Response to Reviewer 1

Responses are in red

The authors present a TEX86-based reconstruction of sea surface temperatures (SSTs) for the Queensland Plateau (NE-Australia, ODP Site 811) for the period between 11 and 2 Ma. The dataset presented is a synthesis of new and published data by the authors (Petrick et al., 2023, Sci. Repts.). The described SSTs clearly document the transient cooling in the late Miocene between 7 and 5 Ma known as Late Miocene Cooling (LMC). However, the magnitude of the transient cooling documented is unusually large at ~4 °C. The authors discuss in detail a possible mechanistic relationship between LMC cooling and subsequent reef drowning known as "Pliocene Reef Gap". All biochemical analytical procedures are described in detail and the quality of the data is discussed; technically speaking, the data set presented can be considered excellent.

I want to thank the reviewer for their comments, for which we will provide more detailed responses below. We want to preface this reply that due to the considerable amount of rearrangements of text, we generally recommend you use the clean version of the article rather than the track changes version for assessment. We have provided detailed information on all changes below.

Science

Chapter 1 (Introduction) provides an overview of global reef patterns in the Miocene and Pliocene on the basis of current literature knowledge. However, the structure of the chapter is not stringent and suffers from duplication and repetition.

We appreciate this general information on the readability of the introduction. To alleviate these concerns, we have condensed the text body and streamlined the whole manuscript to avoid repetitions. Hopefully, this makes the article clearer to read.

Also, in many sentences the content is difficult to follow or the relationships between sentences are illogical (e.g. "Therefore", line 39). More problematic, however, is the fact that the corresponding author does not always summarise cited work adequately. For this reason, I checked some of the cited publications again. For example, it is not true that C4 grasses spread worldwide during the LMC (the C3/C4 transition was at ~40° latitude in both hemispheres, and the C4 grasses never reached western Europe and the Mediterranean region; Cerling et al., 1997, Nature)

We were referring to the statement in Herbert et al. (2016) about the expansion of the C4 grasses. After re-reading the statement, we agree that it may have been phrased in a way that could have made it into a more generalized statement, which we never intended. Hence, we have revised this paragraph accordingly to emphasize that the statement only refers to the tropical expansion C4 grasses (Line 30-36). We also included several new citations, including (Huang et al., 2007; Strömberg & Strömberg, 2011; Wen et al., 2023), to support better and contextualize this argument. We also reviewed all citations to make sure they made the points we were trying to make.

Another example is the interesting Brachert et al. (2020) paper, where I found no discussion of a link between biomineralization performance and sea level changes (line 74), nor modern reef corals to be "hypercalcified" (line 70) or the coral reef habitat to have changed (line 295).

In Brachert et al. (2020), the authors state: “The Neogene corals generally display hypo-calcification, whereas optimal calcification or even hyper-calcification virtually do not occur …” (first two lines of section 3.5). In contrast, Fig. 10 and 11 show that modern corals show a wide range but are often characterized by optimal calcification or even hyper-calcification, which is absent in Neogene corals. This is what we wanted to stress here, but we agree that the wording we used might have been partly misleading. However, after reading some of the other comments, we have decided to deemphasize the adaptation argument as it does not fit the edited paper as well.

Furthermore, in the abstract (lines 9-10) the author claims that little is known about changes in “aquatic ecosystems” relative to terrestrial ecosystems – this statement simply denies the incredible wealth of knowledge that exists about ancient oceanic ecosystems.

We intended to address the knowledge about the impact of Late Miocene cooling on shallow-water carbonate ecosystems and apologize for the ambiguous wording. In general, the revised version hopefully provides a clearer definition of ecosystems, processes, and the period we focus on. We tried to clarify that we are looking at the specific impact on Coral reefs and shifts in these (See lines 9-10 and 74-83).

The results and discussion chapters (chapters 2 and 3) lack any description of lithologies and depositional sequences encountered at site 811 for the period discussed. As the authors are using a new age model, the lithological information is not readily available from the literature.

Thank you, we fully understand this concern. To alleviate this problem, we now provide additional information on the age model of Petrick et al. (2023). To achieve this, we added a section where we present the findings of the before mentioned work, including the age model improvement (see section 3.1 and 4.1). We added a figure showing the lithological changes discussed in the preceding publication to the main article (see Figure 2). However, as stated in Line 225, we do not think that the LMC was the only controlling factor defining the structure and extent of reefs in the vicinity of ODP Site 811. Instead, we focused on the more wide-scale losses during this time.

For an assessment of the TEX86 results, however, it should rather be an integral part of this publication. As is, chapter 3.2 of the results presents a SST reconstruction for the periods before, during and after the LMC. However, this text is rather confusing and the results remain irreproducible, as they lack clear definitions of the time intervals used and the number of measurements used for calculating average SSTs. I suggest the authors insert a table listing all of the information needed.

