Reply to interactive comments

Manuscript cp-2016-51 “Maastrichtian carbon isotope stratigraphy and cyclostratigraphy of the Newfoundland Margin (Site U1403, IODP Leg 342)”

Dear Zhengtang Guo, Nicolas Thibault, Helmut Weissert, and anonymous reviewers,

thank you very much for handling and commenting on our paper. Your suggestions are much appreciated and we will do our utmost best to incorporate them and to improve the manuscript. All comments mention the need for a more detailed discussion on the carbon isotope stratigraphy, as well as a more expansive correlation of Maastrichtian carbon isotope records, and we will gladly provide this. To reflect changed author contributions related to the reviewer comments, we have decided to list SJB as first author and OF as second.
Please find below our response to the comments and suggestions made by the reviewers and Nicolas Thibault. We have copied the interactive comments in grey and written our replies in black. We hope that our comments below are sufficiently clear, but do not hesitate to contact us if there are any problems.
We would like to thank you in advance for further consideration of our manuscript for publication in Earth and Planetary Science Letters.
Thank you very much and kind regards, on behalf of all authors,

Sietske Batenburg and Oliver Friedrich

SC1 Interactive comment by Nicolas Thibault

Congratulations for a new very useful dataset on the Maastrichtian. This is a nice work. However, I have a few comments on your article and I think that there is a number of issues that need to be dealt with: Minor comments: 1) First of all, I think the title should be slightly modified by adding "Late" before Maastrichtian because your record covers only about half of the stage. Moreover, the base of the "late" Maastrichtian may be placed at the FO of nannofossil L. quadratus (base UC20aTP) so your record is clearly within the late Maastrichtian.

We fully agree and will change the title accordingly.

2) I understand the caution with not calculating absolute temperatures based on bulk oxygen isotope. However, it seems like temperatures reconstructed from the bulk oxygen isotopes of well-preserved chalk actually reflect better sea-surface temperatures than previous studies on Maastrichtian planktic foraminifera such as those from Li and Keller (see Thibault et al., 2016, CP 12, 1-10). I think that a lot of analysis on PF from the pioneer studies of Barrera and Savin and Li and Keller are compromised because diagenetic screening was simply not as thorough as today at that time. Yet, most compilations and/or modelling studies still use those values for SSTs but hesitate using well-preserved bulk chalk values. It is likely that values of the bulk chalk are also slightly overprinted by diagenesis but apparently not much more than PF. Moreover, the argument of bulk chalk being a mix of multispecies with potential vital effects is not valid anymore (see discussion in chapter 3.2 of Thibault et al., 2016, CP). See also discussion in Jarvis et al. (2011, 2015; Reghellin et al., 2015).

We will include a discussion of the stable oxygen isotope record, and compare the results with previously published Maastrichtian sea-surface temperature studies. Despite the presence of scatter in our $\delta^{18}O$ record from U1403, the overall decrease in oxygen isotope ratios prior to the K/Pg boundary is in good agreement with the oxygen isotope pattern at Stevns-1, as well as the temperature estimates derived from nannofossils (Thibault et al, 2016). For the lower part of the studied interval, planktic foraminiferal SST estimates based on well-preserved foraminiferal calcite are also available (e.g., Friedrich et al., 2009).

3) There is also diachroneity for base C. kamptneri (see attached pdf for full references)

Thank you, we will include this information, and the full references, in the revised version of the manuscript.

4) Thibault et al., 2015, Lethaia is a miscitation. The discussion on base Micula prinsii is in Thibault et al., 2016, IJES.

Thank you, we will correct this.

5) On Figures 2 and 3, and throughout the text, precise "TP" in superscript after each nannofossil subzone (UC20dTP). TP stands for Tethyan and Intermediate Provinces in the Burnett Scheme. This is important because there are also UC20bBP, UC20cBP and UC20dBP in the Boreal realm whose definitions are different.

We will specify that we used the Tethyan and Intermediate Provinces of the Burnett Scheme by adding TP in superscript in the text and the figures (2 and 3).

6) On Figure 7, it would be useful to add the nannofossil bio-events on each section to highlight the small mismatch mentioned in the text. you also mention the acme and last occurrence of inoceramids. Any data on that in Newfoundland?

To show the generally good agreement of the nannofossil bioevents, these will be added to Figure 7. Since data for this study are related to toothpick samples and XRF-scanning, studies on inoceramids haven't been done so far.

