Biogeosciences
Discussions

Open Access

BGD

Interactive
comment

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

Malin Ödalen et al.

malin.odalen@misu.su.se

Received and published: 14 October 2019

We thank Pearse James Buchanan (henceforth PJB) for a thourough and helpful review of our paper. In this response, we will respond to the comments by the referee in the order they were presented in the referee report.

1   General comments

As suggested by PJB, we will work to make the writing clearer. Specifically, PJB points out the common usage of e.g. mid–sentence, and questions whether this is intentionally used *"to not discuss all the evidence/processes/knowledge on a subject"*. This was not our intention, but rather an attempt to avoid the text getting too long. This is why we decided in some cases to only bring up the most relevant examples. We will go through these parts of the text in detail, and add more information.

Both Referee #1 and PJB find the discussion of Section 4.2 (Implications of changed average C/P) unclear, and both give useful suggestions on how to clarify this section. The section will thus be re–written based on these suggestions.

PJB details how the Introduction could be extended with a more extensive discussion of concepts, in order to help the reader. We agree that this could be helpful, and we will make additions according to PJB's suggestions.

2   Specific comments

2.1   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.***
  Yes, "decreases" should be corrected to "increases", and "nutrients" to "phosphorus" (as we are discussing elemental ratios).

2.2 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.**
  We agree and will add this information, along with relevant references.

- **Page 2, line 5 : What is the e.g. here referring to?**
  This is a formatting issue. The *e.g.,* should be placed before the reference (Broecker, 1982a), which was intended to be given as an example of a key study for the role of ocean sedimentary processes for increased glacial oceanic storage of carbon.

2.3 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.**
  We thank PJB for the suggestion and will add this reference.

- **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.**
  We thank PJB for the suggestions and add the reference to Moore et al. (2013) here. The paper by Garcia et al. (2018) is certainly relevant to our work, and this reference will be added already in Section 2.2 (Page 3).

- **Page 6, line 17 : "see Table ??"... Please make sure your document is**

properly formatted before submitting.
We apologise for the formatting error. *Table ??* should be corrected to *Table 2*.

- *Page 6, line 22 : Why not use more recent WOA 2018 product?*
  When the paper was submitted, WOA18 was only available as a pre–release. We therefore chose to stay with the most up–to–date official release for the initial submission. The official release is now available, and we will therefore use WOA18 for the updated manuscript.

- *Page 7, line 13 : Again a question mark is present. Please format properly before submitting.*
  We are very sorry for having overlooked the formatting errors in our final check before submission. The missing reference marked by *(?)* should be (Ödalen et al, 2018).

  2.4   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 O_2 to remineralise that organic matter. I would like an explanation of why O_2 is higher in $Ctrl_{121}$ than for $Ctrl_{RED}$. See Paulmier, Kriest & Oschlies (2009): Stoichiometries of remineralisation and denitrification in global biogeochemical ocean models. Biogeosciences.*
  We thank PJB for having identified this inconsistency in our results, which we had failed to notice. This was caused by an issue in the code, which caused an unintentional change in O_2:C when the fixed C:P stoichiometry was changed. This issue was not present in the runs with flexible stoichiometry, and thus only affected the O_2 in the 121–ensemble. We have re–run $Ctrl_{121}$ after having corrected the model code. The new resulting average O_2-concentration is 152 $\mu mol kg^{-1}$, which is lower than $Ctrl_{RED}$ (166 $\mu mol kg^{-1}$), but higher than $Ctrl_{GAM}$ (144 $\mu mol kg^{-1}$).

- *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.*
  In line with PJBs suggestion in the General comments, this will be described in more detail in the Introduction, based on the results of Matsumoto (2007).

- *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.*
  We agree with PJB and in order to clarify the sentence, we will change it to:
  About a third (~ 10 % of 30 %) of the increase in drawdown can be explained by a change in average C/P composition of the organic material that is exported out of the surface ocean. This will then be discussed in more detail in Section 4.2, where we will gather all the results and discussion that concerns the $121-$ ensemble (as suggested by Referee #1).

- *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.*
  We will add a note on this, and clarify that we are not applying all forcings that are expected to be needed to reproduce a full glacial state.

