Answer to RC 2 :

We thank the reviewer for his/her helpful comments and suggestions that will help us revise and improve the manuscript. We hope the answers and modifications proposed satisfactorily address his/her remarks.

In the following, the reviewer's comments are in black and our answer in blue.

G. Leloup and D. Paillard

First, as a general comment, we would like to emphasize that our simple model is obviously not designed to be a faithful representation of reality. From a practical point of view, the actual processes involved are far too numerous, they depend on quite local and specific phenomena, and more importantly current knowledge of the long term organic carbon cycle is far too incomplete. We therefore fully agree with both reviewers that in many ways this model is certainly oversimplified. In particular, it is certainly not suited to describe faithfully all the variations in carbon isotopes observed in the geological record.

But our objective is much more modest : we are trying to provide a new framework to explain the persistent long-term (8-9 Myr) oscillations observed over the Cenozoïc and Mesozoïc. The main difficulty is that there is no known external forcing at this particular periodicity. This stands in sharp contrast with the 400 kyr and the 2.4 Myr 13C oscillations that can easily be related to the astronomical (eccentricity) forcing. Still, these long-term (8-9 Myr) 13C oscillations appear remarkably persistent despite major changes in continental configuration, biological evolution or climate. The suggestion that they might also be astronomically paced is therefore worth examining. Unfortunately, current carbon models do not allow for dynamical behaviors like period doubling or frequency locking : they can generally produce oscillations only at the same frequency as the forcing. If we still wish to explain the observed 8-9 Myr oscillations by some astronomical forcing, we need a model with more varied dynamical behaviors. Our model exemplifies such a possibility.

In a revised version of the manuscript, we would emphasize more on the philosophy of our model and its purpose.

Peer review of „Multi-million year cycles in modelled d13C as a response to astronomical forcing of organic matter fluxes".

In this paper, the authors built a simplified numerical representation of the carbon cycle, assuming a mass balance without carbon reservoirs (and hence no lag-times there), unlimited nutrients (otherwise organic burial B would also depend on weathering W), and with constant $[Ca^{2+}]$ concentration in the ocean.

>> assuming a mass balance without carbon reservoirs (and hence no lag-times there)

In the model, we have one global carbon reservoir and one global oxygen reservoir, with the associated time lags : in particular, this is what enables a self-sustained oscillation regime.

>> unlimited nutrients (otherwise organic burial B would also depend on weathering W),

We have no « explicit » weathering, but we are assuming that climate warms when the global carbon content C increases, therefore the traditional Walker feedback through an increase in the carbonate precipitation. We actually have the same feedback on the organic carbon via the relationship between burial B and carbon C : when climate warms, this induces several processes (hence a non-monotonous relation) among which the increase in weathering and nutrient supply. This is further detailed in the dedicated comment on the decoupling between B and W.

>>with constant [Ca^{2+}] concentration in the ocean

We indeed assume a constant [Ca^{2+}] concentration in the ocean. This is a limitation of our model. In particular, including calcium variations could change (and complexify) the relationship between global carbon content C and atmospheric CO_2, and therefore the climate forcings. This will be explained more clearly in the revised manuscript.

Without applying any forcing, their model evolves into steady-state equilibrium when the oxidation of other elements than organic carbon (Ox) increases steeply with oxygen content (O). When the Ox term increases less steeply with O, the model produces oscillations in d13C without any astronomical forcing. Finally, the authors add an eccentricity forcing to the burial of organic carbon and they observe that the resulting d13C is oscillating with preferential periodicities of 2.4, 4.8 and 7.2 Myr. The authors thus built a model that is prone to oscillate at multi-million-year timescales between multiple equilibria, and by adding the forcing they are making sure that the model resides around one equilibrium value until the astronomical forcing becomes strong enough to push the system towards the second equilibrium. Finally, the authors compare their model results to the Westerhold et al. (2020) benthic d13C compilation and point out to the reader that the multi-million-year oscillations in this record could be the result of self-sustained oscillations in the Earth system.

We would like to clarify that in our study, we do not suggest that the multi-million year oscillations observed in the $\delta^{13}C$ record are the direct result of self sustained / internal oscillations in the Earth system.

Rather, we suggest that the addition of astronomical forcing to a system with multiple equilibria can produce oscillations in the $\delta^{13}C$ with periodicities that are different from the astronomical periodicities.

In our case, the carbon-oxygen system without astronomical forcing can produce self-sustained oscillations under certain parameter values (when the Ox term increases less steeply with O, ie if $a < a_{lim}$) but does not necessarily (no oscillations are obtained if $a > a_{lim}$). However, in both cases (self sustained oscillations or not), the addition of astronomical forcing to the system changes its behaviour and leads to oscillations in the $\delta^{13}C$ that can have different frequencies than the astronomical one, and that have a different period than the self sustained oscillations in the case where they exist.