Major comment: 7) The comparison between Site U1403 and Zumaia in the time domain is problematic. Nannofossil events are considered as slightly diachronous between the two sites. I would not be surprised that this is the case, most especially because the biostrat in Zumaia is an issue due to the bad preservation of nannos and the presence of micro-turbidites. However, your two carbon-isotopes curves do not match either. So, in the end, you really don't have any good stratigraphic marker that supports your tuning in time of Site U1403 and allows for comparison to Zumaia. You consider in the text that Zumaia probably represents a more local response whereas U1403 represents a more global signal. This claim could be easily checked and would be more solid if you try to correlate your U1403 curve to other carbon isotope curves of the late Maastrichtian which are well-constrained: Site 1210B, Gubbio, Stevns-1. I attach hereby a possible alternative correlation of carbon-isotopes between U1403 and Zumaia. I'm not saying this is the correct solution. There are other possibilities. But such a correlation still appears more likely than the actual comparison of Zumaia and U1403 on Figure 7. I think that this is a serious shortcoming of the paper as for now, there is not much that actually supports your cyclostratigraphic age-model of U1403. Moreover, the spectra of the signals in the time domain give pretty bad results besides the 405 which is of course a direct result from the tuning to the 405 kyr. The periodicities at 143 kyr and 35 to 50 kyr for Ln Fe/Ca are not super convincing. The same is true for the Mag. Sus with bands at 85-97 and 34-54 kyr. I think that the study would be more solid with a comparison of frequency peaks to La2004 using the average spectral misfit of Meyers. It would also be worthwhile to make tests such as different possible correlations of carbon-isotopes to Zumaia, allowing to apply the Zumaia age-model to U1403 and then test the cyclostratigraphy on such age-models. Maybe, you would come up with a different cyclostratigraphic age-model and a better match of carbon-isotope curves.

In the revised version of the manuscript, we will include a comparison to the bulk stable carbon isotope curves of Gubbio (Tethys) and Site 1210B (Shatsky Rise, equatorial Pacific). The pattern of changes at Site U1403 agrees very well with these open-ocean sites and therefore demonstrates the usefulness of Site U1403 as a reference site for the late Maastrichtian in the North Atlantic, and the limitations of the stable carbon isotope curve from Zumaia. Also, it provides an independent check of the cyclostratigraphic framework of Site U1403 in our study. The carbon isotope curves of U1403, Gubbio and Shatsky Rise agree well when plotted on their own age models, although nannofossil events are slightly diachronous between sites. This information will be included in Figure 7 and more detailed discussion will be added in Section 4.4. For the time series analyses, we prefer to use the selected techniques which allow for a good comparison of the analyses outcomes with regard to patterns in the data and the core photographs. Based on the comment by Nicolas Thibault, we will, however, evaluate the proposed correlation, and potentially discuss it in the manuscript, depending on the outcome of the time series analyses.

I attach a pdf with my comments in the supplement. Best regards, Nicolas Thibault
Please also note the supplement to this comment: http://www.clim-past-discuss.net/cp-2016-51/cp-2016-51-SC1-supplement.pdf

Thank you for the detailed comments in the annotated manuscript which will be added and incorporated into the revised version of the manuscript as outlined in detail above.

References

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Thibault, N., Harlou, R., Schovsbo, N.H., Stemmerik, L. and Surlyk, F., 2015. Late Cretaceous (Late Campanian–Maastrichtian) sea surface temperature record of the Boreal Chalk Sea. *Climate of the Past Discussions*, *11*, pp.5049-5071.

RC1 Interactive comment by H. Weissert

The authors present a new isotope- and cyclostratigraphy of the upper Maastrichtian from the W. North Atlantic. Geochemical data, based on XRF –Scanner analyses, were used for the establishment of a new cyclostratigraphy, C-isotopes were used as a chemostratigraphic tool, oxygen isotope data were interpreted as a proxy for paleotemperature. The paper is well written, data are clearly presented and a comparison with data from the Zumaya section broadens the contents of the paper and it provides information on correlation potential of the new data set. In the following paragraphs I add a few comments which may help to further improve this manuscript: (1) Not everybody is familiar with the details of Maastrichtian stratigraphy. A graph showing the state of the art in Maastrichtian bio- and chemostratigraphy (and magnetostratigraphy) and highlighting the interval which is studied by this research group will be very useful.

