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Climate of the Past
Discussions

Interactive comment on "Southern Ocean bottom water cooling and ice sheet expansion during the middle Miocene climate transition" *by* Thomas J. Leutert et al.

Anonymous Referee #2

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Leutert et al., present the first high-latitude independent bottom water temperature records for the mid Miocene, spanning the bulk of the Miocene climatic optimum and the Miocene climate transition. By using clumped isotope thermometry, the authors circumvent known issues that affect more traditional BWT proxies, such as Mg/Ca.

The main contributions of this manuscript are twofold: 1) By providing independent mid Miocene BWT records, they can evaluate the reliability of deep sea benthic foraminiferal D47 and Mg/Ca records as a BWT proxy. This comparison confirms that D47 is likely an independent temperature proxy that predominantly records BWT, whereas Mg/Ca is affected by non-thermal effects. 2) They show that the main trends in mid Miocene BWT, as reconstructed by D47, are observed at both high and low latitude sites, but are somewhat decoupled from the main trends in ice growth across the mmct. They speculate that regional freshening in the upper water column may be a mechanism to explain this decoupling.

My main concerns are to do with the organization of the manuscript (I have made more specific comments about this below), which can be easily address: - The authors have spread methodological information across the methods, results/discussion and appendix. I found this confusing and will make it hard for readers to later find their methodological approaches. - I found it especially distracting to have Section 3.1 and 3.2 jump between methods, results and discussion. If I were coming back to this paper to find either methodological information or reread the scientific discussion, I would find this frustrating. I think the manuscript would be clearer if the authors could reorganize and group this information better. - The authors also discuss supplementary figures in quite a lot of detail in the main text, so I don't understand why some of those figures aren't incorporated. I am fine with supplementary figures, if the main discussion of those figures is also in the supplement. I found it frustrating to have to go back and forth between the main text & supplement where SI figures were being discussed in detail in the main text. In some cases, the supplementary figures are also only slightly more expanded versions of the main figures, so I don't understand why the supplementary version isn't used instead of the current main version.

Structure concerns aside, the manuscript presents one of the few high-latitude deep sea temperature records of the mid Miocene. I think the manuscript represents a good contribution to Climate of the Past, with interesting implications for both Mg/Ca thermometry and mid Miocene climate reconstructions.

Suggestions in order of appearance:

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Ln 16: Could you specify which other regions/sites you compare to?

Ln 28: In my experience the mmct is defined as the specific benthic isotope excursion ∼13.9 Ma (eg Holbourn et al., 2005), much like you'd recognize CM6 or CM5a/b. Could the authors provide a reference for where their definition comes from?

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Ln 32: Can the authors include the original publications that produced CO2 records across the mmct, for instance Foster et al., 2012 EPSL?

Lns 39-41: I don't think the Fairbanks equation is appropriate here, considering that the isotopic composition of the Miocene ice itself was likely different. Can values reported in more recent modelling studies (specifically Gasson et al., 2016, PNAS) possibly provide a better estimate of this?

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Lns 74-75: Can you introduce here what you mean by low data density? Are you hoping to track orbital-scale (eccentricity, obliquity, precession) variability over this time period? This may become apparent later on, but it would be good to introduce this more clearly here, as low vs high data density can mean very different things to different people.

Ln 77: "calcites" should be "calcite".

Ln 78 / Ln 83: Please specify that this is in the Indian sector of the Southern Ocean.

Ln 88: It would be useful to incorporate the target sample resolution earlier on, especially if you can link the temporal sampling resolution to your goal (still at this stage not clear whether you aim to just get a grasp of long-term changes, or also want to pick up orbital scale variability).

Lns 88-90: Can you clarify this statement? Do you mean you are using a composite depth scale? Based on the supplementary tables, the authors use a revised mbsf (rmbsf), but they don't actually define that anywhere. It's great to see the authors include the full sample ID and depth, but if they could additionally include the original

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mbsf as a column in Table S5, that would be better, especially as they seem to refer to the mbsf not rmbsf depths in Section 2.1.

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Figure 1: - It took me a while to understand this figure, and especially to understand that the two maps are of the same area. I was confused as 747 is only highlighted on the left map & 761 only on the right map. Could you potentially adapt this figure to show the full map? Or annotate more clearly on the figure that 1700 m / 2200 m depth are the modern 747/761 water depths respectively? - Also, could the authors add 1171, and ideally U1335/7/8 on the map? I understand the eastern equatorial Pacific sites are hard to fit on with the globe shown as is, but 1171 can definitely be added.

Lns 106-107: There is some evidence that these species can have different d13C signatures, although this isn't often seen. Has there been any research on clumped isotopes being comparable between the two species? Did you measure the species separately in any of your samples to check they are comparable? Later comment: much of this is later included in the results/discussion section. I found that very confusing and would recommend the authors address interspecies offsets in d18O/d13C/D47 in the methods.

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Ln 110: Rinsed in DI water?

Section 2.3 vs Appendix A: As D47 is a key contribution here, I found it confusing that the clumped isotope methods were split between this section and Appendix A.

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Lns 143-145: Can the authors provide these recalibrated records in their supplementary data for community use, of course appropriately referencing the original studies? This would greatly help update those records to this more recent calibration. From the supplementary information, it seems the 761 data is included, but not the 1171 data.

