

Liu et al. (2021) – Incorporating the stable carbon isotope ^{13}C in the ocean biogeochemical component of the Max Planck Institute Earth System Model.

Review by Pearse J. Buchanan.

General comments

Liu and colleagues have made a useful contribution to ongoing efforts to develop carbon isotope routines within ocean biogeochemical models. They use the biogeochemical model HAMOCC6, which is part of the MPI earth system model, to make two main findings:

1. They compare two accepted parameterisations for the biological fractionation of DI^{13}C by phytoplankton to show that the Popp parameterisation performs much better than the Laws parameterisation. This judgement of skill is made by comparing with observed $\text{d}13\text{C}_{\text{POC}}$ values.
2. They perform hindcast simulations from 1920-2010 using the ERA20C reanalysis to quantify the Suess effect, and then use the model to explore why the Suess effect might be underestimated by a recent observation-based approach to quantify it (Eide et al., 2017a).

I find the paper to be a valuable contribution and certainly worthy of publication. The authors have done a lot of work and their approach is rigorous. I have two major requests:

- a) The agreement with $\text{d}13\text{C}_{\text{POC}}$ data using Popp parameterisation is compelling as shown by Figure 4. Meanwhile, the consistent underestimation of $\text{d}13\text{C}_{\text{POC}}$ by the Laws parameterisation is very clear, but it remains the only parameterisation (to my knowledge) that includes the effect of growth rate, which is an important effect as shown by yourselves in the Subantarctic. You discuss this in your section 3, noting that for the Laws parameterisation to be improved, we would need to alter the slope and intercept of that relationship (although it may actually not be able to perform as well as the Popp parameterisation if it is in fact limited by the nature of the equation (inverse vs. logarithm)). I wonder whether this study could be made even more valuable by refitting the Laws parameterisation? Given that you have the observations, and at each point you have model $\text{CO}_2(\text{aq})$ and growth rate, could you optimise the Laws parameterisation by re-solving for the slope and intercept? As a fellow biogeochemical model developer, I would find this paper incredibly valuable if it offered a means to improve the Laws parameterisation, which is commonly used.
- b) I might be slow, but I only understood things in section 4.2 with multiple re-readings of this paper using more brain power than I'd like. I understood everything more or less easily up until the paragraph beginning at line 489, at which point you explain why the method of Eide (SE_{pref}) underestimates the Suess effect compared to your method with perfect knowledge of the preformed $\text{d}13\text{C}_{\text{DIC}}$ (SE_{total}). In the Indian Ocean, North Pacific and South Atlantic, you show that Eide's method underestimates your model method. You explain that there are two reasons for this:
 1. That preformed $\text{d}13\text{C}_{\text{DIC}}$ at 1940 (the intercept) is constant in the regression equation, but in reality this is spatially variable and decreases with depth, such that

use of a constant value over a subregion like the Indian Ocean will bias near-surface values as underestimated and deeper values as overestimated (?).

2. That the neglect of the suess effect in waters without CFC-12 prior to 1940 is going to underestimate the effect.

Point 2 I get easily, and also appears to be the main reason for difference between Eide's method and the "best" possible estimate using linear regression with the modelled CFC-12. Point 1 if I'm honest I still don't understand well, if at all. Part of this confusion comes from the fact that your earlier explanation of error between SE_{total} and SE_{mod} must take into account the error around the linear regression, which includes **both** coefficients. I would really appreciate a clearer explanation, not only here as a response to myself, but also I think if I am not getting this easily, it would be worth re-visiting section 4.2 and editing how this information is presented so that it is more easily digested by others. I think section 4.2 needs to be made more concise and more clear.

Specific comments

- Line 92: So the input of nutrients at the ocean surface happens where exactly? Everywhere or just where rivers are?
- Paragraph beginning at line 164: This needs simplifying. I would detail the spin-ups first, then talk about the reanalysis runs from 1850-2010.
- Line 275: I would expect the Laws parameterisation to have a higher global mean $d13C_{DIC}$ because of burial, not lower. Yes more ^{13}C -deplete material is remineralised in the interior, but over time more ^{13}C -depleted material would have been buried in the sediments. Meanwhile, at the surface the $d13C_{DIC}$ is higher in Laws, which according to your model's balancing of budgets would add more ^{13}C to the surface than was buried. Over time, this would lead to an increase in $d13C_{DIC}$ in the parameterisation that produced more ^{13}C -deplete organic matter (i.e. Laws). A positive excursion of deep ocean $d13C_{DIC}$ in the paleoceanographic record is actually explained by an increase in the biological flux of material to the sediments. This suggests to me that you may not have run these simulations to steady state. The good news about this is that it doesn't affect your conclusions of which parameterisation is better at calculating $d13C_{POC}$. Popp is clearly the better parameterisation and should continue to be even if your simulations were run to steady state.
- Paragraph beginning line 315: But too strong upwelling and too shallow remineralisation is not an explanation for too high biological fractionation in the Southern Ocean. Macronutrients are already unlimiting to primary production here, so I would say that iron concentrations are too high, much like they are in the North Pacific.
- Paragraph beginning 352: Please see Figure 2 in Buchanan et al., (2019) in Geoscientific Model Development for other model-data measures of agreement with $d13C_{DIC}$.
- Equation 23: Surely the intercept from SE_{pref} is important here? Why is it excluded? Some error between SE_{pref} and SE_{total} must be due to differences in the intercepts?
- Line 562: The underestimation of global mean $d13C_{DIC}$ by Laws parameterisation may in fact change if you preindustrial spin-ups were run for longer. Might be worth noting that here.

Technical corrections:

- Can the authors double check the coefficients in equations 5 and 7 please? The kinetic fractionation should be -0.88 per mil, and the second coefficient in equation 7 should be -1.07 according to Orr et al., (2017). Also, even though I'm sure it doesn't make much difference to the results, I find it strange that they didn't just include the full equations as detailed by Orr et al., (2017) when this has absolutely miniscule effect on computation time. In other words, there doesn't seem to be any real reason to not include the full equation that other modelling groups have implemented.
- Line 132: "preferentially utilised over".
- Line 335: "negative" should be "positive".
- Line 364: "generally" should be "general"
- Line 421: should "above" be "below"? deeper than?
- Line 440: you mean global mean surface ocean-atmosphere right? Not global mean ocean-atmosphere, which would include all depths.
- Line 508: "CFC-12" should be "CFC-12-free"
- Line 574: "Model" should be "Mode"

Thank you for considering my input to your research,

Pearse