Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-225-RC3, 2019

Geoscientific Model Development Discussions
Open Access

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.

Anonymous Referee #3

Received and published: 2 January 2019

Buchanan et al., describe the configuration of carbon and nitrogen isotopes in the CSIRO Mk3L-COAL Earth system model for the specific application to paleoceanographic records. The manuscript also presents a series of experiments demonstrating the impact of parameter values and model configuration. The introduction provides a really good background to isotopes and their use in paleoceanography, and the model itself is described in a good level of detail. I think the choice of experiments is particularly useful and really helps demonstrate the significance of various configurations of the model as well as highlighting potential future uses and scientific questions. As such, I think the manuscript will provide a really useful future resource for modellers as well as paleoceanographers. In general, I don't have any major comments but have

Printer-friendly version

Discussion paper

provided some minor comments and suggestions for the authors. Otherwise I recommend the manuscript for publication in Geoscientific Model Developments.

Specific comments:

Pg 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 transient paleo experiments)?

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

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

Pg 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?

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

Pg 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.

Pg 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?

Pg 11, lines 6 - 7: Please elaborate on the reason why it may be overestimated in the lower latitudes.

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 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

Pg 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?

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 learnt 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 measured foram isotopes and the model output if this is an intended use of the model in the future.

Pg 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 a model grid-box in the equation that is 100 m in depth for example, if the regridded observations fell predominantly in the upper part of the depth range.

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

Pg 20 , lines 6 - 15: Is there any significance of these changes to potential paleo-applications?

Pg 21, line 4: “loss of alkalinity”, I’m guessing this in the surface ocean not the global ocean inventory?

Pg 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 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.

Pg 29, line 24: are the results in the manuscript run with the static or variable remineralisation scheme?

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

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-225, 2018.