Reply to review comments

Revised manuscript #egusphere-2025-2996

"Penultimate glacial sea surface temperature and hydrologic variability in the tropical South Pacific from 150 ka Tahiti corals" *by *R. Asami, T. Felis, R. Shinjo, M. Murayama, Y. Iryu (*Corresponding author: R.A.)*

We deeply thank the reviewer #1 for providing useful comments on our manuscript. Following the comments, we will address all of them and improve the manuscript accordingly. You will see the changes highlighted in red in the revised manuscript (to be submitted later) and the responses to reviewer's comments in the reply letter (see below).

Reply to the comments (Reviewer #1)

Comment (#1-1, Line 15) *"Abstract: The new monthly-to-bimonthly resolved Sr/Ca and $\delta^{18}O$ records from fossil Tahiti corals spanning MIS 6b, MIS 3a, and the last glacial provide valuable insights into past tropical-subtropical Pacific climate variability. However, the manuscript could more clearly separate the influence of large-scale atmospheric changes (e.g., SPCZ/ITCZ shifts) from local oceanographic processes such as upwelling or current-driven changes. While uncertainties related to intercolony $\delta^{18}O$ variability are acknowledged, it would be helpful to clarify how consistent the signals are across coral samples and how representative the records are. The comparison with YD and HS1 is interesting—could the authors elaborate on whether similar forcing mechanisms, such as freshwater fluxes or AMOC slowdowns, might explain the patterns observed across these distinct time intervals?"*

----- Thank you so much for providing useful and positive comments. Following your comment, we will explain the influence of large-scale atmospheric changes from local oceanographic changes more carefully [Line XXX-XXX of the revised manuscript with highlighted]. As you pointed out, uncertainties of intercolony d18O variability (±0.12‰, Sayani et al., 2019)

enable to present the significant difference of d18Osw between modern and MIS 6b (and 3a), but not to discuss the difference between MIS 6b and 3a. Following your comment, we will add the explanations to limit our climatic interpretation on d18Osw differences by considering the intercolony d18O variability in the Discussion [Line XXX-XXX of the revised manuscript with highlighted]. Thank you again for providing your useful suggestions on a relation between AMOC slowdowns and our fossil coral records. As you pointed out, εNd and Pa/Th records from the Atlantic Ocean show higher values during 153 ka and 148 ka (MIS 6b) and 30 ka (MIS 3a) relative to interglacial periods, indicating weaker AMOC activity (Bohm et al., 2015 Nature; Deaney et al., 2017 Nature Communications). So, we will add the possible explanations in the Discussion [Line XXX-XXX of the revised manuscript with highlighted].

"Questions:

1. How were uncertainties propagated in the reconstruction of $\delta^{18}O\_sw$, particularly given the sparse number of fossil coral samples?

2. What independent evidence (e.g., model outputs or other paleoclimate proxies) supports the proposed contraction or weakening of the SPCZ during MIS 6b and MIS 3a?

3. Given the resolution of the coral records, were any spectral or wavelet analyses performed to identify interannual variability (e.g., ENSO frequencies)? If not, could the authors comment on the feasibility of such analysis?

4. Could the authors clarify how seasonal SST and $\delta^{18}O\_sw$ values were extracted from the records? Was a sinusoidal fit or another method used to determine summer vs. winter means?"

----- [Q1] The errors in d18Osw reconstructions were calculated to be maximum in this study. As you pointed out, it is expected that future works will reduce the uncertainty by increasing the number of fossil coral samples during the same periods and reducing the errors of past sea level estimations.

[Q2] Unfortunately, we cannot find no direct evidence of model outputs and paleoclimate records around the study site during the time intervals. However,

some indirect evidence of climate simulations and ice core records during glacial cold periods can support our interpretations of SPCZ variability (Lambert et al., 2008; Cao et al., 2019; Krätschmer et al., 2022). PMIP3/4 simulations show that LGM tropical precipitation decreases, and the deficit in the Northern Hemisphere is larger than that in the Southern Hemisphere, resulting in the southward shift, narrowing, and weakening of the ITCZ at the global scale (Wang et al., 2023 JGR). The Indonesia stalagmite d18O records show that the ITCZ convection strength was weaker during the LGM than the Holocene (Yuan et al., 2023 PNAS). Furthermore, a recent study of biomarker and pollen proxies from French Polynesia indicates that the tropical region (8.9°S, 140.1°W) in the central South Pacific was colder and drier during the glacial period, especially 35-25 ka, than today (Peaple et al., preprint in PNAS). Results of these previous studies can further support our climate interpretations and we will include the additional explanations in the discussion [Line XXX-XXX of the revised manuscript with highlighted].

