Climate
of the Past

Discussions

Interactive comment on "The Plio-Pleistocene climatic evolution as a consequence of orbital forcing on the carbon cycle" *by* Didier Paillard

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Response to anonymous referee #2

First, I would like to thank the referee for his comments and support. He addresses several important technical points listed below and makes a more general remark, that my conceptual model is rather generic and could correspond to other geomorphological mechanisms than the one described in the manuscript. I believe that most of his comments can easily be addressed by a more explicit description of the model, its parameters, and its results, as explained below on a point-by-point basis (RC: the reviewer comment; AC: my response).

RC1 : For component (1), I see no error in the carbon cycle equations as written,

but there are a few steps/assumptions that are not clearly articulated. Adding more details deriving each equation would make the paper easier to follow. In equation (1a) it is implicitly assumed that the weathering and volcanic fluxes can be lumped together (which is fine based on the assumption that both approximate the mantle isotopic value), though this is not stated. (Otherwise the equation should be dC/dt = V + W - B − D).

AC1 : I somewhat disagree on this point. Silicate weathering W takes one CO2 molecule from the atmosphere (or more precisely H2CO3 from precipitation and runoff) and transforms it into HCO3- (through acido-basic reaction or proton exchange) in the river system and finally the ocean. When considering the oceanic carbon budget alone, W indeed adds one carbon in the ocean. But I am considering the "global" Earth surface budget (ocean + atmosphere) and therefore W has no net effect on C. Therefore W does not appear in equation (1a) for dC/dt. Its impact on the global carbon cycle arises only through the ocean alkalinity budget (dA/dt) and carbonate compensation, which leads to carbonate deposition D being directly linked to silicate weathering through D = W-V+B.

=> I will insist on the fact that C includes both the ocean and atmosphere, and better explain the underlying mechanisms.

RC2 : Next, I think it would be helpful to start with the full version of equation (2b): d/dt(δC*C) = V*δV - B*δB - D*δD Then it would be more straightforward to see how the final version is obtained through the product rule and assumption that δc = δD as well as constant values of δV = -5‰ and δB = -25‰. This is particularly important because it is more typical to describe a constant fractionation of organic carbon with respect to δC, rather than a constant δB.

AC2 : This is indeed a good idea. This corresponds also to the remark from Peter Köhler (doi:10.5194/cp-2017-3-SC1) that the equations should be clarified, and the underlying assumptions should be more explicit.

=> I will add the derivation of equation (2b) and explicit choices for $\delta 13V$, $\delta 13B$, $\delta 13D$.

RC3 : On that note, adding an appropriate subscript to the δ notation (rather than writing as $\delta 13$) would be helpful to differentiate between the δ values for each flux.

AC3 : I will follow this suggestion and write the final equation (2b) as: $d\delta 13C/dt$ = ($V(\delta 13V - \delta 13C)$ - $B(\delta 13B - \delta 13C)$)/C

RC4 : Finally, there should be explanation of scaling between pCO2 and total C (namely, that the assumptions are being made that the ocean inventory of Ca2+ does not change and that the mass of carbon in the system is well-approximated by the ocean bicarbonate pool).

AC4 : This corresponds also to the remark from Peter Köhler. As explained in my response (doi:10.5194/cp-2017-3-AC1), this will be justified in the revised version.

RC5 : For component (2), it would be helpful to provide the chosen value for the scaling term a in equation (3) in the text and not just the caption to Fig. 2. Later in the paper, it is mentioned that a has to be of the same order as the equilibrium organic C burial flux, but the value in the caption is in fact double the equilibrium burial flux. There should also be a description of how this value was determined (presumably to get the right amplitude in the modeled δc)?

AC5 : I agree that a better discussion of parameter values could be included in the text, though these values are indeed determined empirically in order to get a qualitatively correct response. The amplitude a is indeed the double of the equilibrium flux B0 for the particular experiments shown on Fig.2. The comment in the text was slightly more generic (" the strength of the forcing a needs to be of the same order than the baseline value B0. This is a robust feature, which does not depend on model setting or parameters ").

=> I will add a short discussion on the choices made for a. I will rewrite the above sentence somewhat differently, as " when variations in B (or equivalently the parameter

a) are smaller than its baseline value B0, the model cannot reproduce the amplitude of $\delta 13C$ observed in marine benthic records ".

RC6 : To me, component (3) is the most novel element of this conceptual model. This thresh- old term allows for a switch between two styles of periodic forcing of the organic carbon burial flux. In general, the periodic forcing reduces the value of B, except if the sedimentary reservoir is near to its maximum size, in which case periodic forcing switches to increasing the value of B. . . Next, what is the basis for setting the threshold condition at S < 0.85 SMAX? The text notes that this threshold mechanism causes a switch in organic carbon burial after significant sea level drops at 2.4-2.5 Myr and 0.35-0.65 Myr, but was the threshold set in order to provide this result?

AC6 : I should certainly also be more explicit here. The "normal" (pre-Quaternary) situation (progradation) is indeed when the periodic forcing reduces the value of B. Then the sedimentary reservoir S is typically at its maximum (we have S=Smax) as shown on Fig.2. But after every significant new sea-level drop (from the zmin "river incision" curve based on Lisiecki and Raymo, 2005), Smax = zmin3 increases significantly and the situation is switched to "aggradation". This first switch (switch ON) is not strongly dependent of the 0.85 threshold parameter, since a sea-level drop as small as about 5% will induce a sufficient increase in Smax (=15%) to trigger the change. But the switch back to normal (switch OFF) and therefore the duration of the "aggradation" phase, will depend more strongly on this threshold choice. In other words, concerning the two major "aggradation" phase discussed in the text (2.4-2.5 Myr and 0.35-0.65 Myr), their starts are directly linked to the significant sea level drops (at 2.5 Myr and 0.65 Myr): they are independent of the threshold value. But their duration is rather directly linked to this threshold value of 0.85 and also to the choice of parameter b. The choice of a different threshold value than 0.85 will consequently affect the amplitude of the differences between experiments (b) and (c), but not the timing of these differences.

