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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 #2

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Using a "classical" ocean carbon cycle box model and parameterizations of flexible elemental stoichiometry in surface ocean particulate, the authors examine the role of elemental composition in mediating the response of atmospheric CO2 to ocean temperature change. There is a significantly different sensitivirty when the particulate C:P ratio is represented by a "multi-environmental" (Ecological Stoichiometry and temperature dependent) framework compared to a fixed, Redfieldian particulate composition. Most notably, compensation between temperature sensitivity of solubility and biological pumps reduces the sensitivity to subtropical temperature change as well as reversing and enhancing the response to tropical perturbations. Variable elemental composition and phosphorus storage modify the sensitivity of atmsopheric pCO2 to the efficiency

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of phosphate utilization in subtropics and tropics. The key message for is that variable elemental ratios have non-negligible impacts on the ocean's control of atmospheric CO_2 and that the temperature sensitivity of solubility and stoichiometrically-mediated biological pumps have some interesting regional dependences.

I enjoyed reading this paper. I found it stimulating and thought provoking. There is alot going on. The authors combine classical carbon cycle box model with several parameterizations of elemental stoichiometry of the sinking particulate (and/or primary producers). The authors connect cellular scale physiology and global carbon cycle and have used and developed an appropriate framework with which to do so.

My criticism of the paper is that the multi-environmental model is presented very much at face value. The assumptions and construction seem very logical but the choice and constraint of the parameters is by and large opaque. In particular the relationship between the storage component of the multi-environmental model and the Galbraith and Martiny parameterization seems interesting and important but is not really discussed. How important is the storage term in controlling the overall response of the multi-environmental model? It is not at all clear from the manuscript. I feel that some clarification and discussion along these lines is important for the reader.

I found the manuscript very interesting and thought provoking. I had a number of questions, comments and need clarification on certain points which I will detail here. Some are more important than others. While my recommendation is major revision, it is clarification that I would like to see, not changes to what has been done.

1. The Sarmiento and Toggweiler and contemporary carbon cycle models focused alot on the sensitivity of atmospheric pCO_2 to "high latitude" changes. This isnt discussed here - perhaps there wasnt any as configured? Some comment would be useful and interesting in this regard.

2. The stoichiometry of sinking particulate and of primary producers is certainly connected but not necessarily the same. The multi-environmental model is founded on

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primary producer physiology. Perhaps this potential difference should be flagged?

3. I would have been interested to see a Droop-style model in the mix as its a relatively common tool - but there is more than enough going on here anyway.

4. I very much like the spirit of the multi-environmental model. (Though I find the name a little odd). It accounts for the role of cell size in mediating nutrient affinity and cell composition (contribution of cell wall material). It was not made clear how sensitive the final parameterization or the outcomes of the box model are to the assumed cell size. Nor could I find any information about the cell size assumed (or modeled?) in the simulations.

5. The photosynthesis parameterization and allocation scheme is very reminiscent of Geider's models in spirit and mathematical form. What is the relationship?

6. The statement at line 235 that the "unique maximum of the growth rate occurs for the set of parameters that lead to co-limitation by nutrients, photosynthesis and biosynthesis" is very interesting and intriguing. Is that an emergent property? Is it obvious that it should be this way? I would have liked to hear more about this.

7. Phosphorus storage seems to be very important. Equation (13) controls a residual storage pool that constrains the parameterized stoichiometry to match the observed relationship between phosphate and particulate stoichiometry, as I understand it. Thus it strongly mirrors the Galbraith and Martiny model of equation (1). For me, some key questions concern this aspect of the model: How significant in the overall control of model stoichiometry is this component? If it dominates, then I could view the multi-environmental model in some way as a combination of the Galbraith and Martiny model with a temperature sensitivity. Or does the more mechanistic and detailed physiology have a significant role? Either way, I think the mechanistic model is valuable and interesting but I would like to understand how much the results are driven by the storage of phosphorus. Its important for a number of reasons and I feel that this should be clearly discussed.

8. A small thing, but I had to stop and think about equation (1) because [P]o has different dimensions than [P]: the former is a ratio and the latter a concentration. I think it would be much clearer and more appropriate to denote [P]o as in (13), with a symbol in accord with other variables that are ratios.

9. The box model formulation makes sense; the inclusion of the thermocline reservoir is important for the sensitivity to changes in the subtropical surface. Some small details: how is the carbonate system solved? Is alkalinity fixed or is there an implicit carbonate pump?

10. The model doesnt resolve nitrogen, and I would expect that the allocation of nitrogen in proteins and pigments would be an important factor, perhaps more so than phosphorus. Does this actually matter? A comment on this would be helpful.

11. Why does temperature affect biosynthesis but not photosynthesis (line 398) imposed from empirical observations? The model description tells us that Q10=2 for temperature dependences, but there is no discussion with regard to phootosynthesis. Why this choice?

12. The discussion of sensitivity to cell radius in lines 400-410 doesnt tell us what is the cell radius (or distribution of) in the model? Is it imposed or modeled (I presume the former but nothing is said in the paper). This should be clear.

13. Figures 3,4,5 are a bit small and fuzzy when printed.

14. I'd really like to understand how important the storage term is in the overall control of figures 4 and 5. We see the variation in C:P and the relative allocation to biosynthesis and photosynthesis, but its not clear how important the latter is to the former.

15. What is the cell size in the box model simulations? Is it imposed? Does it vary? How sensitive are results to r?

16. The model is P based. However, as is alluded to in the manuscript, nitrogen and iron dynamics are important. Indeed P is found to be the proximal limiting resource

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in only a few areas of the global ocean, with N and Fe controlling things locally. So how does this affect the relevance of the model? Wouldnt N and Fe dynamics be more important at the individual scale? Would this (does this) mean that storage is most signficant for P:C? Again, understanding the significance of storage for the outcomes here is very important.

17. Line 464: "nutirent"

18. The contrasting temperature sensitivities of tropical and subtropical perturbations is very interesting. The dominance of the solubility term in subtropical responses is ascribed to the "large surface area" of the subtropical region (line 563). I dont think thats true: I think its because the subtropical surface feeds the subtropical thermocline which reprsents a sigifnicant contribution to global water volume. Hence, changes in subtropical solubility have significant leverage. Since tropical waters dont directly feed into any subsurface water mass, they do not have the same leverage. This is why the resolution of the thermocline box is important. The classic Harvardton Bear box models did not resolve the thermocline and so found very low sensitivity to subtropical perturbations relative to 3D circulation models. Resolving the thermocline in the box models brings them into consistency (this was the point of Follows et al, 2002). I thought this was why the authors had chosen the configuration which resolves a thermocline reservoir.

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