We added the table with the two different definitions of the LMC. We also simplified the paragraph before to clarify what age windows we used for the various reconstructions and why we chose them (See section 3.3). For the figures, we used the definition of LMC by Herbert et al. (2016), which is 7.0-5.4 Ma. Please see Figure 4, which has now also been adapted to reflect these aspects better in order to alleviate the concerns of the reviewer.

Adding an error bar in the figures (2, 4) may also help the reader in evaluating the data.

The calibration error for the TEX_{86}^{H} temperature reconstruction of 2.5 degrees (Kim et al., 2010) was added in Figure 4 and into the captions.

In the discussion chapter (chapter 4), the authors do not present any discussion of their findings related to lithological data from ODP site 811 nor any other site located nearby, e.g., ODP sites 824 and 825.

We have added a new figure for ODP Site 811 (Figure 2). As can be seen, the unit boundaries do not correspond to the LMC, which is the focus of this study. We also added section 3.1 were we discuss the lithologic changes in detail. The other sites nearby (ODP Site 824 and 825) could not be included in the study for the following reasons: The top of ODP Site 825 is too old (12 Ma) to address changes in the site during the LMC (7.0-5.4), and we did not do any temperature reconstructions for this site.

ODP site 824 has only two age datums between 2-12 Ma, i.e., at 5 and 11 Ma (Davies & Mckenzie, 1990), respectively. Furthermore, the site is characterized by non-continuous sedimentation and gravity flow deposits. Finally, we did not do any SST reconstructions on this site, as the noted gravity flow deposits make it a poor candidate for time-continuous geochemical work. Therefore, we consider it inappropriate to include these records given, especially the age uncertainties of Site 824. Finally, as shown in Petrick et al. (2023) and repeated here (See Lines 225-229), we interpret the collapse and drowning of the local reef to have occurred between 11-8 Ma and having been initiated by warm temperatures and local changes. Therefore, in this article, we focus more on the large-scale changes in the Central Pacific during the LMC.

Rather, the authors step directly into a more global discussion with a Mg/Ca dataset from the “nearby” northern Indian Ocean. I agree that the similarity of the two datasets is impressive (Fig. 4). However, given the fact that SSTs ~31 °C before and after the LMC were critically high for coral reef growth (and 3-4 °C warmer than modern SSTs; line 154) and SSTs of the LMC were within the classical reef window range (and modern SSTs), I had expected a discussion on the role of a hot ocean for reef health, both, in the geological past and recent future. This deficit leaves the reader to speculate, whether the Pliocene Reef Gap might be due to the global warming following the LMC?

Given that coral reefs are lost across the Indo-Pacific during the LMC, we do not find the comparisons inappropriate or too far away. However, we hope that the new changes to the article make this point clearer (Section 4.5). We also strengthen the arguments that we do not assume that all reef loss during the LMC resulted from SST cooling (See lines 292-295) – these arguments may not have been presented clearly enough in the original article. To emphasize again, we are of the firm opinion that a combination of forcings, including reduction of the latitudinal extent of corals, increased ocean current flow, and changes in rainfall patterns, caused numerous stressors on the coral reefs across the indo-pacific (See lines 290-315).

However, the Impacts of high SST on the coral reef in the area of ODP Site 811 are discussed in detail in Petrick et al. (2023). We, nevertheless, realize the need to add a more detailed explanation here as well. We added section 4.1, where we summarised and re-iterate the impact of the warm SSTs on the reef based on our previous findings. To briefly summarize, it can be stated that SSTs between 8-11 Ma were a stressor for the coral reefs, leading to lower coral extension and, therefore, ultimately, reef accretion rates. Because of these low accretion rates, the coral reefs in the area could not keep up with a relative sea-level increase caused by a pulse in subsidence.

Here, we also want to expressly thank the reviewer for making an interesting suggestion that post-LMC warming could have caused reef loss. The age of the loss of many of the reefs in this region is admittedly not clear from the geological record. However, in the better-dated reefs (NW Shelf of Australia, Marion Plateau), the final drowning of the reefs seems to occur between 7-6 Ma (Bashah et al., 2024; Ehrenberg et al., 2006; Rosleff-Soerensen et al., 2012, 2016). Furthermore, as we point out in Lines 260-264, the timing and amount of the post-LMC warming is not synchronous. This means that the numerous changes caused by the SST cooling during the LMC are a more likely candidate to explain the drowning of the reefs compared to the later warming.

