Rebuttal to reviewer 1

Reviewer comments R1:

Review of the manuscript „A comparison of two astronomical tuning approaches for the Oligocene - Miocene Transition from Pacific Ocean Site U1334 and implications for the carbon cycle“ by Helen Beddow et al.

Dear Authors,

With great interest, I have read your manuscript. You review the Oligocene-Miocene boundary magnetic and cyclostratigraphic time scale, and test it by tuning two proxies (CaCO3, d13C) from IODP Site 1264. Using these tuning options, you test their consistency with sea floor spreading rates. Finally, your approach provides astronomically tuned chron ages and implications for the Carbon cycle This manuscript has a stratigraphic and carbon cycle focus and discusses the effect of orbital tuning for time scales and carbon cycle interpretations, which are without doubt relevant for paleoceanographic and paleoclimatic studies. You propose an orbitally tuned magnetic polarity time scale for the relevant time interval, which is relevant and a valuable outcome of your study. All over the manuscript is clearly structured and written. High quality figures complement the text in a logical way. The manuscript is in my opinion clearly in the scope of Climate of the Past. A revised version of this manuscript suits the scope of CP, and I recommend publication after below mentioned clarifications/revisions. I hope the comments below help to make your manuscript more clear and relevant to a wide readership. It is clearly meant as constructive

A small correction: we have used IODP Site U1334 instead of ODP Site 1264.

‘General comments’

To be honest, it took me quite some reading to realise why you use the approach presented in your manuscript, and I ask you to clarify this earlier and clearer. You compare tuned ages, based on CaCO3 and d13C records. I was wondering what is your initial argument for using d13C as signal for in-phase tuning? Several studies have shown that this assumption may be problematic in the Neogene and Oligocene (as you also state), and that d13C signals are time-delayed relative to other proxies and eccentricity. Personally, I would expect a delay of this signal relative to physical and/or chemical proxy data. Importantly, Liebrand et al., (2016) demonstrated that at Site 1264 the d13C signal has a ~5-10 kyr time offset relative to CaCO3, therefore it is hard to understand why you would knowingly use an offset signal as tuning target.

The main reasons to use $\delta^{13}C$ for tuning is (1) to test if the generally strong expression of the 405 ky eccentricity cycle in benthic $\delta^{13}C$ yields similar/comparable ages to tuning approaches based on lithological proxies, and 2) to independently test the (previously presumed) phase lag of the benthic $\delta^{13}C$ signal w.r.t. eccentricity with independent evidence that is free from tuning assumptions. The spreading rates provide independent evidence that the lag of the benthic isotopes is a real feature and that tuning to the 405-ky cycle in benthic foraminiferal $\delta^{13}C$ does not yield ages that are in agreement with spreading rates.

Please make this choice clearer early in the manuscript (as I read the manuscript parts of your reason are rather hidden around lines 335-343). Your intention to test phase relations and their stability regarding the phase comes rather late in your manuscript. In any case it may need to be clarified that one tuning option is rather artificial and only used as test, with the expected outcome that it will not be good/valid.

With the knowledge of hindsight, we agree that the $\delta^{13}C$ tuning option seems rather artificial. However, by showing the implications of this tuning option for spreading rates, we can rule it out completely. For the sake of argument (i.e., comparing tuning options and provide independent evidence for chron ages and leads and lags in the climate-carbon cycle system) it is best to keep an open mind about both tuning options, until we reach the discussion and are able to discuss the implications of these tunings. We note that, in the introduction, we clearly state the aim of the research (see last paragraph of the introduction). Sentence beginning with "We evaluate…".

According to your Fig. 7, and the preferred age model, a short interval around 23.1-23.1 Ma experiences sedimentation rates two times as high as previously and afterwards. In my opinion, the exact doubling for one 100 kyr cycle mean that actually two cycles were combined. Having less experience with this specific dataset than you, in my opinion the data structure would allow such an interpretation in this interval, also when considering eccentricity being expressed as precession amplitude. Please discuss this option (or why this may not be the case) in the manuscript.