Major concern.

This is a nice "back-of-the-envelope" carbon cycle exercise, but I do not see the immediate merit in this paper. The authors set the model variables such that it is prone to produce multi-million-year cycles. They force it with an eccentricity cycle (including the 2.4 Myr component) and come back home with a simulated d13C signal that emphasizes these same 2.4 Myr cycles, as well as multiples of that cycle. I would be interested to read why the authors believe their approach provides additional insights into the behavior of the carbon cycle in addition to other previous attempts to simulate the global carbon cycle.

In contrast to many previous attempts, we simply do not attempt to « simulate » the global carbon cycle. We try to provide a possible theory that could explain the occurrence of persistent long-term oscillations, at periodicities roughly a multiple of the forcing. To our knowledge, this has never been attempted in carbon cycle studies before.

We want to emphasize that our major modeling assumption (multiple equilibria) is rather natural though unconventional. Indeed, most carbon cycle models are built on the premises that they should exhibit one equilibrium (and if possible an equilibrium that resembles the current state when submitted to present-day forcing). But net organic matter burial depends upon climate in numerous fashion that acts either ways, with warming favoring burial or favoring old carbon remineralization. Overall, it is unlikely that the relationship between organic matter burial and climate is always monotonous. As shown in our manuscript, assuming such a non-monotonous relationship leads quite naturally to multiple equilibria in our simple carbon-oxygen model, something which may explain some features of past carbon cycle changes.

We force our model with an eccentricity cycle, that includes a 2.4 Myr component, and produce δ^{13}C cycles of 2.4 Myr, and preferentially multiples of 2.4 Myr. However, this finding already differs from previous studies, such as Paillard (2017) or Kocken et al (2019). In these studies, the organic matter burial was also forced with an eccentricity cycle, but the obtained δ^{13}C cycles did not contain periodicities longer than 2.4 Myr. Being able to produce periodicities longer than 2.4 Myr with an eccentricity forcing is not a trivial result. It is possible in our case, because the model is non linear and contains multiple equilibria. In the models of Paillard (2017) and Kocken (2019), where the formulations are mostly linear it is not possible to produce δ^{13}C oscillations with periods longer than 2.4 Myr by forcing solely with the eccentricity.

I am especially thinking about Bachan et al. (2017), who reports on carbon cycle stabilization pathways in response to a sinusoidal forcing.

We thank the reviewer for the suggestion of the Bachan et al (2017) article. Our study shares a similar philosophy with the one of Bachan (2017). Indeed, Bachan (2017) states that "*Many sophisticated models have been put forth to interpret geochemical record and simulate global biocheochemical dynamics (BLAG, Berner and others 1983, Copse, Bergman and others 2004, MAGic, Arvidson and others 2006). The goal here is not to replicate these models. Rather, our goal is to produce the* ***simplest*** *possible model that still bears a semblance of the physical system being modeled, and can produce results that are qualitatively similar to the carbon isotope record*".

In this study, we also look for the simplest possible model that can explain observed features of the δ^{13}C record. However, our model and the one of Bachan (2017) have different goals and make different assumptions. Bachan (2017) focuses on δ^{13}C excursions, having durations of 0.5 to 10 Myr, and declining amplitude over time, taking place mostly in the

Paleozoic and the earliest part of the Mesozoic. There are no marked excursions in the $\delta^{13}C$ record over the last 200 Myr.

In our study, we focus on multi-million year cycles in the $\delta^{13}C$ over the last ~200 Myr (Cenozoic and latest Mesozoic), as it is the period on which oscillations of 8-9 Myr in the $\delta^{13}C$ have been observed (Boulila et al (2012) for the Cenozoic, and Martinez and Dera (2015) for the period from 130 Myr BP to 200 Myr BP).

The observed $\delta^{13}C$ oscillations are of lower amplitude than the $\delta^{13}C$ excursions. The amplitude of the oscillations is around 2‰, while the positive $\delta^{13}C$ excursions in the Earliest Phanerozoic have amplitudes of 5-10‰.

In the study of Bachan (2017), there are no multiple equilibria. The system is linear, forced with a sinusoidal forcing. In the Bachan (2017) study, a resonance behaviour is observed. Larger amplitudes of $\delta^{13}C$ oscillations are obtained for larger amplitudes of the sinusoidal forcing, and this is especially true for input frequencies close to the resonant frequency, where the $\delta^{13}C$ oscillations amplitude changes due to amplitude variation of the input forcing are amplified. However, this differs from our study, as the output $\delta^{13}C$ signal oscillations obtained in Bachan (2017) always have the same frequency as the sinusoidal input forcing. In our case, changing the amplitude of the input forcing (the a_f parameter) does not change much the amplitude of the $\delta^{13}C$ oscillations, But the novelty of our study is that by changing the amplitude of the input forcing (by modifying the a_f parameters that controls the strength of the astronomical forcing, the eccentricity, in our case) we produce $\delta^{13}C$ oscillations that have a dominant frequency that is not present in the input forcing (the eccentricity in our case). Bachan (2017) suggests that linear resonance might be an important concept to explain some high amplitude 13C excursions in the Paleozoïc. We are suggesting that period-doubling and multiple equilibria might be an important concept to explain the persistent long-term 13C oscillations observed at least since the Mesozoïc up to now.