Technical aspects

Many sentences are linguistically imprecise and the use of tenses is not always logical (historical facts and developments should be presented in past tense, for example). Although being a rather general bad habit in many scientific publications, I mention wordings like "… the end of the LMC at **our** site…" as inappropriate – it must read as "… the end of the LMC at ODP site 811…" or "… at the site studied…". Spaces are used quite liberally in the text (especially when a citation is given at the end of a sentence). The formatting of the headings (line 213) or the bibliography is "free-style". Overall, I think the manuscript has the character of a draft and needs to be strongly revised or re-written (and shortened).

We are sorry about the shortcomings in proper phrasing and have done our utmost to correct this.

Recommendation

The merit of this publication is the validation of the TEX86 proxy as a robust method for SST reconstructions of shallow-water carbonates. This new approach will allow for a better, direct understanding of the temperature regimes behind shallow-water carbonate deposits and contribute to the ongoing debate on ancient cool, warm, and hot carbonate systems. The publication must be improved technically, however, which requires a major revision.

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Response to Reveiwer 2

Review of “Impact of the Late Miocene Cooling on the loss of coral reefs in the Central Indo-Pacific” By generating the TEX86-based sea surface temperature (SST) record at the ODP Site 811 from the coral sea between ~ 2-6 Ma, combined with the published data between ~ 6-11 Ma from Petrick et al., (2023), the authors explored the impact of the late Miocene cooling on the regional coral reef loss. Compared to previously published Uk37-based SST records in the region, SST reconstructed in this study documented an unusually strong cooling in the Central Indo-Pacific, showing ~ 4 - 5°C drop from ~ 7 to 6 Ma, which is consistent with the cooling shown in a Mg/Ca-based SST record from the northern Indian Ocean. The authors made efforts to discuss how the temperature change (rapid cooling) could potentially act as a final stressor causing the collapse of coral reef and the known “Pliocene Reef Gap”. I think the dataset presented here is neat and compelling, contributing to the understanding of the temperature changes impacts on shallow water carbonate systems.

However, this manuscript is poorly written and does not read smoothly. It appears to be a casual draft that lacks proper polishing and organization. Additionally, the text suffers from a lack of clear structure, specifically for the introduction and discussion sections (see major comments). In terms of science, the manuscript does not cite enough related literature and definitely needs a deeper and re-organized discussion section. The discussion section in this manuscript fails to provide sufficient discussion related to the presented data and lacks clear explanations when comparing to other studies (see major comments). Overall, I believe this manuscript requires major revisions before it is ready for publication. Here I attached my major and specific comments, hoping to help the authors to revise.

The anonymous reviewer is gratefully acknowledged for their insightful comments. We want to apologize for the organizational issues and consider all comments. Please see the responses below.

Major comments:

Introduction:

1) The introduction is poorly structured, and it lacks leading or summary sentences for paragraphs, resulting in unclear logical connections between them. The introduction is a wired blend of LMC, site background, and the coral reef gap issue. It would be better to move the site background to a later section of the article. To improve clarity and conciseness, I suggest considering combining related content and ensuring a logical flow throughout the introduction.

The site background, though it is of high importance for this contribution due to the scarcity of GDGT-based temperature reconstructions from reefal sites, has been moved to the site description in the results section (See section 3.1). We have now focused on the potential causal relation between the changes associated across the central Indo-Pacific during the LMC and the region's coral reef gap, which is the main focus of our contribution.

2) The authors insufficiently cite other people's work in this section; there are several places that require additional references to support the statements (see the specific comments). 2. I think it is worth to add a section to introduce the oceanographic setting for Site 811 in the main context to offer basic information like the location, water depth, SST, salinity and regional currents in the modern ocean, as well as information about site migration and coral reef history in this region (also put the related repetitive materials from the "Introduction" and "Discussion" sections to this section).
Reviewers 1 and 2 asked for more details on the site, so we have added a section (See section 3.1) to our manuscript. We also added a section on the history of coal in the Miocene (See section 1.3). We have also reviewed the citations and agree that some seminal works were not adequately referenced in the introduction. We have, therefore, added additional citations as appropriate in the revised version.

3. Discussion: This section is poorly structured and lacks organization. It fails to provide sufficient discussion related to the presented data and lacks clear explanations when comparing to other studies. 1) For section 4.1, the authors need to put more effort into explaining the driving mechanisms behind the cooling observed at other sites, as documented in relevant literature, instead of solely relying on comparisons of data and proxies. It's quite confusing when they compare the SST at Site 811 to the SST stack from Liu et al., (2022) without specifying the site locations included in the stack and the proxies used.