- *Page 12, lines 21–25 : Again, I am unsure how you are treating P:O$_2$ 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.*
  In remineralisation, there was an issue in the code which affected the C:O$_2$ ratio in the 121–experiments. This made the P:O$_2$ requirements appear strange, which

C5

PJB noticed. This has been corrected, and the 121–experiments re–run. After correction, C:O_2 remains the same in all experiments, while the P:O_2 ratio, as a result, changes between experiments. We will clarify this in the Methods section. After the correction described abobe, $GLcomb_{121}$ has a lower global average O_2 concentration than $GLcomb_{RED}$ (96 compared to 122 $\mu mol kg^{-1}$), but higher than $GLcomb_{GAM}$ (74 $\mu mol kg^{-1}$).

- ***Figure 5 : The panels in this figure do not seem to be arranged correctly.***
  We are grateful to both referees for having identified this error. We here provide the updated figure (see Fig. 1), where the sub–panels have been re–arranged in the correct order. Per request of Referee #1, we will also clarify the caption of this figure (see Section 5).

- ***Page 13, lines 1–8 : What conditions affect the fractionation strength of biological carbon assimilation? Is it constant or variable?***
  The fractionation strength is variable and will be detailed in an appendix (see below, in author's response to the final bullet point of Discussion).

- ***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 CO_2? 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 CO_2. Stephens & Keeling (2000) The influence of Antarctic sea ice on glacial–interglacial CO_2 variations. Nature. Kurahashi–Nakamura et al. (2007) Compound effects of Antarctic sea ice on atmospheric pCO_2 change during glacial–interglacial cycle. Geophysical Research Letters. Sun & Matsumoto (2010) Effects of sea ice on atmopsheric pCO_2: A re-***

vised 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.

We will add a figure of the sea ice anomaly ($GLcomb$-$Ctrl$) to the supplement. The separation of the effects of sea ice cover expansion from the other physical changes would be highly interesting to isolate. However, we find them to be beyond the scope of this paper, and we wish to keep this paper focused on the effects of flexible C/P on pCO_2. If PJB is interested in the separation of physical effects in cGENIE, this is explored in Ödalen et al. (2018). There, we want to point specifically to the simulation $AD/2$ in Figure 2, where panel e) details the contributions from the changes in each of the different carbon capture processes to the net change in pCO_2. The simulation $AD/2$ has a reduced biological pump compared to the pre–industrial control, but an enhanced carbon capture due to increased disequilibrium and saturation carbon. This is attributed mainly to ocean cooling and a resulting expansion of sea ice. However, no further separation of the effect of sea ice was made.

- *Page 13, lines 10–23 : This paragraph would benefit from being clearer in its findings of CO_2 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.*

  This paragraph will be clarified by concentrating all the results and discussion concerning C/P = 121 to Section 4.2, as suggested by Referee #1. We are confident that this paragraph will become clearer when it is rewritten to focus only on the difference in CO_2 sequestration between Redfield (RED) and flexible C/P (GAM).

2.5 Discussion

- **Page 13, line 28 : your reference to "model" should be an "empirical model" to avoid confusion with the Earth System Model, GCM, etc.**
  Agreed. We will add "empirical".

- **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.**
  We thank PJB for pointing us to this paper, and find the conclusions very interesting. They are well aligned with the study we perform in this paper, and by citing this paper, we will strengthen our arguments. We will thus include the suggested paper in our discussion.

- **Page 14, line 32 : the conclusions of Odalen et al (2018)... which were? What did Odalen et al (2018) do?**
  Here, lines 30–33 should read: *Note that $Ctrl_{GAM}$ has a larger inventory of DIC, as well as C_{soft}, compared to $Ctrl_{RED}$. Ödalen et al. (2018) found that drawdown of CO_2 in response to a perturbation is larger when the control state has a smaller inventory of DIC and C_{soft}. Yet, the effect of applying the same perturbation results in a larger drawdown of CO_2 in GAM than in RED. This is thus opposite of the conclusions of Ödalen et al. (2018). The reason is that the flexible stoichiometry in effect increases the drawdown potential, which more than compensates for the increased carbon inventory in the control state.* As stated

above, Section 4.2 will be re–written, and clarified. In this way, we will assure that the conclusions of Ödalen et al. (2018) are clearly stated.

