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Interactive comment on "Ocean carbon and nitrogen isotopes in CSIRO Mk3L-COAL version 1.0: A tool for palaeoceanographic research" *by* Pearse J. Buchanan et al.

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Response to Reviewer 3

Reviewer 3 provided helpful suggestions and some very encouraging comments regarding the writing and choice of experiments. Although they had no major concerns with the work, they had minor suggestions that have been helpful to improve the manuscript.

— Page 4, lines 4-5: Does running with the offline OGCM restrict experiments to steady-state / timeslice experiments? What is the speed when the OGCM is online (relevant for paleo experiments)?

Rewritten. We added the following in parentheses: "(compared to ∼10 years per day in fully coupled mode)."

— Page 4, lines 25-27: I found the term "phytoplankton functional types" confusing as this usually refers to ecological models that explicit plankton biomass state variables whereas this model parameterises the biological transformations of biogeochemical tracers (e.g. Hulse et al., 2017).

Rewritten. The sentence containing "phytoplankton functional types" has been replaced with a new sentence.

This is: "Briefly, the ecosystem model simulates the production, remineralisation and stoichiometry (elemental composition) of a general phytoplankton group, diazotrophs (N2 fixers) and calcifiers."

— Figure 1: PGorg and PDorg have not been defined so were unclear until I had read more of the manuscript.

Rewritten. Number 3 is now: "Biological uptake of nutrients and production of organic and inorganic matter. Particulate organic carbon (POC) is produced by the general phytoplankton group and N2 fixers (diazotrophs), while particulate inorganic carbon (PIC) as calcium carbonate (CaCO3) is produced by calcifiers. Export of POC by the general (G) phytoplankton group and N2 fixers (D) are herein referred to as CGorg and CDorg (see appendix A1), respectively."

— Page 10, lines 1-2: There are other isotope enabled earth system models (e.g. Hulse et al, 2017, Understanding the causes and consequences of past marine carbon cycling variability through models, Earth Science Reviews, 171, pp. 349-382) but I guess these are those with comparable resolution or similar?

We acknowledge that there are other isotope enabled models out there that include box models and Earth System Models of Intermediate Complexity, but we choose to

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restrict our comparison to other ocean general circulation models in this instance.

We will clarify that we have chosen GCMs specifically and that our comparison is not exhaustive. “We make these model-data comparisons alongside other isotope-enabled ocean general circulation models, for which we chose a non-exhaustive selection (Table 1).”

— Page 10, lines 5-6: I do not really understand what this sentence: “. . .because many solutions were cumulatively run for many tens of thousands of years over the full course of development”.

Rewritten. To reduce confusion as to what this means, we have altered the text to convey the important information.

“Each experiment was run towards steady-state under pre-industrial atmospheric conditions. All results presented in this paper therefore reflect tracers that have achieved an equilibrium solution. We present annual averages of the equilibrium state in the following analysis.”

— Page 10, lines 20-23: Is there oxygen-dependent remineralisation in the model affecting this? If so, this could be stated more explicit here, perhaps linking to the relevant part of the appendices.

Oxygen-dependent remineralisation is included in the model. We chose to conserve oxygen, nitrate and organic matter in the treatment of remineralisation in all situations, with lots of O2 or no O2. So, when there is no O2, denitrification occurs, but some organic matter will go unremineralised and will fall into the grid cell below.

Conservative treatment of organic matter remineralisation in low O2 zones therefore causes a vertical expansion of the oxygen minimum zones. However, there are many reasons to suspect that the coarse resolution ocean model is not adequately resolving the complex tropical ocean currents, and this is the true cause of the unrealistic expansion of the OMZs. In contrast, there is no reason to suspect that the rates of export production in the tropics are too large and driving too great oxygen demand. Moreover, the choice to conserve organic matter remineralisation is mechanistically important for paleoclimate simulations where different conditions evolve.

