Review “Southern Ocean bottom water cooling and ice sheet expansion during the middle Miocene climate transition” by Leutert et al.

Response to Referee #2

Please find below the referee’s comments in blue font and the authors’ response in black font.

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.

Reply: We thank Referee #2 for the detailed and helpful feedback. We will make a number of adjustments including the reorganization of the manuscript and the inclusion of data from the supplement into the main manuscript. More details can be found below.

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?

Reply: We will add "from different latitudes" here in the abstract. Although we compile Mg/Ca-based BWT records from a number of sites, we mainly compare our new record to the infaunal benthic foraminifer-based records from Sites 806 and 761 (Lear et al., 2010; 2015) and, of course, the Mg/Ca-based record from the site used in our study, Site 747 (Billups and Schrag, 2002). Therefore, we would consider it potentially confusing for the reader to either list the main sites we used (Sites 747, 806 and 761) or all sites Mg/Ca-based BWT data were compiled from (Sites 747, 806, 761 and 1171) so prominently in the abstract.

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?

Reply: We agree with Referee #2 that the MMCT is centered at the stepped increase in benthic $\delta^{18}O$ around 13.9–13.8 Ma (e.g., Kochhann et al., 2016), but the onset of this transitional phase was likely earlier and the end later. We will add a reference here (Super et al., 2018) that defines the MMCT from roughly 14.5 Ma to 13 Ma corresponding to our rounded values. We note, however, that there are global compilations suggesting that the

MMCT can be recorded to some extent differently in isotope records (e.g., depending on latitude) (e.g., Mudelsee et al., 2014).

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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?

Reply: Will be done.

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?

Reply: Similar to Lear et al. (2015, Pages 12–13), we intend to state with this sentence that assuming a simple linear $\delta^{18}O$-sea level relationship, as proposed by Fairbanks and Matthews (1978), would imply a drop in sea level of roughly 30–110 m. This simplified but also very illustrative example, which is based on transparent assumptions, is followed by a sentence stating a narrower and likely more realistic range for the sea level drop (~20–40 m). This latter (backstripping- and) model-based range is in excellent agreement with the estimate of Gasson et al. (2016, PNAS), who estimated a middle Miocene sea level variability of 30–36 m for a range of atmospheric CO_2 between 280 and 500 ppm in combination with a changing astronomical configuration. We will thus include Gasson et al. (2016) as an additional reference here. We will also clarify that the sea level drop estimate of ~20–40 m is based on more advanced methods (than the preceding estimate of ~30–110 m).

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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.

Reply: With low data density we mean here low temporal resolution and potential hiatuses in the middle Miocene record from Site 761 (that is only based on one hole). We agree that our formulation was not precise enough, and will thus adjust this sentence. We are not aiming for orbital-scale resolution with the clumped isotope data (see also response below), but we prefer to not provide a quantitative estimate of the temporal resolution of Site 761, as we find it difficult to quantify this resolution due to potential hiatuses at the core breaks.

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

Reply: "calcites" will be replaced by "calcite", as suggested.

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

Reply: We will add "Indian Ocean sector of" to specify that it is in the Indian sector of the Southern Ocean, as requested.

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).

Reply: Following the advice of Referee #2, we will add a sentence in the Introduction to clarify earlier on that our study does not aim at reconstructing orbital-scale variability in BWT.

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 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.

Reply: We will add the original mbsf as a column in this supplementary tables, and specify more precisely where the core expansion at Site 747 is given and has been taken from. We did not use a composite scale for our Site 747 record that is only based on one hole (Hole 747A as specified in Material and Methods).

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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.

Reply: We will revise Fig. 1 making the titles of Panels a and b more informative (annotating on the figure that 1700 m/2200 m depths are the modern 747/761 water depths, respectively) to more clearly point out that Panel a illustrates the water temperature at the approximate modern water depth of Site 747 and Panel b the water temperature at the approximate modern water depth of Site 761. Amongst other things, this figure aims at providing an overview of water temperature at the depths of the two main sites compared here (Sites 747 and 761 where both Mg/Ca- and absolute clumped isotope-based BWT estimates do exist (this study; Lear et al., 2010; Modestou et al., 2020)). We will also add Sites 806 and 1171 on the inset map with the paleogeographic reconstruction for 14 Ma. However, we prefer to not include Sites U1335/1337/1338 here, as they were only used for the age model and not for our paleoceanographic interpretation.

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.