We reorganized this section. The sites of the Liu et al. (2022) SST-stack are shown in Figure 1, but we provide more detail to improve clarity, including links in the figures (Fig. 5,6). We will also expand our explanation of the previously proposed driving mechanisms behind the LMC, though we think that changes in CO_2 are the most likely explanation based on the work of numerous authors (e.g., Herbert et al., 2016; Holbourn et al., 2018; Martinot et al., 2022) We explain why we think this (Line 250-255) We, however, explain the alternative hypothesis, such as gateway closures see line 45-51.

Similarly, the alignment of SST data at Site 811 with the model from Burls et al. (2021) and the absence of an anomaly in cooling, as noted by Martinot et al., (2022) when compared to the SST record at Site U1443, lack clear explanation.

We removed the links to the Burls et al. (2021) and only compared them to the model shown in the Martinot et al. (2022) paper. Overall, the point of this paper is not to evaluate model reconstructions of the LMC (See Lines 248-254). Therefore, we have emphasized that while our record does fit the model prediction of Martinot et al. (2022), the complexity of the site means that it cannot be compared to open ocean data/model predictions without any further qualifications and considerations.

Moreover, only SST data at Site 811 exhibit full recovery after 5 Ma (unlike U1443, which shows a similar cooling trend between 9-5 Ma but lacks data after 5 Ma). What are the potential mechanisms behind this? Is it related to the proxy used or is it a local signal?

Several records have been published (see Herbert et al., 2016), and the figure attached below emphasizes the breadth of SST records. Furthermore, there are at least four tropical sites across the equatorial to subtropical Indo-Pacific that further confirm this SST recovery: Both the Mg/Ca record from U1448 from the Andaman Sea (Jöhnck et al., 2020) and most individual records from the WPWP-stack (Liu et al., 2022; Zhang et al., 2014) show a recovery between 5-3 Ma. While the exact scale of this recovery varies for different sites, SSTs increased between 5-4 Ma at all these sites (Jöhnck et al., 2020; Liu et al., 2022). The cause of the recovery has not yet been identified, but changes in gateways or $p\text{CO}_2$ have been suggested (Jöhnck et al., 2020). However, we do note (see Lines 260-264) that the recovery is not homogenous across the LMC. We agree that this may have been unclear in the original manuscript, hence why we now note this in more detail and have also added Site U1448 and ODP site 1146 SST data to Figure 5.

