. We have revised a large part of our manuscript. Furthermore, we performed an additional analysis and added a discussion regarding the mechanisms responsible for the elevated advective iron transport (see sections 3.4 and 4.2; Figures 12–15). We also added a separate discussion about other factors that change iron cycling in the future ocean (see section 4.4). We think these modifications made our manuscript much better than the original version. Detailed responses to your comments are given below.

Please note that the color scales of the iron budget analysis were changed from log-scale to linear in the revised manuscript to make it easier to compare the results quantitatively.

Reply to General comments

Mechanisms of Change

To my mind there are three mechanisms by which physical iron supply might increase in HNLC regions: i) the direct effect of physical changes (i.e., with little change in the iron field), ii) 'downstream' effects whereby lateral fluxes of iron increase due to reduced iron consumption in adjacent macronutrient limited regions and iii) local changes in the vertical profile of iron over time. Of course, these three mechanisms can interact with each other (and probably do). At present there is little attribution to governing processes beyond the different physical terms themselves, but within these terms different processes can be dominant. Casting the modeled changes within the above framework would help the reader enormously in understanding what processes and mechanisms are at work. For example, comparing the change in physical fluxes of iron with those of macronutrients, or inspecting how vertical profiles have changed, etc might help isolate these three terms.

We consider that the three effects you mentioned can be condensed into two, because effects ii) and iii) are basically the same; that is, they are both driven by changes in the dissolved iron distribution although their directions (horizontal or vertical) are different. Equation (8), presented in the original manuscript, is a good metric for separating the direct effect (mechanism i) from the indirect effect (mechanisms ii and iii), but our previous presentation and analysis did not sufficiently explain these mechanisms. Therefore, we did an additional analysis based on eq. (8). We added a separate section showing the results (section 3.4; Figs. 12–15) as well as a discussion as to why the "downstream" effect was so small in our simulation. We think this approach is a more direct and quantitative way of investigating the mechanisms, rather than investigating macronutrient fluxes and vertical profiles.

Vertical Profile of Iron

There is no discussion whatsoever of the role of changes in the iron distribution in regulating the physical transport of iron in and out of HNLC regions. The paper expends a lot of energy discussing the physical changes, but to my mind there is superficial description/discussion of the changes to the iron field. As the authors demonstrate in their equations, the change in the iron field itself, especially the vertical profile cannot be neglected in the understanding of how the physical fluxes of iron change with time.

The effect of changes in the vertical iron distribution is described in section 3.4.1 and Fig. 13 in the revised manuscript. The result clearly shows that the development of a vertical concentration gradient intensifies vertical iron advection in the eastern equatorial Pacific. We did not show changes in the vertical profile in the Southern Ocean because the change in the horizontal component is dominant there (Fig. 7f).

Regional Decomposition

I would agree with reviewer #1 that care should be taken in deploying a relatively coarse resolution model into regions where small scale features (either associated with circulation or topography) are important. I would propose to shorten these parts of the MS to concentrate on the main points without delving too deeply into specific regions. If the authors wish to retain this section 'as is' then more effort must be made to demonstrate that the model is doing a good job in reproducing local physics and iron dynamics.

In the revised manuscript, we concentrate on changes in gyre-scale circulations that are relatively robust in the projected changes, and we discuss only two regions: the Southern Ocean as a whole and the Equatorial Pacific.

Iron Distribution

The fidelity of the model field is relatively poor, with a low R2 of 0.31. While things indeed look better when the 'basin averages' are presented I would like more detail to be attached to this. For example, how were the basins delineated? And how were the averages calculated? Did the authors extract the model field at identical locations as the data and then average, or was the model 'basin averaged' and then this compared to the data average? IN any case, the authors must acknowledge that the model is not performing very well in terms of its iron distribution. A surface plot of the biases could help in understanding/appraising when and how these errors might impinge upon the conclusions of the study.

We now show the regions used for calculating basin averages (Fig. 4a), and we explain the method used to calculate the averages in the figure caption. We also added a map showing the spatial distribution of the biases of the simulated dissolved iron concentrations (Fig. 4b). See also L. 290- L. 304, in the revised manuscript.

Caveats

The authors concentrate on the role of ocean physics, while in reality it is also likely that climate change will alter a number of aspects of the iron cycle, which will modify iron distributions and hence the effect of changes in ocean circulation on the physical fluxes of iron. These are not discussed. For example, does a warmer ocean imply greater rates of iron recycling and modifications of how it is distributed with depth? How does the change in particle production with climate change impact the scavenging of iron? Are there likely to be shifts in community structure that will modify recycling rates of iron? These would be better mentioned in the end of the MS in place of the space given to discussing regional specific of the west Pacific region perhaps?

Changes in iron recycling rates, particle production, and community structure are considered in the model (we added some explanations to the methodology section, L. 174-197, and the results section, section 3.4), but they had only a minor impact on the simulated results. We added a separate section discussing other factors that can contribute to future ocean iron cycle changes in section 4.4 of the revised manuscript.

Reply to Specific Comments

P8507, L21-26: What about chemistry, we know that ligands play an important role in regulating the residence time, and hence 'transport potential' of iron from a point source.

We added a description of the role played by ligands (see L. 61-69).

P8508, L5-6: Why only mention dust here when you have already introduced a number of other exogenous sources?

We changed "dust" to "exogenous sources" (see L. 77).

