

Response to the comments of Referee 1

We are thankful for the constructive remarks of Pearse J. Buchanan. In the following we reply to the comments point by point.

Two major requests

Comment 1 The agreement with $d13C_{POC}$ data using Popp parameterisation is compelling as shown by Figure 4. Meanwhile, the consistent underestimation of $d13C_{POC}$ 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 $CO_2(aq)$ 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.

Response: Thank you for the inspiring question. As the optimisation of the Laws parameterisation is beyond the scope of this study, we use a back-of-the-envelope calculation to refit the slope and intercept of ϵ_p^{Laws} (Eq. 7).

Because $\alpha_{Phy \leftarrow DIC}$, $\alpha_{aq \leftarrow g}$ and $\alpha_{DIC \leftarrow g}$ are close to unity, $\epsilon_{Phy \leftarrow DIC} \approx \delta^{13}C_{Phy} - \delta^{13}C_{DIC}$ and $\epsilon_{Phy \leftarrow DIC} \approx \epsilon_p + \epsilon_{aq \leftarrow DIC} \approx \epsilon_p + \epsilon_{aq \leftarrow g} - \epsilon_{DIC \leftarrow g}$. As $\delta^{13}C_{Phy} \approx \delta^{13}C_{POC}$, $\delta^{13}C_{POC}$ can be approximated as

$$\delta^{13}C_{POC} \approx \epsilon_p + \epsilon_{aq \leftarrow g} - \epsilon_{DIC \leftarrow g} + \delta^{13}C_{DIC}. \quad (R1)$$

As sea water temperature (determining $\epsilon_{DIC \leftarrow g}$), $CO_2(aq)$ and growth rate are independent of the choice of ϵ_p , and the surface $d13DIC$ is only marginally affected by the choice of ϵ_p (see Section 3.2 and Figs. 5a and 5b), we can approximate ϵ_p^{Laws} using existing monthly output of the above model variables. The surface $d13POC$ obtained with Eq. (R1) ($r=0.74$, $NRMSE=2.8$, Figs. R1a-R1c) is indeed very close to the simulated $d13POC$ ($r=0.71$, $NRMSR=2.5$, Figs. 4b, 4d and 4f).

$$\epsilon_p^{Laws} = 68.3 \frac{\mu}{CO_2(aq)} - 24.7, \quad (7)$$

$$\epsilon_p^{Laws.V1} = 68.3 \frac{\mu}{CO_2(aq)} - 20, \quad (R2)$$

$$\epsilon_p^{\text{Laws-V2}} = 68.3 \frac{\mu}{\text{CO}_2(\text{aq})} - 16.7, \quad (\text{R3})$$

$$\epsilon_p^{\text{Laws-V3}} = 100 \frac{\mu}{\text{CO}_2(\text{aq})} - 20. \quad (\text{R4})$$

As the intercept increases (comparing Eq. 7, R2 and R3), d13POC generally increases (Figs. R1a-R1i) and accordingly NRMSE decreases from 2.8 for Eq. (7) to 1.4 for Eq. (R2) and 0.75 for Eq. (R3), while the spatial relation coefficient remains unchanged.