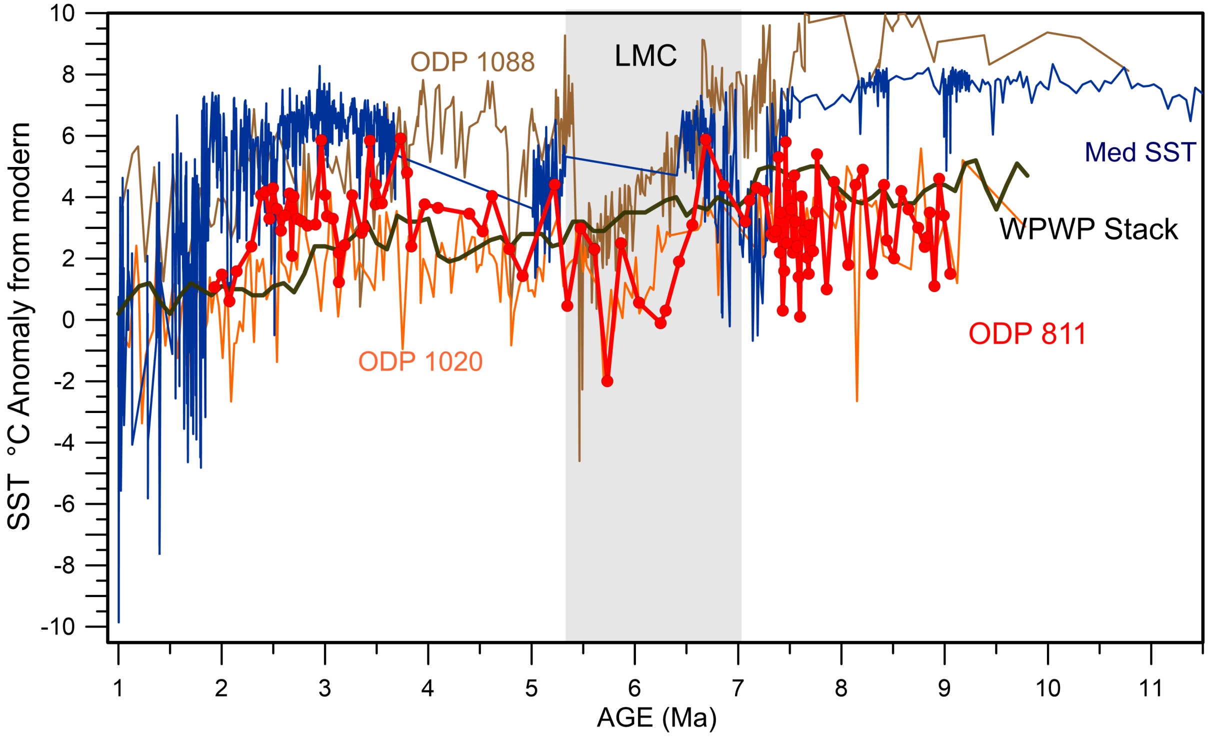


Figure (for reply to reviewer only): This figure collates records that show a recovery in SST similar to ODP site 811 ranging from the higher southern latitudes (ODP Site 1088) to the intermediate norhtern latitudes (Med SST). Besides the ODP Site 811 record (red), the WPWP-stack is based on TEX_{86} values. A composite Mediterranean record (dark blue) (Herbert et al., 2016), ODP site 1088 from the South Atlantic (brown) (Herbert et al., 2016), and ODP 1021 NE Pacific (orange) (LaRiviere et al., 2012). The latter three SST records are based on U^{K}_{37}'.

Furthermore, compared to SST at U1443, SST at Site 811 generally indicates lower values during cold intervals but agrees with high-temperature peaks, a point that has not been discussed.

Some caution should be used here because both TEX_{86} and Mg/Ca-based SST-proxies have individual errors, and choices on proxy calibration at either site could alter the relationship. For instance, although located in close proximity to each other, U1448 and U1443 have a completely different SST relationship with ODP site 811, even though both are based on the Mg/Ca-proxy (See the new Figure 5). This may be due to variations in proxy calibrations, including assumptions on pH, carbonate ion concentration and carbonate dissolution, and assumptions regarding secular trends in the Mg/Ca of surface waters (Jöhnck et al., 2020; Martinot et al., 2022). The most likely explanation for the temperature relationship of the lower values during cold intervals is that ODP site 811 was further south during this time (25-20 ° southern latitude). Therefore, cold Southern Ocean-sourced intermediate waters could have affected the site (Isern et al., 1993), especially at the height of the LMC when currents shifted northward, as is shown in the attached figure taken from Petrick et al. (2023). Site U1443 remained in the tropics during this time, with little Southern Ocean influence (Martinot et al., 2022). However, because of the uncertainty of the error decussed above, we decided that this relationship is interesting but not robust enough for a more in-depth discussion.

