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Climate of the Past Discussions

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Interactive comment

Interactive comment on "Astronomical Calibration of the Ypresian Time Scale: Implications for Seafloor Spreading Rates and the Chaotic Behaviour of the Solar System?" *by* Thomas Westerhold et al.

F. Hilgen (Referee)

f.j.hilgen@uu.nl

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The ms provides an important contribution to the construction of an/the Astronomical Time Scale for the early Paleogene and its implications for seafloor spreading rates and the chaotic behavior of the Solar System, and is as such well suitable for publication in Climate of the Past. The presented integrated high-resolution stratigraphy is truly impressive and it is good to see that some naughty issues now seem to be finally solved. Nevertheless, several issues have to be addressed before the ms can be accepted for publication.

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Major points

1) Statistical identification of the very long period ∼2-Myr eccentricity minima. The eccentricity nodes associated with the very long, 2-Myr, eccentricity cycle are now visually determined in their proxy records, but preferably they should also be pinpointed by means of statistical analysis, such as complex amplitude modulation or the method outlined in Meyers (2015). Such an independent statistical confirmation of the position of these nodes is critically important next to their visual determination to convince the reader of the correctness of the conclusions drawn in the ms. Otherwise the authors have to clearly state why they did not carry out the necessary and logical statistical analysis. Especially the statistical method introduced by Meyers (2015) seems very helpful to reconstruct eccentricity and capture the nodes associated with the very long term eccentricity cycle, so the question is why such statistical methods have not been applied. I guess that the authors may well have given it a try, so in that case, why was it not included in the ms (even to show that these statistical approaches do not work well in this particular case)? The authors may thus wish to discuss in some detail the (dis)advantages of a visual versus a statistical approach. This topic is discussed in some detail by Hinnov (2013) and has been presented in more detail by Steve Meyers in some of his presentations. The point here is that visual recognition of cycle patterns albeit being subjective can be considered an expert system, being able to identify distortions of the signal that are very commonly encountered in cyclostratigraphic records (see e.g. point 2) and that may cause problems when applying statistical techniques (see also Hilgen et al., 2015)

2) Potential distortion by non-linear response of the climate system. The authors have to explain that the amplitude changes they see in their proxies are related to the amplitude of the ∼100-kyr eccentricity cycle and not caused by a non-linear response of the climate system to the eccentricity forcing through associated changes in the precession amplitude. This issue might become critical when dealing with the proxy expression of early Eocene hyperthermals. Evidently, the "distortion" caused by such a non-linear

response will also have consequences for the outcome of the statistics as I guess that these usually start from linear relationships. This issue has to be addressed in the discussion.

3) Exclusion of expression of 1.0-Myr eccentricity cycle. The authors claim that they have detected the expression of the transition from libration to circulation of the very long period eccentricity cycle in the geological record. However, to be sure, they have to address the following two points. In the first place, what is the role of the relatively strong ∼1.0 Myr eccentricity component (related to g5-g1, and can also be written as a combination of ∼100-kyr components) , especially in determining the node around 53 Ma that they attribute to the ∼2-Myr cycle. They should thus make clear what the exact expression of the ∼2-Myr cycle (related g4-g3) both in the solutions and their records is.

4) Reliability of astronomical solution 1. And secondly, how certain are the authors now that the preferred solution of La2010b (or c) is reliable back to ∼56 Ma, as before they have stated (in Westerhold et al., 2015) that the solution is only reliable to 48 Ma. Indeed more and better records are now available, which seem to have led to their different appreciation of the solution. However, the pattern of the ∼100-kyr eccentricity cyclicity also needs to be reliable before the ∼2-Myr cycle can be thrusted as the latter cycle can also be written as a combination of two ∼100-kyr eccentricity components (95 and 99, and 124 and 132 kyr). One reason that Lauretano et al. (2016) had a preference for the 2 cycle age model rather than the alternative 3 cycle model for C23n was the apparently good fit of the distinct four 100-kyr maxima in the d13C records with the pattern in 400-kyr cycle no. 127 now (correctly) tuned to no. 126. However, this 400-kyr cycle (i.e., no. 126) does not show the expected 4 relatively strong ∼100-kyr maxima in its maximum in addition to less distinct ∼100-kyr maxima in d13C in the 400-kyr minima above and below. To me this suggests that the pattern of the ∼100-kyr eccentricity cyclicity might already not be fully reliable around 50.8 Ma, so this raises doubts about the reliability of the solution further back in time. This uncertainty and

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lack of perfect fit should be addressed. The authors should know how careful you have to be when comparing the details observed in proxy records with the solution when its reliability becomes less certain, as they also state in the ms.

5) Reliability of astronomical solution 2. The authors discuss shortly the origins of the different La2010 and La2011 solutions. This is an important issue as their from a cyclostratigraphic perspective preferred La2010b (and c) solutions have been adjusted to the short-term INPOP08 ephemeris solution, which is considered less stable and reliable than either INPOP06 (La2010a) or INPOP10 (La2011 solution), as there is a bias in INPOP08 regarding the position of Jupiter. This point should preferably be elaborated in somewhat more detail as the authors claim that they find the best fit with the La2010b (or c) solutions which are considered less reliable from an astronomical point of view. But see also points 3 and 4.

6) Paleomagnetic data and interpretation: It would be good if a paleomagnetic expert could have a look at the data and interpretation. This is critical and I am not an expert in this field.

Minor points:

The use of the word random in l.1, p.14. This is not a correct word/term to describe the outcome of non-linear complex systems such as the Solar System, as such systems do not behave in a random way.

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