

Interactive comment on "Variable C/P composition of organic production and its effects on ocean carbon storage in glacial model simulations" by Malin Odalen et al.

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In their manuscript, Odalen and coauthors explore the role of variable C:P stoichiometry for carbon storage in the ocean under glacial conditions using an Earth System Model of Intermediate Complexity. Oceanic sequestration of carbon is almost certainly responsible for the recurring glacial-interglacial cycles that have been observed in sediment and ice-core records. While we know that carbon entered and exited the ocean over these cycles, we are still understanding why. What mechanisms caused the regular recurring reorganisation of the carbon system? Many mechanisms have been proposed and are still being proposed, but the suite of mechanisms that are of primary importance has alluded the palaeo-community for some time. Now, many scientists are working steadily towards “putting the pieces of this grand puzzle” together. Variable stoichiometry of organic matter, a phenomenon known for many years and even acknowledged by Redfield himself (often forgotten), is one possible piece that may help to explain why carbon entered (and more importantly stayed!) in the ocean for many thousands of years. It offers a positive-feedback under stratified and iron-rich glacial conditions to drive and **hold** CO₂ within the ocean. Variable stoichiometry achieves this by increasing the carbon that is assimilated into organic matter per unit of phosphorus, making the biological carbon pump more efficient. Effectively, a higher mean C:P ratio “tightens the lid”.

The manuscript presents results from numerous simulations that have importantly been run to steady state in a logical progression. The simulations involve controls, physical perturbations and biological perturbations that are run in isolation and in combination. In this sense, the manuscript is easy to follow. The overarching finding of the paper is that variable C:P ratios make an important contribution to $p\text{CO}_2$ decline under glacial conditions of roughly 15 ppm. This phenomenon therefore cannot be ignored, and the manuscript makes a meaningful contribution to the growing body of work that is highlighting the importance of biological stoichiometry on biogeochemistry and climate.

I therefore **advocate** for publication in *Climate of the Past*. However, I suggest *further*

revisions that I believe would improve the readability and hence its dissemination within the field. My comments for improvement (and sometimes clarification) are below.

1 General comments

The manuscript is well organised in how it presents the methods and results. In this sense, it is very easy as a reader to understand what the authors have done. However, the writing itself requires a lot of polishing. The common usage of “e.g.”, as an example (pun not intended), in mid-sentence makes things simultaneously difficult to read and also leaves me thinking that the authors are purposely choosing to not discuss all the evidence/processes/knowledge on a subject. I strongly suggest that the authors take some time to improve the writing.

One particularly important part of the manuscript that is not conveyed clearly is their diagnosis of CO₂ capture via variable C:P ratios, as opposed to a simple whole ocean increase in C:P. For instance, in the conclusion the authors state that “About half of the increased drawdown of CO₂ results from different global average C/P in the export production. In addition, flexible stoichiometry allows increased carbon capture through the biological pump, while maintaining or even decreasing the fraction of remineralised to total nutrients in the deep ocean.”. The reader is therefore left confused about what C:P ratios are actually doing. Aren’t they contributing to the *whole* of the *increased* drawdown? I think what the authors are trying to say is that the spatial and temporal variations in C:P are important for strengthening the biological pump, because they react dynamically to changes in nutrient supply to reinforce further CO₂ drawdown over a simple prescribed whole ocean increase. I would therefore advocate for the authors to make this more clear.

I also think that many of the concepts discussed in this paper with relevance for the sensitivity experiments could be introduced better in the Introduction. This includes previous glacial modelling studies, paleoproxy evidence and the theories that have been consequently generated for increased oceanic carbon storage due to these prior studies. Temp-dependent remin rates, changes in wind stresses, overturning circulation, polar stratification, solubility (salt and heat), Si-leakage, CaCO₃ compensation and production rates, Fe fertilisation, increased N₂ fixation, sea ice expansion, even volcanism. All of these, except volcanism, are feedbacks that were somehow kick-started by changes in solar insolation. While you obviously do not need to discuss all of these in detail, laying out the current “pieces of the puzzle” would help to ground your work in the current stream of consciousness in the paleoclimate community.

2 Specific comments

Abstract

- Page 1, line 8 : surely you mean increases rather than decreases? And also you mean Phosphate. Because an increase in Fe deposition, which is a nutrient, has been linked to an increase in C:P ratios. Garcia et al (2018) Nutrient supply controls particulate elemental concentrations in the low latitude eastern Indian Ocean. *Nature Communications*.

