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The paper of Paillard investigates the Plio-Pleistocene carbon cycle by setting up a conceptual model, consisting of differential equation for the carbon content of the atmosphere-ocean-biosphere $C$, the alkalinity of the ocean, $A$, and the stable carbon isotope values of $C$, $\delta^{13}C$.

The analysis starts with the following equation for temporal changes in the carbon content of the system

$$\frac{\delta}{\delta t}C = V - B - D$$ (1)

with $V$ being the volcanic carbon input, $B$ the organic carbon burial, $D$ the oceanic carbonate deposition flux. Furthermore, the assumption that carbonate compensation will restore on multi-millennial years time scale the carbonate ion concentration is used. Here, the implicit used knowledge that alkalinity $A$ changes might be approximated after $\frac{\delta}{\delta t}A = W - 2D$ was used (but not mentioned explicitly) to final end with

$$\frac{\delta}{\delta t}C = 2(V - B) - W$$ (2)  
$$\frac{\delta}{\delta t}(\delta^{13}C) = \frac{(V(-5 - \delta^{13}C) - B(-25 - \delta^{13}C))/C}{\delta^{13}C}$$ (3)

with $W$ being the silicate weathering rate. According to the manuscript, the terms in brackets in Equation 3 are meant to be the following:

- $(-5 - \delta^{13}C)$: a volcanic source with constant isotopic signature of $-5\%$,
- $(-25 - \delta^{13}C)$: a constant $-25\%$ fractionation of organic matter with respect to the mean $\delta^{13}C$ of the considered system.
I find the conceptual idea how to understand the observed long-term changes in the carbon cycle very interesting. However, I have some fundamental comments to Equation 3 describing the evolution of the carbon isotope of the system:

1. The term \((-5 - \delta^{13}C)\) in Equation (2) does not serve to describe the volcanic source with the constant isotopic signature of \(-5\%\) as intended, but as some isotopic fractionation by \(-5\%\) with respect to the negative of the mean isotopic values \(\delta^{13}C\) of the atmosphere-ocean-biosphere system.

2. The author decides to follow the initial Equation 1 when setting up the changes in the carbon isotopes. This approach is not wrong, but neglecting any impacts of the carbonate deposition flux \(D\) on \(\delta^{13}C\) might be too simple.

3. It is not clear to me, why in Equation 3 the isotopic signature of both fluxes \(B\) and \(V\) are described as a function of negative \(\delta^{13}C\).

4. Changes in the isotopic value are always also depending on the content of the system. This implies that the differential equation has to be treated with care. In detail, one has to take into account, that when solving \(\frac{\delta}{\delta t}(\delta^{13}C)\), one has to find a solution for \(\frac{\delta}{\delta t}(C \cdot \delta^{13}C)\). Following the product rule, it follows:

\[
\frac{\delta}{\delta t}(C \cdot \delta^{13}C) = C \cdot \frac{\delta}{\delta t}(\delta^{13}C) + \delta^{13}C \cdot \frac{\delta}{\delta t}(C)
\]

Solving for \(\frac{\delta}{\delta t}(\delta^{13}C)\) gives:

\[
\frac{\delta}{\delta t}(\delta^{13}C) = \left(\frac{\delta}{\delta t}(C \cdot \delta^{13}C) - \delta^{13}C \cdot \frac{\delta}{\delta t}(C)\right) \cdot \frac{1}{C}
\]

The first term in equation (5), \(\frac{\delta}{\delta t}(C \cdot \delta^{13}C)\), is what is typically found on right-hand sides of differential equations (RHS of DE). The 2nd term in equations (5,6), \(-\delta^{13}C \cdot \frac{\delta}{\delta t}(C)\), is probably small and might be negligible. However, its existence and any assumptions on neglecting it should in my view be mentioned for the sake of completeness.

5. When setting up differential equations for isotopes in the so-called \(\delta\)-notation one typically starts with the equation for the matter fluxes and multiplies each matter flux with the assumed isotopic signature of the flux, including any potential isotopic fractionation. With respect to the problem at hand (starting with equation 1) I end up with the following differential equation for \(\delta^{13}C\):

\[
\frac{\delta}{\delta t}(\delta^{13}C) = \left(V \cdot \delta^{13}C_{V} - B \cdot \delta^{13}C_{B} - D \cdot \delta^{13}C_{D} - \delta^{13}C \cdot \frac{\delta}{\delta t}(C)\right) \cdot \frac{1}{C}
\]
According to the manuscript I get $\delta^{13}C_V = -5\%$ and $\delta^{13}C_B = \delta^{13}C - 25$. The argument that isotopic signature of the carbonate burial flux $D$ is identical to $\delta^{13}C$ of the system can be used to define $\delta^{13}C_D = \delta^{13}C$. Using the knowledge from the carbonate compensation that $D = W + B - V$ finally gives me a new equation for changes in the isotopic signature:

$$\frac{\delta}{\delta t}(\delta^{13}C) = \left( V \cdot (-5) - B \cdot (\delta^{13}C - 25) - (W + B - V) \cdot \delta^{13}C - \delta^{13}C \cdot \frac{\delta}{\delta t}(C) \right) \cdot \frac{1}{C} \quad (8)$$

