Author's response: Batenburg et al., 2016

Reviewer #1

1. Does the paper address relevant scientific questions within the scope of CP? Yes, the authors want to show and explain climate control on the development of poor oxygenation conditions in the ocean during the Late Cretaceous.
2. Does the paper present novel concepts, ideas, tools, or data? This paper doesn't really present any new ideas, but it has many new data and a slightly different approach from previous papers on the same subject. This paper attempts to define the time frame of the Cenomanian-Turonian interval by integrating new radiochronologic data and using more recent astronomical data. Cyclostratigraphic analysis is performed on data in part different than previously.
3. Are substantial conclusions reached? No, because this article does not stand out enough from that of Mitchell et al., 2008 and the differences in interpretation are not sufficiently justified.

We are happy to read that the reviewer recognizes the significant amount of new paleoclimate proxy data that is presented in our manuscript, as well as a new radioisotopic date for the Cenomanian. Still, the reviewer has the opinion that that our manuscript does not stand out enough from Mitchell et al. (2008). In the original version of the manuscript, we have listed a number of differences in tuning approach and interpretation between Mitchell et al. (2008) and our manuscript. One important difference is that we solely use the stable 405 kyr periodicity of eccentricity as a tuning target, whereas Mitchell et al. (2008) also tuned to 100-kyr eccentricity. We adopt this tuning strategy because the 405-kyr component is the only astronomical tuning target that can be used beyond ~50 Ma, and is thus the prime target in the Mesozoic. The C/T boundary age might seem similar between the two papers, but this is in fact not the case. Mitchell et al. (2008) used radioisotopic ages of Sageman et al. (2006), who used the Fish Canyon sanidine standard age of 28.02 ± 0.28 Ma of Renne et al. (1998), while we use the 28.201 ± 0.046 Ma of Kuiper et al. (2008). At around 94 Ma, this makes a difference of more than 600 kyr or 1.5 x 405-kyr cycle. This improvement is critical for extending the astronomical time scale from the K/Pg boundary back to the C/T boundary and beyond.

The discussion of differences in approach between Mitchell et al. (2008) and our paper, however, did not seem to be effective in indicating the fundamental differences that exists between both works. Therefore, we recognize that this part of the manuscript should be improved. In the revised version of the manuscript, we are paying special attention to the discussion of the differences in cyclostratigraphic approach and paleoenvironmental interpretation between Mitchell et al. (2008) and Batenburg et al. (2016). This involves a fundamental rewriting of the paragraphs involved.

4. Are the scientific methods and assumptions valid and clearly outlined? More or less The assumptions seem to be more or less valid, but there are too many assumptions. For example: - The correlation between MS et chert ; - The link between the different proxies studied and the carbon cycle - The contribution of nutrients from Caribbean plateau activity. One may ask how the transfer of material in view of the cenomanian paleogeographic configuration is. I think, as authors, both the climate and the Caribbean plateau activity are at the origin of the Cenomanian-Turonian anoxic nevertheless this paper does not really show it.

Figure 3 shows the relationship between MS and cherts, as well as the relation with different proxies. The link between this study and the long-term behavior of the carbon cycle is discussed in paragraph 4.4, as is the likely supply of nutrients from the Caribbean LIP. Volcanism was probably the ultimate driver of oceanic anoxia, but, based on our findings, astronomical forcing likely determined the exact timing. We focus on the astronomical forcing aspect and consider the precise oceanographic processes and geochemical pathways beyond the scope of this paper.

5. Are the results sufficient to support the interpretations and conclusions? No

5.1 Because there is no discussion on the choice and the climatic significance of the different proxies studied. Why do studied proxies differ according to stratigraphic interval? Unfortunately these proxies do not have the same meaning: The reflectance is controlled by the lithology. The SiO2 concentration is function of both detrital influx variations and authigenic / biogenic silica content. The concentration of Al2O3 and TiO2 reflects changes in detrital flow. Magnetic Susceptibility (MS) variations are function of the concentration in dia-, para and ferromagnetic minerals. How do you explain the increase in MS in levels rich in diamagnetic minerals? Is it strange? Have you done a statistical analysis which shows the correlation between MS and authigenic/ biogenic SiO2 content?

