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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

Anonymous Referee #2

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This paper presents a nice conceptual model for interpreting changes in orbitally-paced variations in pCO2 and carbonate δ13C through the past few million years. There are three components to the model. 1) A model of steady state carbon cycle fluxes and their isotopic composition. 2) A periodic term (linked to precession) modulating organic C burial. 3) A threshold term for the relationship between organic C burial and precession based on the global sea level curve.

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). 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‰Ṫhis 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. On that note, adding an appropriate subscript to the δ notation (rather than writing as δ13) would be helpful to differentiate between the δ values for each flux. 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).

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)?

To me, component (3) is the most novel element of this conceptual model. This threshold 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. 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. Next, what is the basis for setting the threshold condition at S < 0.85SMAX? 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? 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?

However, it does not seem that the conceptual model is particularly linked to the mechanism 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.

Also, more discussion about the relationship between pCO2 and δ13C cycles represented 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? Also, why is the 100 kyr term added only to the modeled δ13C and not pCO2? Perhaps add the eccentricity and filtered eccentricity to the same figure as the modeled curves.

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.

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