[Q3] As you know, the ENSO variability has a periodicity of 3-to-8 year/cycle. Unfortunately, the fossil corals have very short records (only 10-yr time series at most from a 148 ka coral). At least 20-year-long time series should be needed to quantitatively verify the ENSO signals using MTM or wavelet spectral methods. So, in this study, we focus the investigation on the mean state and seasonality of the climate.

[Q4] The summer maximum and winter minimum SSTs were derived from the lowest and highest coral Sr/Ca values in any given annual Sr/Ca cycle. And, the d18Osw values in the summer SST maximum and winter SST minimum months were used to discuss thermal and hydrological differences in summer and winter between the past and today. Following your comment, we will add the explanations in the method [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-2, Line 41-42) *"The phrase "tropical-to-subtropical South Pacific" is a bit vague. You might consider adding specific coordinates or boundaries (e.g., 10°S–25°S, or regions between Tahiti and the GBR) to help the reader understand the spatial focus."*

----- Following the comment, we will add "0°S–20°S" in the sentence [Line XXX of the revised manuscript with highlighted].

Comment (#1-3, Line 58-62) *"While you state that the fossil coral records are monthly-to-bimonthly resolved and U-Th dated, I am curious whether, given the chronological uncertainty of U-Th dating, the monthly signal can be confidently resolved in glacial-age corals. Since most readers are more familiar with the Last Glacial Maximum (LGM), it would help to briefly define or reference the time range of the penultimate glacial period (~MIS 6) to reinforce its significance."*

----- We apologize for using the expressions that may have misled you. The time accuracy is guaranteed in the monthly-to-bimonthly coral time series because the geochemical record was continuously extracted from a transect along coral growth direction. The U-Th dating result (with 2σ errors) is the period when the coral was alive. To avoid misunderstandings among readers, we will delete the phrase "precisely U-Th dated" in the sentence [Line XXX of the revised manuscript with highlighted]. The definition and reference of the time range was described in the manuscript (Line 90-92 in version 1).

Comment (#1-4, Line 57) *"Change to "51-57 cm"*

----- Thank you for pointing it out. We will correct it to "51-57 cm" [Line XXX of the revised manuscript with highlighted].

Comment (#1-5, Line 89-90) *"Please clarify whether this interpolation assumes stratigraphic continuity or if the coral was directly dated and found unsuitable."*

----- Following the comment, we will correct the sentence to "From the perspective of stratigraphic succession, the mean of these two ages is used as best estimate for the age of our last glacial coral because our sample is located between those two samples in the sediment core (Fig. 2)" [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-6, Line 90-92) *"You may consider briefly describing the climatic relevance of these MIS intervals (e.g., "a transitional phase preceding the PGM") to aid*

general readers."

----- Following the comment, we will add the explanation "..., corresponding to transitional phases preceding the PGM and the LGM, respectively." [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-7, Line 98) *"Please specify the thickness of slabs."*

----- Following the comment, we will add the explanation "into about 1 cm thick slices" [Line XXX of the revised manuscript with highlighted].

Comment (#1-8, Line 100-101) *"Please clarify whether these segments were taken along the red sampling transect shown in Fig. 3 and how many data points this yielded for each core."*

----- Following the comment, we will add the explanation "(see the rectangular areas with numbers in Fig. 3" [Line XXX of the revised manuscript with highlighted].

