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Geoscientific Model Development Discussions

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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.

Somes (Referee)

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Overview

This manuscript from Buchanan et al. describes and evaluates carbon and nitrogen isotopes in a computationally efficient Earth System Model designed for paleoceanography. The new isotopic components are described including its equations. It is validated against modern dissolved and core-top observations and shown to generally reproduce the observations. A mini model intercomparison is done to show COAL performs similarly to other Earth system models. A few additional experiments show the sensitivity to non-Redfield stoichiometry, iron limitation of diazotrophy, and calcite

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saturation state.

Overall I think this is a satisfactory evaluation of the new isotopic components of the model. The description of the model is well done. I thought it was well written and there was a good balance of technical information including equations in the main text versus the Appendix. However, I do have comments that should be addressed before I would recommend publication.

Cheers,

Christopher Somes

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Comments

page 7, lines 9-13: Biological carbon fractionation

There should be more discussion justifying why you only account for a species effect and not aqueous CO2 concentration (Popp et al., 1989;Rau et al., 1989) and/or phytoplankton growth rate (Laws et al., 1995). There are of course large uncertainties, but there seems to be some general relationship with aqueous CO2 so I am surprised that is not included in a model designed for paleoceanography.

page 8: N2 fixation fractionation

Since N2 fixers have a lower del15N value than the atmospheric N2, this implies some fractionation, right? Does the del15N value go into diazotrophs biomass and then remineralize or go directly into NO3?

page 9, lines 5-20: NO3 utilization

Please show the model equation used for the calculation of utilization in the model (i.e. “u” in equation 15) since it is not straightforward exactly how this is calculated.

page10, Table 1: UVic model

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Although the model is based on UVic, the University of Victoria group has not been involved in the C13 and N15 development. Please replace “UVic” with “UVic-MOBI” (Model of Ocean Biogeochemistry and Isotopes) and “University of Victoria” with “Oregon State University/GEOMAR Kiel”.

page 10, line 22: “weak undercurrents that are important for reducing nutrient trapping at the equator”

Strong undercurrents and so-called nutrient trapping occur in the upper kilometer (mostly upper 400 meters), whereas your largest bias is between 1500-3000 meters, so something is missing here. I guess the main problem is that you switch off organic matter remineralization when oxygen runs out which allows the organic matter to sink and remineralize much too deep? If so, this should be pointed out here.

page 11, lines 3-7: “. . . far exceed reconstructions of Eide et al., (2017) . . . it is possible the upper ocean values of Eide et al. (2017) underestimate the preindustrial del13C-DIC field”

I think the robustness of the reconstruction deserves a discussion paragraph if you are going to raise this point. Perhaps there is reason to be somewhat skeptical of this reconstruction in the upper ocean. One important aspect I think they have not accounted for is the anthropogenic effect on biological uptake and remineralization.

My C13 model simulations predict this anthropogenic effect lowers d13C by $\sim$0.5 per mil in the Pacific at 700 meters (compare “Modern” versus “PreInd” differences at 15uM NO3 in Figure 3 of Glock et al. (2018)), which is due to phytoplankton incorporating the lighter anthropogenic CO2 and remineralizing at depth, whereas their reconstruction suggests basically negligible anthropogenic effect at these depths. Note this effect is required for my model’s ability to reproduce the range of modern observations there (see Figure S5 in Glock et al., 2018) and becomes even more important as you approach the surface.

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Do all of the models significantly overestimate these upper ocean values? It would be really interesting if you could also run a hindcast simulation forced by observed decreasing atmospheric del13CO2 and reproduce the modern observations. If so, I think you would have a legitimate argument that errors/uncertanties in the reconstruction may be significantly contributing to the large model-data misfit. I leave this up to you if it is feasible to accomplish, but I believe it is an important issue to discuss if this dataset is going to be the standard for model comparison.

That said, I still believe your decision not to include an aqueous CO2 dependency in your phytoplankton carbon fractionation is also likely contributing to your overestimated del13DIC, since that reduces phytoplankton fractionation in the warm open ocean gyres.

page 12: Figure 2

Something seems to be wrong with your color bar scale as it does not match the contours, which I assume are correct.

pages 13-15: Denitrification parameterizations

It is important to be more transparent about the artificial parameterizations to account for known model biases on both water column (i.e. NO3 reduction value) and sedimentary denitrification (i.e. amplification) in the main text. I have no problem including them, but I think it is fair to at least briefly note the effect they have on your simulations (e.g. how much the global rates change because of them).

It is not really a fair comparison to include models that include these artificial parameterization (COAL) to model's that don't (your chosen version of UVic-MOBI, PISCES). For example, our following paper with UVic-MOBI (Somes et al., 2017) with improved nitrogen cycle dynamics including sedimentary amplification better reproduces global mean del15NO3 similarly to COAL. It is not important which version of UVic-MOBI you decide to include, but these key denitrification parameterizations in COAL should be stated in the main text given its importance for del15N.

I would argue that if water column denitrification cannot react naturally to climate-induced changes to oxygen and remineralization, it significantly limits the model's ability as a tool for paleoceanographic research from a nitrogen isotope perspective. This has led our group to implement physical parameterizations to better mimic equatorial undercurrents (Large et al., 2001;Getzlaff and Dietze, 2013), so we do not have to do rely on this artificial water column denitrification reduction parameterization anymore. This topic should be discussed.

page 14, line 9: del15N in PISCES

Please cite the paper that describes del15N in PISCES; I am unaware of any publication on del15N in PISCES.

pages 17-24: Section 5. Ecosystem effects

I liked the sensitivity experiments focusing on a few key parameters/processes. However, I think they would benefit from an extra table (or two) that summarizes their key results. There are so many numbers mentioned directly in the text, I found it difficult to "digest" them all in a comparative context.

page 18: Variable stoichiometry

Please cite the key studies here and refer to the specific Appendix section that describes this so readers can quickly find it.

page 37: Acknowledgements

Will your published code and model output be accessible to the public?

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