P8508, L10: see also Boyd et al., (2012).

We added a citation to this reference here (see L. 90), and we have compared our results with their estimate (see section 4.1).

P8508: I think the text could be streamlined a bit here by dividing this long paragraph into the different HNLC regions rather than dealing with them all 'en masse'.

We separated the paragraph (see L. 76-96).

P8509, L25: what do you mean by 'surface waters'?

We changed all occurrences of "HNLC surface waters" to "HNLC waters".

P8509, L28: what are these 'previous estimations'? They are few and far between aren't they?

We eliminated this sentence and the discussion related to this topic.

P8510, L3: I presume you mean that productivity will be enhanced 'in HNLC areas'?

That is correct. We added "in HNLC waters" (see L. 121).

P8511, L29: Does this imply even atmospheric fluxes are annual mean and have no seasonal variation?

No it does not. There is a seasonal cycle. We changed the description to reflect this fact.

"BEC uses the monthly mean climatology of aeolian dust and the annual mean climatology of the sedimentary iron flux. We assumed that these fluxes did not change throughout the simulation period (from 1850 to 2100)."

to

"The BEC model uses the monthly mean climatology of aeolian dust and the annual mean climatology of the sedimentary iron flux. We assumed that these fluxes remained constant during the simulation period (from 1850 to 2100).". (see L.169-173)

P8512, as also mentioned by reviewer #1, some more details of the iron cycle should be mentioned here, how is recycling treated? Sinking of scavenged iron? Zooplankton recycling?

We added appropriate descriptions of to the methods section. (see L. 174-197). Detailed analysis of these recycling pathways is beyond the scope of this paper.

Sec 3.2 Decomposing the budget terms is actually not this straightforward is it? For Dust vs PHYS it is simple, but much of the sediment supplied iron is actually present in the PHYS term, which also includes regenerated iron. Thus in comparing FRC (which is dust + sediments) with PHYS (regenerated + transported sediment iron) you are actually counting some iron twice.

When the *PHYS* term receives iron from sediment (from *FRC*) or from regeneration (from *BGC*), the *PHYS* values are negative (divergence of physical flux), and they are positive when physical processes deliver iron (convergence of physical flux). Thus, integration of *PHYS* values in the upper 100 m excludes the iron supply from the *FRC* and *BGC* terms and yields the net iron supply due only to ocean dynamics. We noted this point in L. 342-349. If we double-counted some iron, then the sum of the terms (*TEND*) would not be so small. We double-checked our result by comparing the *TEND* values directly output by the model with the sum of the individual terms (*PHYS* + *BGC* + *FRC*) and found that they were exactly the same.

P8519, L8-onwards, Surely the boundaries of these regions (especially the SO) as set by ocean fronts will have moved by the 2090s? Also why was 60S chosen for the boundary between the high and low SO? Why not have a dynamic boundary set by the modeled Polar Front and sub-Tropical Front?

As we described in the original manuscript, we defined regions mainly as the simulated iron-limited area for diatoms in the 1990s (Fig. 5c). We used latitude (30S) only as an additional line for separating the iron-limited area of the Equatorial Pacific from that of the Southern Ocean. Note that we do not discuss high versus low latitudes of the Southern Ocean separately in the revised manuscript.

P8524, L4: what are these previous studies to which your iron budget compares favorably with? Do you mean the compilation of estimates of external inputs that are detailed subsequently? This is only one aspect of the 'iron budget'. Estimates of the PHYS and BGC terms are few and far between, even from models.

We eliminated this discussion.

P8526, L5, what about also considering whether your vertical iron profile is constrained?

We eliminated the discussion about the high latitudes of the Southern Ocean, because it concerned a local-scale phenomenon that is hard to discuss using a low-resolution model.

P8527, much discussion of Moore et al. (2013), this paper is not even published yet! This makes these aspects a bit frustrating for the reader. If this MS is to be published beforehand it should work 'stand alone'.

Moore et al. (2013) is now available on-line at the following URL:
http://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-12-00566.1?af=R

P8527, L17-19: how do you define 'iron limited'?

We added a definition to the figure caption (Fig. 5).

P8528, L6, the increase of 13% is actually rather small though isn't it? Is this change specific for iron? How does the physical supply of other tracers change in the HNLC regions? This might be a way to deconvolve the three mechanisms of change in outlined above.

The value is specific for iron. Yes, 13% is small. However, please do not forget that we (and maybe also you and most people) expected the iron supply to decrease in the future because of enhanced stratification. So, although it is only a small increase, this response is opposite to the expected response and thus well worth discussing. The mechanisms causing the change are fully described in sections 3.4 and 4.2 in the revised manuscript.

P8528, L 15: is it really only dust effects that you believe are crucial in order to have a better understanding of the future evolution of the iron cycle? I would imagine this list could be rather longer (see also at end of conclusions).

We added a separate section (section 4.4) to discuss other factors in the revised manuscript.

P8529, L5-10: this part was not clear to me

We have added some additional text to clarify this section (revised ms lines 590-597).

P8529, L11 onwards, why do you choose to home in on such a specific region here? If you wish to *make this kind of analysis then it should be extended to all the other HNLC regions (and probably other CMIP5 models) and the comparison of the model's ability to reproduce observations should be conducted quantitatively.*

We eliminated this figure. Model intercomparisons are outside the scope of this paper.

P8530, L8, what is 'reasonably well'?

We eliminated this phrase here.