Answer to RC 1 :

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 $\delta^{13}C$ oscillations that can easily be related to the astronomical (eccentricity) forcing. Still, these long-term (8-9 Myr) $\delta^{13}C$ 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.

Leloup and Paillard present a new model to link astronomical forcing with multi-million-year oscillations in Earth's carbon cycle. I will immediately admit to not being an expert on astronomical forcing of Earth's surface environment or the mathematical modelling of how different modulations may have influenced surface processes. As such, it is difficult for me to make detailed comments about the modelled approach. However, I do have some general thoughts on the assumptions made by the model.

The authors are very open about the fact that this is a very simple model and that they have been unable to include several processes that may complicate the relationship between astronomical forcing and the carbon cycle. I am the first to acknowledge that any model of geological processes has to make simplifications, and that it is impossible to consider every possible control. However, I do worry that there are some potentially major factors that have not been considered, and whose exclusion from the model makes it potentially unrealistic.

Firstly, and very importantly, whilst the rate of organic-carbon burial is indeed related to global oxygen levels, the reverse is also true. Several studies have highlighted that a large increase in organic-matter deposition will cause surface oxygen levels to rise (see e.g., Lenton and Watson, 2000, Global Biogeochemical Cycles; Berner, 2004, Oxford University Press; but there are many others). It's not clear whether the authors have considered this as a two-way process.

In our study, not only are the organic carbon burial rates dependent on oxygen levels, but oxygen levels also depend on organic matter burial and oxidation. Lines 103 - 105, we state that "On one side, the burial of organic matter is facilitated in low oxygen zones. On the other side, organic matter oxidation reduces the oxygen quantity, while burial of organic matter adds oxygen to the surface system". The influence of organic matter burial on oxygen levels is then reflected in Eq. (5) : dO/dt = B - Ox. For positive organic carbon fluxes (net burial higher than net oxidation), the oxygen quantity increases and conversely. The influence of oxygen on organic matter burial is reflected in Eq. (6) : $B(C, O_1) = B(C, O_2) - \delta (O_1 - O_2)$. This dependance to the global oxygen level O is simple in our model (as discussed afterwards) : the organic matter burial decreases linearly with global oxygen contents.

On the subject of oxygen, at the moment the model seems to consider surface oxygen as a single inventory of oceanic and atmospheric oxygen levels, but the reality is that different parts of the marine realm can be very oxygen depleted regardless of overall oxygen levels. This is particularly the case for for small restricted epicontinental basins, and these varied in abundance due to tectonic configuration at various times in Earth's past, and were highly influenced by local processes and sea level changes, both of which are related to astronomical forcing.

Indeed, we fully agree with the reviewer's comment. Higher oxygen levels globally do not necessarily lead to higher oxygen levels in marine parts relevant to organic matter burial. However, it is extremely difficult to account for the numerous important local processes that control oxygen levels and ultimately the burial of organic matter. The simplest possible assumption is therefore to use a global oxygen inventory. But more importantly, our goal is not to have a realistic complex model that represents the oxygen concentration spatially. As explained in the first paragraph, our model is an illustration of the possible role of non-linearities and multiple equilibria to address the question of very long term $\delta^{13}C$ variations, a possibility that, to our knowledge, was never considered before.

Also, the authors consider oxidation of non-carbon elements as an important control, but not the potential reduction of these elements, which I find curious. And what about sulfur and phosphorus?

We make no assumption on the sign of our « Ox » term in equation (5) and it therefore implicitly represents the net effect of all processes other that organic burial. These include both the oxidation of non-carbon elements, but also reduction processes. Though the net flux represents on average an « oxidation », it is probably misleading to call it « Ox ». In a revised version of the paper, we will replace « Ox » by « Redox » to avoid misunderstanding.

I also wonder if the authors have considered the potential role of terrestrial organic-matter burial in their model. Of course, organic carbon burial in the ocean will typically be the more important sink, but there are times in Earth's history when terrestrial burial is thought to have had a massive influence on the global cycle, most famously during the Late Devonian–Carboniferous, but also in the Mesozoic (e.g., Valanginian; see Westermann et al., 2010, EPSL). This is important because the terrestrial sink is likely controlled by very different factors (not directly linked to surface oxygen) than the marine sink.

