Biogeosciences
Discussions

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. Völker (Referee #3) Introductory Comment 1: Quite seldom, when reviewing manuscripts, I encounter a study where I wished I had had the idea myself. This manuscript, of which I have already reviewed an earlier version for another journal, is one of them. It introduces several new diagostics for the iron cycle that help to understand the fate of iron stemming from different external sources as it is advected through the ocean and every now and then gets taken up by a phytoplankton cell. Over the last decade or so it has been realized that other sources of iron besides dust (sediments, hydrothermal sources, volcanos,...) contribute substantially to the inventory of iron in the ocean, and estmates of iron's residence time had to

be corrected downwards. But there remain major uncertainties on the relative magnitude of these sources, and models with very different source strengths and residence times have been equally, albeit only moderately, successful in modelling the measured iron distribution. At this state of things it is an important step forward to have tools at hand that allow to quantify the biological effect of different iron sources, even if it is only for one specific iron model and assumes steady state.

Authors' response: We are delighted by Dr. Völker's (Referee #3) comments. *No changes to the manuscript in response to these Introductory Comments.*

Referee #3 General Comment 1: The new diagnostics are based on a linearization of optimized steady-state solutions from a global ocean biogeochemical model to separate the effect of the different iron sources. The idea is to take the resulting iron distribution, and to linearize the nonlinear parts in the evolution equation for iron around that solution. The linearized equations are not only the basis for separating the effect of the different iron sources, but also allow, by splitting the linear operators, to separate in a second step, how often a specific concentration of iron has passed through biological uptake and remineralization, and how often it will do so in the future.

Authors' response: Just to clarify, we do not linearize our nonlinear nutrient model in the usual sense, but instead construct an equivalent linear model for tracer labels as explained in our response to Referee #1, Minor Point 3. There is a fundamental difference: Linearization usually refers to the first-order Taylor expansion of the nonlinear model around a suitable base state, which captures the system behaviour for small perturbations about the base state. Here, we instead construct the equations for passive tracers that follow the DFe and that have the same solution as the nonlinear equations. The resulting model is linear in the sense that the labels participate in the physical processes in proportion to the local DFe abundance; hence all nonlinear processes are replaced by the DFe-label concentration multiplied by a rate coefficient diagnosed so that the linear and nonlinear models have identical solutions. The passive labelling tracers of the linear model then allow a rigorous partitioning according to

source type, number of regenerations, and so on, because the superposition principle applies to the linear equivalent model.

In response, we will add a sentence or two to the section on the linear equivalent model that cautions the reader not to mistake the linear model as a linearization of the nonlinear model.

Referee #3 General Comment 2: It is important that this exercise is not done for one specific model run, but for a whole family of model runs, differing mostly in the relative strength of iron sources, and correspondingly, in the timescale for scavenging, all reproducing iron observations about equally well (or badly). Of course this means that the linearized equations are different for every member of the ensemble, and the results obtained that way also differ to some extent. Nevertheless, the authors show quite convincingly that some results are quite robust and consistent between the different ensemble members.

Authors' response: Agreed.

No change to the manuscript in response to this comment.

Referee #3 Minor Point 1: The main results obtained in that way are that for reasonable strength of external iron sources, the average number of past and future passages through biological uptake of a given iron concentration is less than one, meaning that most iron has not or will not pass through biology before getting scavenged. This number is significantly higher for dust-deposited iron, since it enters the ocean closer to the place of biological uptake. Regardless of source the pattern of iron concentration that has passed through several uptake-recycling cycles converges towards a Southern Ocean-trapped pattern. Not all results are equally important or surprising: for example the result that "total DFe is more likely to have been regenerated in the past than it is to be regenerated in the future" (page 17, line 30) is fairly obvious from the fact that remineralisation happens deeper in the water column than e.g. dust deposition.

Authors' response: We agree that not all our results are equally important and

some may seem, after the fact, "fairly obvious". The likelihood of passing through the biological pump depends subtly on how transport to surface uptake from either external source or from internal regeneration samples the scavenging field. Our diagnostics allow this to be quantified rigorously for our model. The results can be rationalized after the fact, but we think that both qualitatively and certainly quantitatively, the finding of more likely past than future regeneration is far from obvious before one does the calculation. For example, it is not obvious that sedimentary iron, which has deep sources, is more likely to have passed through the pump in the past than in the future.

No change to the manuscript in response to this minor comment.

Referee #3 Minor Point 2: Some results may also have to do with the specific iron model: that iron fertilization is most efficient in the equatorial Pacific may also have to do with the particular parameterisation of a variable Fe:P quota in the model by Pasquier and Holzer, which basically follows a Michaelis-Menten-type curve, meaning that for Fe tending towards zero, the Fe:P ratio will also tend towards zero; the linearisation of the iron uptake would then ascribe a very high ratio of P to Fe export in regions with very low Fe. Most iron models produce an extremely low surface Fe concentration in the equatorial Pacific, which is far away from dust sources and where the upwelling waters are quite old, meaning thy are low in Fe.

The dependency on Fe:P is mentioned on page 22, line 11, and the sensitivity of the results on this is discussed briefly in the subsequent paragraph. In the discussion section this is however, discussed maybe a bit too briefly (page 25, line 30).

Authors' response: We agree that the DFe dependence of the Fe:P uptake ratio is a major control on our model results. We would not characterize our discussion of the issue on page 22 as particularly brief: An entire paragraph (lines 13–21) is devoted to it. We agree, however, that we should reiterate the importance of the Fe:P ratio in the Discussion and Caveats section.

In response, we will add a sentence or two to the Discussion and Caveats section reiterating the importance of the Fe:P ratio for shaping the pattern of the diagnosed natural iron fertilization efficiency.

Referee #3 Minor Point 3: In summary I think that this is quite a significant paper for understanding the iron cycle in the ocean, and it should be published in Biogeosciences after minor revision.

The paper is quite well written (although it probably appeals more to a reader with some background in linear algebra) and I have checked the mathematical argumentation in depth and it is clear and correct. The authors attempt, and usually succeed in connecting the mathematically rigorous description of their results with what these results mean in terms of biogeochemistry. Nevertheless, here and there, the authors could do a bit more to make the explanations more palatable to the readership of Biogeosciences.

One example that I have is in the beginning of the section on future passages through the biological pump, where the authors explain that "the natural way to formulate the necessary equations is to consider the time-reversed adjoint flow... The adjoints are defined for the volume-weighted inner product." While this is probably clear to a mathematically trained physicist, it may less be so for the average reader of Biogeosciences. Maybe the authors could add a few lines here on what the adjoints are, what the inner product.

My small remarks to the earlier version of this manuscript have been taken into account already, so I stop here.

Authors' response: We are delighted that Dr. Völker appreciates our work, and we certainly agree that our paper should be as accessible as possible to the Biogeosciences audience.

In response, we will rephrase the introduction at the start of Section 4 (Future Contributions to Export) to explain and motivate why we use the adjoint, which is to track DFe backwards in time "from" death with computational efficiency.