Rebuttal to reviewer 2

Reviewer comments R2:

Review: Beddow et al.: "A comparison of two astronomical tuning approaches for the Oligocene-Miocene Transition from Pacific Ocean Site U1334 and implications for the carbon cycle" submitted to Climate of the Past.

General Comments:

Dear Beddow et al.,

The idea of using two different proxy series for astronomical tuning, and especially evaluating the differences between them in detail and the palaeoclimatological implications is excellent. I believe that the spirit of the paper, and the material presented, fall perfectly within the scope of the "Climate of the Past" journal. There are however some aspects that deserve further elaboration or better explanation to further improve the quality of the manuscript before publication. Below you can find my comments and suggestions.

Specific Comments:

Title:

"approaches" was confusing to me, first thought was that you used two different techniques to do the tuning, while you used two different signals/proxies (and corresponding phase relationships)from the same record. Suggest rephrasing of the title to make this clearer.

We have shortened the title and removed the word "approaches" from it.

Abstract:

Lines 25-30: confusing, you mention two different phase-assumption, but both are inverse and in-phase???

We have rephrased this sentence and clarified the difference between the two tuned age models.

L30-32: Not convinced that the two-end member idea is that well known to the average CP reader, and as such might not be clear in an abstract, more for later in the manuscript.

We have deleted this sentence from the abstract.

L33: what is 'correct'?, maybe use something like 'the most consistent with other data', the most probable etc…

We have rephrased this sentence. See also comment R1.

Introduction:

L62: 'tuning signal' and 'target curves', while in L19-20 you use 'climate proxy records and astronomical solutions', to mean (as I understand it) the same thing. A consistent use of terminology might avoid needles potential confusion.

We use this terminology interchangeably. We have clarified this in the text.

Methods:

Not clear/obvious why you estimate CaCO3 from the MS signal. A motivation for this should be given in the introduction, so that the reader is not confused. Doesn't one loose information, quality of data, by this extra step. The correlation is good, but not one-to-one.

We have not computed a new improved transfer function between MS and $CaCO_3$. See also comments to R1. We have clarified this in the text. See section 2.2.

Explain better sources data (place Wilson citation not optimal), and motivate selection of, plate-paired spreading rates. Maybe instructive to indicate those on your Fig. 1?

We have clarified the text, but not Fig.1. We refer readers interested in spreading rate histories to the study of Wilson. Here we focus predominantly on astronomical age calibrations and carbon cycle implications.

Results:

Presentation of the CaCO3 data is absolutely not clear, different numbers in diff figures, and the text. Adding stages in the plots would make reading much easier.
Not convinced by the mentioned higher frequencies in the CaCO3 record. Would be curious to see the MS spectra too. Could discuss the evolutionary spectrum also more, change over the boundary? Climate dynamics, changing sed rates?

We have updated the correlation between MS and $CaCO_3$, and the figures. The new linear correlation makes MS and $CaCO_3$ interchangeable in terms of cyclic patterns. Hence, their spectra are indistinguishable. We have also added periods to all figs (where relevant). The discussion of evolutive spectra has largely been revised. Also sedimentation rates are discussed in more detail. We discuss the climatic-carbon cycle dynamics in section 6.3.

Astronomical Tuning:

Side: Why are the sedimentation rate reconstructions for the CaCO3 done on the full eccentricity scale, and for the d13C on a higher resolution???

The $CaCO_3$ signal (as the MS signal) is predominantly a clipped (or skewed) signal with high $CaCO_3$ values dominating and a minimum $CaCO_3$ value every ~110 ky or so. Therefore, for this record, we visually selected tuning tie points in the $CaCO_3$ minima (i.e., MS maxima) only. The cycles in the benthic $\delta^{13}C$ are much more sinusoidal in shape. Therefore, we have visually selected tuning tie points in both ~110-ky cycle maxima and minima, which were selected in a Gaussian filter of the data on a polynomial age model through the GTS2012 assigned ages of the magnetic reversals. The higher number of tuning tie-points also affects the linear sedimentation rates computed based on the tie points. Section 4.3 explains the tuning procedures in much detail.

Side question. How is the tuning done? Manually extremum per extremum or with a software/script? Explain somewhere. Suggestion.

We tuned manually by visually selecting tie points in case of the $CaCO_3$ tuning and based on a ~110-ky filter in case of the $\delta^{13}C$ tuning. This is detailed in section 4.3.