We agree with the reviewer that the manuscript should incorporate a more complete overview of the available data. We propose to include the shipboard bio- and magnetostratigraphic data from Site U1403 (Norris et al., 2014) in Figure 2, with the stable carbon and oxygen isotope data in the depth domain. The correlation scheme (Figure 7) will be expanded to include the carbon isotope stratigraphies of Gubbio and Shatsky Rise (see also our reply above to point 7 of SC1 N. Thibault). In this last figure, we will also display the magneto- and biostratigraphic information of all sites, with separate columns of planktonic foraminifera and nannofossil zonations, and specific events highlighted in the main figure panel.

(2) Chemostratigraphy: Again, the authors chose the short upper Maastrichtian interval in their correlation graph, it would be easier to read the correlation, if a longer segment of the Zumaya curve would be shown, starting, for example, at the Campanian-Maastrichtian boundary (with corresponding CIE). This graph would further indicate that the observed fluctuations in the Upper Maastrichtian record are mostly of very small amplitude. It is not surprising, that fluctuations around 0.2permil or even less are not of any reliable use in chemostratigraphy. The authors correctly mention the importance of regional factors controlling this isotope pattern. Here, reference to variations in the isotopic composition in modern oceans could/should be made.

We will incorporate other records in the correlation plot and display a longer period of time, as suggested by the reviewer This will put the observed bulk carbon isotope variations in a broader context. We will also briefly discuss modern variations in the distribution of stable isotope signals.

(3) Oxygen isotopes – the oxygen isotope data are, correctly, used as a proxy for paleotemperature and the authors correctly point at possible alteration during diagenesis (all limestones/chalks consist, by definition, of original marine calcite and of cement formed during burial diagenesis). However, the authors may make a comparison with other oxygen isotope data sets from the literature and they may even comment of ranges in paleotemperature calculated from them in comparison with other data, assuming that, despite of diagenesis, the pattern of change is still preserved.

We will include a discussion the stable oxygen isotope data, which show a clear trend, despite the scatter on a small temporal scale. This trend compares well with some previous bulk isotope studies and well-preserved foraminiferal stable oxygen isotope datasets, and contrasts with older foraminiferal isotope studies. See also the point 2 of SC1 by N Thibault and our reply.

Another question concerns the impact of changing lithologies on the oxygen isotope pattern. Do samples with lower carbonate content show any lithology-related changes in carbon or oxygen isotope composition? Did you make a carbonate content – oxygen isotope data cross-plot? This plot does not have to be published, but you may mention, if any correlation between carbonate content and oxygen isotope composition is recognized.

To evaluate the stable isotope data as suggested in this comment, we will assess cross-plots of carbon and oxygen isotopes and evaluate variations in the carbonate isotope data with respect to lithology (i.e., cross-plot with Ca counts as a measure of carbonate content).

(4) A summary graph showing the new isotope data within a larger set of data from different localities could nicely round up this paper.

As mentioned in our reply to SC1, we will include a comparison to the carbon isotope stratigraphies of Gubbio and Site 1210B on Shatsky Rise to provide a robust overview and correlation.

References

Norris, R.D., Wilson, P.A, Blum, P., and the Expedition 342 Scientists: Proc. IODP, 342, College Station, TX (Integrated Ocean Drilling Program), doi:10.2204/iodp.proc.342.2014, 2014.

RC2 Interactive comment by Referee #2

My first impression on this manuscript which made me exciting is "carbon isotope stratigraphy" of the title. To me, a researcher on paleoceanography focusing on Neogene, relies on foraminiferal oxygen isotope too much. Not like foraminiferal oxygen isotope in the Neogene period of which the major controlling factor is global ice volume, carbon isotope is hard to explain due to multi-factor influences. It is not easy to distinguish various influences on carbon isotope, and therefore not easy to make global comparison based on carbon isotopes. However, the core of "stratigraphy" should be global comparison. Putting astronomical tuning or the age model aside, discussion on carbon isotope stratigraphy is less than one sixth the length of whole manuscript. The comparison of the bulk d13C record between Site U1403 and the Zumaia section in northern Spain displays not only good correlation on long-term changes in 66-67.5 Ma but also anti-correlation on 405 Kyr cycles in the period older than 67.5 Ma. The great difference in d13C which the authors ascribed to discrepancies of global and regional carbon cycle demonstrates again that the bulk carbon isotope is not a reliable proxy to make a common timescale for the marine or terrestrial sedimentary sequences for global comparison. Thus, the "carbon isotope Stratigraphy" as a tool for global comparison seems to be overestimated and should be reconsidered in the revision particularly its role in the title. Obviously, carbon isotope stratigraphy is not the core of this manuscript. The in depth discussion on it is weak.

