Dear Referee #1,

Thank you very much for your detailed review of the submitted manuscript.

Below find our reply (red) to your comments (*black*).

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.*

As written in the submitted manuscript on page 3, line 11: "The purpose of this report is to establish high-resolution age models for the BBCP drill cores based on cyclostratigraphy and integrate existing age models from outcrops". From this it should be clear that we found on the outcrop "old" age models to develop "new" age models for the drill cores.

To avoid further confusion, we will change the title of the manuscript to "Synchronizing early Eocene deep-sea and continental records – cyclostratigraphic age models for the Bighorn Basin Coring Project drill cores".

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 ~2 and ~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.

All MTM spectra will be replaced by mtmML96 spectra using the Astrochron software package. As an example we show here (Figure 1) the effect mentioned by the referee.

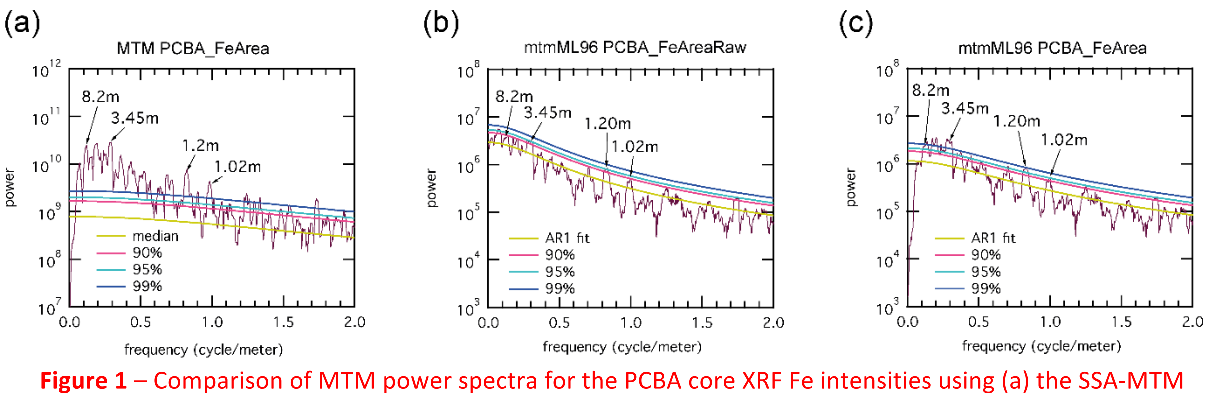

Figure 1 – Comparison of MTM power spectra for the PCBA core XRF Fe intensities using (a) the SSA-MTM toolkit and (b, c) the Mann and Lees (1996) robust red noise MTM analysis within the *Astrochron* software package. For (b) the raw XRF Fe intensities were used, for (c) the detrended XRF Fe intensities were used as done in (a). Clearly the the ML96 approach is much more appropriate for the data analysis.

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.

We agree. Due to the high resolution of the expanded records the spectra show lots of details. Because the spectra have been calculated for the entire length of each record changes in sedimentation rates will produce several closely spaced peaks. To be able to identify changes in sedimentation rates the evolutive spectra were calculated. For the extraction of cycles by filtering of the signal we applied a 30% bandwidth to compensate for these variations in cycle thickness as described in the manuscript.

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).*

We will provide MTM spectra using the mtmML96 Astrochron routine for all data. As an example we show the effect of detrending on the data in Figure 1 b and c. The detrending removed cycles longer than 10m.

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.*

We will correct this in a resubmitted version of the manuscript. However, it is not important for the cyclostratigraphy which is based on the recognition of the precession cycle only.