- ***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.***
The referee has correctly identified that, depsite the fact there is no change in remineralised P (P_{rem}) in $GLcomb_{GAM}$ compared to $Ctrl_{GAM}$, there is an increase in remineralised C. However, the process described by the referee focuses on what happens after remineralisation, while we suggest that this decoupling happens before the organic material is exported to the deep ocean (see lines 25–31). The forcing components applied to $GLcomb_{GAM}$ have competing effects on the amount of organic matter that remineralises in the deep ocean. The net effect is that this amount does not change globally (reflected by a constant P_{rem} compared to $Ctrl_{GAM}$). Meanwhile, changes in ocean circulation, remineralisation depth and dust deposition still cause the local nutrient availability in the surface waters to change. This affects the elemental composition of the exported organic material. In $Ctrl_{GAM}$, the average elemental C/P composition is 121/1. In $GLcomb_{GAM}$, this average is 134/1. This means that even though the

same amount of P is exported to the deep ocean, the organic molecules carry more carbon, which is released in the deep ocean during remineralisation. In $Ctrl_{GAM}$, the global average concentration of P_{rem} is 1.16 $\mu mol kg^{-1}$ (c.f. 1.17 $\mu mol kg^{-1}$ in $GLcomb_{GAM}$). By increasing the average C/P composition of 1.16 $\mu mol kg^{-1}$ organic molecules from 121 to 134 (i.e. by 13 units), this causes an increase in C_{rem} by ~ 15 $\mu mol kg^{-1}$, which corresponds to the observed increase in C_{rem}. In summary, we suggest this is a result of changes in surface P fields (see Fig. 7), rather than a change in the partitioning between C_{rem} and P_{pre} in the recirculation area in the Antarctic Zone. We will clarify this part of the discussion, which comprises lines 19–31 on page 15.

- **Page 16, lines 1–14 :**
  Here, each of the questions will be treated separately.

  - **I would like to see how variations in P:O_2 requirements were treated in this model.**
    C:O_2 requirements were meant to be held constant throughout the simulations, consequently causing changes in P:O_2. However, an inconsistency in the code caused C:O_2 requirements to change in the simulations of the 121–ensemble, causing an inconsistent behaviour of P:O_2. This has been corrected, as outlined above.

  - **Also, can you please explain why the deep water formation characteristics of a model affects O_2?**
    Deep water formation characteristics of a model affects the amount of time available for remineralisation and, consequently, the oxygen consumption. In addition, due to a lack of resolution deep water formation in climate models generally happens as open water convection, rather than as dense plumes along slopes. This causes too much oxygen to be entrained into the deep ocean. We will add this explanation in the updated manuscript.

– *Overall, I find this section a bit sparse and I'm not entirely sure what the point of it is.*
The section aims to discuss 1) to what extent our $GLcomb$ simulations reproduce proxy observations, in this case for O_2, and 2) to discuss one of the problems that arose from applying the flexible C/P in GENIE, i.e. that O_2 concentrations in $Ctrl_{GAM}$ are too low, and its implications for the glacial–like simulation. We agree that the section could be expanded, and we will add the information requested by PJB to make the discussion more comprehensive.

– *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*
In the Atlantic, $Ctrl_{GAM}$ (Fig. 4 e) reproduces the observed extent of the OMZ (Fig. 4 a) better than $Ctrl_{RED}$ (Fig. 4 c) does. In the Pacific Ocean, the O_2 gradient in the observations (Fig. 4 b) is more gradual compared to that of the control states (Fig. d, f), but the core of the OMZ is well reproduced by the model. The forcings applied to $GLcomb$ are not sufficient to reproduce a full glacial state (see also author's response to the next main point, regarding Page 16, line 22). Still, we do get a vertical expansion of the OMZ in $GLcomb_{RED}$ (Fig. 4 h) compared to $Ctrl_{RED}$ (Fig. 4 c), in agreement with the findings of Hoogakker et al (2018). In $GLcomb_{GAM}$, oxygen depletion is too extensive, but the tendency of vertical expansion compared to the control state is present here as well.

• *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?

The forcings applied to $GLcomb$ are factors that are likely to be important for the glacial ocean circulation and biogeochemistry. However, these forcings are not sufficient to reproduce a full glacial state (i.e. the use of the term "glacial–like simulations", rather than "LGM simulation"). Other forcings that have shown to be important for modelling of glacial $\delta^{13}C$ are, for example, brine rejection (Bouttes et al., 2010; Bouttes et al., 2011), and freshwater forcing (e.g., Schmittner et al., 2002; Hewitt et al., 2006; Bouttes et al., 2012). The fact that some important forcings are missing (mentioned on lines 27–28) is likely the main cause for the model–data discrepancy, and the reason for why we do not achieve an accurate glacial Pacific Ocean circulation. This will be clarified in the section. We will also clarify, throughout the paper, the fact that we do not aim to produce a full LGM state. This may also call for adjusting the title of the paper.

