

Response to Peter Köhler

First, I would like to thank Peter Köhler for providing these thoughtful comments and for rebuilding and reproducing my model results. I acknowledge that the model description and the presentation of some equations or parameters was sometimes not explicit enough in the submitted manuscript. I therefore want to clarify some points below.

1. The ^{13}C equation.

The main point raised by Peter Köhler concerns the ^{13}C equation. It turns out that we are both using (almost) the same equation, but just written differently. The only true difference stands in the organic matter fractionation : while Peter Köhler uses a constant fractionation with respect to the environment, I implicitly considered an organic matter sink with a constant isotopic signature of -25‰.

More precisely, equation (8) from Peter Köhler reads :

$$\frac{d}{dt}(\delta^{13}\text{C}) = \frac{1}{C} \left[V(-5) - B(\delta^{13}\text{C} - 25) - (W + B - V)\delta^{13}\text{C} - \delta^{13}\text{C} \cdot \frac{dC}{dt} \right]$$

When substituting the last term using the equation for dC/dt :

$$\frac{d}{dt}C = V - B - D$$

we get :

$$\frac{d}{dt}(\delta^{13}\text{C}) = \frac{1}{C} [V(-5) - B(\delta^{13}\text{C} - 25) - (W + B - V)\delta^{13}\text{C} - \delta^{13}\text{C} \cdot (V - B - D)]$$

And after simplification, and using $D = W+B-V$, this leads to :

$$\frac{d}{dt}(\delta^{13}\text{C}) = \frac{1}{C} [V(-5 - \delta^{13}\text{C}) - B(-25)]$$

As mentioned above, this is very similar to my equation (2b) :

$$\frac{d}{dt}(\delta^{13}\text{C}) = \frac{1}{C} [V(-5 - \delta^{13}\text{C}) - B(-25 - \delta^{13}\text{C})]$$

the only difference being that, implicitly, I used a constant organic matter sink of -25‰. Since the ^{13}C of carbonates remains close to 0‰, these different choices lead to a very small difference in the numerical experiments, as demonstrated by Peter Köhler.

In any case, this point should be clarified in a revised manuscript.

2. The $p\text{CO}_2$ scaling equation.

In the manuscript, C represent the total carbon content at the « Earth surface », which means mostly the ocean reservoir, plus a minor contribution from the biosphere and atmosphere. I used a simple scaling to translate these changes in carbon content C (expressed in GtC) in terms of atmospheric $p\text{CO}_2$ (in ppm) :

$$p\text{CO}_2 = 280 \left(\frac{C}{40,000} \right)^2$$

As explained by Peter Köhler, this might be supported by model experiments for long time scales, but this lacks some justification in the manuscript. Such a scaling can be

obtained when considering that C represent the carbon content of a well-mixed ocean. Then, from chemical equilibrium, we obtain :

$$pCO_2 = k \frac{[HCO_3^-]^2}{[CO_3^{2-}]}$$

where the constant k includes the solubility of CO_2 , and the first and second dissociation constants of carbonate and bicarbonate ions. When considering only the long time scale response, we can assume that carbonate compensation will restore $[CO_3^{2-}]$ to a constant initial value. Furthermore, under standard oceanic pH conditions, bicarbonate ions $[HCO_3^-]$ represent about 90% of the total carbon content C . If we assume, to first order, that $C \approx [HCO_3^-]$, then the above equation means that pCO_2 should, on long time scales, increase approximately as the square of C . Though this is certainly a rough approximation, it is sufficient to provide a reasonable magnitude of the implied pCO_2 changes associated with this simple model.

Again, this point should be clarified in a revised manuscript

3. I indeed also used the rather implicit assumptions that ocean alkalinity is approximated by carbonate alkalinity, therefore equation (1b) in the manuscript. This could be discussed a bit more in the manuscript, though it is quite a classical approximation.

Concerning the choice of precessional forcing $F_0(t) = \max(0, -e \sin\omega)$, I am not sure that any proxy comparison would either backup or dismiss such a choice. Furthermore, there is little hope to find any proxy for global organic carbon preservation, since individual proxies of preservation are often very dependent of the local or regional context. The choice of this forcing is simply based on two premises: 1 – monsoon are primarily driven by precession, something demonstrated by paleoclimatic data and simulated by climate model. 2 – the conceptual model needs a rectifying mechanism to reproduce the envelope of precession, something consistent with the averaged values of river sedimentary carbon discharges being largely dominated by the largest or extreme events. The expression above is the simplest possible choice along these lines.

The isotopic signatures used for volcanic outgassing (-5‰) and for buried organic matter (-25‰) are rather standard values used in geochemical textbooks and treatises. For instance:

Porcelli, D. and Turekian, K.K., The History of Planetary Degassing as Recorded by Noble Gases, §6.6.1 in Readings from the Treatise on Geochemistry, edited by Holland, H.D. and Turekian, K.K., (2010).

These numbers are somewhat conventional with actual measurements varying from about -1‰ to -8‰ for volcanoes or mid-ocean ridges outgassing, depending on location. Similarly, -25‰ is a conventional value for organic matter $\delta^{13}C$ used for instance as a normalization for reporting ^{14}C activities, while actual values vary from roughly -10‰ to -30‰ depending on organic materials.

4. There is no need to specify the carbon content of the model, since it is explicitly computed by the equations. As mentioned in Figure caption 2, the model is integrated from an arbitrary condition (that is carbon content, and isotopic value) at 5 MyrBP and the first 1 Myr is discarded, since it correspond to the transient part of the simulation.

5. I believe all parameter values are given in Figure caption 2, but there has been an unfortunate typesetting change from greek to latin alphabet. This also needs to be corrected in a revised manuscript.