Comment (#1-9, Line 106-107) *"This phrasing is vague. Consider rewording to: "Portions of the samples exhibited secondary aragonite and/or calcite cementation," and quantify if possible. Given that some cements were observed, it would strengthen the methodology section to explicitly describe the criteria used to distinguish acceptable from altered portions. For example: "Transects showing more than X% calcite or altered aragonite under SEM/XRD were excluded."*

----- Following the comments, we will correct the sentences to "The samples (Core 310-M0009B-17R-1W, 44-53 cm, 310-M0009D-25R-1W, 65-75cm, and 310-M0009D-25R-2W, 43-51 cm and 51-57 cm) have portions of well-preserved skeleton and secondary aragonite and/or calcite cements (Fig. S1). Consequently, we performed geochemical analyses on selected transects with well-preserved skeleton without any traces of diagenetic alteration. Transects showing >0% calcite or presence of aragonite cements were excluded for geochemistry in this study." [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-10, Line 113) *"In the X-ray images, the growth direction is somewhat difficult to discern. Could you clarify how you determined the growth direction? Additionally, please mark the locations selected for U-Th dating on the sample images."*

----- Unfortunately, the growth direction is difficult to discern partially on X-ray images because the core slabs are about 1 cm thick. In order to take clear X-ray images, the samples should be cut to a thickness of 3-to-5 mm. However, considering the preciousness of the samples and the necessity for diagenesis screening and geochemical analyses, it was impossible to cut and shape the samples. So, we determine the growth direction using a magnifying glass. Please note that the samples for U-Th dating is not collected from our slab samples. A coral core was cut into two slabs and one is used for our study and the other one was used for U-Th dating (Thomas et al., 2009). Therefore, we cannot mark the locations of U-Th dating on the sample images.

Comment (#1-11, Line 120) *"Since $\delta^{13}C$ and $\delta^{18}O$ analyses were performed at multiple institutions (JOGMEC and Kochi University), please comment on how consistency between labs was ensured—e.g., did both use the same calibration materials, and were inter-lab replicates analyzed?"*

----- Following the comment, we will add the explanation "The analytical consistency between the two laboratories was ensured by replicate measurements of NBS-19 and JCp-1 standard materials" in the method. [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-12, Line 122) *"While calibration against NBS-19 is mentioned, it would be useful to briefly note whether a two-point or linear correction was applied, especially for oxygen isotope ratios."*

----- The oxygen isotope value of reference gas was determined using a 5-point calibration from 5 international standards. The standards NBS-19 and JCp-1 were analyzed multiple times for every sequence and we confirmed that the measured values were consistent with the recommendation values.

Comment (#1-13, Line 122) *"replace "was" with "were""*

----- Following the comment, we will correct it accordingly [Line XXX of the revised manuscript with highlighted].

Comment (#1-14, Line 126-128) *"Consider revising the sentence for clarity and precision as follows:*

"Each ~0.2 mg coral powder sample was dissolved in 5 mL of 0.5 mol/L high-purity HNO3, prepared using ultrapure Milli-Q water.""

----- Following the comment, we will correct it accordingly [Line XXX of the revised manuscript with highlighted].

Comment (#1-15, Line 128) *"The use of Sc, Y, and Yb as internal standards, along with the application of a Ca-matched reference solution every three samples, are appropriate strategies. However, could you clarify whether internal standard correction and drift correction were applied sequentially or in combination?"*

----- Following the comment, we will add "in combination" in the sentence [Line XXX of the revised manuscript with highlighted].

Comment (#1-16, Line 133) *"The Sr/Ca reproducibility reported (better than 0.30% RSD) and agreement with JCp-1 reference values is strong. To provide additional context, you might briefly mention whether this level of precision is sufficient to resolve seasonal or interannual SST variations in your specific corals."*

----- Following the comment, we will add "(equivalent to SST errors of <0.5 °C)" in the sentence [Line XXX of the revised manuscript with highlighted].

Comment (#1-17, Line 136) *"Consider breaking the paragraph into two for readability: one focusing on resolution and averaging effects, and the other on the SST/$\delta^{18}O$_sw uncertainty estimation."*

----- Following the comment, we will correct the paragraph accordingly [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-18, Line 137-138) *"Here is somewhat vague. Consider clarifying what*

drives the variability—e.g., is it due to differing coral growth rates or diagenetic screening? Additionally, it may be helpful to specify the typical linear extension rate assumed to translate mm to time."

