

Review No 1 (anonymous)

I am pleased to see the authors address my comments so comprehensively (although I do not know why they assume I am male!). The discussion about the interpretation of the 1092 record as a global signal is very thoughtful. The whole paper is now much clearer as the authors have explained the nature of CM6 in the context of the ice sheet growth at the MMCT. The only change I now suggest is that "falling sea level" on line 45 should be "rising sea level."

[AC: We thank the referee for his/her effort to review our manuscript again. And thanks for spotting this slip, we have changed "falling" to "rising" sea level \(l. 45\).](#)

Review No 2 (anonymous)

Raitzsch et al. present us a relatively high resolution record of atmospheric CO₂ from a Southern Ocean site (ODP Site 1092) for the middle Miocene climate transition. The manuscript, both data processing and interpretation, has been substantially improved, and it will be a great addition to the scientific toolkit on climate-carbon-ice sheet interactions during the Neogene and beyond.

I only have a few additional comments and suggestions, for which indicated line numbers refer to the marked version of the manuscript.

[AC: We thank the referee for his/her effort to review our manuscript again. The replies to the additional comments are listed below.](#)

1.1 Sampling strategy: please include the size fraction selected.

[AC: We used the 250-315 \$\mu\text{m}\$ size fraction, and added this information \(l. 83-84\).](#)

Line 165: Please consider rephrasing as (or similar to): "By contrast, for *T. trilobus* d11B the *T. sacculifer* calibration is applied, with four available equations (.....). The calibrations refer to different size fractions of *T. sac* and different analytical techniques..."

T. sacculifer d11B calibrations are dependent on the size fraction of the foraminifera shells (e.g. see summary in Foster et al. 2012). Comparing individual records with different calibrations without considering the size fraction of the foraminifera used might be misleading.

[AC: Good point. The sentence has been rephrased accordingly \(l. 152-155\).](#)

Lines 209-212: Mg/Ca is pH insensitive for *T. sac* (Gray and Evans 2019). Therefore the pH correction should not be applied to *T. trilobus*, but it is not clear if it is.

[AC: For the sake of consistency, we applied the modified 'MgCaRB' model for the *T. trilobus* records, too. But, as the reviewer stated correctly, there is no discernible pH effect on Mg/Ca of this species. This is now clear in the text \(l. 186-189\).](#)

Lines 261-262. The CO₂ record has large uncertainties on the scale of ~ 50-170 ppm. How confidently could we interpret variations on the scale of 50-100 ppm? The ~100 ppm step change at 13.82 Ma is probably meaningful as it displays some consistency with the following CO₂ estimates and the general cyclicity of the record, but the 50 ppm higher frequency variations remain a question. Relative CO₂ changes must carry a smaller uncertainty, and an estimate of this would be very useful to demonstrate that such variations are indeed meaningful and interpretable.

AC: That is an interesting point raised by the reviewer, and indeed the uncertainty of pCO₂ estimates is much smaller if we neglect the uncertainties of parameters that are unlikely to change on shorter timescales, i.e. those for the calibration equation and d11B of seawater. This is now addressed in the manuscript (l. 219-222).

Lines 355 and below:

At the risk of “wobble matching”, comparing the B/Ca record of Sosdian et al., 2020 with this CO₂ record one might see some striking similarities. There is a long term decline in CO₂, just as the B/Ca record displays a long term increase (and thus decline in surface water DIC). Within the overlapping period, there are two clear ~ 400ky larger cycles, each of which include a CM. The cycle that includes CM5b is characterised by both lower B/Ca (higher DIC) and higher CO₂, thus both records appear consistent. The cycle that includes CM6 is intriguing because of the structure within the B/Ca record of Sosdian et al., 2020 in comparison to the d11B and B/Ca in Badger et al., 2013 and this CO₂ record. If we ignore the point at ~13.8 Ma of the record in Badger et al., 2013, and focus on the period transitioning into CM6, B/Ca and d11B/CO₂ follow the expected covariation so as higher d11B (higher pH and lower CO₂) occurs when B/Ca is higher (lower DIC). But then, the expected minima in B/Ca at CM6, when compared to this CO₂ record here, and the correlation of records within CM5b, collapses at the peak of the d13C positive excursion but recovers shortly after. Of course, the disagreement could be driven by issues on either of the two proxies, e.g. non-DIC effects on B/Ca, or the assumptions regarding the d11B-CO₂ proxy, or regional effects on either of these two records, or a more global scale change. It would be great if both the similarities and the disagreements can be explored, and in that context identify what could make CM6 different to CM5b.

AC: We fully agree that a deeper comparison of our record with the B/Ca record of Sosdian et al. (2020) is beneficial, and have therefore extended section 4.2 (l. 314-320).

Line 422 and below:

Clearly CM6 is characterized by a more pronounced d13C change compared to all the other CM events. However, this is not something observed in this CO₂ record. If anything, comparing the maximum and minimum one might say CM5b CO₂ is more pronounced than CM6, since the maximum is ~equivalent but the minimum of CM5b CO₂ is clearly higher, and the amplitude of change is either equivalent or larger for CM5b than CM6. This comparison gets further complicated by the argument of enhanced deep water formation during CM6 (which is plausible), which would likely place Site 1092 into a frontal system in the Southern Ocean, recording higher [CO₂] than atmospheric CO₂ (and possibly also contributing to the divergence in the records of B/Ca and CO₂). What would drive the difference in amplitude of change between the d13C and CO₂ records for CM6 in comparison to the other CM?

AC: While we agree that answering the questions about the exceptional nature of CM6 in comparison to the other CM events would be a great step forward in Miocene climate research, we think that, based on our data, the room for new speculations is quite limited. However, we added a concluding remark at the end of section 4.4 (l. 413-419) emphasizing again that Site 1092 may bear a regional signal that differs from global climate evolution, and that further research on this topic is required.

Other changes

In addition to the changes suggested by the reviewers, we found that in the supplemental Fig. S2 "G. trilobus" should be "T. trilobus". We have corrected the figure accordingly.