Reply: We measured both *C. mundulus* and *C. wuellerstorfi* separately in 36 sediment samples to assess inter-species $\delta^{18}O$ and $\delta^{13}C$ offsets. We find that in the context of our limited knowledge on benthic foraminiferal $\delta^{13}C$ (and $\delta^{18}O$) offsets in the middle Miocene, the clear offset in $\delta^{13}C$ represents an interesting finding, although not at the very heart of the our paleotemperature study. We thus include this finding in the results rather than in the methods.

In terms of foraminiferal Δ_{47}, a number of studies using different approaches did not detect species-specific vital effects across a range of species (e.g., Leutert et al., 2019; Peral et al., 2018; Piasecki et al., 2019; Tripati et al., 2015; Watkins and Hunt, 2015). Specifically, the studies of Piasecki et al. (2019) and Modestou et al. (2020) included *C. mundulus* and/or *C. wuellerstorfi* in their assessment. No vital effects on Δ_{47} are observed even between species with very different stable isotope compositions, i.e. infaunal and epifaunal species. The lack of observed species-specific offsets in Δ_{47} makes biases caused by pooling measurements from different species unlikely. With this background, we feel confident that we can assume negligible vital effects on benthic foraminiferal Δ_{47}. Another line of evidence supporting negligible Δ_{47} offsets between the two different species at Site 747 is that individual Δ_{47} measurements show no discernible offsets between the two species in our data. We will add these information (parallel measurements on both species in 36 samples to assess inter-species $\delta^{18}O$ and $\delta^{13}C$ offsets; no inter-species offsets in benthic foraminiferal Δ_{47} found in previous studies) including relevant references also in the Chapter "2.3 Sample material" for clarification.

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Ln 110: Rinsed in DI water?

Reply: Yes, the test fragments were rinsed with deionized water once between each ultrasonication step and at least three times at the end of the cleaning. The sentence will be adjusted accordingly.

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.

Reply: Some of the specific details on the methodology and data treatment are only relevant for clumped isotope experts who like to reprocess our data, and would in our opinion be too detailed in the main text, given that our study does not have a technical and methodological focus but rather represents one of the first applications of clumped isotope thermometry to middle Miocene foraminifera. The clumped isotope technique was developed, described in detail and tested for such applications in earlier studies (e.g., Fernandez et al., 2017; Hu et al., 2014; Leutert et al. 2019; Meckler et al., 2014; Piasecki et al. 2019; Schmid and Bernasconi, 2010). Therefore, we would like to keep the clumped isotope methods relatively concise in the main text, and provide all methodological details needed for reprocessing our dataset in the appendix, supplementary tables and the EarthChem database. For clarification, we will add a sentence in the Section "Isotope measurements and data processing" linking Appendix A more prominently to this section.

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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.

Reply: This will be done. Similar to the clumped isotope temperature data from Site 761 (Modestou et al., 2020), we will include the recalculated Site 1171 clumped isotope temperature record (Leutert et al., 2020) using the calibration of Meinicke et al. (2020) as a supplementary table. In this context, we note that although we will update the Site 1171 upper ocean temperature record for consistency; there are no significant differences between these calibrations. Also, we note that the data from our study that is required for recalculation will be made publicly available at the EarthChem archive.

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.

Reply: For consistency and comparability, we have tied all records we use to the widely used GTS2012 timescale of Gradstein et al. (2012), similar to Steinthorsdottir et al. (2020) reviewing the state-of-the-start in Miocene climate and ocean circulation. Differences between GTS2012 and GTS2020 (Raffi et al., 2020) were found to be nonexistent to insignificant for the time period of interest in this paper. For example, the Site 1171 age model of Leutert et al. (2020), which is based on magnetostratigraphic tie points is identical on GTS2012 and GTS2020. In case of Site 747, four of five magnetostratigraphic tie points from ~16 Ma to ~12 Ma (covering the study interval) have identical GTS2012- and GTS2020-ages (C5r/C5An, C5An/C5Ar, C5AA/C5AB, C5AB/C5AC), whereas only one magnetostratigraphic tie point is very slightly (0.020 Myr) younger on the GTS2012 timescale compared to GTS2020 (C5Br/C5Cn). The age of another magnetostratigraphic tie point (C5Cn/C5Cr; age of 16.721 Ma (GTS2012)) that has been used to extend our age model beyond 15.974 Ma for two sediment samples would change slightly more (by 0.084 Myr) when adopting the GTS2020 timescale. Concretely, using GTS2020 ages for the Site 747 magnetostratigraphic tie points instead of GTS2012 would result in the following changes: the ages of 397 (out of 500 measurements) measurements would stay identical, 38 measurements would change by <0.01 Myr and 65 measurements would change by 0.01-0.02 Myr; no age would change more than 0.02 Myr. $\delta^{13}C$-based ties were only considered with a precision of 0.1 Myr. The effect of changing from GTS2012 to GTS2020 would thus be negligible for the timescales of our study and not affecting interpretation and conclusions. However, we would like to point out once more the importance of having all records consistently tied to the same timescale (which is ideally also widely used such as the GTS2012 timescale), wherever possible.