In a new version of the manuscript, we would emphasize on the fact that our interest lies in oscillations in $\delta^{13}C$ over the last ~200 Myr, period for which 8-9 Myr oscillations of the $\delta^{13}C$ have been reported [Boulila et al (2012), Martinez and Dera (2015)].

I also feel that some simplifications in the model need to be more clearly justified. It seems contra-intuitive to de-couple silicate weathering from the organic carbon flux (B does not depend on W). The ocean cannot recycle the same nutrients ad infinitum. You have to introduce new nutrients to compensate for the ones lost to mineralization and burial. Those nutrients come from terrestrial weathering.

Indeed, weathering and nutrients availability influence primary productivity and have thus the potential to impact marine organic matter burial. A lack of nutrients would lead to a lower primary productivity in the ocean. If the preservation efficiency of marine organic carbon (the ratio of marine organic carbon buried to the marine organic initially produced - the organic carbon primary productivity) remains constant, a lower primary productivity would lead to a lower burial. However, as the preservation efficiency of marine organic matter is very low, only around 0.2 - 1.3% (Burdige 2007, Kandasamy and Nagender Nath 2016), small changes of the organic carbon preservation efficiency can also highly influence the organic matter burial. Thus, the influence of weathering on marine organic carbon burial is not so straightforward, as a decreased marine primary productivity does not necessarily lead to changes in organic matter burial, if there are changes in organic matter burial preservation efficiency due to other environmental factors.

Also, our organic carbon flux term B is a sum of organic matter burial (B+) and oxidation (B-). The organic matter burial can take place on land, or on the ocean. The organic matter buried in the ocean can be of both terrestrial or marine origin. The organic matter of terrestrial origin (approximately one third of organic matter buried in the oceans at present, Burdige 2005) is not influenced by the nutrient changes in the ocean. The organic matter oxidation (B-) does not depend on the nutrient availability in the ocean.

Therefore, there is not a direct dependence of the organic matter flux term (B) to the nutrients in the ocean. Rather in this study, we chose to look at the dependance of organic matter fluxes to climate (through the surface carbon content C), and oxygen O. And climate can influence weathering and thus nutrient availability and primary productivity in the ocean.

In our model, larger carbon values and hotter, wetter climate lead to more marine organic carbon burial for different reasons. First, for warmer temperatures the solubility of oxygen on surface water is decreased (Bopp et al, 2002) and ocean stratification is increased, leading to expansion of oxygen minimum zones (Stramma et al 2008). This increases organic matter preservation. Second, warmer and wetter climate can increase weathering, and thus the delivery of nutrients to the ocean. The consequent increase in primary productivity and oxygen consumption can lead to regional deoxygenation, and thus enhance organic matter preservation (Baroni et al 2020).

However, the global organic matter flux B, that is the difference of the total organic burial B+ and organic matter oxidation B-, does not increase in a monotonous way with warmer climate as marine organic matter burial is not the only process varying with climate.

In our study, we have made the assumption that oxidation of petrogenic organic carbon (B-) also increases with warmer temperatures (larger C contents). Thus, the evolution of organic matter fluxes with climate depends on both the relative dependence to C of burial and oxidation.

In our study, we make the assumption that for low carbon values, and thus colder climate, the organic matter flux does not vary much with climate. For intermediate carbon C values, we make the assumption that the increase in organic matter oxidation (B^-) with temperature and C is steeper than the increase in organic matter burial (B^+), which leads to a lower organic matter flux (B) with increasing C. On the contrary, we make the assumption that for higher carbon values, the increase in organic matter burial (B^+) is steeper than the increase in organic carbon oxidation ($B^{-)}$, leading to an increase of organic matter flux (B) with increasing C. This is schematized in Fig. RC2.