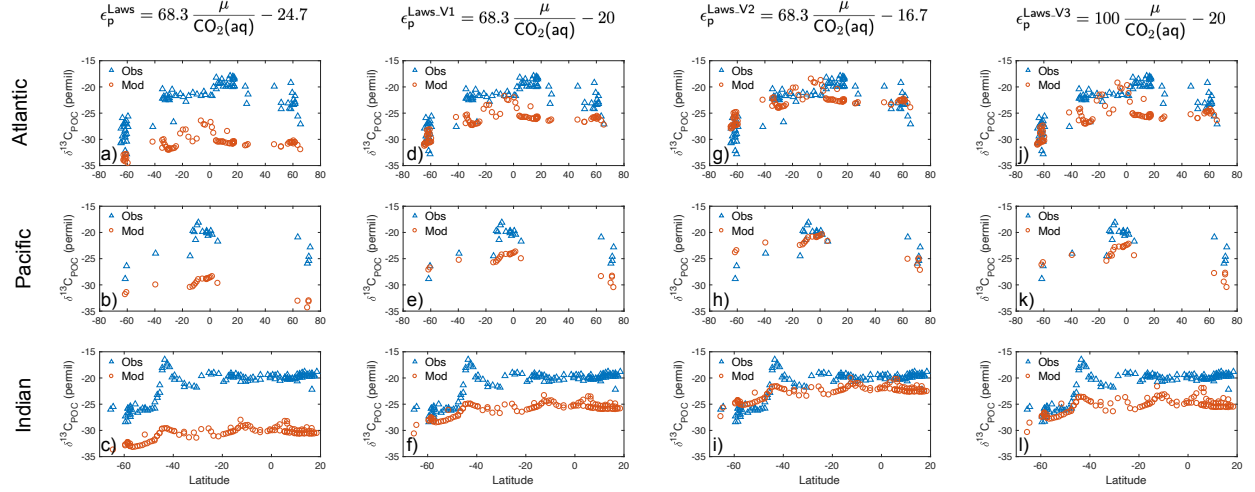


Figure R1: As Figure 4b, 4d and 4f in the manuscript, but for d13POC estimated for ϵ_p^{Laws} (Eq. 7) using Eq. (R1) (a-c). (d-f), (g-i) and (j-l): As (a-c), but for $\epsilon_p^{\text{Laws-V1}}$ (Eq. R2), $\epsilon_p^{\text{Laws-V2}}$ (Eq. R3) and $\epsilon_p^{\text{Laws-V3}}$ (Eq. R4), respectively.

As the slope increases (comparing Eq. R2 and R4), the d13POC difference between low and high latitudes increases (Figs. R1d-R1f, R1j-R1l) and NRMSE slightly decrease from 1.4 to 1.3. However, d13POC in the low latitude of the Atlantic shows too high variability compared to the observation. Accordingly, the spatial correlation coefficient decreases from 0.74 to 0.66.

In summary, a systematic refitting of the Laws parameterisation could be an interesting step to improve the model performance and might be considered in our future work.

Comment 2 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 d13CDIC (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 $\delta^{13}\text{C}_{\text{DIC}}$ 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.

Response: Both ^{13}C Suess effect $\delta^{13}\text{C}_{\text{SE}(t-1940)}$ and pCFC-12 in the ocean result from the invasion of atmospheric signal. Thus their spatial distribution resembles each other: both show larger absolute values at the surface than in the interior ocean. $\delta^{13}\text{C}_{1940}^{\text{pref}}$ also has a specific vertical structure in our model: it is generally more positive in the upper ocean than the deep ocean. According to the equation

$$\delta^{13}\text{C}_t^{\text{pref}} = \delta^{13}\text{C}_{\text{SE}(t-1940)} + \delta^{13}\text{C}_{1940}^{\text{pref}},$$

and $\delta^{13}\text{C}_{\text{SE}(t-1940)} < 0$, $\delta^{13}\text{C}_t^{\text{pref}}$ shows a smaller vertical gradient than $\delta^{13}\text{C}_{\text{SE}(t-1940)}$. Thus, a linear regression for $\delta^{13}\text{C}_t^{\text{pref}}$ and pCFC-12 results in a less negative slope a_{pref} than a slope obtained with a spatially-uniform $\delta^{13}\text{C}_{1940}^{\text{pref}}$.

In the revised manuscript the above explanation is incorporated to clarify Point 1.

To improve the readability of this section, we restructure it and divide it into subsections. We also move the detailed description of the E17 approach and calculation procedures to the Appendix to make this section more focused on the results and discussion.

Specific comments

Comment 3 Line 92: So the input of nutrients at the ocean surface happens where exactly? Everywhere or just where rivers are?

Response: The input of nutrients are added uniformly at the ocean surface. This is now specified in the revised manuscript.

Comment 4 Paragraph beginning at line 164: This needs simplifying. I would detail the spin-ups first, then talk about the reanalysis runs from 1850-2010.

Response: Agree. In the revised manuscript this paragraph only focus on the spin-up runs.

Comment 5 Line 275: I would expect the Laws parameterisation to have a higher global mean $d_{13C_{DIC}}$ 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 $d_{13C_{DIC}}$ 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 $d_{13C_{DIC}}$ in the parameterisation that produced more ^{13}C -deplete organic matter (i.e. Laws). A positive excursion of deep ocean $d_{13C_{DIC}}$ 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 $d_{13C_{POC}}$. Popp is clearly the better parameterisation and should continue to be even if your simulations were run to steady state.

