Reply to Referee #1:

Thank you for your review and insightful comments. We have substantially revised the manuscript. In the revised version, we eliminated descriptions for the higher latitudes of the Southern Ocean and subarctic North Pacific, where small-scale responses dominate, and concentrated on describing large-scale responses in the equatorial Pacific and Southern Ocean. Other reviewers advised us to investigate mechanisms that intensified advective iron transport, so we did an additional analysis, which we describe in sections 3.4 and 4.2. As a result, the overall length of the manuscript is a little bit longer than the previous version. We believe, however, that the revised version is simpler than the previous version because it concentrates more on specific issues. 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.

I am not a modeller so I will not attempt to evaluate the details of the modelling results reported here, although I note the modelling system appears to be widely used and reasonably well validated. I was surprised by the scale of flow changes projected to occur over a 100 year time scale (p8516) which seem very large, and the comparison of modelled and observed Fe concentrations (Fig 4) are not particularly convincing. The ocean Fe distribution depends of course on inputs and circulation discussed here, but also and removal rates which depend on the Fe complexation and scavenging modelling which is not I think described here.

The model skill in simulating dissolved iron concentrations is still relatively poor compared with that of simulating macronutrients (which also have much better observational climatologies). We added a new figure showing the spatial distribution of the concentration biases (Fig. 4b). We think the biases are relatively small in HNLC waters. We also added a short description of how we calculated iron removal processes to the methodology section (see L. 174-197).

Despite these comments I think the authors explore an interesting idea of whether circulation changes will alter Fe cycling. As they note both circulation and Fe supply will change over coming decades as discussed by Mark Moore et al in their recent Nature Geosciences paper. That paper did not consider the subtler question considered here of whether the circulation changes can alter the internal cycling and supply of Fe. In that sense I was not sure that the comparison of physical supply and FRC (p8518) is particularly useful because this is a bit like comparing new and net production, *although of course for the phytoplankton it is simply an issue of Fe supply.*

We think that comparing the *PHYS* and *FRC* terms is useful because it indicates which regions are more susceptible to changes in ocean dynamics.

The overall conclusion section of the paper I think elegantly make the main points from this paper, that a general warming and increase of stratification, need not lead to lower supply of Fe from deep water to the euphotic zone everywhere, and indeed in some of the critical HNLC regions Fe supply from below could be enhanced and so also primary production. However, this conclusion is extended within this paper to a detailed discussion of how circulation may change around the Antarctic and Indonesia and with it Fe supply. My feeling is that this kind of large scale model is not well suited to these detailed local scale changes in circulation, and indeed the authors explicitly note the problems they have simulating the west Pacific region. Furthermore in the southern part of the Southern Ocean (in their terminology) we do know that local circulation and local topography play a key role in Fe supply from the blooms around the peninsula and South Georgia for example, so the argument that most of the Fe supply comes from below does not seem to accord with the field data. The authors basically seem to acknowledge this in their conclusions where they say that the physics in these models need to be better, although I wonder if the best strategy might not be to use more detailed regional models, rather than global ones, but that is a discussion for the modelling community.

We eliminated the discussion about the southern part of the Southern Ocean because a local-scale response was mainly responsible for that change. For the equatorial Pacific and the Southern Ocean as a whole, we think that our model simulated iron transport processes reasonably well in comparison with previous observational and modeling studies (see section 4.1). Using regional models with higher resolutions will make circulation fields better for present-climate simulations, but preparing reasonable boundary conditions (for both the surface and lateral boundaries) for global warming experiments will be difficult. Currently it is not computationally feasible to use global, high-resolution models for climate change simulations. The introduction of a flexible mesh-refinement technique to the CESM framework is planned (e.g., Ringler et al., 2013). This technique may enable us to overcome this problem. Climate projections in IPCC AR5 that include ocean biogeochemistry are conducted with low-resolution global models, and investigating the ocean iron cycle in such models is meaningful. Our analysis revealed the mechanisms responsible for the different model predictions of marine productivity under the future climate shown by Steinacher et al. (2010) (see section 4.3).

However, I would say that if the main points of the paper are as summarised in the conclusions, and if the authors hope to reach a wide audience, then the paper could perhaps be reduced in size and simplified (perhaps with details in supplementary material) and try in some way to help the reader through the mass of acronyms (MIXn and MIXv etc.) which really began to confuse me. This simplification might encourage a wide readership from beyond the modelling community to consider the potentially rather important conclusions within this paper.

We combined *MIXh*, *MIXv* and *MIXn* into a single *MIX* term, which hopefully simplifies things and makes the manuscript much easier to understand.

Reference

Ringler, T., Petersen, M., Higdon, R. L., Jacobsen, D., Jones, P. W., and Maltrud, M.: A multi-resolution approach to global ocean modeling, Ocean Modelling, 69, 211-232, 2013.