----- Following the comment, we will add the explanation "due to differing coral growth rates" in the sentence [Line XXX-XXX of the revised manuscript with highlighted]. The typical linear extension rate was described in the Results and Discussion because results of annual Sr/Ca cycles are needed to estimate the growth rates (see Line XXX-XXX of the manuscript version 1).

Comment (#1-19, Line 139-140) *"Although SST differences were calculated using OISST v2.1, the method for converting Sr/Ca values to SST should be described more explicitly—for example, specifying the calibration equation or slope used. If this conversion is based on modern Porites calibrations, please cite the source directly here or reaffirm the slope presented in earlier sections."*

----- Following the comment, we will add the explanation about Sr/Ca-SST calibration used in this study [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-20, Line 141) *"The calculated SST offsets (e.g., +0.07 °C and −0.09 °C) are indeed small. Still, it would be useful to state explicitly how these compare to the observed amplitude of seasonal SST changes in the fossil records. This will help readers assess whether the offsets are indeed negligible relative to the climate signals of interest."*

----- Following the comment, we will add the explanation "...because the amplitude of seasonal Sr/Ca-derived SST changes in the fossil coral records is much larger (3.4–4.6 °C, see the details in Results and Discussion) than the SST offsets derived from different sampling resolutions" in the sentence [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-21, Line 144-145) *"This setence could be strengthened by briefly specifying whether uncertainties were combined in quadrature and whether interannual SST variability was considered in addition to analytical and calibration errors."*

----- Following the comment, we will add the brief explanation about the method [Line XXX-XXX of the revised manuscript with highlighted]. However, we do not think that the technical method should be described in this study because it was already published in Cahyarini et al. (2018) and we referred the paper in the manuscript.

Comment (#1-22, Line 146) *"Please provide 230Th age table."*

----- Please note that the U-Th measurements are not carried out in this study. The ^{230}Th age results (Table) were already published in Thomas et al. (2009). So, we cannot show the table in this study.

Comment (#1-23, Line 147) *"The paragraph is detailed and informative but could benefit from breaking into shorter sections for better readability. Consider separating the data description, interpretation, and growth rate discussion into distinct paragraphs."*

----- Following the comment, we will divide the paragraph (section 3.1) into three paragraphs [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-24, Line 155-156) *"The explanation about the complex interpretation of coral δ13C on glacial-interglacial timescales is helpful. If space allows, a brief summary or rationale would aid readers unfamiliar with Felis et al. (2022)."*

----- Following the comment, we will add a brief explanation in this paragraph [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-25, Line 159-160) *"You might consider adding brief comments on the sample size (n) or the robustness of these statistics."*

----- Following the comment, we will add the sentence "There exist significant correlations of coral Sr/Ca vs. d18O records for 153 ka (r = 0.74, n = 54, p < 0.01), 148 ka (r = 0.71, n = 89, p < 0.01), and 30 ka (r = 0.71, n = 20, p < 0.01)" in this paragraph [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-26, Line 165) *"The comparison of glacial vs. modern coral growth rates is important. Consider elaborating slightly on the implications of lower growth rates for proxy reliability or potential diagenetic effects."*

----- The coral growth rates are not related to diagenetic effects, but the coral density may be related to the effects because it could depend on potential space for cements to precipitate. We do not discuss about the possibility because we do not use geochemical records of diagenetically altered coral skeleton for climate interpretation. On the other hand, we will add the explanations on the relationship between coral growth rates and geochemical records in the section 3.2 [Line XXX-XXX of the revised manuscript with highlighted]. Furthermore, we will add a brief implication "The coral growth effects on geochemical records should be carefully considered for paleo-tempearture estimations (see the details in the section 3.2" in this paragraph [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-27, Line 193) *"Could you provide more detailed information on how uncertainties were propagated through the multiple correction steps? Are error bars or confidence intervals included with the final SST and δ^{18}Osw estimates?"*