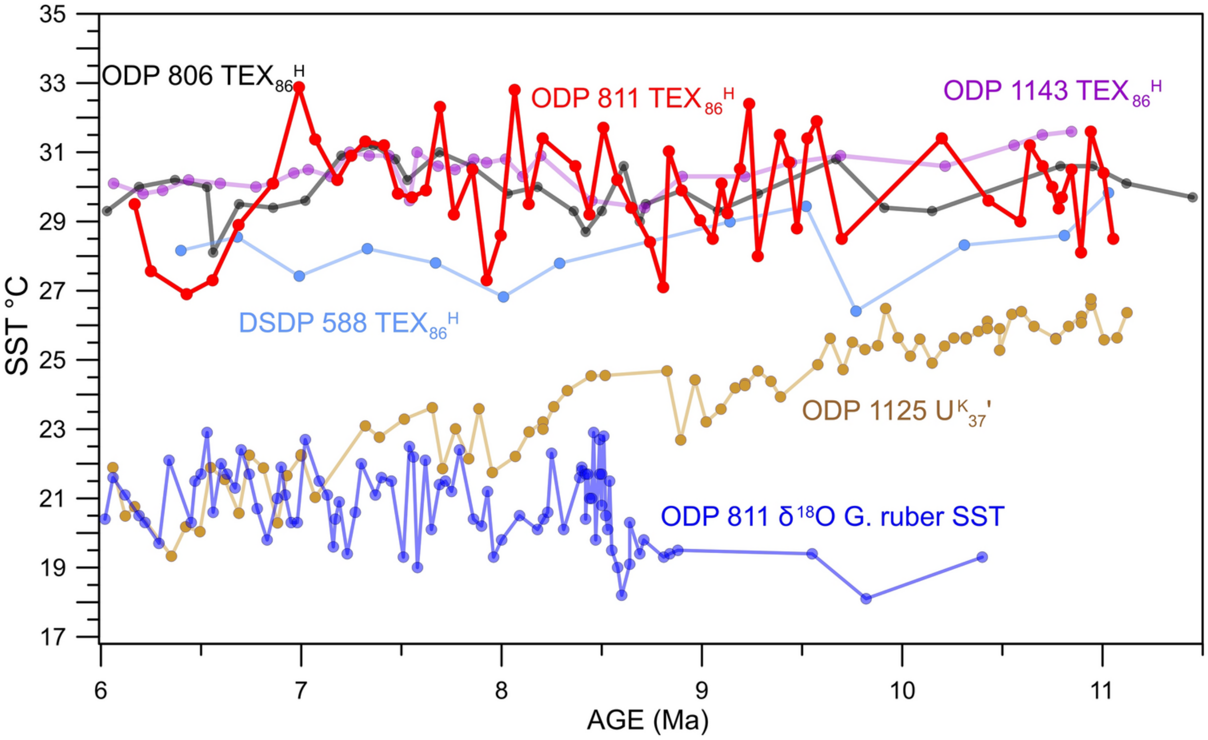


Figure from Petrick et al. (2023): A comparison of different SST records from the western Pacific. The new 811 TEX_{86}^{H} records from ODP site 811 (red) and the old δ^{18}O record (dark blue)(Isern et al., 1996)are compared to other Sites from the western Pacific (Fig. 1). These include ODP site 806 from the WPWP to the north of the Coral Sea (black), ODP site 1143 from the southern South China Sea (purple) (Zhang et al., 2014), and DSDP site 588 from the Lord Howe Rise at the southern limit of the Coral Sea (light blue) (Auderset et al., 2022) all of which are TEX_{86}^{H} records. ODP site 1125 east of New Zealand south of the Tasman front (gold)(Herbert et al., 2016) is a U^{K}_{37}' record.

To improve this section, I suggest: 1) Clearly state the related stacks/records (including the proxy and site information) from other studies at the outset when comparing data and refer to Figure 1 when necessary. 2) Rewrite the second paragraph, adding more details on how the models can support the SST data and its relation to CO2 decrease.

We added references to Figure 1 to clarify this point (See Lines 122-125). However, when looking over the data, we do not think it is appropriate to talk about model SST relationships based on the data presented here. We base this on the complex location of the site. We also note that the intent of conducting the present study at ODP Site 811 was not to understand model data mismatches but to ascertain the impact of SST change on the coral reefs – although that is inarguable to be of high topical importance. Therefore, besides ascertaining that our data roughly fits the expected decrease in temperature due to atmospheric pCO_2 changes as presented in Martinot et al. (2022), (See Lines 248-251) we do not feel confident to make further-reaching statements on the Late Miocene oceanic conditions based on our results. This would need to be tackled by a more exhaustive study focussing specifically on model-data intercomparison during the LCM.

2) For section 4.2, this section primarily delves into the historical context of coral reef loss in the region since the Miocene. However, the authors fail to link other studies to the data in their study until the end of this section, with only the last two sentences referencing their own results. Much of this background material should be condensed and summarized in the discussion section, with a closer connection to their own data throughout the text. Furthermore, the presence of many illogical transitions (e.g., 'however') disrupts the coherence of the section.

This section has been eliminated, and the key parts have been moved to the discussion section (See section 1.3 and 1.4). Thereby, we will also make every effort to remove the noted “illogical” and “superfluous” words or phrases.

3) For section 4.3, this section includes seven paragraphs that are poorly organized and somewhat chaotic. For instance, the first three paragraphs need to be merged into one, and the sixth paragraph, which also discusses the impact of cooling to the loss of the coral reef, should be moved to the beginning too. After that, the discussion should flow into other stressors related to the cooling (e.g., changes in currents and terrigenous input). In the third paragraph, It is not clear how the major SST drop became the final trigger for coral reef collapse in terms of the coral's ecological and physical characteristics, which requires more discussion. In the fourth paragraph, additional explanation is needed on how changes in terrigenous input caused by cooling would impact the loss of coral reefs.

The organization and conciseness of the section will be improved following reviewer recommendations, emphasizing on the following aspects:
Modern studies show that many species of coral exhibit lower carbonate extension rates at lower temperatures (Lough & Barnes, 2000; Lough & Cantin, 2014). Such a cooling effect may be responsible for the global latitudinal reduction of the coral reef belt during the end of the Late Miocene (Perrin & Kiessling, 2012). Sites to the south of ODP Site 811 – such as the Marion Plateau, which today is about 4 °C cooler than the central Coral Sea – would have experienced temperatures below the ideal coral growth window. As we pointed out in the manuscript, the climatic impacts of the LMC go beyond just lower SST. For instance, there was a southward displacement of the Monsoon belt, which would have increased the rainfall in Indonesia and Australia (Holbourn et al., 2018; Jöhnck et al., 2020). This likely would have led to an increase in detrimental siliciclastic input into the reef systems in this area. Evidence for increased primary planktonic productivity in the Pacific has been proposed by Holbourn et al. (2018). Both of these factors have been shown to be major stressors on coral reefs (Hallock & Schlager, 1986; Hinestrosa et al., 2016; Sanborn et al., 2024; Webster et al., 2018; Wiedenmann et al., 2012). A potential reason that these impacts have not been recognized previously is that coral reef reconstructions often rely on benthic d^{18}O reconstructions to investigate climatic changes (Harrison et al., 2023). Therefore, the discussion we intend to raise is that the full impact of cooling on the reef gap has never really been evaluated. We Added these aspects and clarified this point in the manuscript, and are now present in Lines 291-315 in the revised version

