

***Interactive comment on* “Iron fertilization efficiency and the number of past and future regenerations of iron in the ocean” by Benoît Pasquier and Mark Holzer**

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In this manuscript, Pasquier and Holzer present a series of diagnostics to document the “life cycle” of dissolved iron in the ocean. Depending on the total source of iron (from an ensemble of nearly 300 solutions that “equally well” resemble oceanic nutrient distributions) they find that the majority of iron molecules are scavenged permanently from the ocean before they have had a chance to be biologically utilized. Of those that are taken up by phytoplankton, the majority will only have one circuit of the “ferrous wheel” before they too are permanently buried in the sediments.

I thought this was a really interesting paper, that certainly fits the criteria for publication

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in Biogeosciences. I would like to suggest a few points that the authors might consider:

1.) Although fairly well written overall, in places I found the manuscript overly technical. For example, on page 9: lines 14-19 where there are 4 equivalences in as many lines, and only the last one (or two) are relevant. Perhaps there is a way to simplify? Furthermore, I appreciated where the authors had split their prose to identify the “physical” cause or effect and then the “mathematical” proof (page 7: line 20-22). Can this clarity be afforded elsewhere in the manuscript?

2.) Adding to the slightly overwhelming number of symbols used in this manuscript, I did come across something that looks like a mistake, or maybe requires clarification: Figure 1 suggests that “D” is the reversible scavenging process – after the first regeneration, iron is transported to the near-surface where it is scavenged onto a sinking particle and released at depth to be then transported into the euphotic layer and biologically utilized. Similarly, “D~” is used for future reversible scavenging. However, section 2.3 defines “D” as “iron scavenging minus redissolution of scavenged iron” and the “permanent loss of iron due to burial in the sediments” (page 4: lines 6-7), which appears to correspond to “d” in the schematic.

3.) The phrase “Southern Ocean nutrient trapping” is frequently used, and I wondered if the authors could check that all uses are appropriate. For example, page 7: lines 17-19, I think the authors have the correct explanation that hydrothermal iron is added to density classes that upwell in the Southern Ocean, but is this really “nutrient trapping” and not just transport?

4.) Another paper that considered the iron fertilization efficiency was Dutkiewicz et al. (2006; GRL; doi: 10.1029/2005GL024987). Using an adjoint of the MITgcm biogeochemistry model, they found a similar pattern of tropical-Pacific-dominated primary production and carbon uptake when iron is added to the ocean.

5.) Finally, I wonder if the authors could comment on the caveat that their biogeochemical model may not capture the full array of interactions that might lead to en-

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hanced iron regenerations through grazing by zooplankton, or bacteria/virus interactions, for instance. This is in regards to the “ferrous wheel” idea where recycling of iron is considered important (e.g. Kirchman (1996, Nature, doi: 10.1038/383303a0; Maldonado et al., 2005, GBC, doi: 10.1029/2005GB002481; Strzepek et al., 2005, GBC, doi: 10.1029/2005GB002490; Boyd et al., 2017, Nature Geoscience, doi:10.1038/ngeo2876). Maybe these views can be reconciled, with reference to figure 3?

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