- ***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.***
  We agree that temperature in itself is not chemical. Here, we were referring to the changes in solubility of CO_2, which is a chemical response to changes in temperature. This will be clarified in the revised version of the paper. The water mass distribution in cGENIE is strongly constrained by the resolution of the model, especially in the vertical. Changes in temperature and salinity that should cause

changes in water mass volume may not be sufficient to allow a water mass to extend to the next vertical level of the model. As a consequence, while the gradient between water masses may become more or less pronounced, the interface of water masses may still remain at the same depth. The section will be clarified, including the above description of why Pacific glacial circulation is not fully reproduced.

- *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 $\delta^{13}C$ change across the glaciation and into the Holocene? $\delta^{13}C$ 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 $\delta^{13}C$ data over the last 150000 years. Climate of the Past.)? Moreover, $\delta^{13}C$ 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 $\delta^{13}C$ and $\delta^{18}O$ 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 proxy data do imply a change in $\delta^{13}C$ across the deglaciation (whole ocean change 0.34 ± 0.19 ‰, Peterson et al., 2014). What we are trying to say is that our model simulations do not fully reproduce this change. Here we are referring to the fact that the correlation of the HOL proxy records with $Ctrl_{RED}$, $Ctrl_{GAM}$, $GLcomb_{RED}$, and $GLcomb_{GAM}$, is in all cases between 0.76–0.78. On the other hand, the correlation of LGM proxy records with the same four simulations is in all cases between 0.55–0.58. As our $GLcomb$–simulations still correlate so well

with the HOL dataset, this suggests the applied forcings have not caused these simulations to be clearly different from $Ctrl$ in terms of water mass distribution. For the same reason, the correlation with LGM proxy data does not significantly improve from $Ctrl$ to $GLcomb$. Thus, the deglacial change in $\delta^{13}C$ reflected in the proxy data is not fully captured by the model. This will be clarified in the updated version of the manuscript.

- *(Section 4.5 ?): The ability for simulated $\delta^{13}C$ 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.*

  The fractionation is dependent on both aqueous CO_2 and growth rate (represented in K_Q, see appendix). This dependence and its consequences will be detailed by adding the following text in an Appendix (see Section 2.6)

2.6   Appendix: $\delta^{13}C$ in cGENIE

cGENIE represents ^{13}C as an explicit tracer (separate from and in addition to bulk carbon) in the model, tracking its concentration in all the same gaseous, dissolved, and solid forms that carbon exists in, reporting $\delta^{13}C$ in ‰ relative to the standard VPDB. The current scheme is based on that described in Ridgwell (2001) and updated as described in Ridgwell et al. (2007), and is evaluated for the modern ocean (alongside simulated $\Delta^{14}C$) in cGENIE, in Kirtland–Turner and Ridgwell (2016).

In the aqueous phase, the isotopic partitioning of carbon between $CO_2(aq)$, HCO_3^-, and CO_3^{2-} is resolved and follows Zeebe and Wolf–Gladrow (2001) (their Section 3.2). The empirical fractionation factors used are from Zhang et al. (1995). The air–sea fractionation scheme follows that of Marchal et al. (1998) with the individual fractionation factors again taken from Zhang et al. (1995).

For the isotopic composition of organic carbon ($\delta^{13}C_{POC}$), the model of Rau et al. (1996) is adapted, assuming that the isotopic signature of exported POC reflects that of phytoplankton biomass. Following Ridgwell (2001), the full equation of Rau et al. (1996) is simplified to:

$$\delta^{13}C^{POC} = \delta^{13}C^{CO_{2(aq)}} - \epsilon_f + (\epsilon_f - \epsilon_d) \cdot \frac{K_Q}{[CO_{2(aq)}]} \tag{1}$$

where [CO_2(aq)] is the ambient concentration of aqueous CO_2 and $\delta^{13}C_{aq}$ is its isotopic composition. K_Q is a temperature–only dependent approximation of the full cell–dependent size and growth rate parameterization in the Rau et al. (1996) model (see Ridgwell, 2001). We take an intermediate value for the enzymatic isotope fractionation factor associated with intracellular C fixation (ϵ_f) of -25‰ following Rau et al. (1996,1997), and assume a temperature–invariant value for ϵ_d of 0.7 ‰.

