Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-379-AC1, 2018

Biogeosciences
Discussions
Open Access

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

Benoît Pasquier and Mark Holzer

pasquieb@uci.edu

Received and published: 17 September 2018

Referee #1 General Comments: This is a review of "Iron fertilization efficiency and the number of past and future regenerations of iron in the ocean" by B. Pasquier and M. Holzer. The authors present a novel technique to track the life-cycle of dissolved iron (DFe) in the ocean. The authors apply the technique to multiple instances of data-constrained representations of the iron cycle and investigate how many cycles DFe parcels experience before and after participating in the biological pump. The authors use their technique to quantify the efficiency of iron fertilization on biological export, one of the motivations for their study. The manuscript is fairly well written, laying out clearly their novel technique, and they present well-designed

Printer-friendly version

Discussion paper

experiments to utilize their technique. I think this manuscript will be a useful addition to the literature. I am making suggestions for some minor changes and/or additions.
Authors' response: We thank Referee #1 for these general comments. *No changes to the manuscript in response to these general comments.*

Referee #1 Minor Point 1: The abstract doesn't mention that the model used by the authors includes multiple types of external sources of DFe. I think it would be useful to mention in the abstract the types of external sources of DFe under consideration.
Authors' response: We agree that it would be good to mention the modelled external iron sources in the abstract. *In response, we will revise the abstract to explicitly state that aeolian, sedimentary, and hydrothermal iron sources are modelled.*

Referee #1 Minor Point 2: The notation for the nonlinear model in Section 2.1 deviates from the notation in the author's previous work (Pasquier & Holzer, Biogeosciences, 2017) (e.g., removal of the Redfield ratio for the uptake terms, and changed notation for the particle transport terms). I suggest that the authors either use the same notation as their previous work, or describe how and why the notation in the current work differs from the previous work.
Authors' response: Referee #1 is correct that we changed notation from what we used to describe our fully coupled Fe—P—Si model [*Pasquier and Holzer*, 2017]. Here, we use simplified notation for extra clarity and readability, exploiting the fact that not all the complexity of the fully coupled model is needed to develop our new iron-cycle diagnostics. *In response, we will add a brief statement to Section 2.1 which points this out and makes the connection with the corresponding symbols in* Pasquier and Holzer*, [2017].*

Referee #1 Minor Point 3: There is no motivation given for the definition of the equivalent linear model in Section 2.2. I think the paper would benefit from having a paragraph describing what the goals/requirements of the equivalent linear model are, and how goals/requirements lead to the model that the authors are using.

Authors' response: The opening paragraph of Section 2.3 ("In order to track iron from its birth at the source to its eventual death...") was meant to motivate the need for the equivalent linear model, but we agree with Referee #1 that we do not explicitly point out why the nonlinear model itself cannot be used for tracing *partitions* of DFe or, equivalently, iron *labels*. The iron labels, unlike iron itself, are passive tracers and obey a linear equation of motion. Mathematically, only these linear tracers allow for correct partitioning of the DFe distribution because the superposition principle only applies to linear systems. This is often overlooked in the bigeoscience literature where studies typically evaluate the contribution of a given process by computing the anomaly that results from the removal of the process (e.g., removing a source). Such an anomaly approach results in errors that scale with the degree of nonlinearity of the system. For example, *Holzer et al.* [GBC, 2016] showed that omitting an iron source to evaluate its contribution to the DFe distribution underestimates the true contribution (evaluated using the equivalent linear model) by a factor of ~ 2. For the iron cycle, the nonlinearities of the uptake and scavenging processes are the reason why an equivalent linear model is required. *In response, we will add a paragraph to the beginning of Section 2.3 that includes the arguments above to explain the necessity of the equivalent nonlinear model.*

Referee #1 Minor Point 4: While some symbols chosen for the various terms in the nonlinear and equivalent linear models do seem related to the processes being represented by the terms (e.g., U for uptake, $\mathcal{R}$ for regeneration, $\mathcal{D}$ for death), not all of the connections are clear (e.g., J for scavenging, $\mathcal{L}$ for uptake that gets exported). This makes is hard for the reader to keep track of which terms mean what. I suggest adding a table that describes, in terms of processes, what each symbol denotes.

Authors' response: We strongly agree with Referee #1 that good notation is important, and when possible we do use symbols whose meaning is self-evident. However, the desire for simple notation needs to be balanced with precision and clarity. For example, we need to use different symbols for linear and nonlinear operators, and we do not want to use an "s"-based symbol for both particle transport (“sinking") and scavenging. We think the notation in our manuscript is a reasonable compromise. We agree that a table describing our symbols could be helpful to the reader. *In response, we will add a short glossary of symbols as suggested by Referee #1.*

Referee #1 Minor Point 5: Have you considered how a particular instance of the nonlinear model would respond to a substantial change to aeolian input, such as would happen in the LGM or a future climate change scenario. Does the technique presented shed light on how the nonlinear model would respond to this change in forcing? This could be mentioned in the Discussion section.

Authors’ response: Thank you for the suggestion. We think that exploring the response of the iron cycle to changes in iron input is a rich subject deserving a separate study (and beyond the scope of the current manuscript). The diagnostics developed here probe the iron cycle without perturbing it and are therefore by themselves insufficient to infer the response to source changes. To examine the steady-state response to source changes, e.g., aeolian input for an LGM or future climate scenario, one would first have to use the fully coupled Fe—P—Si model of *Pasquier and Holzer* [2017] to solve for the perturbed steady-state nutrient cycles. Once these have been calculated, our diagnostics can readily be applied to the perturbed iron cycle to elucidate, for example, what the mean number of iron passages through the biological pump was during the LGM. *In response, we will add some discussion to the Discussion and Caveats section to explain that while we diagnosed the unperturbed iron cycle, one can also apply the diagnostics to perturbed states resulting from added iron, e.g., to shed light on paleo or future climate scenarios.*

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-379, 2018.