4. Data description: The authors' description of their data is inconsistent throughout the text. The SST record exhibits a temperature drop of around 4-5°C and the authors mention it as a stronger cooling compared other studies (Section 4.1). However, they also state that the average temperature drop is around 2°C (and consistent with other records) by using confusing average calculation (Section 3.2). I suggest showing the error margins of the temperature reconstruction and include a smoothed line of the data to help identify the absolute SST drop.

We added a table (Table 1) to clarify both the windows used to average and why we averaged the record and did not just use the 4-5 °C SST drop. This is mainly because of the large sample size precluded high-resolution sampling. Therefore, we can not tell if our cold SSTs are a result of a period of lower SST or just a cold snap in the record. This can also be seen in the IODP site U1445 Mg/Ca record, where the change from maximum SSTs pre-LMC to the coldest SST is about 5-7 °C as well. Also, see the new running average we added to Figure 4.

SST-averaging was conducted to minimize the effects of cold or warm spikes in raw data, potentially biasing the record. Therefore, we used both the periods described in Martinot et al. (2022) and Herbert et al. (2016) and a window based on our records to show that the cooling was compatible, regardless of the averaging procedures applied. We selected all points within the windows described and then calculated the average SSTs in that window. We then subtracted the average LMC temperatures from pre-LMC temperatures to understand the SST change during the LMC. SST-averaging yielded an average temperature drop of 2 °C. For visual improvement of the figure, we added error bars of 2.5 °C taken from the literature to Figure 4, as was suggested by reviewer 1, and add a smoothed line to the data points again to Figure 4. We have also removed references to stronger cooling and stuck with 2 °C cooling.

5.

Figures: The LMC time interval boundary is inconsistent in all their figures. The blueshaded LMC in figure 2 covers a different time interval than that in figures 4 and 5, and the gray bar indicating LMC in figure 4 differs from that in figure 5. I suggest combining figures 2, 4, and 5. Presenting all the records on the same time scale will facilitate a better evaluation of the data and related events.

The revised version consistently applies the traditional definition of the duration of the LMC, given by Herbert et al. (2016) as 7-5.4 Ma, to the figures. However, while we appreciate the idea presented by the reviewer, given the number of records, the different proxies, and the calibrations, we think the figures need to remain separate to avoid oversaturating the prospective reader with various datasets of different origins.

Specific comments:

1. Lines 16-18: This sentence should be excluded from the abstract but put it in introduction instead since the “reef gap” has been explained in the abstract already.

We did this

2. The first part of abstract can be more concise, and it should address more about the indication from the data/results of this study in the second half of the abstract.

We did this

3. Lines 30: Using Herbert et al., (2016) as the main and only ref. in the first paragraph to introduce LMC is not enough, need more recent refs.

We did this and included (Herbert et al., 2016; Holbourn et al., 2018; Martinot et al., 2022; Tanner et al., 2020; Wen et al., 2023) (See Lines 25-26)

4. Lines 37: Need to add ref for benthic d13C shift associated with biogenic bloom.

We did this by adding citations for this event (Grant & Dickens, 2002; Pillot et al., 2023)

5. Line 59-60: This sentence should be combined with the text later and it does not make sense as a leading sentence for this paragraph.

We did this (Moved to Section 1.3)

6. Line 60-62: Too many “however” transitions are used in the whole article and several instances do not align with the logical flow of the text (e.g., line 60).

We did this

7. Lines 67-68: Need to add ref when stating that LMC was muted in the benthic d18O record

We did this (See Line 27)

8. Lines 82-83: Need to add ref for the “records produced”.

We expanded this section with citations (See section 1.4)

9. Lines 112: Not clear. What is the standard error related to? how about error of calculated temperature?

   We made it clear that the errors are based on the Kim et al. (2010) (2.5 C +/-) paper and describe in more detail what this is. Also, based on the feedback from reviewer 1 we added the error bar to Figure 4.