=> This needs to be explained in the revised manuscript.

RC7 : Again, the value of the scaling factor for the growth rate of the sedimentary reservoir, b, should be provided in the text, along with an explanation of how this value was determined.

AC7 : I agree. And again, the value of b is a rather empirical choice. As explained above, its value will affect the duration of "aggradation" phases, and consequently the amplitude of the differences between experiments (b) and (c).

=> I will add a short discussion on the choices made for b.

RC8 : Also, in Figure 2, it is clear to see why the addition of this threshold term appreciably changes model behavior around 0.6 Myr, but not obviously earlier in the record. Maybe this is just hard to see because of the scale on the axes?

AC8 : There is indeed a significant change around 0.6 Myr that explains why the 400 kyr 13C cycles are disturbed at this time. There is also a significant change at about 2.4 MyrBP in the 13C results on Fig.2 (experiment (c): red curve) whereas the results without this mechanism (experiment (b): blue curve) the simulated 13C values are significantly out of the range of observed values. Interestingly, the switch model was designed to address the disturbed "400 kyr 13C cycles" of the last 1MyrBP. The better agreement with data at 2.4 MyrBP was not expected, and comes as a bonus.

=> I will clarify the role of the threshold mechanism when discussing results shown on Fig.2, and I will add a short comment on this last point in the conclusion.

RC9 : However, it does not seem that the conceptual model is particularly linked to the mech- anism proposed (a shift between progradational to aggradational river systems). Paillard suggests in the introduction that "astronomical parameters are influencing climate through other mechanisms than the growth and decay of ice sheets", but it seems to me that what's been done is to link organic carbon burial to the growth and decay of ice sheets via the impact on sea level. This means the conceptual model is equally applicable to any process related to sea level that can drive a threshold response in

organic carbon burial. This is not a flaw in the conceptual model, but parts of the text could be rewritten to emphasize that the geomorphological mechanism is only one possible physical interpretation of what the model actually describes.

AC9 : The first aim of this model is to link the observed 400-kyr 13C oscillations and the associated carbon cycle changes to the astronomical forcing, through the dynamics of organic matter burial. This is in general fully independent of sea level changes, except for the most recent Quaternary period. Since our knowledge of the carbon cycle is much more detailed over this recent period (pCO2 data, numerous 13C records, ...), it is necessary to explain both the rather generic 400-kyr 13C oscillations observed during the Cenozoïc and beyond, but also why the Quaternary 13C oscillation look different and how this relates to observed pCO2 fluctuations. As explained in the introductory part of the paper, I am using a deductive line of thought. I certainly agree with the reviewer that the mechanism suggested here is probably not the only possible one. It is nevertheless (to my knowledge) the first one suggested so far that may explain both the recent past and the more remote one, in the same conceptual framework.

=> I will add a short comment on this last point in the conclusion, together with the following point (AC10 below).

RC10 : Also, more discussion about the relationship between pCO2 and δ13C cycles rep- resented by this conceptual model would be welcome. Based on the introduction, I expected further explanation of phasing between simulated cycles and eccentricity. In particular, how well has the model accounted for a change in the nature of the 400 kyr δ13C oscillation in the last million years?

AC10 : Indeed, it is probably important in the discussion to re-state the main objective of this model: reproducing not only the 400 kyr δ13C oscillation seen during the pre-Quaternary, but also explaining why it is perturbed during the last million years, and to insist on the final δ13C conclusion: assuming that this perturbation is caused by major sea level drops, as performed in this model, leads not only to a better agreement for

the δ13C curves, but also explains several features of the CO2 changes.

=> This will be discussed in more details and more clearly re-stated in the conclusion.

RC11 : Also, why is the 100 kyr term added only to the modeled δ13C and not pCO2?

AC11 : The 100-kyr term added to the 13C results (orange curve) is just an "ad-hoc" addition to improve the match with data, based on the (usually accepted) hypothesis that this 100-kyr oscillation in the 13C is liked to the varying size of the biosphere. There is no such data for the pCO2 over the last 4 million years, and there is no simple explanation for the observed pCO2 100-kyr cycles: adding this cycle a posteriori is therefore certainly not justified for pCO2. More importantly, the 100-kyr cycle is not the subject of this manuscript, so may be I should simply remove the orange curve to simplify the figure.

RC12 : Perhaps add the eccentricity and filtered eccentricity to the same figure as the modeled curves.

AC12 : Yes. This would indeed simplify the discussion of the results in terms of phasing, according to the above comments (RC10).

RC13 : Finally, in the results section of the text, comparison between blue and black curves in Figure 2 is cited as evidence for good agreement between model results and observations, but both these curves are model results.

AC13: This was a bad formulation in the original text. I meant that experiments a and b, with and without the long-term trend, (ie. the black and blue curves) were very similar in terms of 13C, and both were comparable to the data (the grey curves).

=> This sentence will be changed in the revised manuscript.

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C7