In the d13C tuning this 50 kyr period will be close to 41 kyr, could this be an argument in favor of the d13C tuning (because tuning on the eccentricity makes the obliquity come out stronger, and you would expect an obliquity component no?)???

This could be an argument for the $\delta^{13}C$ tuned age model; however, there is no way of knowing. The test for the best astronomical age models, that we have constructed, uses independent evidence from plate-pair spreading rates. Assuming that obliquity must have had an affect on the global marine carbon cycle is more speculative, and not supported by the $CaCO_3$ tuned age model that is in best agreement with spreading rates.

L343: earlier on you mentioned the variability, sensibility of band pass filters to varying parameters, now you use the band pass filters to discuss phase relationships. How robust is this, or is there no problem? Also the phase relationships on Fig. 5 seem to be very sensitive to the used age models…

The exact position of filter minima and maxima is indeed dependant on the bandwidth of the filter. However, the filters depicted here were computed using broad bandwidths and qualitatively show the affect tuning has on the position of 405 ky minima and maxima in the benthic $\delta^{13}C$ wrt to those of eccentricity. The shift in these positions between the two age models is very robust. Unfortunately, the length of the time series was too short to compute phase evolution using, for example, Blackman-Tukey cross-spectral analysis (see e.g. Liebrand et al. 2017, PNAS). Therefore, we discuss the increased phase lag of the benthic $\delta^{13}C$ 405 ky cycle based on visual description, which is supported by filters.

And see ‘other comments’ too please.

See our reply below.

Discussion:

L443: I expected this discussion much earlier… it affects the interpretation of the previous paragraphs

Good point. We have reordered parts of the discussion and results in such a manner that we now first compare the to tuned age model to each other, and then use the plate pair spreading rate histories distinguish between the two tunings. Most of the content stayed the same, we now present the arguments in a more logical order.

L509-510: What might be the influence of the detrending (or not fully) of this d13C shift? Might it effect the BP filtering, be related to this peak in SR in C6Cn2r, add in the end an offset in age models??? Just an observation/thought…

The large positive shift in absolute benthic $\delta^{13}C$ values associated with the onset of the Oligocene-Miocene Carbon Maximum is also linked to the increase in phase lag of the 405 ky cycle. We have taken the bandwidths for filtering very broadly, which visualizes the observed phase lag. Detrending does not affect the phase lag of the 405-ky cycle in $\delta^{13}C$, because only periodicities >600 ky were removed using a notch filter.

Conclusions:

L525: 'insolation forcing' (actually also in your discussion), you tune on eccentricity, but eccentricity as such is only a very small component in the insolation term, eccentricity kicks in as amplitude modulator, non-linear feedbacks etc… should we be careful with the terminology?

Good point. We have added a sentence in the discussion to clarify this. All eccentricity signals in the U1334 records are of course a nonlinear response to the amplitude of precession (i.e. one of the two main insolation components that directly modulate seasonality). When we refer to insolation responses and eccentricity signals, the mechanistic link must be nonlinear and indirect.

References:

Not all consistent (for later, editing)

We have reviewed the references.

Missing in my opinion: Laurin et al., 2017, Paleoceanography

Suggestion, because very recent, Khider et al., 2017, Paleoceanography.

We have included these references in the manuscript.

Other Comments:

L24: again 'tuning approaches'

We have rephrased "approaches" throughout the manuscript.

L58: submitted, in ref list as 'in review', published in CPD, be consistent.

We have updated this reference.

L62: 'tuning signal' and 'target curves', while in L19-20 you use 'climate proxy records and astronomical solutions', to mean (as I understand it) the same thing. A consistent use of terminology might avoid needles potential confusion.

We have clarified the text concerned with tuning signal and target curves.

L83-84: now you specify ODP and IODP, while you already referred to the concept of 'Sites' in the previous paragraph, maybe do this specification earlier in the manuscript.

We have corrected this in the text. The introduction of ODP is now moved forward, before we discuss the first sites.

L84: capital needed for 'Middle Miocene'? is this an official term?

It is, but our use here is informal, hence the small letter.

L85: strange place to refer to Laskar et al., 2004, you didn't specify the tuning sources for the previous paragraph, be consistent.

Indeed. We have removed this reference.

L88: be consistent in your referencing style, and make at the same clear which reference is for which record.

