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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 et al.

Anonymous Referee #1

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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

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Discussion paper

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-1 and 170 Tmol Si yr-1 global exports.

My two main comments concern the sensitivity of their calculated global carbon 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 ∼10 Pg C yr-1 flux? Or is the DOC export flux somehow already included in their computations from their model solution?

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 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.

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?

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.

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?

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