10. Lines 130: Not clear. What does it mean by “we used the 2/3 index to ensure…..”

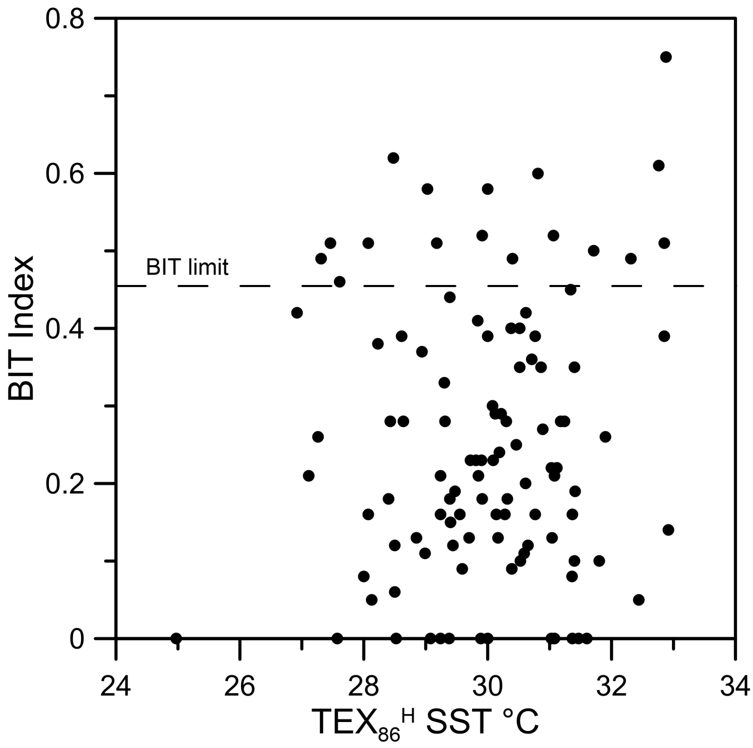
This is about the use of the 2/3-index, meaning the ratio of GDGTs with 2 versus 3 rings, in order to check the GDGT-based SST reconstruction for environmental influences other than temperature. The 2/3-index is used to evaluate the depth of production and associated water temperature of archaeal GDGTs, which impacts on the TEX_{86} (Rattanasriampaipong et al., 2022). Unlike the other accuracy tests for molecular SST-proxies, it does not have a defined cut-off published and is only used to eliminate data where the 2/3-ratio is exceptionally high. In our study, the 2/3-index was very low, suggesting mainly surface production of archaeal GDGTs (Rattanasriampaipong et al., 2022) We expand on this in the paper (see Lines 177-180).

Lines 132: Is it “supplemental data 1” (line 125) or “supplement 1”?

We intended to refer to “see the supplemental data in Petrick et al. (2023).”

Lines 137-138: I would love to see a covariance plot between SST and BIT. BIT index is high for the dataset and the cutoff the authors using in Figure 3c is about 0.5 (but not exactly at 0.5), which is weird. Based on the similarity between the SST at site 811 and site U1443, I doubt there is serious terrigenous input impact on the samples before ~ 5 Ma but not sure about the younger part. However, after seeing the high BIT index values (most of them higher than 0.2), I think it would be helpful if the authors can offer some other evidence to support that there is little terrigenous derived source influence at site 811 during the Miocene (e.g., information from other studies like organic carbon isotopes) or using lower cutoff in the discussion (if removing those samples still doesn’t affect the major trends or conclusions of the paper).

The presently accepted cut-off for the BIT should be 0.45, which will be corrected in the figure. A covariance plot (see below), for space limitations within the text body, has been added as a supplementary figure. We also include a figure showing % carbonate as part of the new lithological column, demonstrating that the amount of terrestrial input to the site is very low. See response to reviewer 1.



Supplemental Figure 1: BIT compared to TEX_{86}^{H} SST values.

11. Lines 160-162: I am confused about the sentences describing the SST change at site 811. Is it decreasing around 5 °C from ~ 30°C to ~25°C from 7 Ma to 5.9 Ma? I think the temperature is keep decreasing since around 7 Ma and it is not reasonable to calculate

the average temperature between 6.7-5.9 Ma and stating that it is about 2 °C cooling at site 811.

We used the 6.7-5.9 Ma window to match data shown in Martinot et al. (2021) and included a further time-averaging window by Herbert et al. (2016) to identify the averaged SST change, as explained above. While the changes at ODP site 811 might be outside of these widows, we wanted to use these definitions to match what has been done before. To help organize this data better, we added Table 1, which shows the different windows and the average SST drop. All average calculations yielded a 1.8-2.2 °C shift in SSTs. Please see above for explanations on spikes in the SST-record.

Line 161: Same as "however", too many "finally" transitions are used in the whole article, which does not help with logical transitions.

To our own regret, the previously submitted manuscript version contained an unduly number of unneeded and superfluous "filling or transition phrases", which are omitted in the revised version.

Citation List

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