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.*

We will clarify this in the revised version of the manuscript. Looking at the initial manuscript figure 5, this can be explained. The onset of the PETM in the PCB core (Bowen et al. 2015) is between two paleosols showing higher Fe and AStar values at roughly 115 mcd ('purple 0') and 123 mcd. For making an age model we (page 7, line 19): "... also assume that the onset of the PETM is located in the minimum between cycle -1 and 1". Regarding the phase relationship, carbon isotope data from PCB soil nodules (Bowen et al., 2015) in the paleosols are more negative then before and after a paleosol horizon. This is similar to records from the deep-sea around the PETM showing more negative bulk carbonate carbon isotope values in more clay rich layers (higher Fe XRF intensities, Zachos et al. 2010, Littler et al. 2014, Zeebe et al. 2017). Whether the lighter carbon isotopes values and the paleosols correspond to precession minima or maxima (e. g. Lourens et al. 2005) is still unknown, and probably will never be known for certain. This is not relevant for establishing a cyclostratigraphy based on cycle counting.

The onset of the PETM was set into a precession minimum in the PCB cyclostratigraphy model. To correlate deep-sea and terrestrial records, the onset and the top of the initial rapid recovery of the CIE are commonly used (McInerney and Wing, 2011). The PCB cyclostratigraphy indicates that the duration of this interval covers six precession cycles or ~120 kyr (assuming an average duration of 21 kyr for one precession cycle). Therefore, the previous estimate by Röhl et al. 2007 from deep-sea records for this interval of ~95kyr is too short by ~25 kyr or about one precession cycle at the onset of the event. We concluded that a precession cycle is missing in the deep-sea records and needs to be added to those age models. Subtracting 21kyr (one precession cycle) from the missing ~25 kyr leaves us with ~4 kyr. This discrepancy is within any uncertainty of all the age models, as mentioned by the referee. We will clarify this in the revised manuscript.

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.).*

We thank the referee for this comment. A sentence will be added mentioning the Wotzlaw et al. study. We think discussing or showing the effects of your new age model compared to the Helium age model is important and could be basis for further research, but we refrain from going into too much detail because this is not the scope of our study.

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.*

This section will be rewritten as the position has been revised slightly including a proper error discussion.

The best constraint on the stratigraphic position of Biohorizon B comes from the Gilmore Hill section where it falls between locality MP167 (LAD of Haplomylus) and MP166 (FAD of Bunophorus), both of which fall directly in the line of section. The mid-level of locality MP167 is at 807 meters (above PETM) of that section and the mid-level of locality MP166 is 840 meters so Biohorizon B must fall somewhere in the interval between ~807 and ~840 meters of the Gilmore Hill section (see Abels et al., 2012 and D'Ambrosia et al., 2017 for details). Another locality, MP122, that is not located directly in the line of section but has been physically correlated to the 825-835 meter level in this section via bed tracing contains both Haplomylus and Bunophorus so provides a more precise biostratigraphic estimate of Biohorizon B but with additional stratigraphic uncertainty due to the long distance correlation. The 33 meters of section between 807 and 840 meters represent 4-5 precession cycles (#79 to #83 in table 2 of the ms) from 54.151 to 54.254 Ma and the 10 meters of section between 825 to 835 meters represent 1.5 precession cycles (between #81 and #83, centered at #82 in table 2 of the ms) from 54.165 To 54.195. We will change the Figure 8 to more accurately represent these uncertainties – see Figure 2 in this reply to the referee.

We will rewrite the chapter and be more critical about temporal uncertainties. However, we think it is worthwhile to point out that the biotic event on land and the deep sea are happening around the same time. It will be made clear that the biotic turnover in the deep-sea is a series of events (representing the fast evolutionary change) rather than a single event. We hope that our manuscript sparks new effort to investigate these biotic turnovers in much more detail to find out how “major” it was.