We agree with the reviewer that the generated carbon isotope curve should be discussed in more detail, and we will include a comparison to other Sites in the discussion (i.e., Site 1210B and Gubbio; see also replies to SC1 and RC1 above). Site U1403 correlates well with Site 1210B (Shatsky Rise, equatorial Pacific) and Gubbio (Tethys Ocean) and seems to reflect an open ocean setting where the carbon isotope signature is mostly determined by global processes. That stable carbon isotope stratigraphy is a reliable tool especially in the Cretaceous has been demonstrated in numerous studies (e.g., Voigt et al., 2010, 2012; Wendler, 2013; to name only a few) and will be further supported by the new comparison between our new data and published open-ocean records. In contrast, at Zumaia, the signal is strongly overprinted by regional carbon inputs and the two curves do not compare well beyond 67.5, as noted by the reviewer (see also the reply to point 7 raised by SC1 N Thibault). Based on the reviewers comment, the discussion about carbon isotope stratigraphy will take up a much more prominent part of the revised manuscript and carbon isotope stratigraphy will merit a place in the title.

Cyclostratigraphy which the authors depended on to construct the timescale of the Maastrichtian interval of Site U1430 is obviously the core of this manuscript. The tuning material is the constant elemental ration (Fe/Ca on natural Logarithm scale) obtained by high resolution XRF core scanning and the Magnetic Susceptibility. The important steps of tuning are the initial identification of the orbital cycles in the elemental and MS records and the subsequent comparison with the 405-kyr components of the eccentricity. The authors did a good job on mathematics for orbital tuning on which I have no doubts, but there are some questions left to be clarified. I believe that the manuscript will become easier for the readers to understand after carefully responds to these questions.
A precondition for the construction of a timescale by orbital tuning is the continuity of the sedimentary sequence. Detailed introduction of this content seems to be missing in the text except for the reminding that the reviewers and readers should refer to other references. Part 2, the geological setting, is too simple and short to be treated as an independent section. Actually, they can be integrated into Part 3, the materials and methods. But I believe that most readers hope to see a detailed introduction on the lithology in this manuscript.

Following the suggestion of Referee #2, we will provide more background information in the separate section dedicated to the geologic setting. Here we will discuss the lithological variations and reasons for why we think that it is a continuous section in more detail and include the available bio- and magnetostratigraphic information (Norris et al, 2014), which will also be displayed in Figure 2.

The authors calculated the cycles of the variability in elemental and MS records in depth domain and corresponded the dominant cycles ranging from big to small to 405 kyr, 100 kyr, 41 kyr and 21 kyr orbital cycles. The fundamental of making such a comparison is that the ratio of the cycles in depth is similar to the ratio of the dominant cycles of eccentricity, obliquity and precession in age. However, similarity of the ratios of dominant cycle between in depth and in age is a necessary not a sufficient condition of orbital tuning. The initial age model derived from biostratigraphy and magnetostratigraphy and absolute dating is the sufficient condition of the orbital tuning. The authors didn't mention them in their manuscript. They need to prove that why the dominant cycles of the proxy records in depth domain correspond to orbital cycles based on the initial age model.

We will provide the available bio- and magnetostratigraphic information in the revised text and in Figure 2, which provides the initial age constraints on the studied succession. Rather than assigning ages to the record based on this initial age control, we prefer to use the bio-, magneto-, and chemostratigraphic information as an independent test of the constructed age model. We will compare the bulk carbon isotope stratigraphy, magnetostratigraphy and biostratigraphy of Site U1403 with those of Gubbio, Zumaia and Shatsky Rise in Figure 7, and will discuss similarities and discrepancies in the revised manuscript. This will allow us to evaluate the cyclostratigraphic interpretation.

The 405-kyr long eccentricity cycle is the basic tuning component which determines that the accuracy of their timescale is less than 405 kyr. Why don't they increase the time resolution of the astronomically tuned timescale to obliquity and precession since they have so high resolution records of elemental ratio and MS?

