

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

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We appreciate the referee’s comments. The referee’s main points concern our statements on carbon export, specifically the lack of DOC and constant C:P stoichiometry. Before we address these in detail, we would like to emphasize that we model the phosphorus, silicon, and iron cycles and not the carbon cycle. Carbon export is plotted merely to show the phosphorus export in convenient units that make it easy to compare to other model and observational estimates. We therefore used a constant C:P ratio as a convenient unit conversion not intended to quantitatively estimate carbon export. The advantage of a constant Redfield ratio is that it does not distort the pattern of the phosphorus export. While we did point out that the carbon export flux is merely meant as a conversion of the phosphorus export after Eq. (27), we should have been

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more explicit that we are discussing phosphorus export in the abstract and in some of the figure discussions. In response, we will revise the manuscript (especially the abstract) to make clear that we quantify phosphorus export and to mention throughout that the plots of carbon export show phosphorus export expressed in carbon units.

The referee's comments raise the interesting questions as to how much dissolved organic matter transport is implicitly captured by our approach, and how different the inferred global carbon export would be if one used more careful estimates of the C:P ratio for the export of particulate and dissolved organic matter. We will address these issues in our point-by-point responses below, which follow the referee's comments reproduced in bold.

**This manuscript presents the formulation of a global biogeochemistry-ocean circulation model that considers the phosphorus, silica, and iron cycles. Results are presented from a family of solutions that fit the data (dissolved phosphate, silicate, iron, and phytoplankton distributions) equally well but explore the sensitivity to the unconstrained external iron sources to the ocean. Metrics related to global carbon and opal export, limiting nutrients, and iron based export production patterns are presented. The presented modeling framework is at the state-of-the-art for building a 3D global biogeochemical model with the solution computed in offline mode and is of high interest to the ocean modeling and marine biogeochemistry communities. The main advance of the work is to show that the global biogenic carbon and opal exports are well constrained by the available nutrient and satellite phytoplankton data even though the external and internal ocean iron fluxes are not. The family of most probable model solutions given the sensitivity in assumptions on iron cycling mostly converge on 10 Pg C yr<sup>-1</sup> and 170 Tmol Si yr<sup>-1</sup> global exports.**

**My two main comments concern the sensitivity of their calculated global carbon**

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**export flux to their omission of DOM cycling and variable C:P stoichiometry in organic matter production/export. DOC has been estimated to contribute 20% (Hansell et al., 2009, Oceanography) to 25% (Letscher et al., 2015, Biogeosciences) of global carbon export production. In the model presented by the authors they chose to omit DOP cycling, with their argument being that DOP cycling represents a small to negligible contribution to the biological phosphorus cycle. They also rationalize that DOP typically has lifetimes <1 year in surface waters such that it is not significantly advected with the ocean circulation and can instead focus on vertical redistribution of particles as the dominant export process in their model. However DOC has longer lifetimes in surface waters on the order of a couple years and does accumulate to large enough quantities to be an important part of the carbon export term. Can the authors address the sensitivity of their calculated global carbon export flux to this omission of DOM cycling in their model? Should DOC export be considered as an addition to the computed  $\sim 10 \text{ Pg C yr}^{-1}$  flux? Or is the DOC export flux somehow already included in their computations from their model solution?**

Our estimates of phosphorus export effectively capture the export of both POP and DOP, despite DOP not being explicitly represented as a separate tracer. This is because the phosphate export of our model is set by the strength of  $\text{PO}_4$  uptake and by the Martin exponent  $b$  of the remineralization profile, both of which are optimized by minimizing the mismatch between modelled and observed  $\text{PO}_4$  concentrations. Because the real ocean's (regenerated)  $\text{PO}_4$  concentrations result from both POP and DOP export, the effect of DOP export on the remineralization field is implicitly accounted for in our optimized states.

We agree that the lack of explicit representation of DOC results in an underestimation of the carbon export as inferred from the phosphorus export. We estimate the "missing" carbon export by explicitly calculating both POP and (semilabile) DOP export using the

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data-assimilated phosphorus cycle of Primeau et al. (JGR, 2013), which carries explicit  $\text{PO}_4$  and DOP tracers and uses the same circulation as in our manuscript. (The DOP export was computed as the aphotic remineralization rate of DOP produced in the euphotic layer.) We find a DOP export of  $0.70 \text{ Tmol P yr}^{-1}$  and a total phosphorus export of  $5.79 \text{ Tmol P yr}^{-1}$ . If one applies C:P=106:1 to the total phosphorus export, this converts to a total carbon export of  $7.37 \text{ Pg C yr}^{-1}$ . However, if one uses C:P=225:1 for DOP (as determined by the DOM OPT simulation of Letscher et al. (2015) for semilabile DOM), the total carbon export becomes  $8.37 \text{ Pg C yr}^{-1}$ . Thus, by not representing DOC explicitly, one “misses”  $\sim 12\%$  of the total carbon export. (In this calculation, the DOC export represents 23% of the total carbon export, consistent with the estimate of Letscher et al., 2015.)

