

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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Dr. Lauderdale’s (Referee #2) General Comments: 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 in Biogeosciences. I

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would like to suggest a few points that the authors might consider.

Authors’ response: We thank Dr. Lauderdale (Referee #2) for these positive remarks. *No changes to the manuscript in response to the General Comments.*

Referee #2 Minor Point 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?

Authors’ response: We think the equations in this instance are useful and should not be avoided because they define and clarify convenient notation (the χ -weighted global averages). This notation is used later on multiple occasions in both the manuscript and figures and enhances subsequent readability. However, we do agree with Referee #2 that our manuscript need not be overly technical.

In response, we will rephrase the second last sentence of this passage using more succinct expressions to ensure its point is clearly made with a minimum of symbols:

“Note that this fraction can be considered to be the χ_k -weighted global average of the local unused fraction $f_k^{0\downarrow} \equiv \chi_k^{0\downarrow} / \chi_k$. This weighted average is defined as $\langle f_k^{0\downarrow} \rangle_{\chi_k} = \langle f_k^{0\downarrow} \chi_k \rangle / \langle \chi_k \rangle$, where we introduced the $\langle \cdot \rangle_{\chi_k}$ notation, which will be used throughout.”

We will check the revised manuscript everywhere for clarity and employ the “split-prose” approach where appropriate.

Referee #2 Minor Point 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 \mathcal{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.

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Similarly, $\tilde{\mathcal{D}}$ is used for future reversible scavenging. However, section 2.3 defines \mathcal{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.

Authors’ response: We did try to minimize the number of symbols and always introduce new notation only to make the manuscript clearer or to provide precision where we think it is important. However, we agree with Referee #2 that we should have been clearer when introducing the operator \mathcal{D} . (The details of \mathcal{D} are provided in Appendix A.) \mathcal{D} is the linear integral operator that, applied to the DFe concentration field χ , gives the local rate of scavenging minus the local rate with which scavenged iron is redissolved. The net local rate $(-\mathcal{D}\chi)(r)$ in Equation (2) can thus be locally positive (net DFe re-dissolution) but its vertical integral is always negative (a finite fraction is scavenged out of the system). Hence, \mathcal{D} provides both the scavenging-pump transport for DFe (conservative “reversible scavenging”) as well as the permanent DFe sink (non-conservative “death”). The field d is the local iron death rate, which is calculated from $\mathcal{D}\chi$ as detailed in Appendix A and introduced later in Section 4.1. $d(r)$ is the rate at which DFe is removed at r by scavenging *and* instant sediment burial. Thus, d (unlike \mathcal{D}) does not capture the recycling of scavenged DFe.

In response, we will revise the text to clarify the action of \mathcal{D} where it is introduced. For clarity and simplicity, we will also revise Figure 1 slightly by removing the adjoint operator symbols (they come too early for this introductory schematic) and to use the same type of \mathcal{D} -labelled arrow for both mid-stream reversible scavenging and for permanent burial, as \mathcal{D} accomplishes both. We will also label the uptake process in the euphotic zone with \mathcal{L} , so that all physical processes are labelled on the figure.

Referee #2 Minor Point 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?

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Authors’ response: We think that we use “Southern Ocean nutrient trapping” correctly here because we are considering hydrothermal DFe that has already been regenerated once ($n = 1$). We agree with Referee #2 that hydrothermal DFe is first “just transported” to the Southern Ocean surface where the density layers of the hydrothermal vents outcrop, but subsequently part of this hydrothermal DFe is utilized and trapped in the Southern Ocean (the trapping mechanism is described in the cited references). The plots of Figure 2 for hydrothermal DFe (column 3) show that hydrothermal DFe with $n \geq 1$ is found in the Southern Ocean with the characteristic pattern of Southern Ocean nutrient trapping.

In response, we will double check the revised manuscript to ensure that all other occurrences of “Southern Ocean nutrient trapping” are appropriate.

Referee #2 Minor Point 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.

Authors’ response: We thank Referee #2 for reminding us about this paper. We agree that we should have referenced it and that it will provide additional interesting context for our study. It is important, however, to appreciate that the experiments described by Dutkiewicz *et al.* (2006) are finite-amplitude perturbations, while our study quantifies the “natural” fertilization efficiency of the *unperturbed* iron cycle.

In response, we will add references to Dutkiewicz et al. (2006) where relevant.

Referee #2 Minor Point 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 enhanced 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. Kirchner, 1996, Nature, doi:10.1038/383303a0; Maldonado *et al.*, 2005, GBC,

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[doi:10.1029/2005GB002481](https://doi.org/10.1029/2005GB002481); *Strzepek et al.*, 2005, GBC, [doi:10.1029/2005GB002490](https://doi.org/10.1029/2005GB002490); *Boyd et al.*, 2017, Nature Geoscience, [doi:10.1038/ngeo2876](https://doi.org/10.1038/ngeo2876)). Maybe these views can be reconciled, with reference to figure 3?

Authors' response: We thank Referee #2 for bringing these studies to our attention. While the details of the ferrous wheel are beyond the scope of our study (as is their bearing on Figure 3), we agree that it would be appropriate to briefly reference these papers where we comment on the associated issues in relation to our model. Specifically, our simple formulation of the Fe:C uptake ratio may well be unrealistic (e.g., *Kirchman*, 1996; *Strzepek et al.*, 2005), as we acknowledged in the Discussion and Caveats section where we discuss the work of *Rafter et al.*, (2017). Similarly, we acknowledged (with reference to *Twining et al.*, 2014) that different remineralization lengthscales for iron and macronutrients (*Boyd et al.*, 2017) are not modelled. The effect of ligands on iron bioavailability (*Maldonado et al.*, 2005) is also not represented by our model, which does not have dynamic ligands but instead prescribes a ligand distribution.

Importantly, we would like to note that not every process thought to operate on DFe needs to be explicitly modelled for a useful representation of the iron cycle. The inverse model of *Pasquier and Holzer* (2017) is of intermediate complexity, and any effect of the above issues is captured implicitly when parameters are optimized to fit the observed nutrient and phytoplankton fields. Explicit modelling of these complexities may be important for models that try to predict how the system will change in the future, but we do not think this is necessary for constraining and diagnosing the large-scale cycling of DFe in the current state of the ocean as we do here.

In response, we will mention the effect of ligands on iron bioavailability (which was missing in the submitted version), and we will add references to the suggested papers were we discuss the associated issues in the Discussion and Caveats section.

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