Review of Misumi et al. "The iron budget in ocean surface waters in the 20th and 21st centuries: projections by the Community Earth System Model version 1"

1. Overview

In this manuscript ('MS') Misumi et al. conduct an investigation into the present day and end of century iron budget as simulated by the CESMv1 model. The authors find that despite our canonical idea of climate induced increases in stratification retarding nutrient fluxes to the surface ocean, physically mediated iron fluxes actually increase in the (modeled) HNLC regions, thus contributing to increases in primary production. The topic fits well within the scope of biogeosciences and while I feel there are a number of caveats and unexplored aspects of the results, the general conclusion is well articulated and highlights an important question. I therefore recommend the MS be accepted subject to a revision that responds to the following questions and concerns.

2. 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.

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.

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.

Iron Distribution
The fidelity of the model field is relatively poor, with a low R^2 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.

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?

3. 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.

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

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

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'.

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

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

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

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

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?

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.

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?

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.

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

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'.

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

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.

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).

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

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.

P8530, L8, what is ‘reasonably well’?

References

Boyd, P. W., K. R. Arrigo, R. Strzepek, and G. L. van Dijken (2012), Mapping phytoplankton iron utilization: Insights into Southern Ocean supply mechanisms, J. Geophys. Res., 117, C06009, doi:10.1029/2011JC007726.