Introduction

- Page 2, line 2 : Also because of the rapid release of carbon to the atmosphere over the deglaciation, implying its storage somewhere during the glacial.
- Page 2, line 5 : What is the e.g. here referring to?

Methods

- Page 4, line 20 : suggest citing the more recent estimate of 2.6°C by Bereiter et al (2018) Mean global ocean temperatures during the last glacial transition. *Nature*.
- Page 6, line 1 : suggest citing Moore et al (2013) Processes and patterns of oceanic nutrient limitation. *Nature Geoscience*. Another paper of note that would be useful for your work is the recent article by Garcia et al (2018) Nutrient supply controls particulate elemental concentrations in the low latitude eastern Indian Ocean. *Nature Communications*.
- Page 6, line 17 : “see Table ??”... Please make sure your document is properly formatted before submitting.
- Page 6, line 22 : Why not use more recent WOA 2018 product?
- Page 7, line 13 : Again a question mark is present. Please format properly before submitting.

Results

- Page 8, line 29 : The oxygen content of the ocean should be lower in *Ctrl*₁₂₁ than for *Ctrl*_{RED}. This is because a higher C:P ratio of organic matter should also require more O₂ to remineralise that organic matter. I would like an explanation of why O₂ is higher in *Ctrl*₁₂₁ than for *Ctrl*_{RED}. See Paulmier, Kriest & Oschlies (2009) Stoichiometries of remineralisation and denitrification in global biogeochemical ocean models. *Biogeochemistry*.
- Page 11, line 1 : Please explain why a shallow RLS is shallower in a warmer climate. Alternatively, you could provide a more thorough explanation of the effect of temperature on remineralisation rates in the Introduction.
- Page 11, line 9 : This sentence needs to be clearer with what it’s trying to say. Roughly 10% of what? Change in the average composition of what? Of course I can guess what you mean when I stop to think about it, but please make it easier for the reader by saying what you mean.
- Page 11, lines 22-25 : But not cool enough to align with the more recent estimate of Bereiter et al (2018) Mean global ocean temperatures during the last glacial transition. *Nature*.
- Page 12, lines 21-25 : Again, I am unsure how you are treating P:O₂ remineralisation requirements in your variable stoichiometry experiments. I think this should be explained. It is also strange once again that your *GLcomb*₁₂₁ experiment is better oxygenated than your *GLcomb*_{RED} experiment.
- Figure 5 : The panels in this figure do not seem to be arranged correctly.
- Page 13, lines 1-8 : What conditions affect the fractionation strength of biological carbon assimilation? Is it constant or variable?

- Page 13, line 12 : Why not add a figure of sea ice cover in the supplement? Also, can you separate the effects of sea ice cover expansion from the other physical changes in terms of CO₂? A few studies since the Stephens & Keeling (2000) paper have found that an increase in sea ice cover under glacial conditions actually reduces ocean carbon storage because it prevents organic carbon production. It would be worthwhile to separate this effect from temperature and circulation and note if it is positive or negative on atmospheric CO₂. Stephens & Keeling (200) The influence of Antarctic sea ice on glacial-interglacial CO₂ variations. *Nature*. Kurahashi-Nakamura et al (2007) Compound effects of Antarctic sea ice on atmospheric pCO₂ change during glacial-interglacial cycle. *Geophysical Research Letters*. Sun & Matsumoto (2010) Effects of sea ice on atmospheric pCO₂: A revised view and implications for glacial and future climates. *Journal of Geophysical Research*. Buchanan et al (2016) The simulated climate of the Last Glacial Maximum and insights into the global marine carbon cycle. *Climate of the Past*.
- Page 13, lines 10-23 : This paragraph would benefit from being clearer in its findings of CO₂ sequestration regarding C:P ratio changes. I have to read this multiple times to understand what the authors are trying to say when comparing Redfield, variable C:P and C:P=121.