Although the records display shorter scale variability corresponding to the periodicities of obliquity and precession, these periodicities cannot be reliably reconstructed so far back in time. Precession and obliquity reconstructions are provided until ~40 Ma in the La2004 solution (Laskar et al., 2004), and more recent solutions such as La2011 used in our study only reconstruct the eccentricity components. Even then, beyond ~52 Ma the only reliable tuning target is the stable 405 kyr periodicity of eccentricity (Laskar et al., 2011). To clarify this to the reader, we will include a short discussion on this topic in the text of the revised manuscript.

What is the age of the K/Pg boundary in their timescale?

Astronomic calibration uses the K/Pg boundary as starting point with an age of 66.04 Ma based on new bentonite ages from coals near the K/Pg boundary (Renne et al., 2013).

The authors mentioned that "Carbonates of the studied nannofossil oozes consist mainly of coccoliths and benthic and planktic foraminifera (Expedition 342 Scientists, 2012)", and concluded that "bulk oxygen isotope values therefore predominantly represent a surface-water signal". Since there are benthic foraminifera in the carbonates of the sediments which is obviously a signal of bottom water, why the bulk oxygen isotope mainly represents a surface-water signal"?

Due to the open-ocean setting of Site U1403, the investigated sediment is mainly composed out of planktic organisms such as nannofossils and planktic foraminifera. Benthic foraminifera are present but rare in comparison to these planktic microfossil groups (Norris et al., 2014). Therefore, the bulk oxygen isotope values are mainly (but not exclusively) a signal of the upper water column.

References

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Voigt, S., Gale, A. S., Jung, C., and Jenkyns, H. C.: Global correlation of Upper Campanian - Maastrichtian successions using carbon-isotope stratigraphy: development of a new Maastrichtian timescale, Newsl. Stratigr., 45, 25-53, 2012.

Wendler, I.: A critical evaluation of carbon isotope stratigraphy and biostratigraphic implications for Late Cretaceous global correlation, *Earth-Science Reviews*, 126, 116-146, 2013.

RC2 Interactive comment by Referee #3

Review of "Maastrichtian carbon isotope stratigraphy and cyclostratigraphy of the Newfoundland margin (Site U1403, IODP Leg 342)" by Friedrich et al. for Climates of the Past. The ms describes the bulk carbonate carbon isotope stratigraphy of IODP Site U1403 for the late Maastrichtian using cyclostratigraphy based on Fe/Ca and magnetic susceptibility to establish an astronomically calibrated age model. The record is subsequently compared with that of the Zumaia section in northern Spain and claim is made that the new record reflects global climate change more reliably. In that case this would be an important contribution as the latest Cretaceous is a fascinating interval with both short warming and cooling periods, and potentially leading to a delicate state of the climate system that is highly sensitive to the consequences of the impact at the K/Pg boundary. In that respect the ms is suitable for publication in Climates of the Past. However, the ms is in its current state not acceptable for publication. Its needs to undergo at least major revision and a second review.
Main issues:
1) Lack of discussion on carbon isotope stratigraphy. The ms focuses mainly on the astronomical age model for the Site U1403 records, but does not go into the details of the consequences of the age model for the interpretation of the resultant carbon isotope stratigraphy and astrochronology rather than stating that the record provides much more a global signal than the regional signal that is preserved in the Zumaia section to which it is compared. As such it is more suitable to serve as a standard for future carbon isotopic correlations. An astrochronology for this interval by itself is not interesting, as it has already been astronomically calibrated on the basis of both deep-sea cores and land-based marine sections such as Zumaia. Hence, the astrochronology should rather be considered as a tool to discuss and interpret the astronomically dated carbon isotope stratigraphy in detail. However, rather surprisingly, the ms does not provide a detailed discussion and interpretation of the record, while at the same time differences with the regional Zumaia record are very obvious, and should be discussed in detail.

We will improve the discussion on the bulk carbon isotope stratigraphy from Site U1403 (please also refer to the replies above such as to comment 7 of SC1). We will include two other sites in the correlation panel, which correlate well with the record of Site U1403, and we will highlight the consistencies and discrepancies with the Zumaia section in more detail.

Also the potential link to the brief warming and cooling episodes accompanied by perturbations in the global carbon cycle should be discussed if the claim is made that this record represents a global signal (see also next point). In my opinion, the ms is now to meagre to be published in Climates of the Past.