The result of applying this scheme in cGENIE, is a zonal mean profile characterized by $\delta^{13}C_{POC}$ of -22 to -21 ‰ in the tropics, declining with increasing latitude to reach -28 to -30 ‰ in the Southern Ocean. This latitudinal pattern is comparable to measurements made on suspended particulate organic matter as discussed in Ridgwell (2001).

For ^{13}C fractionation into biogenic carbonates at the ocean surface (e.g. foraminiferal tests, and coccolithophorid coccoliths), cGENIE follows Mook (1986) and employs a simple temperature–dependent fractionation between the $\delta^{13}C$ of aqueous HCO_3^- and calcite.

3   Technical corrections

- ***Page 1, line 13 : remove repeated "with"***
  We will correct this typo.

- ***Page 5, line 16 : replace "reduced half" with "halved"***
  The sentence will be changed according to the suggestion.

- ***Table 1 : "witg" to "with"***
  We will correct this typo in the caption of Table 1.

- ***Page 11, line 8 : Inadvisable to begin a sentence with "$\sim$"***
  The sentence has been rewritten (see above comment for Page 11, line 9).

- ***Page 11, line 29 : "SV" to "Sv"***
  We will change "SV" to "Sv" throughout the manuscript.

- ***Page 13, line 26 : "GCMs" this acronym has not been defined previously.***
  We will define the acronym here.

- ***Page 15, line 23 : I assume you mean "0.003" rather than "0003"?***
  Yes, we will correct this to 0.003.

4   References

Bouttes, N., Paillard, D., and Roche, D.: Impact of brine–induced stratification on the glacial carbon cycle, Climate of the Past, 6, 575–589, 2010.

Bouttes, N., Paillard, D., Roche, D. M., Brovkin, V., and Bopp, L.: Last Glacial Maximum CO_2 and δ^13C successfully reconciled, Geophysical Research Letters, 38, 2011.

Bouttes, N., Roche, D. M., and Paillard, D.: Systematic study of the impact of fresh water fluxes on the glacial carbon cycle, Climate of the Past, 8, 589–607, https://doi.org/10.5194/cp-8-589-2012, https://www.clim-past.net/8/589/2012/, 2012.

Hewitt, C. D., Broccoli, A., Crucifix, M., Gregory, J., Mitchell, J., and Stouffer, R.: The effect of a large freshwater perturbation on the glacial North Atlantic Ocean using a coupled general circulation model, Journal of Climate, 19, 4436–4447, 2006.

Marchal, O., Stocker, T. F., and Joos, F.: A latitude-depth, circulation-biogeochemical ocean model for paleoclimate studies. Development and sensitivities, Tellus B: Chemical and Physical Meteorology, 50, 290–316, 1998.

Matsumoto, K.: Biology-mediated temperature control on atmospheric pCO2 and ocean biogeochemistry, Geophysical Research Letters, 34, 2007.

Mook, W.: 13C in atmospheric CO2, Netherlands Journal of Sea Research, 20, 211–223, 1986.

Ödalen, M., Nycander, J., Oliver, K. I. C., Brodeau, L., and Ridgwell, A.: The influence of the ocean circulation state on ocean carbon storage and CO_2 drawdown potential in an Earth system model, Biogeosciences, 15, 1367–1393, https://doi.org/10.5194/bg-15-1367- 2018, https://www.biogeosciences.net/15/1367/2018/, 2018.

Peterson, C. D., Lisiecki, L. E., and Stern, J. V.: Deglacial whole-ocean δ^13C change estimated from 480 benthic foraminiferal records, Paleoceanography, 29, 549–563,

2014.

Rau, G., Riebesell, U., and Wolf–Gladrow, D.: CO_{2aq}–dependent photosynthetic ^{13}C fractionation in the ocean: A model versus measurements, Global Biogeochemical Cycles, 11, 267–278, 1997.

Rau, G. H., Riebesell, U., and Wolf–Gladrow, D.: A model of photosynthetic ^{13}C fractionation by marine phytoplankton based on diffusive molecular CO_2 uptake, Marine Ecology Progress Series, 133, 275–285, 1996.

Ridgwell, A., Hargreaves, J. C., Edwards, N. R., Annan, J. D., Lenton, T. M., Marsh, R., Yool, A., and Watson, A.: Marine geochemical data assimilation in an efficient Earth System Model of global biogeochemical cycling, Biogeosciences, 4, 87–104, 2007.