We have clarified the references here. 3

L89: remove 'very', suggestion

Removed.

L91: first time mentioning 110 and 405 kyr periods, maybe for the first time mention explicit link to eccentricity and explain why you use the number of ~110 kyr.

We now link these cycles to the stable eccentricity solution. The text concerned with tidal dissipation and dynamical ellipticity is moved to the discussion and removed from the introduction.

L95: what exactly was the first advantage? Clear cycles or good agreement?

See previous comments. The fact that eccentricity (in contrast to the obliquity and precession solutions) is stable is the first advantage. This has now been resolved.

L99: miss reference(s), how significant would that effect be (for the OMT)?

We have added references here. It could make a difference of up to a couple precession/obliquity cycles at 23 Ma. We refer the interested reader to *Lourens* et al. 2004.

L102: what about differences in age and duration estimates?

Good point. These are affected by tuning assumptions as well. We have clarified the text.

L114: also the CaCO3?

Yes. We have clarified this in the text.

L117: two end-member concept can use more explanation

We refer to section 4.3. for more explanation of this point. Here we briefly state what the reader will find in the manuscript.

L119: diff methods? More diff proxies (with corresponding phase interpretations), not my favourite formulation.

We changed “methods” for “proxies.

L120: now you talk about “records” (before: proxy or signal), I would prefer one consistent terminology. Mention explicitly, between brackets, which records.
See main comment about motivation for this CaCO3 estimate, and potential loose of quality of your data.

I think this is actually quite clear. Proxy signal, record, curve, target, solutions: these are all synonyms for time series. We have clarified these terms on a few occasions.

L148-153: not really ‘Methodology’

We present this information here because we briefly want to explain what the $CaCO_3$ signal of the sediment may indicate. Understanding the tuning signal is important for the method of age model construction.

L160: “a”, typo?

Removed

L161: “and n” typo?

Removed

L172: resampled? What were the original and new resolutions? From Fig. 2 it seems that not all isotopic data has the same resolution, could this be important?

See also comments R1. We have clarified the text.

L173: small motivation for 6 m and 600 kyr? They don’t seem to represent the same amount of your signal???

These values are approximate, because the notch filter is Gaussian. The exact bandwidth did not crucially affect the detrending. We left the text as is.

L176: maybe mention that the bandwidths are mentioned in the fig captions.

We prefer the method section.

L178-179: window sizes and which method (e.g. FFT?) or evolutive analyses.

See comment R1. We have updated the text. Indeed FFT.

L182: Would make more sense to discuss the Wilson, 1993 paper in the introduction, where it is currently missing. Also is this paper the (original) source of your spreading rates?

We mention in the introduction that spreading rates have previously been used to constrain/check tuned age models. In our opinion, the introduction of the Wilson data is best presented here.

L184: missing “)”

We have removed the brackets altogether.

L184-186: sources rates for all Wilson paper? What is your motivation to select these four sets?

See comments R1. The text has been clarified wrt this point.

L199: which reversals?

We have added this information.

L206-207: the ranges on your plot 2a and 2b, and Fig 3 for CaCO3 and MS are different!!! How is this possible, highly confusing. Is one from core logging and others from discrete sampling? Needs to be clarified.

We have updated this plot, and now show the new, improved, correlation between MS and $CaCO_3$.

L208: below 70%? Also at other places? What is the point? Include the stages on the plots, this will make things much easier for all readers that might not be as familiar with the magnetostratigraphy as you are.

We have removed this sentence. We have added the Oligocene and Miocene Periods in Figure 3.

L213: where is the OMT on your figure?

See previous comment.

L215: refer to (sub)figure, where does this age come from?

Figure reference added. Age removed.

L216: is this higher amplitude variability so convincing? Often single points?

After the OMB clear 110 ky cycles are present. These consist of many data points.

L217: more positive or less? Lower values, reversed axis?

More positive values/higher values. The y-axes of the isotope plots are indeed reversed.

L224: where do you see these 1.83 and 2.8 c/m peaks in the CaCO3 record? I don't…

Agreed. Sentence removed.

L225: which high-amp cycles? Specify, be 100% clear.

We have clarified the text.

L228: any biostrat in addition to the magnetostrat?

No biostratigraphy.

L233: can refer to Table 1. Fig. 4

We have included these references.

