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Interactive comment on "Inverse-model estimates of the ocean's coupled phosphorus, silicon, and iron cycles" *by* Benoît Pasquier and Mark Holzer

Anonymous Referee #2

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The submission by Pasquier and Holzer uses a new ocean bioegeochemical and ecosystem model, embedded within a data-constrained steady-state circulation model, to explore linkages between the phosphorous, silicon, and iron cycles. The model uses interesting methods to represent nutrient uptake by multiple-species phytoplankton communities without explicitly resolving their biomass, allowing for efficient simulations and parameter optimization. Based on previous work, the authors understand that no single optimal solution for the Fe cycle can be obtained because certain source and sink processes have overlapping effects on the Fe distribution. They therefore explore a "family" of solutions with different source strengths, which are independently optimized and then compiled into a "typical" solution and uncertainty range. There are a number of interesting outcomes that are robust across the family of solutions, for

example the patterns of phosphorous export supported by each iron source and the "efficiency" of each source at supporting export. Atmospheric Fe supports most export relative to the magnitude of its source, followed by benthic and then hydrothermal Fe.

I think this paper takes an interesting approach and has the potential to be a valuable contribution to the literature. Nevertheless, I have two main critiques of the paper in its current form. First, the paper lacks a clear direction from the outset. The introduction does not lay out any specific questions or hypotheses that the new model is designed to address, nor does it identify the particular gaps in our understanding of the Fe cycle that the authors aim to close. Instead, the goal is simply states as "to constrain a model of the coupled nutrient cycles by optimizing the biogeochemical parameters against available observations", which does not seem like a strong motivation. The purpose of an inverse model should be to extract new information from the available observations, not just to match the observations. The authors should begin by clearly stating what new information they aim to extract by explicitly simulating the coupling of Si, P and Fe, relative to their previous work.

This same mentality extends throughout the paper, where numerous model-data comparisons are presented without properly highlighting what new has been learned in the process. For example, one of the key benefits of this coupled model is the ability to assess the relative Fe-scavenging efficiency of different particle types (organic, silica, dust), which remains an open question in Fe biogeochemistry. While this result is part of the model solution, it receives very little attention in the text – it is briefly noted that on a global basis, organic matter and silica are equally responsible for Fe removal from the ocean, and a figure is shown in the Appendix. But the authors should discuss which particle type is the stronger Fe scavenger on a per-gram basis, whether this is robust across the family of solutions. This would be a new interesting result of this study.

As another example, the new model seems to be the ideal tool for examining differences in Fe quotas among phytoplankton types – another open question in Fe cycle research. The authors briefly mention that they experimented with different Fe quotas, but abandoned the approach when the parameters converged to similar values. If the model selects similar Fe quotas for all plankton groups, and this is robust across the whole family of solutions, it would be an interesting result indeed and worth of some attention in the paper! Especially if the authors could demonstrate that there is no evidence for enhanced Fe quotas in subtropical gyres where diazotrophic plankton are common, given that there is ongoing debate about the relative Fe requirements of N-fixing and non-fixing plankton.

My second main critique of the paper is that it doesn't present the model-data comparisons for Fe that would be best suited to support the conclusions. If one of the main goals of the paper is to understand the relative contribution of each Fe source to organic matter export, one would want to show that the model accurately reproduces the locations and transport trajectories of the sources. By design, many of the GEOTRACES transects sampled different source regions of Fe, and show clear signatures of these sources and their transport across basins. For example, GA03 and GP16 both traverse benthic and hydrothermal source regions. Plotting cross-sections of modeled and observed Fe along these transects would give a clearer visual impression of the model's performance than the summary statistics and basin-wide profiles that are presented. The reader would want to ensure that these source signatures and transport trajectories are well reproduced, before considering the export contribution analysis.

In addition, I have the following minor comments:

1. I agree with Reviewer #1 that caveats of neglecting DOP cycling need to be more carefully considered. Ignoring DOP will not only bias the total estimated export, but also its pattern and therefore potentially the contribution of different Fe sources to export. Particularly, DOP convergence is thought to provide a significant P supply to subtropical gyres, and essentially "relocates" export downstream, from tropical and coastal upwelling zones into the gyres. Given that benthic Fe supports most export in upwelling zones and atmospheric Fe supports most export in gyres, relocating exporting between those two regimes seems important.

2. What is the justification for choosing such widely different export ratios between plankton types (page 5, line 13)? The authors cite Dunne 2005, but there have been other studies since (e.g. Richardson 2007) that suggest small plankton contribute as much export, relative to their NPP, as large plankton.

3. What is the justification for not prescribing a minimum Fe:P quota in equation 14? It is impossible to sustain phytoplankton growth with no Fe, so if the model is optimizing towards zero it means that model is straying into unrealistic parameter space, not that this parameter can be neglected. The authors should set a reasonable lower limit during the optimization (e.g. low end of the range shown in Moore et al 2013), rather than allowing the Fe quota to approach zero at low [Fe].

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