Ridgwell, A. J.: Glacial-interglacial perturbations in the global carbon cycle., Ph.D. thesis, University of East Anglia, 2001.

Schmittner, A., Meissner, K., Eby, M., and Weaver, A.: Forcing of the deep ocean circulation in simulations of the Last Glacial Maximum, Paleoceanography, 17, 5–1, 2002.

Turner, S. K. and Ridgwell, A.: Development of a novel empirical framework for interpreting geological carbon isotope excursions, with implications for the rate of carbon injection across the PETM, Earth and Planetary Science Letters, 435, 1–13, 2016.

Zeebe, R. E. and Wolf–Gladrow, D. A.: CO_2 in seawater: equilibrium, kinetics, isotopes, 65, Gulf Professional Publishing, 2001.

Zhang, J., Quay, P., and Wilbur, D.: Carbon isotope fractionation during gas-water exchange and dissolution of CO2, Geochimica et Cosmochimica Acta, 59, 107–114, 1995.

5 Figures

5.1 Fig. 5 - updated caption

Model ocean $\delta^{13}C$ (contours) compared to the two proxy record time slices (HOL and LGM) of benthic $\delta^{13}C$ (circles) of Peterson et al. (2014). The upper half of the figure shows the Atlantic Ocean (panels a–d), while the lower half shows the Pacific Ocean (panels e–h). The columns represent the model simulations ($Ctrl_{RED}$ or $Ctrl_{GAM}$), while each row represents one of the proxy record time slices (HOL or LGM). The left hand column shows $Ctrl_{RED}$ (panels a, c, e, g), and the right hand column shows $GLcomb_{RED}$ (panels b, d, f, h). The rows show, from top to bottom, a–b) HOL Atlantic, c–d) LGM Atlantic, e–f) HOL Pacific, g–h) LGM Pacific. Note that, before we compare $GLcomb_{RED}$ to LGM observations (panels d and h), a constant of 0.32‰ is subtracted from the simulated $\delta^{13}C$, to account for terrestrial release of $\delta^{13}C$–depleted terrestrial carbon which is not modelled. The corresponding comparison for model version GAM is shown in Fig. S.3.
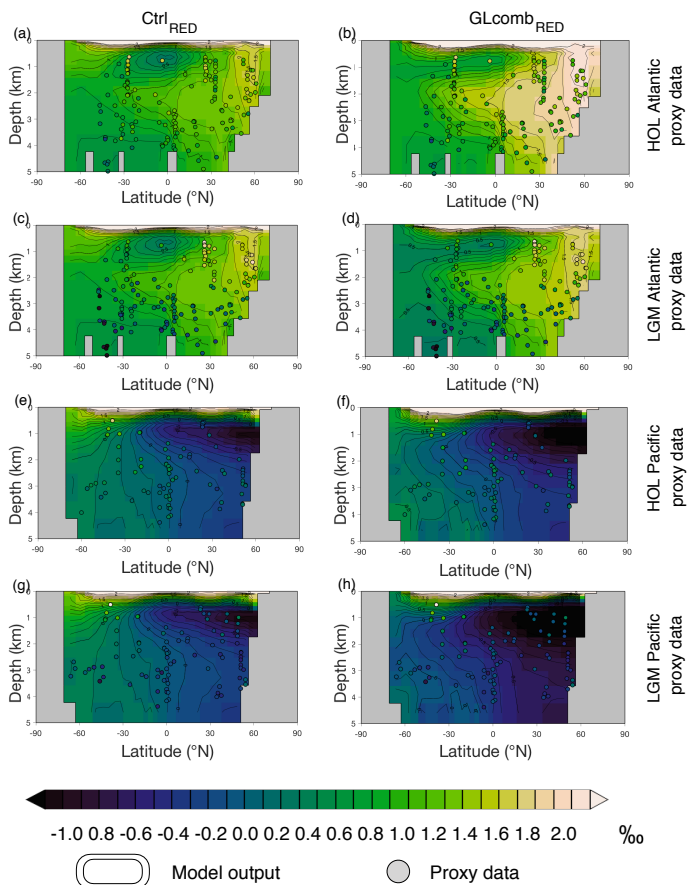
Fig. 1. Updated version of Fig. 5. The caption of Fig. 5 is updated according to the text on the previous page.