

Clim. Past Discuss., author comment AC2  
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## Reply on RC2

Ji-Eun Kim et al.

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Author comment on "Precessional pacing of tropical ocean carbon export during the Late Cretaceous" by Ji-Eun Kim et al., Clim. Past Discuss.,  
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We thank *Referee #2* Mingsong Li for reviewing our manuscript. Here we reply to the comments given 26 Jun 2022, the revised manuscript will be changed accordingly.

Two major and some minor issues were raised by R#2. First we would like to comment on the two major issues:

### *1) Missed information about the tuning in the main paper*

R#2 suggests to insert a chapter about the astrochronology / cyclostratigraphy in the main text as this is a central part of the study. In a revised manuscript we will add a chapter dealing with the age model development. We will move supplementary figures S2 (Basic 405 kyr age mode), S10 (bulk carbon isotope MTM power spectrum and wavelet analysis in depth and age) and S12 (Barium MTM power spectrum and wavelet analysis in depth and age) to the main text.

R#2 points out that *the astronomical tuning strategy is generally straightforward in this case study* and that *the resulting mean sedimentation rate of about 1.25 cm/kyr* from the 405-kyr cyclostratigraphy is supported by other geologic evidence.

We have tested our 405-kyr cyclostratigraphy by plotting the Shatsky Rise bulk  $\delta^{13}\text{C}$  record against the bulk  $\delta^{13}\text{C}$  record of the Zumaia succession from Batenburg et al. (2012) which is the only record with comparable resolution. As given in the main text we updated the tie points of Batenburg et al. (2012) to the Laskar cosine function (Table S2) to make the records consistent with respect to the target curve. This way we can check if the number of 405-kyr cycles identified in the Shatsky Rise record is consistent with the complete record from Zumaia.

R#2 comments that the method we used *can be subjective and it is unclear whether all assigned 405 kyr cycles are fine or not*. One solution is to use complex statistical tools like TimeOpt or COCO, where the latter was developed by R#2. And indeed as given by the test by R#2 using COCO the results are basically the same as ours. We thank R#2 for point out to use these methods. Moreover, R#2 suggests a potential critical interval identified by his method at  $\sim 285\text{m rmcd}$ . This corresponds to between 405-kyr cycles labeled Maa5 and Maa6 in Figure S2 and S3. If there is one more 405-kyr cycle in this interval the carbon isotope minimum at 68.3 Ma would not match the Zumaia record anymore.

In the revised version of the manuscript we will add the COCO approach for testing our age model. It should be pointed out here that the drill cores used are affected by coring disturbance to some extent, particularly when chert layers were encountered. For the composite record from Shatsky Rise we tried to avoid those intervals whenever possible. However, there will be some disturbance and therefore misinterpretation of changes in cycle thickness can occur using an automated analysis routine that assumes constant sedimentation rates. Unpublished precession cycle by precession cycle correlation to Zumaia also reconfirms the basic 405-kyr age model presented in the initial manuscript.

- *Unclear relationship between the thermocline and precession*

*This paper argues that precession paced ocean export production via influencing the thermocline depth. In the text, shoaling of the thermocline is thought to have occurred in response to changes in precession. This is a critical component of the story; however, the mechanism is not presented. In Figure 5, the authors note that "a shallower thermocline at high precession" represents a huge jump. Please fulfill this gap in the main text.*

*Moreover, the paper mentioned that "ENSO-like variability" existed in past greenhouse conditions. It would be better if this variability pattern can be introduced so readers don't have to read other papers (e.g., Davies et al., 2012) to understand this term.*

We will add additional introduction to this variability in the revised manuscript and more clearly lay out the proposed relationship between precession and this ENSO-like variability (proposed revisions *italicized*) "...Previous work suggested a link between past warm climates and a "permanent El Niño state" (Wara et al., 2005; Fedorov et al., 2006) with *weak trade winds along the equator, a deeper thermocline and more stratified tropical Pacific preventing upwelling of nutrients that result in reduced primary production and low carbon export. Today this occurs as an irregular periodic variation along with an opposite state in the tropical Pacific with strong trade winds and more intense upwelling in the eastern tropical Pacific driving increases in productivity on interannual-to-decadal timescales. Changes in the mean state over longer timescales can be recorded in deep sea sediment archives (e.g., Pena et al., 2008; Zhang et al., 2021).* The large range of variations in our carbon export record in the tropical Pacific (*XRF-Ba content and benthic foraminiferal accumulation rates*) during the Maastrichtian greenhouse argues against the hypothesis of a continual El Niño-like state. *The new record exhibits variations which we suggest reflect changes in the mean climate state. We suggest instead that this new record adds to the growing body of work that suggests robust El Niño-Southern Oscillation-like variability existed in past greenhouse conditions (e.g., Davies et al., 2012) and may be sensitive to orbital forcing, especially the effect of orbital precession in the tropics (e.g., Clement et al., 1999; 2000; Lu et al., 2019). The resultant tropical mean climate state and climate variability forced by minima and maxima of precession changing the strength of coupled ocean-atmosphere feedbacks in the tropical Pacific is less clear during the Maastrichtian greenhouse (Fig. 4), and requires focused modeling efforts and new proxy records which test these hypothesized relationships."*

