Reply to Referee #3:

Thank you for your constructive comments. We have revised a large part of our manuscript. We did an additional analysis and added a discussion regarding the mechanisms responsible for the elevated advective iron transport (see sections 3.4 and 4.1; Figures 12-15). We think the modifications make our manuscript much better than the original version. Detailed responses to your comments are given below.

Please note that 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 Overall Evaluation

This paper provides a diagnostic of the iron budget in the ocean surface waters for present day and for 2100 under a RCP8.5 scenario from the CESM v1. The authors diagnose the model flux results to show that in the current day, 70% of the iron supplied in iron-limited regions (HNLC) comes from physical processes, though the specific mechanism differs between different regions of the ocean. At the end of the 21st century the authors find that the model suggest an increase in physically delivered iron to the HNLC regions, which helps increase primary production in these areas. Though this latter point is worth publishing, I do not feel that the authors have fully separated the mechanisms in control of these changes. In contrast to iron, nitrate is reduced everywhere in the surface oceans in the modelled 2100. But the contrast between the two nutrients (and their source and sinks, and where they are limiting) are not developed. The authors suggest that changes to Southern Ocean overturning and the "gyre scale circulations" provide more iron to the Southern Ocean HNLC region. And yet the same mechanism could potentially also supply more nitrate - but it does not. Differences in the source and sinks and where they are limiting, and thus the changes in their distributions will make them respond differently. The authors in passing mention issues of upstream changes to the nutrients and biomass (e.g. even in the conclusions): but I think more attention needs to made to these issues. Changes in upstream productivity and impact on limiting and non-limiting nutrients must play a large role in resetting iron gradients and allowing higher supply. Thus though the authors have done a very detailed job at diagnosing the fluxes, I do not feel they have done a complete job of understanding what has caused these flux changes. In the equatorial Pacific they make a fuller argument about changes in sediment supply. However the marked east/west gradient in primary production changes begs the question of how much of this causes the increased flux in iron.

In the end is it correct to say that the circulation changes have cause the changes in iron supply, or

is that the physics changes are just transferring changes in the iron gradients. I believe that the authors diagnostics tell part of this question - but it is not clear in this current version of the paper how much you have interrogated these diagnostics from this point of view. For instance pg 8522, lInes 7-10 the diagnostic suggests that "intensified mass flux" is responsible for the increase iron supply. But why not nitrate as well? Possibly an investigation of the changes to the nutrients in the upstream regions will offer insight. Many of the inferences in the paper are couched in terms of "probably", "must" - surely with all the diagnostics the authors should be able to be more definitive.

We acknowledge that the previous manuscript did not fully describe the mechanisms responsible for the intensification of advective iron transport. We did an additional analysis to investigate these mechanisms (see sections 3.4 and 4.2; Figures 12-15). We utilized equation (8) to investigate the mechanisms rather than the macronutrient budget. The results clearly show that the direct effect of a circulation change is dominant over the "upstream-downstream" effect.

Reply to Individual Science Questions and issues:

1) I would suggest contrasting changes to the different budget terms in the nitrogen (or phosphorus) budget to start to pull apart some of the impact of upstream biological changes and upstream physical changes. 2) The rather intensive details of the flux separation analysis is a bit difficult to get through. This is especially true of the high latitude Southern Ocean. Though it is commendable that the authors have understood the different terms at play (though see 1 above), I feel that this is potentially wasted detail: I would have little confidence in the model in this region (no iron supply from ice, probably poor representation of circulation). And I'm not sure that the final understanding is useful. 3) In several places (e.g. pg 8509, lines 12-16) the authors mention the enhanced "meridional overturning circulation". It would be good to specify that this is Southern Ocean overturning – Atlantic overturning is projected to decrease instead.

1) See our response given above.
2) We eliminated the discussion about the high latitudes of the Southern Ocean and combined *MIXh*, *MIXv*, and *MIXn* into a single *MIX* term.
3) We eliminated discussions about meridional overturning circulation.

Reply to Technical Corrections:

1) Misumi 2013b is referenced in the text, but absent in the reference list.

We have corrected this mistake.

2) *For non-modellers it would be good to define "non-local convective mixing". Also maybe better to use a non-technical term when discussing it.*

We use “surface boundary-layer mixing” instead “non-local convective mixing” in the revised version.

3) *Figures 8 and 9: the vectors are very hard to see*

We reduced the number of the panels and enlarged them. We also put white borders around each vector to make them easier to see.

4) *Figure 11: confusing with the switching from horizontal and vertical terms between panels.*

This figure was eliminated.