----- The propagation errors were calculated as combined errors by following the methods of Cahyarini et al. (2008) and Knebel et al. (2024). Following the comment, we will add more detailed information on how uncertainties were propagated through the corrections in the paragraph and in the footnote of Table 1 [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-28, Line 198) *"The assumption of a uniform 0.5% increase in seawater Sr/Ca during the LGM is based on previous studies. Is there any evidence that this value varies regionally, especially in the South Pacific? How might regional variability affect the accuracy of corrections?"*

----- There are no published papers showing regional variations of seawater Sr/Ca values in the study site. However, because the residence time of Sr and

Ca in seawater is much longer (>1,000,000 years), it is expected that regional variations of seawater Sr/Ca ratios could be probably small. In this study, the assumption of 0.5% changes in seawater Sr/Ca at glacial-interglacial scale was used, but future studies on regional Sr/Ca variability will evaluate the accuracy of corrections more quantitatively using raw data of our fossil records published in this journal.

Comment (#1-29, Line 200) *"Is the assumption of present-day SST (2000–2008 AD) as the baseline appropriate, given the potential influence of recent anthropogenic warming? Could using a pre-industrial baseline change the inferred SST anomalies?"*

----- In this study, the present baseline was used as 2000-2008 AD because the Sr/Ca-SST calibration of modern Tahiti corals for comparison was established using the records for 2000-2008. As you pointed out, the annual SST for 2000-2008 (27.7±0.2 °C) is 0.7±0.4 °C higher than 1854-1950 (27.0±0.3 °C). Following the comment, we will add the explanation "Taking account for the effects of recent global warming since 1950, our paleo-SST estimates should be corrected using the SST difference of 0.7±0.4 °C between 2000-2008 and 1854-1950 (derived from NOAA NCDC ERSST v5)." in this paragraph [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-30, Line 200-201) *"The Sr/Ca-SST calibration slope used is from modern Tahiti corals. Have you considered possible shifts in calibration slope for glacial period corals due to physiological or environmental differences?"*

----- Possible shifts in calibration slope for glacial period cannot be considered in this study because there are no published researches on glacial coral Sr/Ca-SST calibrations in Tahiti.

Comment (#1-31, Line 220-221) *"The application of LeGrande and Schmidt (2006)'s modern δ^{18}Osw–SSS relationship is common, but is there any evidence or modeling result suggesting this relationship may have changed during glacial periods?"*

----- There is no consensus about the salinity-d18Osw relationship during glacial periods. Some simulation studies show that the salinity-d18Osw

relationship may have changed in the Atlantic Ocean during glacial periods (e.g., Wadley et al., 2002 QSR), but others show that no drastic changes occurred in the relationship (Roche et al., 2004 EPSL). Therefore, we used a widely-accepted relationship in the South Pacific (LeGrande and Schmidt, 2006) on the assumption that it has not changed throughout the glacial-interglacial priods. When a relationship for glacial periods would be established, our paleo-SSS estimates can be re-calculated.

Comment (#1-32, Line 221-223) *"How much of the 3–4 °C cooling estimate is due to the individual corrections (e.g., inter-lab offset, growth-rate adjustment, seawater Sr/Ca)? Can the relative contributions be quantified or discussed?"*

----- The relative contributions of growth-rate adjustment and seawater Sr/Ca difference can be quantified to be 0% and 21-28% of 3-4 °C. The correction of inter-lab offset is not related to this contribution.

Comment (#1-33, Line 221-223) *"How do these coral-based estimates compare with other local or regional paleo-SST/SSS proxies (e.g., foraminifera Mg/Ca, alkenones) in the same time slices?"*

----- Paleo SST and SSS data from foraminifera Mg/Ca and alkenones in ocean sediments show average values for several decades to centuries, which can be understood as annual mean values. Therefore, annual mean values from coral-based estimates can be compared with the other proxy records.