The preferred age model, supported by spreading rates, is the $CaCO_3$ tuned age model (Fig. 6) and not the $\delta^{13}C$ tuned age model (Fig. 7). A brief spike in sedimentation rate near 23.1 Ma (Fig. 7e) is indeed an artefact of misinterpreting two 100-kry cycles as one. The spreading rate history clearly rules this option out. Unfortunately, we are not able to use precession, or its amplitude, as it is not well expressed in the data. We have reordered the discussion and the age model evaluation section (see also comments to R2). Both age model options are now discussed in more detail; including discussion on sedimentation rates, phase assumptions and cyclostratigraphic interpretations.

I would propose to include a more thorough discussion on what the age model differences mean for phase relations, also in context of the recent manuscript by D. Khider 'The role of uncertainty in estimating lead/lag relationships in marine sedimentary archives: A case study from the tropical Pacific: Lead/Lag uncertainties' (Khider et al., n.d.). Your d13C tuning leads to an out-of-phase relationship (lines) of the d13C signal. This is stated, but not discussed. Please discuss why this may be the case, and what it tells about reliability of the signal and tuning.

An error had crept in Fig. 5 depicting the phase relationships. We have updated the figure and text that discusses the phase relationships. The Khider reference is only partially relevant, because they study the last three glacial, which are much better constrained in time compared to the Oligo-Miocene records. We do refer to Khider now, to highlight potential avenues for future research.

Apparently, you use MS data to derive CaCO3. Is there any reason why you do not directly use the MS for tuning then? This is not clear to me, so please clarify this, and demonstrate that the original MS data also supports your conclusions and these signals are in phase when tuned. Results/Figures may go to Supplements. I think demonstrating this will strengthen your manuscript and reasoning. Throughout the manuscript I propose to name the age model CaCO3/MS based, as it is more MS based than CaCO3 based to my understanding.

We have recomputed the conversion of MS into $CaCO_3$ and now use a linear transfer function. The signal structures are identical to one other and the tunings and phase calculations based on MS are similar to those of $CaCO_3$. We convert MS into $CaCO_3$ because ~90% (new improved conversion, using only coulometry data of the study interval) of the variance in the MS record is linked to $CaCO_3$. Variability in $CaCO_3$ estimates, however, is with most readers more strongly associated with lithology (and the processes underlying it: i.e. dissolution, dilution, productivity) than MS variability is. We have clarified the text in section 2.2 to better explain our choice for $CaCO_3$ est. over MS.

Although this is very recent literature, I think that discussing (Laurin et al., in press), their implications and your d13C results would be of advantage – though it would not change your results.

We have included Laurin et al and Khider et al. in the manuscript and reference list.

‘Specific comments
Below you find further remarks. Addressing these would improve your manuscript in my opinion.

Line 33: “correct”: Are you sure one of these is correct in detail? Please rephrase.

We have rephrased this sentence.

Line 34: please explain “anomaly profiles”

We have rephrased this sentence.

39: C6Bn.1n–C6Cn.1: please provide rough age

We have added rough ages in parentheses.

58: Submitted? The paper without data in the reference list is published already, https://www.clim-past.net/13/1129/2017/

We have updated this reference.

81: Here it may be useful to mention that the tuning process can introduce signals into datasets, as has been demonstrated by e.g. (Shackleton et al., 1995)

We discuss introducing spectral power in the data record in section 4.3.1. already. We have added a reference to Shackleton et al. 1995.

83-88: sentence is quite long, please phrase clearer.

We have rephrased this sentence.

144: SI units for MS refer to Volume. Later on you mention units/gram. Both can be correct, but please be careful not to mix the two, and use one consistent unit for the MS through the manuscript, ideally SI units.

We have changed "SI units" with "sensor values" [see: *Westerhold et al*., 2012a, *Pälike et al*., 2010], because our MS record refers to shipboard, whole-round core-logger MS values.

