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Biogeosciences
Discussions

Interactive comment on "Marine Phytoplankton Stoichiometry Mediates Nonlinear Interactions Between Nutrient Supply, Temperature, and Atmospheric CO_2" *by* Allison R. Moreno et al.

Anonymous Referee #1

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Variability in marine phytoplankton stoichiometry can lead to differences in carbon export to the deep ocean. This manuscript expands on previous models (mainly Galbraith and Martiny, 2015 and Yvon-Durocher et al., 2015) to further our understanding of how various environmental factors lead to changes in phytoplankton stoichiometry. The authors show how incorporating factors, such as temperature, light, and phosphorus concentrations, are able to model variations across the global ocean that would otherwise be lost in more mainstream models using fixed C:P ratios. The manuscript overall nicely lays out the difference modeling approaches taken and how they ultimately change the effect on carbon export in the global ocean. I want to point out that I am not a modeler and therefore cannot asses the math presented to its full extend, but I am a

biogeochemist and can provide a critique on the science presented in this manuscript. I believe this manuscript should be published with the revisions I have outlined below.

My first issue with the manuscript in general is the lack of nitrogen. The ocean overall is nitrogen limited and it is a bit worrisome that it is never mentioned. Why was nitrogen left out? There must be a reason, and including a sentence or two to explain why it has been left out would be sufficient without having to incorporate it into the models.

My second issue is the inclusion of iron and iron deposition. It is mentioned several times throughout the manuscript and honestly seems to be thrown in haphazardly. Even calling the one region the iron-limited upwelling zone does not really make sense. I do not disagree that these regions are distinct from the subtropical gyres, but there needs to be another way to separate them. The simplest thing to do would be to remove all talk of iron and iron deposition as it does not add anything to the manuscript. If you choose though to leave it in, there needs to be more discussion and also a few references as there are currently none. I have listed below each mention of iron and have provided some input should you choose to include it.

My third issue is how phospholipids have been defined and treated within the model. The decision to functionally treat phospholipids with the storage pool needs to be justified or expanded upon as it is currently not clear. As the authors state, phospholipids are localized within the cellular membrane (defined in the model as a functional pool) and not as energy storage molecules as suggested in the text. Lipids associated with energy storage, localized within intracellular lipid droplets, are generally non-phosphorus, highly reduced, and non-polar (see Levitan et al 2014 "Remodeling of intermediate metabolism in the diatom Phaeodactylum tricornutum under nitrogen stress"). This raises the question of how the authors have defined the storage pool, is it defined as utilized by organisms for energy storage or is it only in the sense that "this is a pool where some phosphorus is stored within the cell?" L34: First mention of iron-limited tropical upwelling region. Again, I would honestly remove "iron-limited" and just call the region the tropical upwelling region. These regions become macronutrient limited as well once you leave the immediate upwelling zone so its deceiving to just focus on iron. It is also known that the Southern Ocean is iron limited, so, again, it is deceiving to focus on iron in the upwelling regions when there are other regions that are iron limited.

L46: add “ppm” after ∼46

L70: add “et al.” after Durocher

L76-85: Remove iron-stressed and iron-limited. The sentence “Iron deposition in the tropical upwelling. . .” is not correct. There is actually very little iron deposition to the tropics, the North African dust plume deposits iron to the tropical Atlantic but that is the one example (see Jickells et al 2005 “Global Iron Connections Between Desert Dust, Ocean Biogeochemistry, and Climate”). Iron is upwelled along the coasts in these areas along with macronutrients, but it is incorrect to call that iron deposition.

L111-115: Questions 1 and 2 seem redundant, please remove question 1 or give more detail if it is in fact different from question 2.

L135: Where in the water column are you taking the phosphorus concentration?

L207: add “et al.” after Daines

L212: add “et al.” after Daines

L213: Expanding on the phospholipid justification, can you explain more why you choose a zero contribution for phospholipids? Although non-P substitutes can reduce the phosphorus incorporated into P-lipids, observations suggest non-zero quantities remain. For example, 1.3 +/- 0.6% P uptake in the P-limited Sargasso are incorporated into phospholipids (Van Mooy et al 2009 – mentioned in next correction) and phospholipids make up approximately 5% of particulate organic P in the P-limited eastern Mediterranean (Popendorf et al 2011 “Gradients in intact polar diacylglycerolipids across the Mediterranean Sea are related to phosphate availability”). Might it be more appropriate to have two distinct P-lipid/total cellular P values for high and low phospho-

rus regions?

L215-216: For phospholipid substitution, a more appropriate reference would be Van Mooy et al 2009 “Phytoplankton in the ocean use non-phosphorus lipids in response to phosphorus scarcity” instead of Van Mooy et al 2006.

L228: add “et al.” after Daines

L282: . . .that underlies the subtropical gyres and equatorial upwelling regions (labeled M), and deep waters. . .

L314: “Iron limitation is implicitly simulated through its control on the tropical [P]. . .” – how does iron control phosphorus concentrations? This is not clear in the manuscript and I personally have not come across any such research stating such. Again, if you are going to keep iron in the manuscript please provide references of where you have gotten the information and expand on the explanation of how you can make this justification.

Table 2: Please switch the columns so that Range of fhd (sv) is first and references is second (will be consistent with Table 1).

L350: “This set of experimental runs was intended to capture the effects of changing levels of iron deposition. . .” – Again, talking about iron deposition in these tropical upwelling regions does not make sense and as you have not provided references I would just remove it all together. This experiment wanted to test the sensitivity of pCO2 to nutrient availability, I believe that is a good enough reason and there is no need to mention iron limitation.

L377: Change variables to variable

L379: Remove “iron stressed”

Figures 6 and 10: I really like these figures and think you could include more in the discussion about the implications of how global temperatures will affect export. It is a nice way to tie your work with large scale impacts on biogeochemical cycles and reiterate the importance of the study.

L477: add “the” before data

L589: remove iron-limited

L596: remove “iron deposition or”

L600: remove sentence “This observation suggests that pCO2 may have a complex link. . .”. You honestly have not shown anything to do with iron delivery and its link to pCO2, there is nothing included in the model that I saw and again have provided zero references about iron deposition

L650: remove “thus”

L674: remove “which might be influenced by increased atmospheric iron deposition,”

L680: change separating to separate

References: There are a few references that are not mentioned in the manuscript. A couple are about iron cycling and I am curious if and where they were originally included and also possibly had more of an explanation associated with them of why you link iron to phosphorus? Cunningham and John 2017 Moore 2004 Raven and Falkowski 1999 Also, please move Van Bogelen and Neidhardt 1990 and Van Mooy et al 2008 references to after the Toseland et al 2013 reference

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