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.

Reply: We will add a sentence in the methods to point out that both species were measured in the same samples in parallel to quantify interspecies offsets in $\delta^{18}O$ and $\delta^{13}C$.

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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.

Reply: Following the referee's advice, we will include the relevant $\delta^{18}O$ and $\delta^{13}C$ values from the Site U1337 record from Tian et al. (2018) to complement the previous record in the interval younger than 16 Ma (in Fig. 2a and c). We will also include isotope data from Nathan and Leckie (2009). In addition to updating the caption of Fig. 2 with the relevant references, we will generally increase the size of the symbols in Fig. 2 for better readability.

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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.

Reply: As requested by Referee #2, we will substantially reorganize Section 3 to, amongst other things, separate methods, results and discussion, and also adjust the corresponding titles (including the title of Section 3.2). Notably, the passage from Line 190 to Line 199 (previously submitted version) will be moved in Material and Methods. Furthermore, we will separate Results and Discussion, as also suggested by Referee #1.

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.

Reply: We prefer to have Fig. S3a in the supplement, as, in our opinion, single Δ_{47} values do not represent the final result and are simply too imprecise to be interpreted in terms of paleoclimate (without averaging). An individual measurement by itself is basically meaningless with our analytical approach (small sample, Kiel device method) and we think that showing the data at individual measurement level in the main manuscript would give the reader a false impression. Note that we will visualize the Δ_{47} signal using two independent approaches (including fully propagated errors and 68% and 95% confidence intervals). Furthermore, we will generally restructure the Results and Discussion to avoid that the reader needs to consult the supplement to follow the arguments.

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.

Reply: We agree and will move the passage from Line 190 to Line 199 to Material and Methods. Also, the Results and Discussion will be restructured and separated for clarification (as described above).

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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.

Reply: A typical error for Mg/Ca-based temperatures introduced by sample reproducibility and calibration at Site 806 (Lear et al., 2015) will be added in Fig. 3.

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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?

Reply: Yes, we referred to the difference between the sites. We propose to rephrase the sentence as follows for clarification and correctness: "Since modern BWTs at Sites 747 and 761 are similar (~1–3°C; see Fig. 1), we expect middle Miocene temperature differences between Sites 747 and 761 to also be small, although the middle Miocene water depths of these sites may have been somewhat different from today."

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

Reply: We are hesitant to show the Site 747 and 806 BWT records overlaid (see Fig. 1 of this reply), due to remaining uncertainties in the absolute ages (especially at Site 806) and also for clarity. Instead, we prefer to focus on the overall pattern of temperature change without stating too strongly that the BWT evolution at Sites 747 and 806 has been the same, also regarding the timing.