To place our findings in a larger paleoclimatic context, we will include a more detailed discussion of the temperature signal revealed by the oxygen isotope records of Site U1403. This record shows a warming trend at the end of the Cretaceous which is comparable to other paleotemperature estimates (see reply to point 2 of SC1 N Thibault) from this time-period and may reflect the main phase of Deccan Trap volcanism.

2) Inconsistency between introduction and discussion. This point is partly linked to the previous one. The introduction goes into the details of the carbon isotope events in the Maastrichtian stating that these events have been extensively studied but that causal mechanisms are poorly understood due to poor age constraints. However, their record only seems to cover part of the MME of the events mentioned in the introduction, which as such is thus inconsistent with the remainder of the ms. It is further not made clear at all why this particular excursion should be the MME (see also next point)?

We agree with Referee #3 that part of the introduction chapter is only loosely related to the following discussion of our new records. We will account for this by shifting the focus of the introduction more towards the interval that is present in the U1403 record (Late Maastrichtian). Furthermore, we will include a detailed correlation of our carbon isotope record with the ones of Zumaia, Site 1210B and Gubbio, allowing us to discuss in much more detail the global vs. regional relevance of individual excursions (see also our reply to SC1 and RC1). By including more bio- and magnetostratigraphic details into the discussion and Figure 7, we will also extend the discussion why we believe that our record contains the MME.

3) Comparison with other records. The astronomically dated carbon isotope record is only compared with the independently bulk carbon isotope record of the Zumaia section. However, no attempt is made to correlate the record to other carbon isotope records that are available, in which for instance the MME has been identified (see Thibault et al., 2012; Voigt et al., 2015 as mentioned in the introduction). It would be very logical to make and discuss these correlations, although astronomically calibrated age models may not have been established for all these records.

In the planned bulk carbon isotope correlation panel in the revised version of Figure 7 (see detailed reply to point 7 of SC1), we will incorporate the records from open ocean sections in the Tethys (Gubbio) and the Pacific (Site 1210B, Shatsky Rise). These records display very similar trends and shifts as the record from Site U1403, indicating its suitability as a reference section for the North Atlantic. The discrepancies with the Zumaia record allow us to distinguish local influences from global processes that affect the Earth's carbon cycle.

Other issues.

4) Astronomical calibration. The astronomical age model is based on the calibration of the inferred 405-kyr cycle in the Fe/Ca ratio and magnetic susceptibility to the 405-kyr eccentricity cycle in the eccentricity time series starting from the K/Pg boundary. However, also the Ba record is shown which seems to contain a distinct cyclicity as well, but why has this record not been included in the cyclostratigraphic analysis and tuning as an extra check on the correctness of the cyclostratigraphic interpretation.

The Ba record is depicted to demonstrate that different components of the climate system respond to different orbital forcing factors. Whereas the Fe and Ca ratios closely follow the lithological alternations paced by eccentricity-modulated precession, Ba responds much more abruptly and frequently, likely reflecting productivity peaks during individual precessional extremes. As Ba does not respond to the longer-term orbital parameters, time series analyses of this record do not provide information which can be used for the construction or testing of an age model based on the 405-kyr periodicity of eccentricity.

5) Ratio. The authors use the ratio of 1:5:20 of spectral peaks as an argument for their cyclostratigraphic interpretation. However, by doing so, they are selective and f.i. a rather strong 200-kyr cycle is observed in many of the spectra, but no attention is paid to this peak and no explanation is provided.

We will discuss the periodicity in more detail and provide potential mechanisms for the existence of this peak in the power spectra. For example, a peak at 200 kyr can occur as a result of the strong expression of every second ~100 kyr cycle. This can be due to variations in climate and sedimentation that are not related to Milankovitch forcing, or it can be a property of the astronomical forcing parameters related to very long (Myr) periodicities of eccentricity. We will also draw more attention to the lithological alternations as observed in the core photographs and place the findings in a broader context by comparison to other sections to facilitate the evaluation of our age model by the reader.

6) Why are there so many co-authors for such a short paper?

The data presented in this manuscript are the result of shipboard sampling during IODP Expedition 342. Since many people provided input for the used biostratigraphic and magnetostratigraphic framework and the generation of the presented stable isotope and XRF-scanning data, this results in a rather long list of people contributing to this study.