However, we acknowledge that this choice to conserve oxygen is causing a vertical expansion of the OMZs. “Alternatively, the expansion oxygen minimum zones could be due to our conservative treatment of organic matter remineralisation (appendix A), where remineralisation will not occur when O2 and NO3 are limiting. Excess, unremineralised organic matter therefore falls deeper in the model in the oxygen-deficient zones.”

— Page 11, lines 4-6: It’s also possible that the model is missing something. An alternative approach here might be to force the model with anthropogenic CO2 and explicitly account for the Suess effect?

We argue that this is outside the scope of this paper. However, future work will involve historical and future scenarios that will explicitly account for the Suess effect, and also paleoclimate experiments where atmospheric δ13CO2 is different.

— Page 11, lines 6-7: Please elaborate on the reason why it may be an overestimate in the lower latitudes. Rewritten.

“It is also equally possible that our fixed biological fractionation of 21 ‰ may be an overestimate in highly productive tropical regions where high growth rates lower the fractionation factor towards 15 ‰ (Laws et al., 1995).”

However, given the concerns of both this reviewer and reviewer 2 (Christopher Somes), we have begun new experiments that include variable biological fractionation, where values between 15 and 25 ‰ are dynamically simulated according to growth rate and [CO2]aq. These experiments will take a few weeks / one month to complete, and so we ask for another month to integrate their results within the paper if the reviewers / editor think this would be a beneficial or necessary addition.

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— Tables 1&2: I find it difficult to really comprehend the comparisons in this table format. You could alternatively plot the data on Taylor Diagrams (so keeping the table data on correlation on one axis and the mean-normalised RMSE as the straight line distance) alongside Target diagrams to include the mean. See Jolliff et al., (2009) Summary diagrams for coupled hydrodynamic-ecosystem model skill assessment. Journal of Marine Systems. 76 (1-2), pp. 64-82.

We have remade both the nitrogen and carbon isotope figures into Taylor Diagrams to better convey the model skill. See figures 2 and 5.

The original tables have been altered to only convey the global and regional means.

We have also included the CESM in the nitrogen isotope comparison. Although we know that carbon isotopes are available for this model, we have yet to hear back from authors of prior studies, and the data is not available online.

— Page 12, lines 3-4: "…suggests that the upper ocean values between 200 and 500 metres of Eide et al. (2017) are too low." Or alternatively there are structural errors common to all models?

We argue that the values of the Eide reconstruction are almost certainly too low. Observations and models both produce values in excess of 2 per mille in the upper ocean. We have added these figures to the supplementary material, and also added a new sentence to the discussion that addresses why this might be the case (see response to reviewer 2). Briefly, the introduction of depleted values via biology is likley biasing the Eide reconstruction too low, even though these authors made attempts to eliminate the suess effect.

— Section 4.2: Of the manuscript, I struggled with this section the most. Firstly, I was not familiar with the Schmittner paper itself and I had to go read it to find out what I needed. Secondly, I'm not sure what extra I have learned here other than the mismatches in Fig. 3 are related to mismatches between modelled DIC and observed DIC, which is not really surprising. I think the section could be improved if it included a brief description of the Schmittner calibration and a brief discussion about the challenges of relating the measures forma isotopes and the model output if this is an intended use of the model in the future.

Rewritten. Hopefully the adjustments made to the following paragraph are sufficient, but if not, then please advise.

"We extended our assessment of modelled del13CDIC by comparing it to a compilation of benthic del13C values taken from the foraminiferal genus Cibicides (Schmittner et al., 2017), a genus on which much of the palaeoceanographic del13C records are based. For this comparison, we adjusted our predicted del13CDIC using the linear dependence on carbonate ion concentration and depth suggested by Schmittner et al. (2017): del13CCib = 0:45 + del13CDIC – 2.2x10-3 * CO3 – 6.6x10-5 * z This adjustment is necessary because the incorporation of DIC into foraminiferal tests is altered by the concentration of CO3 ions and pressure, such that a one to one comparison between del13CDIC and del13CCib introduces error. By adjusting our three-dimensional del13CDIC output using Eq. (19), we thus attain predicted del13CCib.We also computed measures of statistical fit for a traditional one to one comparison between del13CDIC and del13CCib to assess the benefit of the calibration."

