Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-74-RC1, 2017

Climate of the Past
Discussions

Interactive comment on "Synchronizing early Eocene deep-sea and continental records – new cyclostratigraphic age models from the Bighorn Basin Coring Project" *by* Thomas Westerhold et al.

Anonymous Referee #1

Received and published: 5 December 2017

The aim of the ms is to construct astrochronologic age models for the Bighorn Basin Coring Project and to compare and synchronize these age models with those of the deep-sea. This is a valid approach as the continental succession of the Bighorn Basin is marked by high sedimentation rates and the number of precession related cycles might be more easy to count than in the deep-sea records with their associated CaCO3 dissolution especially during the PETM. The "new" age models are not fundamentally different from published age models based on outcrop. Yet, it is timely to compare them in detail with the marine models, also in view of discrepancies with independent He3 based age models. The ms. can probably be accepted after major revision but certain issues definitely need further clarification.

Major points.

Title: Looking at the ms, one may ask why new is included. Are these new cyclostratigraphic age models for the BBCP cores, but then what are the old cyclostratigraphic age models from the project. Moreover, these age models are also not particularly new when compared with the existing age models based on outcrops, as these are largely identical.

MTM spectra 1. The MTM spectra might be somewhat problematical as the null-spectrum and confidence limits do not follow the shape of the spectrum very well. Is this a problem of using the "wrong" model for calculating the null-spectrum (as in Vaughan et al., 2011)? Is it as such logical that the power of all thicknesses between $\sim$2 and $\sim$20 m plot above the 99% CL? This band contains the dominant 3.5 and 8 m cycles, but constant power above 99% for such a broad frequency is not very logical.

In this respect, the authors should preferably not use the Mann and Lees (1996; ML96) robust red noise approach that is in SSA-MTM tookit, as it has a tendency to artificially create low frequency cycles (as documented in Meyers, 2012; example in his Figure 2D illustrates that there is a 90% chance of getting false long period cycles from noise). If they want to use the ML96 approach, they should use the one in Astrochron, which fixes this 'edge-effect' problem.

The MTM spectra often show a bewildering numbering of peaks in the frequency band that is of primary interest for the paper. This large number of peaks likely stems from the very long and high-resolution character of the records, but it might be preferable to attempt reducing the number of spectral peaks in this band, as less peaks / resolution imply greater stability of peak position, and is more easy to interpret.

MTM spectra 2. The dramatic reduction of power at very low frequencies in their MTM spectra is due to the data detrending. My concern is that the frequencies they are interested in are up against this detrended region of the spectrum. So if they want to do this detrending, they should also show power spectra without the trends removed, so one can better evaluate the nature of the peaks (if they are real or a consequence of detrending).

Half-precession. The authors constantly use the term half precession cycle for their 3.5 m cyclicity while their precession related cycles are often more than twice as thick. This problem was also encountered by Abdul-Aziz et al. (2008), when studying the Polecat Bench and Red Butte sections; they concluded that this cycle, which is related to prominent individual paleosols, does not represent half (or semi-) precession, but has a period that is significantly shorter and closer to that of the Heinrich events of the last 100.000 year. This was confirmed by the results of bandpass filtering, and the same is the true for the results of the filtering in the present ms (see their Figure 5 where more than two cycles fit into one precession-related cycle). Hence, the authors should use sub-precession or millennial-scale rather than half-precession.

Precession minimum (l. 24, p. 9). The authors mention that the PETM onset was in a precession minimum, but it is not clear where that comes from (they refer to see above). Do they mean the Fe- minimum between their cycle 1 and -1? But where is the phase relation with precession based on, or it this a mistake? It is also not perfectly clear to me how that might explain the 4-kyr discrepancy in addition of the one precession cycle misfit, also because discrepancies of 4-kyr are within the uncertainty of all the age models.

Comparison between cyclostratigraphic and Helium-based age models. (l.10-28, p.10). This discussion is becoming a bit semantic and potentially far-fetched, going into much detail, which may not all be that relevant to explain the observed major offset of 40-kyr for the initial rapid recovery of the CIE. Also, the average duration of a precession cycle remains an average and longer precession periods are generally found in intervals with high eccentricity (maxima) and shorter cycles during eccentricity minima (as the different cycles are differently modulated), so this plays a role as well. However, again the difference might be either too small to explain the offset or will only (slightly) enhance it. A relevant question to ask is whether the Helium isotope ratio is not affected (or not)

by the enhanced volcanic activity at that time as the East Greenland flood basalts may have formed at the same time (see Wotzlaw et al.).

Almost. A very interesting and also intriguing aspect is the potential causal connection between Biohorizon B in the Bighorn Basin and the calcareous nannofossil events of the same age in the marine realm, intriguing especially as the proxies in the marine record do not indicate that something dramatic is happening. However, the authors use the curious wording almost when comparing the continental faunal turnover with the marine events. But almost is not of the same age, so what is exactly the difference and how does almost the same age translate to potentially having the same origin. In Figure 8, the age difference between the two events is ~40-kyr, but what are the uncertainties in the respective age models and in the position and thus age of the respective bio-events? The uncertainty in the position of Biohorizon B might be quite large compared with that in the marine record, so do these uncertainties overlap? The reason to develop high-resolution astrochronologic age models is meant to increase the temporal resolution and solve possible temporal relationships and chicken-and-egg problems. So what does this almost imply in this case? This should be made more clear in the ms.

Minor points.

Cycle 0. Why is there no cycle 0 in the numbering of the cycles in the PCB cores

405-kyr minimum (p.7, l. 14). Is this also not part of a very long 2.0 Myr eccentricity cycle, see Lourens et al., (2005) and Meyers (2015)? Please check what you mean exactly.

3.2 Time series analysis of BBCP drill cores. The first paragraph before 3.2.1 belongs to the Material & Methods section rather than to Results.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2017-74, 2017.