We agree with the reviewer that terrestrial organic matter certainly plays an important role, in particular at some specific times in the past. But this terrestrial sink is likely to be even more difficult to represent in simple, global terms with an idealized model. Besides, our goal is to investigate the seemingly robust relationship between astronomical forcing and organic matter burial, something which is more likely to originate in the more « stable » oceanic environment. We acknowledge that this model will not be able to represent specific peaks in the $\delta^{13}C$ due to terrestrial organic matter burial variations. This would be emphasized in a revised version of the paper.

If it isn't possible to incorporate these factors into the model, then at the very least there needs to be more open consideration of them (as well as other processes which will vary over time). But as things stand, I worry that the list of missing controls is so long at present that the model cannot really be a strong representation of reality, and that at least some of them need to be included as separate terms regarding the sources and sinks of carbon and oxygen etc.

As explained in the first paragraph, our goal is not to describe all the variations in carbon isotopes observed in the geological record, but to provide a new framework to explain the persistent long-term (8-9 Myr) oscillations observed over the Cenozoïc and Mesozoïc, as a consequence of orbital forcing. We would emphasize on the model objectives in a revised version of the manuscript.

Minor comments:

Line 51: Here 'favour' is written. Elsewhere it is 'favor'. Be consistent.

This will be corrected.

Line 99: A constant fractionation factor of -25 per mil for organic matter is a probably a big assumption given the differences in different organisms, and especially following the rise of C4 plants in the Cenozoic (considering that this paper discusses that time interval).

The value of the fractionation factor could indeed be changed for a lower value, but this would only change the numerical results, not the qualitative oscillations obtained with our model. This remark would be added in a revised version of the manuscript.

Line 117: How is the carbon cycle forced astronomically? Simply invoking an unnamed link feels rather vague to me.

The assumption that the organic matter fluxes are forced astronomically comes from the persistent observation of 400 kyr cycles in $\delta^{13}C$, and the fact that this frequency is the dominant frequency of eccentricity. Therefore, we chose to force the organic matter flux with eccentricity. However, the 8-9 Myr cycles that are the focus of this paper are not easily explained by a forcing by eccentricity, as the 8-9 Myr frequency is absent from the eccentricity spectra.

Different causal mechanisms have been proposed by authors to explain the link between eccentricity and organic matter fluxes, as explained in the introduction. For instance, Kocken et al (2019) suggested that marine organic matter burial is enhanced for low eccentricity values, as they could favor annual wet conditions and clay formation, and that the majority of organic carbon is buried in association with clay particles (Hedges and Keil, 1995). Alternatively, Martinez and Dera (2015) suggested that low eccentricity values lead to favorable conditions for persistent anoxia throughout the year, which leads to higher carbon burial in the ocean. In both these cases, lower eccentricity values are associated with higher organic carbon burial, and conversely high eccentricity values are associated with lower organic carbon burial, and the organic matter fluxes are thus “forced astronomically”. Our study does not allow us to discriminate between these mechanisms and to say if one is more plausible than the other or if both are at play. We deliberately do not choose a specific physical mechanism, and we rather focus on the implications of having organic matter fluxes that are forced astronomically on the output $\delta^{13}C$ signal, in the case where there are multiple equilibria in the (C,O) system.

Lines 132–133: Yes, but this will not be evenly distributed and even when surface oxygen levels rise, there can still be places in the ocean that can be very anoxic.

Indeed, but as explained earlier it is extremely difficult to account for the numerous important local processes that control oxygen levels and ultimately the burial of organic matter and the simplest possible assumption is to use a global oxygen inventory. In a revised version of the manuscript, we will emphasize more on this hypothesis.

Line 154–155: This will then cool the climate and reduce organic-matter oxidation, raising surface oxygen levels, both of which will act to mitigate the organic-carbon burial.

This would be true in a model with a single equilibria, if B increased monotonically with C. However, in our model B does not only represent the marine organic matter burial, it is the difference between organic matter burial B^+ (that includes terrestrial burial, and oceanic burial of organic matter of both terrestrial and marine origin) and oxidation (B^-), $B = B^+ - B^-$.

In the study, 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.

If the oceanic carbon burial B^+ increases with C, but that B^- increases more sharply with C (which is the case for intermediate carbon values, $C_1 < C < C_2$, in our model and depicted in Figure RC1), warmer temperatures (higher C) do not lead to an increased organic matter flux B, but a decreased one.