— Page 12, eqn 18: How variable are the depths of the Cibicides d13C observations? When binning the data to the model grid, do you weight the averages by depth. I'm curious about what error could be introduced if say you compared the d13C calculated using eqn 18 with a mid-depth of the model grid-box in the equation that is 100 m in the depth for example, if the regridded observations fell predominantly in the upper part of the depth range.

The correction of modelled DI13C uses a depth dependent term of 6.6x10-5. Thus, at a depth of 1000 meters, the depth term becomes 0.066 per mille. At 3000-4000 metres it only just begins to be significant at 0.2-0.26 per mille. So first we argue that

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the depth-dependent term is not the significant effect of the calibration throughout the upper ocean, compared with the CO3 term which is more of the order of 0.2 when CO3 is at 100 mmol/m3 and the constant of 0.45. Second, we argue that taking the bottom, top or mid-depth point of the ocean grid box as the depth used in the correction would have negligible effect on the fidelity of our model-data comparison. We say its negligible because using even the tallest boxes of 450 metres would generate a difference of 0.03 per mille in our model-data comparison.

— Page 18: It would help to briefly outline the reasons behind the trends in C:P and N:P when using the variable stoichiometry.

Rewritten. We have added the following text after the first sentence of this section:

“Organic matter had more carbon and nitrogen per unit phosphorus in regions with low PO4, such as the Atlantic Ocean (Fig 8a), which elevated O2 and NO3 demand during oxic and suboxic remineralisation, respectively. Lower ratios were produced in eutrophic regions such as the subarctic Pacific, Southern Ocean and tropical zones of upwelling. Overall, global mean C:P increased from the Redfieldian 106:1 to 117:1, causing an increase in carbon export from 7.6 to 8.0 Pg C yr-1.”

— Page 20, lines 6-15: Is there any significance of these changes to potential paleoapplications?

Yes, but we suggest that this is covered sufficiently in the current version. We neglect to invoke specific examples of changes in nitrogen and carbon isotopes from past climates because simulations under past climate conditions were not performed. We therefore leave it to the reader to think on our results and possibly identify where interesting effects may lie.

— Page 21, line 4: “loss of alkalinity”, I’m guessing this in the surface ocean not the global ocean inventory? Yes. Surface alkalinity. Clarified.

— Page 22, lines 1-3: The general statement that CaCO3 production doesn’t affect the isotopes much is fine but a caveat should be added: you do not have a representation of CaCO3 sediments in the model and so cannot model any subsequent changes in the alkalinity inventory due to burial/dissolution (e.g. Boudreau et al., 2018: The role of calcification in carbonate compensation, Nature Geoscience, 11 (12) pp. 894-900). These changes would be relevant over the timescales you are discussing and may drive further changes.

Agreed. This is a good point. We have added a sentence that this a major caveat and will be addressed in future developments.

“However, we stress that version 1.0 of CSIRO Mk3L-COAL does not include CaCO3 burial or dissolution from the sediments according the calcite saturation state of overlying water (Boudreau, 2013). The neglect of ocean-sediment CaCO3 cycling means the neglect of an important aspect of the global carbon cycle active on millennial timescales Sigman et al. (2010). Changes in CaCO3 burial and dissolution could have a non-negligible effect on δ13C through altering whole ocean alkalinity, which would eventually alter air-sea gas exchanges of CO2 and therein affect surface δ13C. While we do not address these effects here, we aim to do so in upcoming versions of the model.”

— Page 29, line 24: are the results of the manuscript run with the static or variable remineralisation scheme?

Rewritten. This has been clarified by adding (default) to the end of these sentences in the Appendix.

— Page 43, lines 38-39: Should this be the companion paper: Simulations of radiocarbonin a coarse-resolution world ocean model: 1. Steady state prebomb distributions (https://doi.org/10.1029/JC094iC06p08217)?

We interpret the reviewers suggestion as writing another paper describing the implementation of radiocarbon in the ocean model. This could be possible, but we have not attempted to do so as yet.

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