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Interactive comment on "The 405 kyr and 2.4 Myr eccentricity components in Cenozoic carbon isotope records" *by* Ilja J. Kocken et al.

Anonymous Referee #2

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This manuscript presents an interesting modelling study of the eccentricity components in the Cenozoïc carbon isotope records. Overall, the paper is well written and the obtained results are worth publishing. I nevertheless have some major comments on the model-data comparison and the overall discussion, which both appear quite incomplete and insufficient.

Major comments

1 - The model used here computes many variables, but only a few of them are discussed. In particular, since the ocean temperatures (in the Pacific) are computed, I would expect to find a comparison between them and the 18O records presented in the introduction (Figure 1). Such a comparison is not shown and not even discussed. From the legend of Figure 2 (Âń deep Pacific temperature . . . are omitted from this plot

because their pattern mirror that of Ctot), I understand that temperature is dominated by the carbon forcing (pCO2) and exhibits a strong 2.4 Myr oscillation. What about its amplitude ? Is it compatible with data ? I suspect this is not the case. The pCO2 changes are large (about a pCO2 doubling at 2.4 Myr frequency) therefore modelled temperature changes should be in the 3 to 4°C range for rather standard values for climate sensitivity, and the corresponding 18O amplitude should be in the 0.5 to 1‰ range. This does not seem to be compatible with data as shown on Figure 1. In any case, whatever the results, I do not understand the modelling strategy: why using a rather sophisticated model that computes many outputs, but only discussing (cherry picking ?) some of them, and not others ?

2 – Similarly, the CCD output of the model is also not compared to the real world. This is a bit less disturbing, since the authors are not showing any CCD reconstructions over this time span. But such data exist, though probably with low resolution. For instance, according to Paelike et al (Nature, 2012), the Pacific CCD is deep and stable during this time (Oligocene-Miocene) up to about 18.5 Ma. Again, this does not seem consistent with the model results. Interestingly, they also observe rather large changes in the CCD during the Eocene (CAE events), that may, or may not, correspond to the simulated changes in this paper ? In any case, some thorough discussion of the CCD outputs with respect to observations seems to me absolutely necessary. Again, what is the point of computing these variables, if not for performing some comparison with data ?

3 – According to the final sentence, high resolution pCO2 proxies would be necessary to check if a strong 2.4 Myr signal is present or not over the Cenozoïc, as suggested by the model. As mentioned in my first comment, such a strong eccentricity signal in greenhouse gases should probably have already been detected in the (climate) 18O data. But more importantly, such pCO2 proxy data are in fact readily available for the Plio-Pleistocene (eg. Bartoli et al., 2011 ; Seki et al, 2010 ; . . .) and no such large (2x) pCO2 changes are seen, while large climatic changes are obvious. A simple model

also based on organic carbon burial on continental margins was proposed recently (Paillard, CP, 2017) to account both for the 13C and the pCO2 data over this period.

4 – Spectral analysis represents a rather large part of the paper (Figs 3 to 6). Still, I do not quite understand how this helps for the discussion or for the conclusions, beyond the (quite expected) fact that long-term processes are acting as low-pass filters. Either I missed some important point, or probably there is a far too large weigh on spectral analysis in this paper.

5 – The same comment applies for the use of red noise in the experiments. Obviously, there is here some (rather implicit) attempt to Âń fit Âż the spectrum of the data with a deterministic model plus a red noise. But what is the point ? I understand that red noise fitting is useful for spectral line detection. But this is not the topic of the paper. What do the noisy experiments tell us about the dynamics of organic carbon on continental margins ? Does a nice spectral fit help the authors to make their point ? I am personally not convinced.

Other comments

6 – Page 4 line 18 ; Page 10 line 3 and line 10 Âń shifting of spectral power Âż. I certainly do not recommend using this word Âń shifting Âż in the current context. In signal processing, a spectral shift is a change of frequency between input and output. This is not the case here, since output frequencies are exactly the same as input ones. The authors are referring possibly to the fact that high frequencies are damped. This is simply called a low-pass filter, not a frequency shift. Or possibly that the amplitude modulation of the forcing can be extracted thanks to some non-linearity of the model (or “clipping” of the forcing). But again, this is not a “shift” in frequency, but a de-modulation (ie. the most simple tone-combination).

7 – Page 7, line 7 : ETP Âń could be considered more objective Âż. I do not understand why. From a mathematical viewpoint, using an insolation forcing is a parametric choice (eg. choosing latitude and season). ETP is also parametric (relative weigh of tilt and

precession). Furthermore, in ETP, the phasing of precession is arbitrarily fixed to 2 values only (a plus or a minus sign), while choosing a specific season offers more freedom. Insolation is more physically based, ETP is not. Obviously, there is no specific reason to choose 65°N in summer, since there are no ice sheet present at this location. In the context of the model presented here, a much more simple and objective choice would be to use only one parameter : eccentricity, or tilt, or precession only. Then the discussion on mechanisms would be easier. Since the focus is on the 405 kyr and 2.4 Myr eccentricity frequencies in the carbon system, I am not sure that using tilt in the forcing is relevant (except may be to discuss the 1.2 Myr modulation vs the 2.4 Myr eccentricity one, but this is probably not quite the topic).

8 – Legend of Figure 2 : atmospheric pCO2 is shown on the figure, though the legend says just the opposite.

9 – Page 10 line 17 : Âń 180° . . . out-of-phase Âż. No, since Âń out-of-phase Âż means that there is no phase relationship. Here, it is in Âń anti-phase Âż, something very similar to Âń in-phase Âż.

10 – Page 11 line 9 : Âń δ13C minima are phased with ETP maxima Âż. No. The authors probably mean that δ13C minima are compared with ETP maxima, or equivalently that the relationship between δ13C and ETP is measured by the phasing between -δ13C and ETP.

11 – Page 11 line 26 : Âń 0.5 Myr Âż please add exponent -1.

12 – Many figure legends in the supplement are incomplete or inconsistent with the figure. which makes it difficult to understand. . . Figure 1: 3 colored curves but only 2 description. Figure 2: A/The red and purple curves are co2 and pacific temperature, but which one is which ? According to B, purple might be CO2, but what is “tcb” ? Figure 3: What is the green curve ? Figure 6: The legend mentions 18O records, but the figures displays apparently only 13C ones

References

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