Biogeosciences Discuss., 10, C3612–C3614, 2013
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Interactive comment on "The iron budget in ocean surface waters in the 20th and 21st centuries: projections by the Community Earth System Model version 1" *by* K. Misumi et al.

Anonymous Referee #3

Received and published: 19 July 2013

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 every-

where 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.

Individual Science Questions and issues:

1) I would suggest contrasting changes to the different budget terms in the nitrogen

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(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.

Technical Corrections:

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

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.

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

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

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