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

‘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.

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 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.

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.

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.

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 CaCOC/MS based, as it is more MS based than CaCO3 based to my understanding.

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.

'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.

Line 34: please explain "anomaly profiles"

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

58: Submitted? The paper without data in the reference list is published already,

https://www.clim-past.net/13/1129/2017/

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)

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

144: SI units for MS refer to Volume. Lateron 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.

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

179: please specify details of the evolutive spectral method

195: Please explain what survives here.

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

209: please explain 'CCSF'

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

223: Smaller? Weaker?

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

275: 'smallest lag' – relative to what?

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

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

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.

296; Evolutive ? analysis: what 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.

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

484: these references are examples, please use 'e.g.'

506: 1264 → IODP Site 1264?

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

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

Figures: Please indicate which phase represents relative lag/lead

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.

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 datapoint which seems less representative than more lower MS datapoints. Can this influence your results?

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

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.

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.

Fig. 7 heading: … versus age.

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., Meyers, S.R., Galeotti, S., Lanci, L., 2016. Frequency modulation reveals the phasing of orbital eccentricity during Cretaceous Oceanic Anoxic Event II and the Eocene hyperthermals. Earth Planet. Sci. Lett. 442, 143–156. doi:10.1016/j.epsl.2016.02.047

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