172: please state the re-sampling resolution (in depth or time, or both?)

We have added this information.

179: please specify details of the evolutive spectral method

We have added these details.

195: Please explain what survives here.

The Pacific plate survived. The Juan de Fuca plate was subducted. This has been clarified in the text.

207: 88%? In the Figure it looks like more than 90%, please check.

We have recomputed the $CaCO_3$ content of the sediment and adjusted the values in the text.

209: please explain 'CCSF'

We have explained CCSF in the main text of the manuscript.

220f: hard to see in Figures, please see comment on the Figures below.

See our reply below

223: Smaller? Weaker?

We have replaced “smaller” with “weaker”.

238 and elsewhere: significantly? At which confidence level? I cannot read the significance level from Figures, so please rephrase.

We have replaced “significant” with “strong”.

275: ‘smallest lag’ – relative to what?

We have added “with respect to orbital eccentricity”

304f: Tuning is expected to lead to increased power, see e.g. (Huybers and Aharonson, 2010; Shackleton et al., 1995).

We have added these references.

Generally 4.3. Please substantiate why you choose the d13C as tuning signal here. This information is rather hidden in lines 335-343.

This information was already given in this section. We state that $\delta^{13}C$ is one of two end-members (in terms of phase) for tuning [see, e.g. *Liebrand et al*, 2016., *Pälike et al.*, 2006a, 2006b]. Previous studies have implicitly assumed (correctly) that $CaCO_3$ often responds more directly to orbital forcing (though still nonlinearly) than the climatic components reflected by the O and C isotope systems. Here, we test this assumption, by showing that tuning to $\delta^{13}C$ does not yield satisfactory plate-pair spreading rate histories.

288: Ref to Fig. 6c: In Fig 6c the CaCO3 maxima are not really aligned with eccentricity minima (the dashed correlation lines are not consistent with this statement). Please make sure this is the case, I think this is a plotting issue, as data seem aligned.

The plot seems fine to us. The confusion is probably due to the filters of the ~110-ky signal that are sometimes slightly misaligned with the CaCO3 maxima that we manually selected. We have clarified the text.

296; Evolutive ? analysis: what kind of analysis?

We have added information wrt the kind of analysis.

366? More significant? (and 420f: ‘marginally significant’) Now, it is significant at 95% confidence or not? Maybe rather state ‘significant at higher confidence level?’ – if this is the case.

To prevent confusion with statistical significance, we have rephrased these sentences.

454: Can sedimentation rates give you information on a choice here as well? I propose to insert a brief statement/discussion on this.

We have added a brief discussion on the sudden increase in sedimentation rates here. Constant sedimentation rates are probably more likely.

484: these references are examples; please use ‘e.g.’

We have added “e.g.”.

506: 1264 à IODP Site 1264?

We have inserted “Site”. IODP is already mentioned in the introduction.

516: … required to speculate? Please rephrase, as I do not think we need to speculate.

We have rephrased this sentence.

Figures: Please give correct units for the MS, “instrument units” are not reproducible.

Please see previous comment. We have replaced S.I. units with “sensor values” (according to IODP nomenclature). These MS records are measured on whole round multi sensor tracks and are measures per volume.

Figures: Please indicate which phase represents relative lag/lead

We have now indicated leads and lags with respect to eccentricity in Figure 5.

Fig 2a: high CaCO3 data seem to show less variability than low MS data – again, please note why you use CaCO3 data for tuning instead of MS data.

The low MS values are near the detection limit of the whole round sensor. The main features between the MS and CaCO3 are now identical due to the new linear transfer function that we applied. The conversion from MS to $CaCO_3$ did not affect the visual selection of tuning tie-points in the CaCO3 record, because these are all defined in $CaCO_3$ minima (i.e. MS maxima). We have described and clarified our choice for converting MS into $CaCO_3$ in section 2.2. of the main text.