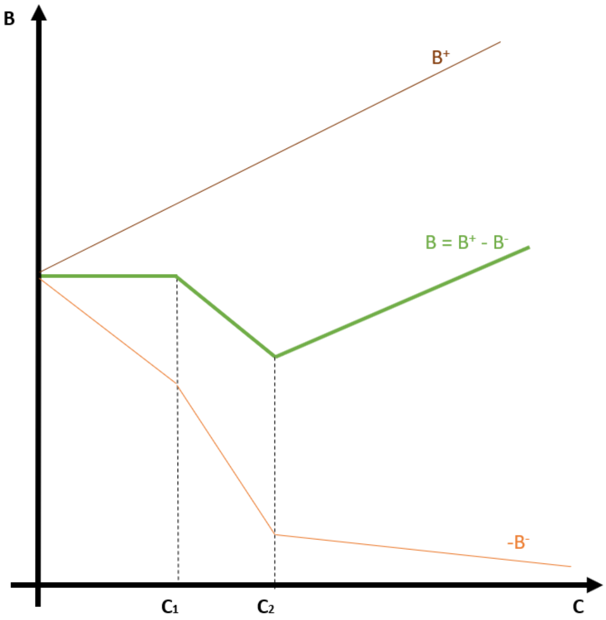

Fig RC1 : Schematic representation of the evolution of organic matter burial (B^+), organic matter oxidation (B^-) and organic matter flux ($B = B^+ - B^-$) with surface carbon.

Lines 162–168: What are 'lower', 'intermediate', and 'higher' carbon values defined as? What range?

In a revised version of the paper, the values of C_1 and C_2, ie the ranges for "lower" ($C < C_1$), "intermediate" ($C_1 < C < C_2$) and "high" carbon values ($C > C_2$) will be indicated more clearly in Section 2.2, by indicating the numerical values. For now, it is written $C_1 = C_{eq1ref} + (1/3) (C_{eq2ref} - C_{eq1ref})$ and $C_2 = C_{eq1ref} + (2/3) (C_{eq2ref} - C_{eq1ref})$. The corresponding numerical value of $C_1 =$ 44 333 PgC and $C_2 =$ 45 667 PgC will be added. Also, the reader will be referred to section 2.2 at the first mention of low, intermediate and high carbon values, for more clarity. However, we emphasize that these values are model parameters that could be changed. This would change the numerical value of the oscillations obtained, but does not change the main result of the paper : being able to obtain longer oscillations than present in the input forcing, due to the presence of multiple equilibria in the carbon cycle.

Line 175: '…we place ourselves here in one of the simplest case possible.' is rather casual language for me.

This formulation will be modified in a revised version of the manuscript.

Line 201: I assume that the organic-matter burial being referred to here is oceanic. What about terrestrial organic-carbon burial?

In our model, the terrestrial organic matter burial does not vary with oxygen, nor does the organic matter oxidation. However, the organic matter burial in the ocean is influenced by the oxygen content O (in a linear way in our model), so the total organic matter flux (sum of marine and terrestrial organic matter burial minus oxidation) varies with oxygen. This should be clarified in a revised version of the manuscript, when introducing the dependence of B to O (starting l. 132).

Line 258: What about reduction of other elements?

In a revised version of the paper, we will replace « Ox » by « Redox » to avoid misunderstanding.

Line 422: I'm not sure a mechanism is being proposed per say. It's been assumed that astronomical forcing influences carbon supply vs burial and oxygen levels, and that long-term cycles can be reproduced for a certain set of parameters. But this is all very theoretical still and there isn't a cause-and-effect link proposed for how the astronomical forcing is influenced these carbon and oxygen sources and sinks. For me, that would be the mechanism.

In this sentence, mechanism does not refer to a physical mechanism linking organic matter fluxes to astronomical forcing. Rather, it is meant as a "dynamic" mechanism, that allows to obtain oscillations with longer periods than the input forcing : in our case, the presence of multiple equilibria in the (C,O) system, that can lead to longer oscillations when an astronomical forcing is added. The sentence could be clarified by using the term "framework" instead of "mechanism", and we could emphasize the link with the explanation in l. 425.

Refs :

Hedges and Keil (1995), Sedimentary organic matter preservation: an assessment and speculative synthesis, Marine Chemistry, 49, 81-115

Kocken et al (2019), The 405 kyr and 2.4 Myr eccentricity components in Cenozoic carbon isotope records, Climate of the Past, 15, 91-104

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