Comment (#1-34, Line 229) *"Can you elaborate on the specific correction factors applied and the uncertainties associated with each? How were the time-weighted averages computed (e.g., did you apply any weighting based on coral age model confidence intervals or sample resolution)?"*

----- Following the comment, we will add the explanations "...offsets relative to the average value of 8.901 mmol/mol for the JCp-1 standard (Knebel et al., 2024) as..." and "...Sr/Ca averages for selected periods during which the U-Th ages of fossil corals are overlapped are..." in this paragraph [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-35, Line 231-232) *"What is the uncertainty ('C) associated with the reconstructed SSTs for each period, considering analytical, calibration, and age model uncertainties?"*

----- Following the comment, we will add the uncertainty "2.7±0.2 °C lower" and "1.0±0.3 °C lower" in this sentence [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-36, Line 234-237) *"Have you attempted to correlate SST with sea level data quantitatively, or is the comparison purely interpretive? Would plotting SST vs. RSL strengthen the climate-state argument?"*

----- Following the comment, we will add the correlation "(r = 0.94, p < 0.01, n = 3)" in this sentence [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-37, Line 237-240) *"Is the salinity conversion based on a local modern δ18Osw-SSS relationship? If so, please cite the calibration and clarify the assumed slope."*

----- Following the comment, we will add the explanation "if a salinity-d18Osw relationship is applied (LeGrande and Schmidt, 2006)" in this sentence [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-38, Line 252) *"Can you elaborate on the specific correction factors applied and the uncertainties associated with each? How were the time-weighted averages computed (e.g., did you apply any weighting based on coral age model confidence intervals or sample resolution)?"*

----- Following the comment, we will add the explanations accordingly. The time-weighted averages were calculated for selected periods during which the U-Th ages of fossil corals are overlapped. We will add the explanation in the Discussion [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-39, Line 270) *"Are the SST differences reported here consistent with broader regional reconstructions from other South Pacific sites (e.g., Vostok, EPICA Dome*

C, or MD06-2986)"

----- Yes, lower SST signals during 153-148 ka and 30 ka are also recorded at the sites Vostok (Petit et al., 1999 Nature), EPICA Dome C (Jouzel et al., 2007 Science), and MD06-2986 (Pahnke et al., 2005 Science). Following the comment, we will add the explanation in this paragraph [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-40, Line 271) *"While the coral-derived SST anomaly is reported with an uncertainty (+/-1.1 'C), the foraminiferal and alkenone records are not. For consistency and better comparison across proxy types, could you report uncertainties for these other proxies as well?"*

----- Following the comment, we will add the explanations "<2±0.6 °C" and "4±0.5 °C" in this paragraph [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-41, Line 271) *"How robust is the SST anomaly at 153–148 ka given the temporal spread and potential uncertainties in coral U-Th ages? Could there be variability within that interval?"*

----- Coral records show snapshots of reconstructed SST time series for selected several years. Considering the interannual and decadal climate variability, the actual SST anomaly during 153-148 ka is likely to be larger or smaller than our estimates in this study.

Comment (#1-42, Line 271-274) *"Have you considered or discussed the potential systematic differences between SST reconstructions from different proxies (Sr/Ca, Mg/Ca, alkenones)? For example, were corrections applied for seawater Mg/Ca changes in the foraminiferal records?"*

----- In previous studies, foraminiferal Mg/Ca-derived SSTs were estimated on the assumption of constant seawater Mg/Ca because residence time of Mg and Ca is much longer (>1,000,000 years) than the glacial-interglacial cycles. In this study, we referred the reconstructed SST values published in Medina-Elizalde and Lea (2005).

Comment (#1-43, Line 274) *"Did you consider ENSO or other interannual variability during the glacial periods, and how might this influence the interpretation of stronger zonal SST gradients?"*

----- Following the comment, we will add the explanation "Paleoclimate records and simulations indicate less frequent and weaker ENSO variability during the LGM relative to the present (e.g., Ford et al., 2015 Science; Thirumalai et al., 2024 Nature). A climate simulation suggests that the equatorial Pacific climate under glacial conditions is characterized by a contracted WPWP and stronger SST gradient together with a deeper mixed layer driven by a stronger Walker circulation (Thirumalai et al., 2024), which can support our interpretation on SST gradients in the subtropical and the mid-latitude regions of the South Pacific." in this paragraph. [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-44, Line 276) *"As the distinction between latitudinal and zonal SST gradients is somewhat ambiguous, could you clarify whether the observed differences are primarily driven by latitudinal (meridional) or zonal (longitudinal) gradients, or both. Could this be better supported with spatial SST maps?"*