Different paleo-environmental proxies have been measured between the Scaglia Rossa/Bianca Formations on the one hand (C and O stable isotopes, magnetic susceptibility, reflectivity and limestone-chert alternations), and the Livello Bonarelli on the other hand (XRF-derived SiO_2, Al_2O_3, TiO_2 and Loss-on-Ignition). The large differences in sedimentary facies imply that different proxy records form the best archives for paleoclimatic variability. For example, reflectance data are very useful in the interval where black shales occur, whereas colour variations in the Scaglia Bianca (above the Livello Bonarelli) are limited. We discuss the characteristics of the different proxies in terms of paleoclimatic and paleo-environmental interpretation in more detail in the revised version of the manuscript. As the individual proxy records are limited to the lithological units, we do not have overlapping magnetic susceptibility and SiO_2 data, and cannot perform statistical analyses.

5.2 How did you measure the δ13 C in chert? These analyzes do not explain what is the minimum carbonate content for valid δ13 C values?

$\delta^{13}C$ values were obtained from powdered samples, of the limestones and marls as well as of the cherts, which still contained carbonate. If, in the first measurement run, carbonate concentrations were too low to obtain a reliable signal, measurements were repeated with a larger volume. This information is being incorporated in the revised manuscript.

5.3 The authors state “we procure insights in the relationship between orbital forcing and the ´ Late Cretaceous carbone cycle by deciphering the imprint of astronomical cycles on lithologic, geophysical and stable isotope records. . . “ but the data shows that the imprint of astronomical cycles in the stable isotope records and specially δ13C is very difficult for deciphering, that’s why, the cyclostratigraphic analysis is applied to others proxies whose link with the carbon cycle is not shown.

The cyclostratigraphic framework is indeed constructed based primarily on the geophysical proxies and the limestone-chert alternations. In Figure 2, we show that eccentricity maxima, as interpreted from these proxies, correspond to high variability in $\delta^{13}C$, as well as with a tendency towards more negative $\delta^{13}C$ values. This is one example of how we link the cyclostratigraphic interpretations with the global carbon cycle. Additionally, one of the significant findings reported in this manuscript, is the fact that the base of the Livello Bonarelli corresponds to the first 100-kyr eccentricity maximum after a 405-kyr eccentricity minimum, which is another clear example of a link between cyclostratigraphy and global carbon cycle perturbations.

Some authors’ conclusions are in agreement with Mitchell et al. (2008) works. Mitchell et al. in particular, show a cyclicity of about 2.4 Ma in the development of anoxia. Unlike Mitchell’s works Batenburg et al. suggest that "the exact timing of major carbon cycle perturbations during the Cretaceous may be linked to increased variability in seasonality partner after the prolonged avoidance of seasonal extreme" at the 2.4 Myr scale. This interpretation is not confirmed on any figure. We don’t see the 2.4 Myr cycles on Figure 3.

A possible role of the 2.4 Myr eccentricity cycle is discussed after the observation that the mid-Cenomanian event, the OAE-2 and the Pewsey excursion are separated by 2.0 - 2.4 Myr respectively, and the observation that black shales are lacking in the interval preceding OAE2. This lithological pattern and the spacing between the three “events” can be observed in Figure 3.

Why are not the insolation variations calculated from La2011 data presented?

This is because the most recent insolation solution that is currently available is La2004. This insolation solution is only valid until ~40 Ma. The La2011 and La2010 solutions that are available at present, are eccentricity-only solutions. Only the 405-kyr eccentricity component of the La2010 and La2011 solutions can be used in the Cenomanian/Turonian interval.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes, but scientific reasoning should be more explicit

We agree with the reviewer that scientific reasoning should be more straight-forward and explicit in the revised version of the manuscript. Therewith, we first and foremost focus on [1] showing the differences with the Mitchell et al. (2008) approach and [2] the hypothesis that OAE2 was favored by a specific sequence of astronomical configurations, with a prolonged period of low eccentricity (2.4 Myr eccentricity minimum) followed by an eccentricity maximum (100-kyr eccentricity maximum).