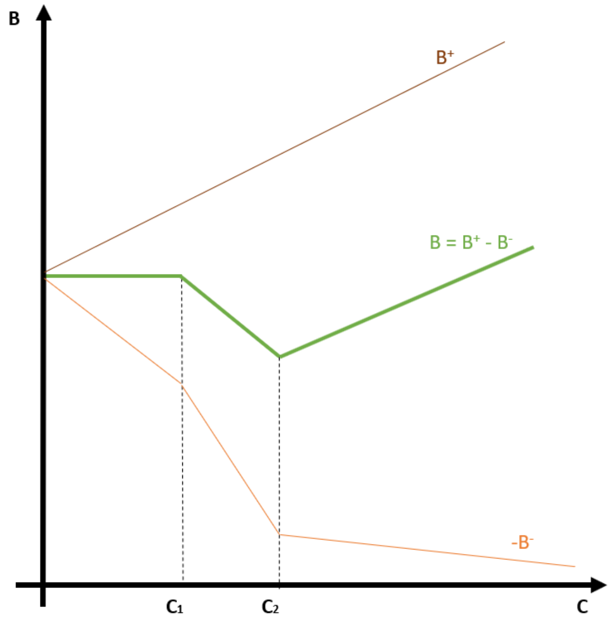

Fig RC2 : Schematic representation of the evolution of organic matter burial (B^+), organic matter oxidation (B^-) and organic matter flux ($B = B^+ - B^-$) with surface carbon.

In a revised version of the manuscript, the description of the organic matter flux term B should be clarified. We will emphasize on B being the difference between organic matter burial, B^+, (that includes terrestrial burial, and oceanic burial of organic matter of both terrestrial and marine origin) and organic matter oxidation, B^-. We do not make particular assumptions on the evolution of terrestrial burial with carbon (climate) and oxygen contents. We assume that both organic matter oxidation (B^-) and organic matter burial in the ocean (and thus B^+) increase with increasing C. However, if B^+ and B^- have different slopes of increase with C, this leads to a non monotonous evolution of $B = B^+ - B^-$ with C. The exact shape of the evolution of B(C) does not impact the results, as long as it is non monotonous, it can lead to multiple equilibria in the carbon cycle (multiple crossing of the red and green curves of Figure 2). We assume that the marine organic matter burial and thus B^+ decreases with increasing oxygen levels, resulting in a decrease of B with increasing O.

Moreover, that weathering also modulates the availability of alkalinity, which balances out the atmospheric CO2, and allows for calcification. One ends up with a triangle of calcification (take alkalinity and nutrients, releases CO2), weathering (take CO2, releases alkalinity and nutrients) and organic matter burial (take CO2 and nutrients). But these three are not in phase with each other, which in itself already results in an oscillatory pattern.

Indeed, but these time scales are to short to account for a ~10^7 year oscillatory behavior, since carbon or phosphorus have residence times ~10^5 years, about 2 order of magnitude more rapid than our phenomenon. This is why the oxygen cycle might play a role in our case. Alternatively, this could be due to other multi million year process.

Bachan, Aviv, et al. "A model for the decrease in amplitude of carbon isotope excursions across the Phanerozoic." American Journal of Science 317.6 (2017): 641-676.

Minor concern.

The y-axes in Figures 2 and 3 are incorrectly labeled.

- In Figure 2, the y-axis represents B, not dC/dt. My suggestion would be that the authors hatch the area in-between the organic and inorganic terms and label them with dC/dt>0 when the inorganic term is larger than the organic term, and vice versa.
- In Figure 3, the y-axis represents B for the green curve and Ox for the blue curve. Not dO/dt. Again, here the authors could hatch

We thank the reviewer for this suggestion that we will follow in the revised version.

References :
Baroni et al (2020), Enhanced Organic Carbon Burial in Sediments of Oxygen Minimum Zones Upon Ocean Deoxygenation, Frontiers in Marine Science, 6
Bopp et al (2002), Climate-induced oceanic oxygen fluxes: Implications for the contemporary carbon budget, Global Biogeochemical Cycles, 16, 6-1-6-13
Boulila et al (2012), A ~9 Myr cycle in Cenozoic $\delta^{13}C$ record and long-term orbital eccentricity modulation: is there a link ? Earth and Planetary Science Letters, 317-318, 273, 281
Burdige (2005), Burial of terrestrial organic matter in marine sediments: A re-assessment, Global Biogeochemical Cycles, 19, 4
Burdige (2007), Preservation of organic matter in marine sediments: controls, mechanisms, and an imbalance in sediment organic carbon budgets?, Chemical reviews, 107, 467-485
Kandasamy and Nagender Nath (2016), Perspectives on the terrestrial organic matter transport and burial along the land-deep sea continuum: caveats in our understanding of biogeochemical processes and future needs, Frontiers in Marine Science, 3, 259
Martinez and Dera (2015), Orbital pacing of carbon fluxes by a ~9 Myr eccentricity cycle during the Mesozoic, Proceedings of the National Academy of Sciences, 112, 12604 - 12609
Stramma et al (2008), Expanding Oxygen-Minimum Zones in the Tropical Oceans, Science, 320, 655-658