L235: isn't evolutive a form of power spectrum?

Correct. We have removed the word "and" in between the two.

L238: what does significant mean?

See R1. "Significant" is replaced by "strong".

L240 CaCO3 est => different CaCO3 values ? never different on your figures…

We are not entirely sure what R2 means with this comment. We have updated Fig. 2b.

L245: I see your point, but here you took twice the same filter and is the different outcome because the different variations in both signals. Remove very.

We have deleted the sentence referring to filter bandwidth. This was out of place here. And removed "very".

L253: be consistent with your spelling of time(-)series

We have removed the dash

L256: reference to wrong figure, not Fig. 7

Changed into Fig. 5.

L257-261: repetition of intro, and this time with reference. Do once expanded in the intro.

We have chosen to remove it from the introduction and discuss tidal dissipation/dynamical ellipticity (and the implications for tuning) here.

L272: again, somewhere in the beginning you referred to different phase relationships???

Yes, both $CaCO_3$ and $\delta^{13}C$ are tuned in-phase with eccentricity, after multiplying the records with -1. However, both records contain different response times to eccentricity, resulting in different age models with differing leads and lags (i.e. exact phase relationships). To clarify the difference between leads and lags (which also contain phase information), and the 'broad correlation' between phases of cycles and how they correspond to one other (e.g. ecc max to $CaCO_3$ min), we have clarified the text.

L273-279: this would have been useful to read much earlier in the manuscript.

We do mention this in the last paragraph of the introduction. This is the section in which we further elaborate on this rationale.

L284-286: maybe shortly explain mechanistic link? Why higher CaCO3 in cooler period?

We now mention the mechanisms underpinning the $CaCO_3$ variability in the equatorial Pacific.

L287: clearly delineated? Before you made the argument that is not always so easy? Some peaks are clear, but not all 23.

We have rephrased this sentence.

L293: where is the OMT? Not so much higher sed rates…

We have indicated the OMT in the figure. We have removed the comment wrt higher sed rates across the OMT.

L296-298: what figure do you refer too? (Fig. 5?) Confusing description, the evolutive shows where you see the cycles over the record, maybe state something that the 405 kyr is the most consistently present over the records for all proxies or something of the sort. Also (L298), it is difficult to see the highest amplitudes on the evolutive??? Maybe on the power spectra, but there I don't see a much stronger short eccentricity cycle for the CaCO3, it seems however more present over the whole record, compared to the stable isotope records.

Yes we do refer to figure 5. We have rewritten and clarified this text.

L301-302: indicate on relevant figure(s)where this OMT, and peak glaciation conditions occur

Because we are talking about evolutive analysis, we have removed the reference to glacial peaks. Not all readers are familiar with the structure of the data post-OMT, so this reference was true, but a bit confusing.

L308: 50 kyr cycle, not immediately clear specific for d13C (continuation), or in general. In the d13C tuning this 50 kyr period will be close to 41 kyr, could this be an argument in favour of the d13C tuning (because tuning on the eccentricity makes the obliquity come out stronger, and you would expect an obliquity component no?)???

It is a relatively weak signal indeed. We would like to mention it, but interpreting it is more difficult. We prefer not to use it to favour one age model to the other.

L315-325: it might not be clear to all readers how by looking at Fig. 5 (which shows phases in degrees) you get to duration in kyr, small clarification (e.g. in methodology) would make it easier to interpret. Suggestion.

We have replotted the phase computations and they are now depicted in ky.

L329: Laurin et al., 2017, Paleoceanography, recent reference, maybe include.

We have added this reference.

L332: Is Early Miocene with capital? Indicate your (sub-)stages on your figures.

We prefer informal use. Periods are now indicated on figures.

L343: earlier on you mentioned the variability, sensibility of bandpass filters to varying parameters, now you use the bandpass filters to discuss phase relationships. How robust is this, or is there no problem? Also the phase relationships on Fig. 5 seem to be very sensitive to the used age models…

We have added a cautionary note about the implications of having a short record and not being able to compute this potential phase-increase.

Side question. How is the tuning done? Manually extremum per extremum or with a software/script? Explain somewhere. Suggestion.

We have already explained this in section 4.3.2., last sentence of the second paragraph.

L357: Fig 7e. Peak in sed rate around C6Cn2r, potentially skipping an eccentricity cycle? How would including another short eccentricity cycle in the d13C tuning affect your outcomes?