----- Probably, both. However, more paleoclimate records and simulation studies are needed to clarify whether the primary driving factor is latitudinal or zonal gradients. Therefore, following the comments, we will correct the explanation "These lines of evidence for the stronger latitudinal and/or zonal SST gradients imply..." [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-45, Line 279) *"Could you elaborate on how well the reconstructed SST gradients match the modeled zonal wind fields or SST gradients? Any specific model experiments used?"*

----- In this study, spatial SST distribution cannot be reconstructed and so we cannot compare directly our data with modeled results. Some climate simulation studies and ice core studies indicate pronounced latitudinal winds

during glacial cold conditions (Lambert et al., 2008; Cao et al., 2019; Krätschmer et al., 2022). Here, we would like to say that our consideration on subtropical to mid-latitude SST gradients is roughly consistent with these views. Therefore, we will correct the sentence to weaken our indication "..., which could be supported by..." [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-46, Line 283-284) *"How were the uncertainties in seasonal Sr/Ca amplitudes calculated, and are they comparable across time intervals?"*

----- The uncertainties in seasonal Sr/Ca amplitudes are standard errors (SE) that was calculated for each month's average which takes into account how many monthly proxy values were included in each month's average. This follows the method of Knebel et al. (2024). Following the comment, we will correct the sentence and Table 1 "Our Tahiti coral Sr/Ca seasonality of 0.23±0.02 and 0.17±0.01 mmol/mol at 153–148 ka and 0.18±0.01 mmol/mol at 30 ka is larger than that previously reported for HS1 (0.13±0.01 mmol/mol), B-A (0.12±0.01 mmol/mol), and the present (0.14±0.01 mmol/mol) (Knebel et al., 2024) (Fig. 6B)." [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-47, Line 290-292) *"Could additional hydrological proxies or isotope-enabled modeling help clarify the dominant processes responsible for δ^{18}Osw changes?"*

----- We cannot find other studies on seasonally-resolved d18Osw model simulation. As you know, sediment core records do not tell us summer and winter d18Osw values because of low data resolution (several decades/sample). Therefore, we cannot clarify the dominant processes in this study and so will correct the phrase "suggest" to "may suggest" [Line XXX of the revised manuscript with highlighted].

Comment (#1-48, Line 293) *"Consider reorganizing the paragraph beginning with "The SPCZ is a diagonal band..." for clarity—it introduces background information somewhat abruptly in the middle of result interpretation."*

----- Following the comment, we will add the explanation "...at that time, possibly associated with the SPCZ variability different from today" in the sentence immediately preceding this paragraph for clarity [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-49, Line 297-298) *"Could you clarify how they distinguish between large-scale atmospheric influences (like SPCZ shifts) and local oceanographic drivers? Are there model simulations or regional paleoclimate reconstructions that support the proposed SPCZ changes during MIS 6b and the last glacial period?"*

----- In this study, it is impossible to distinguish between atmospheric influences and local oceanographic drivers due to lack of paleoclimate proxies and model simulations at 153-148 ka and 30 ka. However, some climate simulation studies indicate pronounced lower SSTs in the Pacific and reduced ENSO variability during the LGM, which can support lower activity of the SPCZ. Following the comment, we will add the explanation "This climatic interpretation could be supported by a simulation study suggesting that the WPWP contracted to the west and SST gradient became stronger in the equatorial Pacific during the LGM (Thirumalai et al., 2024)" in this paragraph [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-50, Line 301-303) *"The comparison with Knebel et al. (2024) and Asami et al. (2009) is valuable, but a more detailed discussion of methodological differences or coral site locations would help contextualize why the reconstructed seasonality is higher in this study."*

----- Following the comment, we will add the explanation "The comparison of SST seasonality between the penultimate glacial and the YD period can be significant because the fossil corals were collected from the same site (Tiarei) in Tahiti and analyzed by the same methodology." in this paragraph [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#1-51, Line 303) *"Revise for clarity in "which was suggested to be resulted from..." → "which was suggested to result from.."*

----- Following the comment, we will correct it accordingly [Line XXX of the revised manuscript with highlighted].