Upon further examination and reflection of the literature, we want to shift the focus of the discussion here (see above). Figure 4 will be revised so that a precession is more generally related to changes in the mean state of the system, shifting from a mean state with a shallower thermocline resulting in higher productivity to a deeper thermocline with lower productivity in the region. Without additional work (outside the scope of this study) it is not possible to know if it is minima or maxima in precession that should result in one mean state or the other.

New references to be added to the main manuscript:

Clement, A. C., Seager, R., and Cane, M. A.: Orbital controls on the El Niño/Southern Oscillation and the tropical climate, *Paleoceanography*, 14, 441– 456, 1999. doi:10.1029/1999PA900013.

Clement, A. C., Seager, R., and Cane, M. A.: Suppression of El Niño during the mid-Holocene by changes in the Earth's orbit, *Paleoceanography*, 15, 731– 737, 2000. doi:10.1029/1999PA000466.

Liu, Z., Lu, Z., Wen, X., Otto-Bliesner, B., Timmermann, A., and Cobb, K.: Evolution and forcing mechanisms of El Niño over the past 21,000 years, *Nature*, 515(7528), 550– 553, (2014). <https://doi.org/10.1038/nature13963>

Lu, Z., Liu, Z., Chen, G., and Guan, J.: Prominent precession band variance in ENSO intensity over the last 300,000 years, *Geophysical Research Letters*, 46, 9786–9795, 2019. doi:10.1029/2019GL083410.

Pena, L. D., Cacho, I., Ferretti, P., and Hall, M. A.: El Niño-Southern Oscillation-like variability during glacial terminations and interlatitudinal teleconnections, *Paleoceanography*, 23, PA3101, 2008. doi:10.1029/2008PA001620

Wara, M. W., Ravelo, A. C., and Delaney, M. L.: Permanent El Niño-like conditions during the Pliocene warm period, *Science*, 309, 758–761, 2005. doi:10.1126/science.1112596.

Zhang, S., Yu, Z., Gong, X., Wang, Y., Chang, F., Lohmman, G., Qi, Y., and Li, T.: Precession cycles of the El Niño/Southern oscillation-like system controlled by Pacific upper-ocean stratification, *Communications Earth & Environment*, 2, 239, 2021. doi:10.1038/s43247-021-00305-05.

#### *Minor issues*

- *Line 28: add "405 kyr," before 100 kyr.*

Will be corrected in the revised manuscript

- *Line 111: sampling time of 12 s. Do you have evidence that 12 s is sufficient for the data quality? I have tested a range of sampling times ranging from 10 s to 120 s, it looks like XRF can usually produce stable results after ca. 20 s.*

The Shatsky Rise cores consist of sediments with very high carbonate content, typically >95%. XRF scanning of very high carbonate sediments is difficult due to artifacts produced by the extremely high calcium peak and interaction with other elements. Thus, after test scans, T. Westerhold decided to scan only at a 50 kV setting because only the barium signal gave a stable and good result using 12 seconds count time. Agreed that count time should be longer to get statistically better results, however there is a time and cost limitation running cores at the repositories. Thus 12 seconds only scanning 50 kV was deemed useful to establish a stratigraphy and look at productivity related changes. Other elements like iron, which normally are pretty robust in acquiring, are difficult to measure on the high carbonate cores in relatively short time. One has to be aware that >95% carbonate content at a 10 kV run (usually the one to measure Al to Fe) will result in a high deadtime prolonging the scan time enormously. From the experience of T. Westerhold with many XRF scanned sections on a large range of sediment types and cores, stable results can be obtained already at 5-7 seconds count time having the right sediment

composition and sediment core quality (not too high water content etc.). Generally, it cannot be stated as done by R#2 that XRF *can usually produce stable results after ca. 20 s* only.

- *Figure 1: Please explain the meaning of shade zones in the map (brown, light olive, blue, and purple).*

Will be included in the revised manuscript

- *Figure 3: obliquity period was not 41 kyr. Alternatively, the La2004 solution predicts a 38 kyr cyclicity for this time interval.*

Will be corrected in the revised manuscript

- *Figure 4: the middle rows are too busy. Why there is no BFAR data at the interval of 66.74- 68.71 Ma?*

BFAR was only done for two cycles (peaks in Ba) to confirm whether or not the organic matter export indicated by the Ba peaks was reaching/impacting the seafloor community. No data was collected for their earlier peak.

- *Line 468: Remove "Strong" in the caption.*

Will be corrected in the revised manuscript