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Lns 150-151: GTS2020 was recently published (Raffi et al., 2020), so I would recommend updating the magnetostratigraphic tie points to the most recent timescale. This may not make much difference for the younger interval, but for the oldest reversals used, GTS2020 uses the Chrons recalibrated by Kochhann et al., 2016. As the authors use the Kochhann ages for the d13C based ties, for consistencies sake, I would recommend using the Kochhann et al., 2016/GTS2020 ages for the magnetostratigraphic tie points as well.

Lns 167-168: Ah, the authors discuss interspecies offsets here. I think it would be helpful to discuss in the methods that both species were measured in the same sample to quantify interspecies offsets.

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Figure 2: The authors have done a good job at compiling stable isotope records from different regions; however, I think they are missing some data in the youngest interval. Nathan and Leckie, 2009 (Palaeo3) published low-resolution benthic stable isotope data at 806 and Tian et al., 2017 (Gcubed) and 2018 (EPSL) provides benthic stable isotope data from U1337 between 0 and 16 Ma. Including relevant parts of these datasets could benefit their inter-regional comparison, as they hardly show any data younger than 12.7 Ma in the eastern/western equatorial Pacific Ocean.

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Section 3.2 title: I'm not sure the title really reflects the content of the section. If the authors do reorganize their current section 3 to separate out methods, results and discussion, subsections of this topic could be helpful.

Ln 189: I appreciate the averaged D47 signal is the one the authors want to take forward in their discussion, but can they incorporate Figure S3a into the main manuscript? The D47 data is a key result of the study and they refer to Figure S3 a few times, so it would be better for the reader not to have to flip back and forth between the main text and supplement so often.

First two paragraphs of 3.2 (Lns 190-199 & 200-204): I found the way methods-results-discussion were all mixed together in section 3 to be confusing. Can the authors re-structure it, so the information is easier to find? I found it distracting to continually jump back and forth between methodological information & scientific discussion. The first paragraph is very methodological (190-199), followed by a results paragraph (200-204), before the authors move on to their discussion. If I were rereading this paper at a later stage for their climatic interpretation and discussion, I would find the inclusion of methodological information in the discussion distracting.

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Figure 3: I appreciate this is not data produced by the authors, but if they could provide error estimates for the Mg/Ca BWT, that would be helpful.

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Lns 220-222: I don't fully understand what the authors are trying to say here. It currently reads like the authors are saying that Miocene BWT at 747/761 were similar to the present day, but then in the following sentence, they say Miocene BWT were considerably warmer. Do they mean the difference between 747 and 761 is similar to the present day?

Lns 229 - 231: Showing these BWT records overlaid would help illustrate this point.

Lns 234-245: Can the authors integrate Figure 3 with Figure S4? The authors discussed the various Mg/Ca records included in Figure S4 quite a lot in this paragraph, but only include 2 in Figure 3. I again found it frustrating to have to switch between the main text and supplement to look at data that was discussed in depth in the main text.

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Ln 257/Figure 4: I don't understand why Figure S5 isn't used in the main manuscript instead of Figure 4, especially as the inclusion of CO32- data is the main difference between the two figures.

Lns 257-258: This sentence seemed out of place, as they only discuss 747 in this paragraph and only briefly mention 1171 in the previous paragraph.

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Lns 280-281: This information feels more appropriate for the methods.

Ln 284: “complicating” should be “complicated”.

Lns 291-293: What could past differences in the isotopic composition of the Miocene ice sheet mean for these estimates? The authors mention absolute estimates in the introduction, so it seems odd that they do not come back to absolute estimates here after reconstructing d18Osw. If the authors considered the potential past isotopic composition of the ice sheet used in Gasson et al., 2016, they could transfer their new d18Osw estimates to absolute sea level estimates. I think this could be worth including, especially as they mention absolute estimates in the introduction.

Lns 293-294: This sentence isn’t entirely clear; can you rephrase it? Also, could you include the equations used in Figure S6 in the caption or figure itself?

Ln 298: Can the authors rephrase the part of the sentence here to do with orbital parameters? When I first read it, it seems to be undermined by the previous sentence where the authors say that they can’t be sure they pick up orbital-scale minima in global ice volume due to the temporal resolution of the D47 records. If they can make it clearer that they mean to look at the influence of longer-term cycles in orbital parameters (e.g., 2.4 Myr/1.2 Myr amplitude modulations), rather than shorter-term orbital cyclicity, that would be helpful.

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Figure 5: - It was hard to read all the details in this figure. Could it be made big-

ger/wider? - Also, I appreciate the authors are not trying to discuss orbital scale variability, but it could be useful to include one of the higher resolution benthic stratigraphies as a reference for the exact timing of different events. A composite of the records shown in Figure 2 could be used, or the authors could include the new astronomically tuned “zachos curve” from Westerhold et al., 2020. - In panel A, could the authors highlight the amplitude modulation of the longer-term eccentricity and obliquity cycles (for instance, as done in Liebrand et al., 2017 PNAS)? - In the caption, it’s unclear whether the authors mean that the “upper ocean temperatures” at 1171 are a shallow bottom water temperature, or a (near)-surface ocean signal, without being aware of the original study.

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Lns 319-320: The main d18O decrease certainly occurs after the big drop in temperatures, but I think it also looks like the 747, 761 d18O record is also increasing between the two purple bars, which might indicate that the decoupling is more nuanced than the authors initially suggest.

Ln 329: Could you include the location of 1171 on Figure 1.

Lns 333-335: Could the authors provide a bit more mechanistic detail here about why expansion of the Antarctic ice sheet would result in a freshening of the upper water column? A greater uptake of fresh water in an ice sheet could arguably increase local salinity, not cause freshening. Further explanation might help explain the link the authors suggest.

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