Comments on the revised Climates of the Past Discussion Paper:
Astronomical Calibration of the Ypresian Time Scale: Implications for Seafloor Spreading Rates and the Chaotic Behaviour of the Solar System?

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This manuscript seeks to address several related objectives: (1) to resolve a controversy about appropriate astronomical tuning within magnetochron C23 (how many 405 ka cycles are present?), (2) to solve the so-called "50 Ma discrepancy", (3) to identify the correct theoretical model(s) for short eccentricity tuning during the Yepresian, (4) to test for the chaotic behavior of the Solar System, and (5) to provide a complete Yepresian Astronomical Time Scale (YATS), yielding time-calibrated biostratigraphic, magnetostratigraphic, and chemostratigraphic data. The study proposes to achieve these goals, and in addition, suggests that an increase in ocean spreading rate between 51-52.5 Ma is linked to chaotic orbital behavior, through an influence on dynamic mantle flow.

The fundamental questions that this study seeks to address are well established in the literature, and they constitute important issues worthy of discussion in Climates of the Past. Therefore, in reviewing this manuscript I have considered two essential questions: is the data synthesis conducted appropriately, and are the interpretations robust? As mentioned in my prior review, the data production and assimilation campaign that is the foundation of this study is an impressive effort. Given the wide range of topics this study covers, however, there are varying degrees to which each hypothesis has been tested, and in my opinion, improvements are needed to more clearly describe the hypothesis testing and the robustness of the results. I provide more information on these recommendations below. Once these issues are addressed, I believe the manuscript will be suitable for acceptance, and it will be an important and valuable contribution to Climates of the Past.

Before proceeding however, I would like to note that there is an important aspect of this study that is not presently emphasized, but has broad relevance to the field of chronostratigraphy. This work provides an excellent case study into the challenges, uncertainties and potential of chemostratigraphy, magnetostratigraphy and biostratigraphy, made possible by the integration of comprehensive data sets spanning two ODP campaigns (Leg 207 & 208). There are valuable lessons from this study that apply more broadly to the science of interpreting magnetostratigraphic, biostratigraphic and chemostratigraphic data. I recommend emphasizing this important aspect in the abstract and introduction. It is already discussed nicely in the main body of the paper.

1. Secular Resonances and Chaos
1.1 Statistical Tests for Chaos
As outlined in Ma et al. (2017, Nature 542, 468-470), there exist several tests for verifying the emergence of the (s4 − s3) − (g4 − g3) secular resonance in the geologic record. Emergence of this secular resonance underlies the chaotic behavior predicted by the theoretical models.

A. For quantification of g4-g3:
- Amplitude modulation of the 405 kyr long eccentricity cycle
- Amplitude modulation of short eccentricity
- Amplitude modulation of precession

B. For quantification of s4-s3:
- Amplitude modulation of ~40 kyr obliquity term

C. Phase relationships:
- For example, the anti-phased relationship between long-eccentricity “grand cycles” and short eccentricity “grand cycles”

D. Radioisotopic time control, or biostratigraphy-magnetostratigraphy-chemostratigraphy calibrated to radioisotopic dates, to guard against missing portions of “grand cycles” that could be erroneously interpreted as indicating a resonance transition.

Note that, for parts A-C, there are numerous ways to go about evaluating the modulations (e.g., complex demodulation, power spectrum integration). An essential revision to the manuscript is the explicit discussion of these 6 tests, an evaluation of which ones the Ypresian data passes, which it fails, and which are not possible. This will make it more obvious to the non-specialist where ambiguity exists, and where it doesn’t.

In the prior review of this study, I recommended the authors construct an analysis similar to that in Table 1 of Ma et al. (2017), to evaluate the possibility that changes in sedimentation rate (including hiatus) are influencing the observed modulation patterns (test D above). It was not my intention to imply that the authors require high-precision radioisotopic data for the success of their study. Rather, I hope that the authors provide some quantification of how much time could be missing, and where in the stratigraphy it is most likely absent, based on independent constraints. To some degree, suggestions of this are woven into the existing text (e.g., pg. 12, lines 1-2), but it is not presented in a comprehensive and linear manner. For the sake of transparency and evaluation by colleagues, this type of information is essential; the resulting uncertainties may be very large, but that is important to explicitly acknowledge. And compared to the tremendous amount of work that has gone into generating and interpreting the impressive records from Leg 207 and 208, this is a relatively small investment of time.