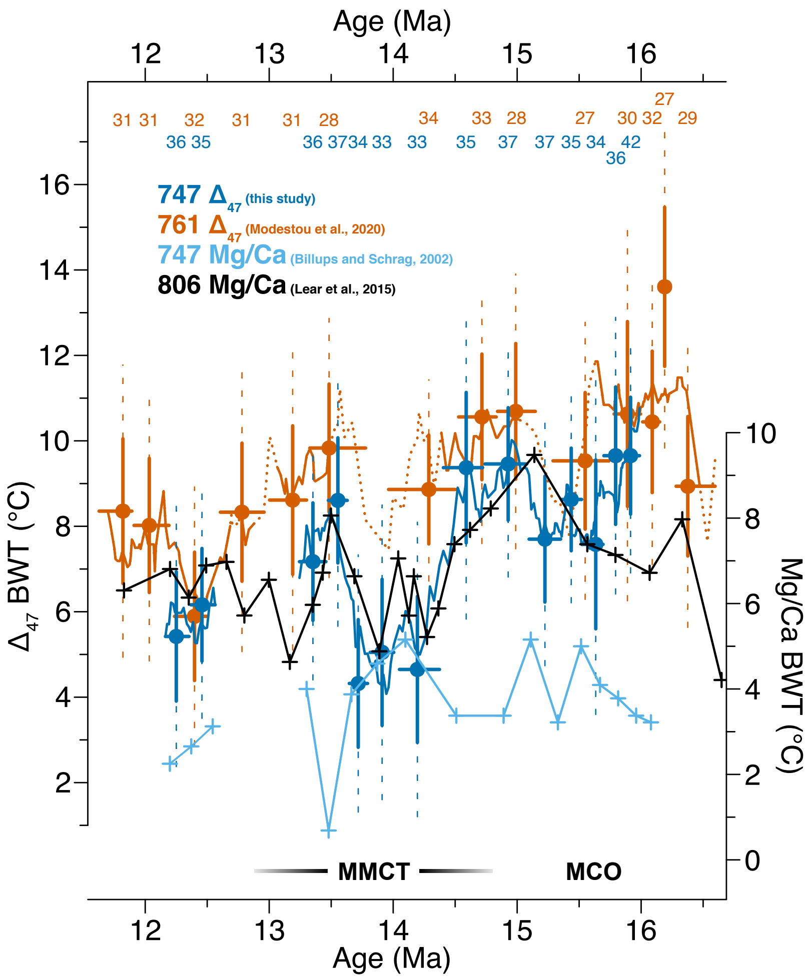

Fig. 1: Overlaid BWT records. *The curves and values of this figure are identical to those shown in Fig. 3 of the main manuscript.*

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.

Reply: We will move the Site 1171 Mg/Ca BWT curve from the supplementary figures (Fig. S4 in the previously submitted version) to Fig. 3 in the main manuscript to avoid the need to switch between main text and supplement here, and we will adjust the corresponding figure captions. We prefer to not move the Site 761 Mg/Ca BWTs to the main manuscript as these BWT estimates are in essence represented by the clumped isotope BWT estimates from the same site shown in Fig. 3; a detailed comparison between Mg/Ca- and Δ_{47}-based BWT estimates for Site 761 has already been carried out by Modestou et al. (2020), who found good agreement between the two paleothermometers at that study site. Nevertheless, we intend to still show the comparison between middle Miocene Mg/Ca- and Δ_{47}-based BWTs (Lear et al., 2010; Modestou et al., 2020) in a supplementary figure. In addition, this figure will also illustrate the (very small) effect of using the recent Meinicke et al. (2020) Δ_{47}-temperature equation instead of the recalculated (Bernasconi et al., 2018) Kele et al. (2015) calibration that has originally been applied by Modestou et al. (2020). We further note that we will integrate benthic $\delta^{18}O$ from Sites 747 and 761 into Fig. 3 for better overview and temporal orientation when looking at the BWT curves.

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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.

Reply: We did not include Fig. S5 (of the previously submitted version) in the main manuscript, as we see these calculations as a sensitivity test rather than reliable quantitative calculations of bottom water carbonate ion saturation changes, and would like to avoid misunderstandings in this regard. Our goal was to examine the magnitude of saturation change required to bring the Δ_{47} and Mg/Ca BWT curves together. At present, we cannot fully exclude a dissolution effect on the Mg/Ca and/or the Δ_{47} signatures of benthic foraminiferal calcite (as pointed out in the main text), largely limiting the practical use of combining benthic foraminiferal Δ_{47} and Mg/Ca as a way to derive quantitative estimates of carbonate ion saturation changes. Furthermore, we note that the Site 747 Mg/Ca record of Billups and Schrag (2002) has been sampled in low temporal resolution and is based on a comparably small number of foraminiferal tests in the relevant interval, decreasing its representativeness of past environmental conditions (e.g., due to potential aliasing) and also the informative value of the calculated changes in bottom water carbonate ion saturation. In summary, we think that our calculations displayed in Fig. S5 illustrate that changes in bottom water carbonate ion saturation within a reasonable range could actually explain (or at least contribute to) the observed divergences between Mg/Ca- and Δ_{47}-based BWTs, but do not feel confident enough to put these in the main manuscript.

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.

Reply: We will remove the sentence.

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

Reply: We agree and will move this information to the methods.

Ln 284: "complicating" should be "complicated".

Reply: We will remove "complicating", which is not essential here.

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.