I show in Figure 1 for the simplest scenario without long-term trend in CO$_2$ and the Amazon-like organic burial that both approaches (Paillard: Eq. 3; this comment: Eq. 8) lead to slightly different results, but they agree on large scale features. This comparison would therefore suggest, that the simplifications done in the setting up of the differential equation for the changes in the carbon isotopes by Paillard might be justified (even if I do not yet understand them in detail). However, I still believe that setting up the differential equation for the carbon isotope the way I describe above might be a way which is at least better to understand and easier to reproduce.

I like to finish with some more general comments:

1. Another simplification of the setup is the estimation of the change in atmospheric CO$_2$ mixing ratio by

$$\text{CO}_2 = 280 \cdot \left( \frac{C}{40,000 \text{ PgC}} \right)^2 \text{ (in ppm)}. \quad (9)$$

This equation was given without any further motivation. However, since all inputs of carbon to the system are given by volcanic CO$_2$ outgassing into the atmosphere, one might also evaluate the corresponding changes in atmospheric CO$_2$ concentration by the so-called airborne fraction, the fraction of injected carbon that stays in the atmosphere. With the given equation for CO$_2$ above (Eq. 9), this airborne fraction turns out to be around 3%. For example, a rise in C by 100 PgC, for example, would lead to a new CO$_2$ mixing ratio of 281.4 ppm. Following the well known relation of 1 ppm of CO$_2 = 2.12$ PgC, this rise in CO$_2$ by 1.4 ppm is similar to a rise in atmospheric carbon by nearly 3 PgC, thus 3% of the initial perturbation. The long-tail of the airborne fraction for potential future CO$_2$ emissions was recently investigated systematically with the GENIE Earth System Model. It was found (Equation S1 and Table S2 in Lord et al. (2016)) that the airborne fraction is around 5% and 1.6% on a timescale of $10^5$ and $10^6$ years, respectively. Thus, the so-far unmotivated assumption for CO$_2$ as given in Equation (7) above might be supported with such results but also illustrates, that variabilities faster than several $10^5$ years are not contained in this approximation of CO$_2$ given in Eq. 9.
2. Some of the assumptions are rather implicit and not supported with any further details or citations. The assumption that ocean alkalinity changes are approximated as changes in carbonate alkalinity by only considering variations in the carbonate ion concentration as $W - 2D$ was already mentioned above, and might find support in Zeebe and Wolf-Gladrow (2001). Furthermore, the assumption that the monsoon response to astronomical forcing as a simple function of the precessional forcing after $F_0 = \max(0, -e \cdot \sin(\omega))$ needs some backup from proxy reconstructions. References for the assumed isotopic signature of $-5\%$ for volcanic outgassing $V$ and for the fractionation of $-25\%_0$ in the organic burial flux $B$ would also be highly welcome.

3. The assumed long-term trend in weathering via the parameter $\gamma$ leads only to a decrease in carbon content and CO$_2$. Thus, to really mimic the multi-million decrease in CO$_2$ from 350 ppm 4 Myr ago to 280 ppm in the preindustrial time one needs also to increase the overall carbon content of the system at the beginning of the simulations. This is not mentioned. Otherwise CO$_2$ would start during scenarios which include this trend ($\gamma > 0$) at 280 ppm at the start of the simulations 4 Myr ago and decrease thereafter.

4. To reconstruct the carbon cycle in detail it would be helpful for the reader to be provided with the finally chosen parameter values.

References


Figure 1: Rebuilding the model of Paillard. A: Dimensionless orbital forcing function $F$ with an amplitude of 1 and a mean value of 0 based on Laskar et al. (2004). B: Change in atmospheric CO$_2$ concentration (following Eq. 9 of this comment) for the Amazon-like burial of organic carbon ($B = B_0 - aF(t)$, $a = 20$). No long-term trend in CO$_2$ is considered ($\gamma = 0$) and the carbon fluxes W, V, B are determined from the carbon turnover time of $\tau_C = 400$ kyr. C) Changes in $\delta^{13}C$ of the same Amazon-like burial scenario following either Paillard (Eq. 3) or this comment (Eq. 8).