It is also worth emphasizing in the manuscript that, in absence of high-precision independent time control, it is essential to combine multiple records from multiple regions, to help safeguard against incompleteness that is otherwise difficult to assess qualitatively and/or quantitatively.

Finally, given that the approach used in this manuscript is to develop an astronomical time scale "anchored" to a theoretical solution, the 6 statistical tests outlined above also provide a critical assessment of the veracity of the hypothesized YATS.

1.2 Additional Comments on Node Identification
One very interesting result of this study is the reinterpretation of cycles in the interval spanning 68-92 rmcd at Site 1258, suggesting the emergence of obliquity forcing. In precession dominated settings such as those investigated here, the emergence of obliquity can occur during eccentricity nodes. So, this provides some independent evidence for the existence of an eccentricity "grand cycle" node at ~80 rmcd (Site 1258).

Is there any indication of obliquity cycles during the other proposed eccentricity nodes? If so, this would provide quantitative evidence supporting correct eccentricity node identification (this is essentially another way of at getting at g4-g3, in settings that are almost exclusively precession forced).

2. XRF Data
As noted in the prior review, the intercalibration results from Site 1267, comparing XRF scanning campaigns conducted on two different instruments, look excellent. But an r^2 value of 0.09 associated with the Site 1262 XRF Fe data sets indicates a surprisingly poor correlation. In their response, the authors argue:

> "There might be a misunderstanding here. Figure S3 shows the intercalibration for Fe intensity data obtained from different generations of XRF core scanners and their distinct hardware for Site 1262 and 1267. NOT reproducibility."

I understand the practical reason for conducting this intercalibration, but this exercise is equivalent to evaluating inter-instrument reproducibility, and the results are not reassuring. The good news is that Site 1262 doesn't appear to be really essential to the conclusions. So rather than using problematic data, my recommendation is to do one of the following: (1) rescan the critical interval, or (2) eliminate the suspect data from the manuscript. But if the authors choose to keep the data in the manuscript, at a bare minimum, they should explicitly acknowledge the problematic nature of this data in the main text, and indicate that their primary conclusions do not rely on it.

Again, I think it would be worthwhile, as a general practice, to adopt an XRF data reporting approach that quantifies instrument stability (e.g., see Figure A.1 of Ma et al, 2014), and reproducibility based on duplicate analyses (e.g., see Figure A.2 and Table A.1 of Ma et al., 2014). It seems inconsistent that there is no discussion of data quality in section 2.1 ("XRF core scanner data"), in contrast to the other geochemical data ("2.2 Bulk stable isotope data").

3. Chaotic orbital influence on dynamic mantle flow

As mentioned in my prior review, I am concerned about the proposed link between chaotic orbital behavior and changes in ocean spreading rate, because no description is made of the physical mechanism by which it is manifested, either qualitatively (how does orbital behavior impact mantle flow, and how would a chaotic transition thus be expressed as an increase in spreading rates?), or quantitatively through modeling. Of course, some level of speculation is important and productive in science; if instead the authors were proposing a new link between chaotic orbital behavior and climate/oceanographic change, this would not be too difficult to justify as plausible, considering that there exist good conceptual and quantitative models for how astronomical cycles influence insolation and climate.

The critical difference with the present study is its speculation that astronomical perturbations are sufficient to influence mantle flow, without discussion of an existing theory to support it, or elaboration on how this could plausibly happen. If the authors were correct, however, this would be an important discovery. I encourage the authors to further develop the underlying theory in this manuscript (at least in a qualitative sense), or to reserve discussion of this speculative hypothesis for a future manuscript where it can be treated in greater detail.

4. Additional comments

pg., 17, Lines 10-11: note that mechanisms have been proposed that account for low-latitude expression of obliquity forcing (see STIG; Bosmans et al., 2015, Climates of the Past 11, 1335–1346).

For individuals not familiar with the site numbers, it would be helpful to explicitly distinguish the Walvis Ridge data (Leg 208; Sites 1261, 1263, 1265, 1267) from the Demerara Rise data (Leg 207, Site 1258), when feasible, throughout the manuscript.

Figure S14. It would also be helpful to put "kyr" next to the numbers in the 2 and 3 cycle model plots (similar to the lower plot labels, "cm").