Fig. 2b: R2 denotes the correlation between MS and CaCO3 or the fit between data and model? Please note that the high MS and low-CaCO3 part seems heavily influenced by a single high MS data point, which seems less representative than lower MS data points. Can this influence your results?

We agree that the correlation between MS and $CaCO_3$ was suboptimal. The entire MS record and coulometry data set for U1334 was used. We have replaced this conversion with one that only considers the data for our study interval, and that removes intervals from Site U1334 with very low and very high MS values. The R^2 value of the MS – coulometric $CaCO_3$ content measurements improved to 0,92. A very convincing relation between MS and calcium carbonate content, as is expected for the deep Pacific. Both MS and $CaCO_3$ estimates were considered during the tuning process and the y-axis conversion of MS values did not affect the tuned ages. In the main body of the text (section 2.2.) we discuss our choice of (new) transfer function.

Fig.3: please indicate the position of the OM boundary

We have now indicated the OMB.

Figs 3, 5: wavelet plots show a lot of irrelevant high-frequency noise. I propose to focus on relevant frequency ranges. This will make readers better able to reconstruct your statements in the manuscript.

Small correction: these are evolutive FFT analysis, not wavelet analysis. We prefer to also show the higher frequencies, because a lot of discussion in the literature is concerned with these cycles. By showing that obliquity and precession are not continuously present/strongly expressed, we visualize one of the main reasons why we tuned solely to the (stable) eccentricity solution.

Fig 5 nicely shows bifurcations of the 100-kyr cycle. These can be used to test phase relationships (Laurin et al., 2016). I encourage you to comment if the pattern is consistent with your assumption.

We have not looked into the details of how bifurcations of the ~110-ky cycle can shed further light on the individual 95 and 110-ky phase relationships to eccentricity. The main aim of this study is to identify the most suitable tuning signal curve and settle the ~110-ky tuning of the OMT interval.

Fig. 7 heading: … versus age.

We have added “age” in the figure caption.

References: I am aware of issues with proposing to cite references during the review process. Please see these as suggestions solely. For some cases, there are other papers which also point in the same direction. I clearly do not require you to cite this specific literature, but I ask you to consider their content, which in my opinion can improve your manuscript. Please decide yourself.

Huybers, P., Aharonson, O., 2010. Orbital tuning, eccentricity, and the frequency modulation of climatic precession. Paleoceanography 25. doi:10.1029/2010PA001952

Khider, D., Ahn, S., Lisiecki, L.E., Lawrence, C.E., Kienast, M., n.d. The role of uncertainty in estimating lead/lag relationships in marine sedimentary archives: A case study from the tropical Pacific. Paleoceanography 2016PA003057. doi:10.1002/2016PA003057

Laurin, J., Růžek, B., Giorgioni, M., n.d. Orbital signals in carbon isotopes: phase distortion as a signature of the carbon cycle. Paleoceanography 2017PA003143. doi:10.1002/2017PA003143

Liebrand, D., Beddow, H.M., Lourens, L.J., Pälike, H., Raffi, I., Bohaty, S.M., Hilgen, F.J., Saes, M.J.M., Wilson, P.A., van Dijk, A.E., Hodell, D.A., Kroon, D., Huck, C.E., Batenburg, S.J., 2016. Cyclostratigraphy and eccentricity tuning of the early Oligocene through early Miocene (30.1–17.1 Ma): Cibicides mundulus stable oxygen and carbon isotope records from Walvis Ridge Site 1264. Earth Planet. Sci. Lett. 450, 392–405. doi:10.1016/j.epsl.2016.06.007

Shackleton, N.J., Hagelberg, T.K., Crowhurst, S.J., 1995. Evaluating the success of astronomical tuning: Pitfalls of using coherence as a criterion for assessing pre-Pleistocene timescales. Paleoceanography 10, 693–697. doi:10.1029/95PA01454

Good suggestions. We have added these references to the text and reference list.

We would like to thank Christian Zeeden (R1) for his constructive comments.