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? I don't doubt the quality of the data, but the choice of these data should be better explained. Their own new contribution is clearly indicated.
8. Does the title clearly reflect the contents of the paper? With this title and content, this article does not stand out enough of Mitchell et al. (2008) works.

See above.

9. Does the abstract provide a concise and complete summary? Yes
10. Is the overall presentation well structured and clear? I think the section "results" requires a total reorganization. Before addressing the proxy data and the link with the lithology, we should discuss the time frame of these series (radioisotopic dating + correlation). Any cyclostratigraphic analysis must begin with an accurate (bio)chronological framework. The authors indicate, correctly, that the stratigraphic timing is not based on biostratigraphic, but chemostratigraphic correlations with well-dated series. I believe in the validity of such correlation, but nevertheless to valid a correlation, two continuous chemostratigraphic records must be correlated, which is not the case in this work (see Figure 9). Figure 9 is not convincing and not valid since it lacks isotopic data of the Bonarelli level. On the other hand, this figure is misplaced. It should be positioned at the beginning of the article. Thus, a part of the results and some figures should be reorganized. Another figure that shows the link between δ 13C and 2.4 kyr orbital cyclicity should be integrated.

We do not agree with the reviewer that our manuscript needs a total reorganization. The reviewer rightfully says that *"any cyclostratigraphic analysis must begin with an accurate (bio)chronological framework*". It goes without saying that we agree with this statement. Hence, the well-studied and well-documented biostratigraphic framework is presented early in the manuscript, in Figure 3, alongside the lithological column. Unfortunately, the reviewer misinterpreted our Figure 9, as this figure is not meant to constrain the initial stratigraphic framework by correlating outstanding features in d13C. Instead, Figure 9 shows a chemostratigraphic comparison between carbon isotope records from contemporaneous sections. In this figure, we show carbon isotope records along their original age-models, as constructed by the authors of the publications in which these data have been presented. In other words, Figure 9 is a figure in which we evaluate our tuning, and therefore, it should be presented after we presented the two tuning options for the studied sections. It is likely that the confusion was caused by the early call-out to Figure 9 in the original manuscript (Section 4.1), before the actual tuning is presented. In the revised version of the manuscript we discuss Figure 9 *after* we presented our tuning options for the studied sections.

11. Is the language fluent and precise? Yes
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes, In "Geological setting and proxy records" paragraph, the choice of proxies studied and their meanings must be explained. The "result" paragraph must be reorganized. Correlations and

2.4 kyr orbital cyclicity must be better argued. The modified Figure 9 should be placed at the beginning of the Article. The synthetic Figure 2 should be placed at end of the article.

These comments have all been addressed above.

14. Are the number and quality of references appropriate? Yes, but it is necessary to include additional references to explain the significance of the studied proxies
15. Is the amount and quality of supplementary material appropriate? There are not any

References

Kuiper, K. F., Deino, A., Hilgen, F. J., Krijgsman, W., Renne, P. R., and Wijbrans, J. R.: Synchronizing rock clocks of Earth history, Science, 320, 500-504, DOI 10.1126/science.1154339, 2008.

Mitchell, R. N., Bice, D. M., Montanari, A., Cleaveland, L. C., Christianson, K. T., Coccioni, R., and Hinnov, L. A.: Oceanic anoxic cycles? Orbital prelude to the Bonarelli Level (OAE 2), Earth and Planetary Science Letters, 267, 1-16, DOI 10.1016/j.epsl.2007.11.026, 2008.

Renne, P. R., Swisher, C. C., Deino, A. L., Karner, D. B., Owens, T. L., and DePaolo, D. J.: Intercalibration of standards, absolute ages and uncertainties in 40Ar/39Ar dating, Chem Geol, 145, 117-152, http://dx.doi.org/10.1016/S0009-2541(97)00159-9, 1998.

Sageman, B. B., Meyers, S. R., and Arthur, M. A.: Orbital time scale and new C-isotope record for Cenomanian-Turonian boundary stratotype, Geology, 34, 125-128, Doi 10.1130/G22074.1, 2006.