While we agree that accounting for DOC export gives more realistic estimates for the total carbon export, we reiterate that we use our model to calculate phosphorus export and merely convert to carbon units when we think this is useful (e.g., for comparison to the results of Primeau et al., 2013). In response to this comment, we will add a paragraph briefly discussing the fact that phosphorus export converted to carbon units underestimates carbon export by roughly 10% due to the missing DOC export.

**Secondly, recent global datasets and model inversions of nutrient data have shown/predicted that the production and export of organic matter from the surface ocean is not constant and exhibits latitudinal and ocean biome-level variability (e.g. Martiny et al., 2013, Nature Geoscience; Teng et al., 2014, Nature Geoscience; Devries & Deutsch, 2014, Nature Geoscience; Galbraith & Martiny, 2015, PNAS). The authors chose to calculate all of their carbon export metrics using a constant Redfield ratio of 106:1 C:P to get carbon units from their model which is in phosphorus units. How much would their global estimates of carbon export change if a variable C:P of organic matter production/export were used? For example, a regionally variable C:P could be computed using**

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the relationship predicted with surface phosphate conditions from Galbraith & Martiny, 2015 (PNAS) using the model simulated phosphate fields. Alternatively, the twelve-biome inferred export C:P ratios from Teng et al., 2014 (Nature Geoscience) could be used to calculate the regionally variable C:P of export from the authors model. It seems given what we now know vis-à-vis regionally variable organic matter stoichiometry, it would be remiss not to include that knowledge to update the global C export flux from the authors' model solution.

We will revise the manuscript to make explicit that we compute phosphorus export converted to carbon units. We agree with the referee that if the objective were to provide an accurate estimate of carbon export, using recently constrained regionally varying C:P ratios would be more realistic. However, using the regionally varying C:P ratios suggested by the referee has only modest effects on the globally integrated carbon export:

(i) Applying the P:C relation of Galbraith and Martiny (2015) to the phosphate export of our typical state gives a carbon export of  $8.5 \pm 0.4 \text{ Pg C yr}^{-1}$  or  $9.4 \pm 0.9 \text{ Pg C yr}^{-1}$  when we use their log-binned parameter values.

(ii) Applying the regional C:P inverse-model estimates of Teng et al. (2014) gives a carbon export of  $10. \pm 2. \text{ Pg C yr}^{-1}$ . Both this estimate and the one based on the log-binned regression agree within their uncertainties with our simple unit-conversion value of  $10.3 \pm 0.4 \text{ Pg C yr}^{-1}$ .

#### Other comments:

**Pg 20 L1-5: The authors blame phytoplankton biomass mismatches between the model and satellite observations based on a lack of seasonality in the model but aren't these steady-state satellite climatologies they are comparing against, and therefore seasonality is averaged over?**

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We are alluding to the fact that there is a difference between the fields produced by a steady-state model and the long-term average of a seasonally varying field. (For example, the circulation we use is *not* a simple annual-mean circulation, but rather a steady-state circulation whose transport reproduces the annually averaged tracers used in the assimilation.) However, we agree that this passage could have been clearer. In response, concerning the mismatch at  $\sim 60^\circ\text{S}$ , we will revise to point out that the strong seasonality of the Southern Ocean, with its large seasonal cycle in sea-ice coverage, is not captured by our model and that this could conceivably be the reason for the mismatches in that region. We will delete the second mention of seasonality in this paragraph, which is unintentional repetition.

**Pg 23 L20-25: The authors argue that the sharper meridional gradient in C export is more realistic because there is a sharp gradient in satellite NPP. But they don't include DOC export. DOC export is estimated to be 1/5 to 1/4 of total global C export with its larger contribution occurring in the subtropical gyre systems due to large-scale downwelling.**

We agree that the inclusion of DOC would result in more realistic estimates of carbon export. However, in this passage we are comparing our phosphorus export (converted to C units) to POC export as estimated from satellite-derived NPP, and by definition POC export does not include DOC export. In response, we will revise this passage to make clear that we are comparing phosphorus export converted to carbon units, and to state that sharp meridional gradients can also be seen in the satellite-derived NPP. We will delete the statement about the sharp gradients being “more realistic” to avoid confusion.

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**Pg 27 L24-30. One statement says the dominant Fe sink is from POP scavenging. The very next statement says that opal scavenging accounts for half of Fe sinks. The next statement says that dust scavenging is negligible for Fe sinks. Why not just say that POP and opal scavenging account about equally to Fe sinks?**

Thank you for catching this — we will revise as suggested. (There was some left-over wording from a previous iteration.)

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