Reply: It is true that we mention absolute BWTs in the abstract/introduction but not absolute bottom water $\delta^{18}O$ (and even less absolute sea level) estimates. We only state that systematic biases in bottom water $\delta^{18}O$ estimates may be smaller when using the clumped isotope technique to constrain the temperature component for the calculation of bottom water $\delta^{18}O$ compared to other approaches such as the Mg/Ca technique. In comparison to estimating absolute BWTs, an estimation of a bottom water $\delta^{18}O$ signature that is representative at larger scales (e.g., as a basis to derive global ice volume/sea level changes) is hampered by additional uncertainties including regional imprints of water masses with different salinities, a possible pH effect on benthic foraminiferal $\delta^{18}O$ and additional uncertainties in the equation linking BWT, benthic foraminiferal $\delta^{18}O$ and bottom water $\delta^{18}O$. Estimating past global ice volume/sea level changes would require even more assumptions such as the isotopic composition of the ice sheet, which may have been different from today (as pointed out by the reviewer). Also, we can at this point not be certain to which extent Site 747 located in the Southern Ocean is representative at global scales (or also influenced by Southern Ocean-specific influences). Precise quantitative estimates of the changes in global ice volume/sea level are thus considered premature. We will extend our discussion of the reconstructed bottom water $\delta^{18}O$ values adding more information on alternative potential influences such as salinity, pH and the $\delta^{18}O$ composition of the ice sheet.

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?

Reply: For clarity, the corresponding sentence will be rephrased. In addition, we will include the equation of Marchitto et al. (2014) in the main manuscript (in Material and Methods) and the equations of Lynch-Stieglitz et al. (1999) and Bemis et al. (1998) in the caption of the supplementary figure, following the referee's advice.

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.

Reply: We will include the amplitude modulations of the 40 kyr obliquity and the 110 kyr eccentricity (Fig. 5a) to put more focus on the influence of the longer-term orbital cycles that are relevant at our timescales. In addition, we will slightly rephrase the sentence addressing the orbital configuration, but prefer to keep our formulation referring to the observation/interpretation of Holbourn et al. (2005) close to the original wording:

"The main $\delta^{18}O$ increase after 13.9 Myr ago occurred during a period when eccentricity declined and amplitude variations in obliquity decreased (Fig. 3). This orbital configuration would have resulted in an extended period of low seasonal contrast over Antarctica, inhibiting summer melting and favouring ice-sheet expansion.” (Holbourn et al., 2005)

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

Reply: We will increase the font sizes, optimize the labels and make the figure more compact, but prefer to keep the overall format/width of the figure to be able to fit in one column since we would like to put emphasis on the variability of our reconstructed temperatures (rather than on high-resolution records).

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.

Reply: We had previously thought about including a higher-resolution benthic stratigraphy as a reference, but prefer to focus on the benthic $\delta^{18}O$ stratigraphies that were measured at the same sites as the available clumped isotope BWT records (Sites 747 and 761). These $\delta^{18}O$ curves are directly comparable to the BWT curves and thus best suitable to assess the (relative) timing of the events on the timescales relevant for this study.

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)?

Reply: We will add the 40 kyr-filtered obliquity and the 110 kyr-filtered eccentricity and highlight their respective amplitude modulations. In an additional supplementary figure, we will compare these values to the unfiltered eccentricity and obliquity timeseries.

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.

Reply: Site 1171 upper ocean temperatures were derived from the planktic foraminiferal species *G. bulloides* that is assumed to dwell around 200 m water depth in the Southern Ocean (Vázquez Riveiros et al., 2016). We will add a sentence with this information in the caption of Fig. 5.

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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.

Reply: We agree with the observation of the referee that $\delta^{18}O$ at Sites 747 and 761 also appears to be broadly increasing between the two purple bars. We will point out in the discussion that the decrease in temperatures indeed coincides with a slow increase in $\delta^{18}O$. However, the temperature decrease derived from Δ_{47} is much more pronounced, suggesting that there is a counteracting influence on $\delta^{18}O$, likely a decrease in bottom water $\delta^{18}O$. This could be ice retreat (i.e., a decoupling between ice volume and temperature), but also a change in salinity. We will expand the discussion of our results in this regard.

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

Reply: Will be done.

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.

Reply: Our interpretation of a freshening in certain parts of the Southern Ocean relates to earlier work suggesting a redistribution of fresh water within polar regions in times of increased ice volume and a possible freshening due to increased meltwater input from the growing land-based Antarctic ice sheet and/or sea ice exported equatorward away from the region of sea ice formation (e.g., Adkins et al., 2002; Sigman et al., 2004; Crampton et al., 2016). We will add this information including the relevant references.