Probably yes. However, this is the result from an initial 405-ky cycle interpretation in $\delta^{13}C$ and subsequent alignment (in phase) of the ~110-ky cycle. At the OMT this tuning approach results in the (with hindsight incorrect) merging of two cycles into one. Spreading rates rule out this approach, and the in-phase response of $\delta^{13}C$ during (especially) the early Miocene. Including another cycle here, would bring the carbonate and $\delta^{13}C$ tunings in better agreement. However, we would then depart from our phase-assumption. Which is the main cycle parameter we aimed to test.

L358: 1) different units of sedimentation rate in text and figures, confusing. 2) 1.7 cm/kyr peak????

We have adjusted the figure axes to correspond with the text. We have corrected the value of this peak in LSR across the OMT in the text.

L361: could this one cycle difference be related to the comment “L357”?

Yes, it is. We have added this information.

L362: refer again to fig 5, helps with following. (for me)

We have added a reference to Fig. 5.

L368: confusing, different frequencies on fig. 5, and kyr-periods in text descriptions. Would be good to mention the frequencies you point at in the text (or plot evolutive diagrams in function of periods)…
Is the 41 kyr the case for all proxies? See also comment on 50 kyr cycle.

We have added the periodicities to Fig. 5. We have clarified the text with respect to the presence of strong obliquity cycles in benthic $\delta^{18}O$.

L371-379: the phase results for the different proxies seem much more similar to each other than for the CaCO3 tuning. Can you comment on that?

We had noticed this ourselves as well. Unfortunately a small error was present in the phase plots. This has now been corrected.

L385: can refer to the Wilson paper for more info on principle, also on uncertainties etc of this type of data, not all readers might be familiar with this.

We have added a reference to two papers by Wilson.

L393: what are the dotted lines on Fig. 8?

The figure caption is revised to define the dotted lines.

How would this plot look like with the additional d13C short eccentricity in your tuning?

This is not a tuning option considered in this paper. See previous comments.

Side: Why are the sedimentation rate reconstructions for the CaCO3 done on the full eccentricity scale, and for the d13C on a higher resolution???

See the sections related to tuning. $CaCO_3$ is tuned to eccentricity minima. $\delta^{13}C$ to both eccentricity minima and maxima. This is also reflected in the sedimentation rates.

L391-393: conclusions before description results…

We have moved this entire paragraph toward the base of section 5.

L412: reference to Tab. 1 would be useful. In the text you use duration difference, which are not includes in the Table… could make it easier for the reader.

We have added this reference.

L426-427: specify which interval.

We have specified this.

L435: On=> One? Typo.

We have rephrased this sentence to clarify the main message.

L443: I expected this discussion much earlier… it affects the interpretation of the previous paragraphs

We prefer the order in which we presented the results and discussion. First we present two tunings. Next we let spreading rates be the judge. Finally, we integrate all results (tuning and spreading rates) and explain the key differences between the two tuned age models.

L468: be consistent in ref style.

Corrected.

L494: Zachos et al. (2010), EPSL interesting additional reference?…

This is a reference regarding the PETM and early Eocene. Carbon cycle dynamics were probably quite different then.

L501: Laurin et al., 2017; Paleoceanography.

We have added Laurin et al to the references here.

L503-505: where do we see this change???

We have added this information between parentheses.

Fig 6 & 7: detrended records? Mention.

Records are not detrended in the plots, but were detrended prior to filtering. This is mentioned in the methods section.

L509-510: What might be the influence of the detrending (or not fully) of this d13C shift? Might it effect the BP filtering, be related to this peak in SR in C6Cn2r, add in the end an offset in age models??? Just an observation/thought…

We have tested the affect of detrending, but this did not affect the BP filtering of the 405 ky or the ~110-kyr signal significantly.

L519: the CacO3 content in this case is a 'derivative' of original MS data…

We have added this information.

L525: 'insolation forcing' (actually also in your discussion), you tune on eccentricity, but eccentricity as such is only a very small component in the insolation term, eccentricity kicks in as amplitude modulator, non-linear feedbacks etc… should we be careful with the terminology?

We have rephrased this.

L533: "C6AAr.r3" = only place in the manuscript where this name occurs???? Typo? Different notation, then explain.

This was indeed an error. We have corrected it.

We would like to thank R2 for their constructive comments.