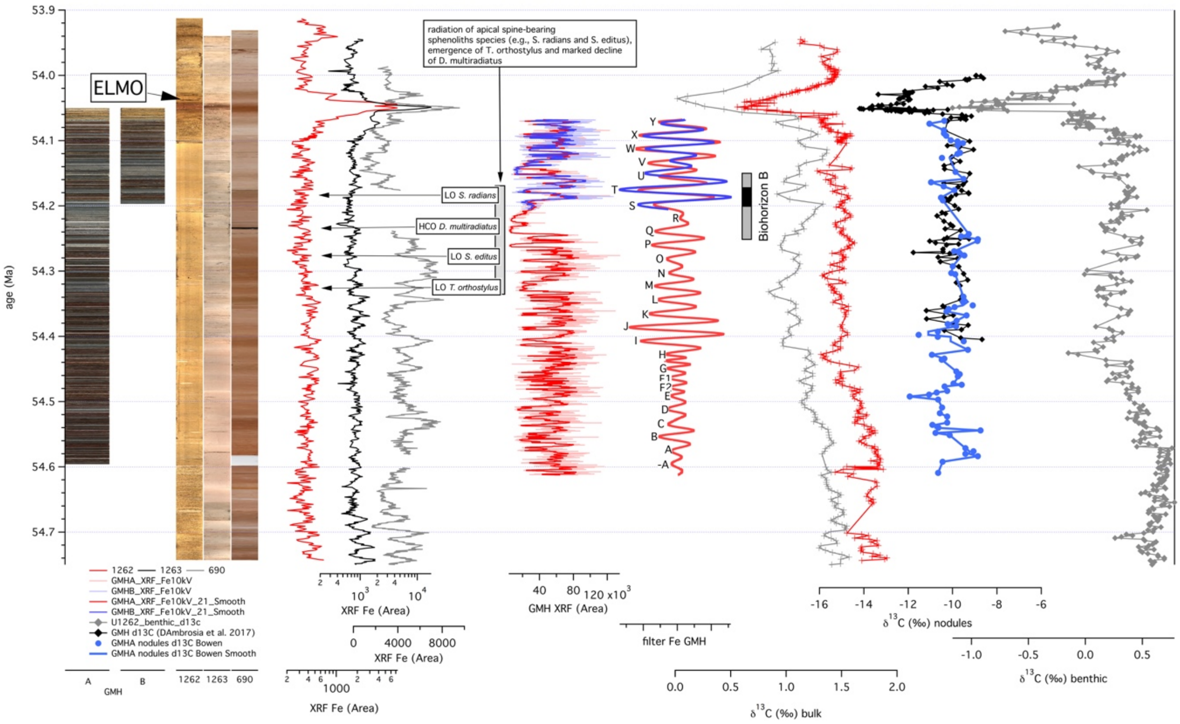

Figure 2 – Proposed revision of Figure 8 now showing the interval where the Biohorizon B in the GMH section is located and more details on the series of events occurring in the deep-sea record. The figure is an overview for the interval prior to the Eocene Thermal Maximum 2 (ETM-2) data from deep-sea records and the terrestrial Gilmore Hill (GMH) drill core against age. Core images for GMH A and B, core images of ODP Sites 1262, 1263 and 690 (aligned from left to right according to the water depth from deep to shallow), XRF Fe core scanning data from 1262 (red), 1263 (black), 690 (grey) (Westerhold et al., 2007), and GMH, extracted Gaussian filter of the GMH XRF Fe intensity data, stable carbon isotope data of soil nodules from the Gilmore Hill area (black – Gilmore Hill section Abels et al., 2012 and D'Ambrosia et al., 2017; blue – GMH drill core) and the deep sea benthic foraminifera (1262 – Littler et al., 2014) and bulk sediment (690 – Cramer et al., 2003; 1262 - Zachos et al., 2010). Position of Biohorizon B is after Abels et al., 2012 and D'Ambrosia et al., 2017 (black bar represents best estimate, gray bars represent conservative estimate – see text for discussion); the change in calcareous nannofossils (gray bar and text box) in ODP Site 1262 from Agnini et al., (2007).

MINOR POINTS

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

Why should there be a cycle 0? It is not clear what the referee is pointing at. The onset of the PETM was chosen as the zero line (Table 1 of the ms), in-between precession cycle -1 (before onset PETM) and 1 (after onset PETM).

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.*

It is correct as written. The cycles occur in a 405-kyr minimum, a time of low amplitude modulation of the precession cycle by eccentricity.

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.*

We would like to keep this section where it is because the time series analysis and thus age model development should ideally be in one chapter.