Response:

Our pre-industrial spin-up simulations have been run to equilibrium regarding $d_{13C_{DIC}}$, according to the OMIP protocol: Equilibrium states are reached with 98% of the ocean volume having a $\delta^{13}C_{DIC}$ drift of less than 0.001‰ year^{-1} .

The difference in $DI^{13}C$ water-column inventory (and therefore difference in global mean $d_{13C_{DIC}}$) between Popp and Laws is determined by the differences in air-sea gas exchange, input of $DO^{13}C$ and $^{13}CO_3^{2-}$, loss of $PO^{13}C$ and $^{13}CO_3^{2-}$ to sediment and the sediment $DI^{13}C$ reflux, see Table A1. Indeed more ^{13}C is added to the surface than buried, which leads to slight increase of ^{13}C inventory over time. This increase is larger in Laws than Popp by 30.5 Gmol C/yr . However, Laws parameterisation leads to a slightly higher mean surface $d_{13C_{DIC}}$. This difference in surface $d_{13C_{DIC}}$ causes Laws to have a smaller air-sea $^{13}CO_2$ flux into the ocean than Popp, with a difference of $-272.1 \text{ Gmol C /yr}$. Thus the lower $DI^{13}C$ inventory in P.Laws than P.LPopp primarily results from the difference in air-sea gas exchange.

We correct an error in Table A1: the numbers 626.6 and 596.1 should be swapped.

Comment 6 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.

Response: We agree with Referee #1 that too high iron concentrations are likely the main cause of the too high primary production in the Southern Ocean. Iron concentration in the Southern Ocean

is 0.2-0.4 nmol/L in our model, compared to observations (generally <0.25 nmol/L according to eGEOTRACES, <https://www.egeotraces.org>). Iron limitation in our simulations occurs in a smaller area (south of 50°S) compared to that suggested by observations (south of 40°S, Moore et al., 2013). However, stronger upwelling and shallower remineralisation in the model are also causes for high iron concentration at the surface.

We add these discussions in this paragraph of the revised manuscript and a figure for the simulated iron limitation in the Appendix.

Comment 7 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}.

Response: Thank you, we add the suggested reference in the revised manuscript.

Comment 8 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?

Response: By definition SE_{pref} has no intercept because in the assumption of the E17 approach 13C Suess effect is proportional to pCFC-12:

$$SE_{\text{pref}} := \delta^{13}\text{C}_{\text{SE}(t-\text{PI})} = f_{\text{atm}} \cdot \delta^{13}\text{C}_{\text{SE}(t-1940)} = f_{\text{atm}} \cdot a \cdot \text{pCFC-12}_t.$$

Comment 9 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.

Response: Our spin-up simulations are long enough for d13C_{DIC} to reach equilibrium, see the reply to Comment 5. Thus, the underestimation of global mean d13C_{DIC} by Laws parameterisation will not change if the spin-ups are run for longer.

Technical corrections

Comment 10 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.

Response: Thank you for catching this error. Indeed equations 5 and 7 do not follow the OMIP protocol (Orr et al., 2017). We actually adopted these equations, as well as the simplification of Eq. (7) from Schmittner et al. (2013). We ran a short simulation and proved the small differences in Eqs. 5 and 7 lead to negligible changes in model results. Thus we didn't rerun and expensive simulations in this study.

In the revised manuscript we remove the statement about following the OMIP protocol. And we make a note for the differences in Eqs. 5 and 7 between this study and OMIP protocol.

Comment 11 Line 132: “preferentially utilised over”.

Response: Thanks, this is modified in the revised manuscript.

Comment 12 Line 335: “negative” should be “positive”.

Response: This is corrected in the revised manuscript.

Comment 13 Line 364: “generally” should be “general”

Response: Thanks, modified.

Comment 14 Line 421: should “above” be “below”? deeper than?

Response: No, here we do mean above the pCFC=20 patm isoline. The domain referred to here is illustrated in Figure 13 (area above the thick grey line). In the revised manuscript we add a reference to Figure 13 to avoid confusion.

Comment 15 Line 440: you mean global mean surface ocean-atmosphere right? Not global mean ocean-atmosphere, which would include all depths.

Response: Yes, “global-mean surface ocean-atmosphere” is used in the revised manuscript.

Comment 16 Line 508: “CFC-12” should be “CFC-12-free”

Response: Corrected.

Comment 17 Line 574: “Model” should be “Mode”

Response: Corrected.

References

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