Discussion

- Page 13, line 28 : your reference to “model” should be an “empirical model” to avoid confusion with the Earth System Model, GCM, etc.
- Page 14, lines 1-5 : The advantages of using empirical/statistical models within biogeochemical ocean GCMs, including the Galbraith & Martiny (2015) parameterisation, was explored rigorously in my 2018 paper in *Global Biogeochemical Cycles*. It not only improved that model’s biogeochemistry significantly, but also altered the long-term behaviour of the carbon cycle as you have also found. It may be interesting, but I of course leave it up to you whether it’s useful. Buchanan et al (2018) The importance of dynamic biological functioning for simulating and stabilizing ocean biogeochemistry. *Global Biogeochemical Cycles*.
- Page 14, line 32 : the conclusions of Odalen et al (2018)... which were? What did Odalen et al (2018) do?
- Page 15, lines 19-24 : So the proportion of remineralised to preformed phosphorus effectively doesn’t change in the simulations? And yet, you find a large increase in respired C? This must mean that the remineralised phosphorus that is exported into the ocean interior in your *GLcomb* simulation is being quickly circulated into the lower overturning cell and returned to the Antarctic Zone, where sea ice prevents gas exchange and biological production, at which point this P is recirculated and becomes preformed, while respired C remains respired and is also recirculated. If this is the case, it merits more discussion in comparison with previous literature on the subject of a more efficient biological pump that invokes more regenerated nutrients as a *must* for a more efficient biological pump. I suggest Hain, Sigman & Haug (2014) The biological pump in the past. *Treatise on Geochemistry, 2nd Ed.*
- Page 16, lines 1-14 : I would like to see how variations in P:O₂ requirements were treated in this model. Also, can you please explain why the deep water formation characteristics of a model affects O₂? Overall, I find this section a bit sparse and I’m not entirely sure what the point of it is. Can you comment on the size of the OMZs? New evidence shows that

the OMZs in the Pacific expanded vertically during the glacial. Hoogakker et al (2018) Glacial expansion of oxygen-depleted seawater in the eastern tropical Pacific. *Nature*.

- Page 16, line 22 : And yet others achieve relatively good correlations in the Pacific basin using the same data? Menviel et al (2017) Poorly ventilated deep ocean at the Last Glacial Maximum inferred from carbon isotopes: A data-model comparison study. *Paleoceanography*. Muglia et al (2018) Weak overturning circulation and high Southern Ocean nutrient utilization maximized glacial ocean carbon. *Earth and Planetary Science Letters*. I think you cannot say that your model doesn't provide good fits to the LGM proxy data because of poor data coverage, and it would be more useful to discuss why the model doesn't fit with the data. It seems like your glacial circulation in the Pacific basin is therefore not accurate?
- Page 16, line 26 : Temperature is not chemical. I also do not understand how you could alter the temperature and salinity of the ocean without altering water mass distributions, and if this is indeed the case, it requires further description as to why earlier in the paper. Also a good spot to talk about why the data in the Pacific are not well reproduced by the model.
- Page 16, lines 24-31 : I don't follow this paragraph. You state that "Each of the two observational datasets (HOL and LGM) display similar correlations across the two model simulations. This implies that our changes in forcings do not achieve any obvious changes in water mass distribution." But doesn't the distribution of $\delta^{13}\text{C}$ change across the glacial and into the Holocene? $\delta^{13}\text{C}$ in the Atlantic, for instance, is often used to show that the Atlantic meridional overturning was shallower during the glacial, and that this change occurred during Marine Isotope Stage 4 (Oliver et al (2010) A synthesis of marine sediment core $\delta^{13}\text{C}$ data over the last 150000 years. *Climate of the Past*)? Moreover, $\delta^{13}\text{C}$ is used as a way to show that the water mass distribution between the Atlantic and Pacific was considerably different during the glacial as compared to the Holocene (Sikes et al (2017) Enhanced $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ Differences Between the South Atlantic and South Pacific During the Last Glaciation: The Deep Gateway Hypothesis. *Paleoceanography*.) These studies conflict with what you are saying.
- The ability for simulated $\delta^{13}\text{C}$ to reproduce the proxy data at the LGM will depend strongly on water mass distribution (which apparently doesn't change appreciably) and how biological fractionation is parameterised. If it is constant, the 10% loss in C fixation will cause the ocean to be more positive overall by some constant factor. However, if the parameterisation contains a dependence on aqueous CO_2 and growth rate, both of which are lower, then the fractionation will vary. It would be worthwhile telling the reader what parameterisation is used and, if it does involve growth rate and aqueous CO_2 , what effect this has.

Conclusion

- No comments

3 Technical corrections

1. Page 1, line 13 : remove repeated "with"
2. Page 5, line 16 : replace "reduced half" with "halved"

3. Table 1 : “witg” to “with”
4. Page 11, line 8 : Inadvisable to begin a sentence with “~”
5. Page 11, line 29 : “SV” to “Sv”
6. Page 13, line 26 : “GCMs” this acronym has not been defined previously.
7. Page 15, line 23 : I assume you mean “0.003” rather than “0003”?