

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 #2 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 #2)

General Comments:

Comment (#2-1) *“This manuscript presents a highly significant study that reconstructs ocean environments during MIS 3 and MIS 6—periods for which paleoclimate records are particularly scarce—using high-precision geochemical analyses of fossil corals. The approach of employing well-preserved coral skeletons as archives of high-resolution climate information is appropriate and timely, and the dataset provides important insights into past oceanographic and climatic variability. I especially appreciate the careful attention the authors have paid to diagenetic screening and to the influence of coral growth rates on analytical resolution. This demonstrates that the study was conducted with great care and methodological rigor, which strengthens the reliability of the results. However, there are several areas where the manuscript can be improved for clarity. First, descriptions of some correction methods are incomplete or missing, which makes it difficult for readers to fully evaluate the robustness of the results. Second, the interpretations of SST and salinity variations occasionally appear overstated given the relatively short duration of the records analyzed. These sections would benefit from a more cautious discussion that explicitly considers the uncertainties involved. Another point concerns the discussion of seasonality. Since corals are among the very few archives that can resolve*

seasonal-scale variations, it would be highly valuable if the manuscript provided a more careful and detailed discussion of seasonality, including error estimates and an assessment of whether observed differences are statistically significant. Finally, while the manuscript includes a discussion of SPCZ migration, it would be strengthened by explicitly considering ITCZ and/or ENSO, and by situating the findings within the broader context of paleoclimate records and model studies. This would allow the results to be more effectively placed in a global climatic framework and would broaden the potential impact of the study. In summary, this is an important and promising manuscript that has the potential to make a substantial contribution to the field.”

----- Thank you so much for providing positive comments. Following comments from you and the reviewer #1, we will improve the manuscript by adding some explanations on the correction methods for paleo-SST and -SSS, the discussion on SST seasonality, and the relation with the ITCZ and/or ENSO variability.

Major comment (#2-2) “Correction methods:

Some descriptions of correction methods are omitted or unclear (e.g., P6 L138–140, L142; P7 L163–165). These should be explicitly stated in the Methods section.”

----- Regarding the effects of different resolution, we followed the method of Asami et al. (2020, GRL) clearly showing the evaluation. Following the comment, we will add the explanation “..., by following the method of Asami et al. (2020)” and also add a supplementary Table (Table S3) representing the evaluation results in this study. [Line XXX-XXX of the revised manuscript with highlighted]. Regarding coral growth effects, we will add a brief explanation “The coral growth effects on geochemical records should be carefully considered for paleo-temperature estimations (see the details in the section 3.2” in Method section [Line XXX-XXX of the revised manuscript with highlighted].

Major comment (#2-3) “Interpretation of short records:

The discussion of SST and salinity variations occasionally appears overstated given the relatively short duration of the analyzed intervals (e.g., 30 ka, 153 ka). The associated

uncertainties should be clearly acknowledged. The record of 30 ka has only for 2 years and a half.”

----- Following the comment, we will correct some explanations on the short coral records in the Discussion, by using weaker expressions such as “(may) indicate”, “(may) suggest”, “appear”, “seem”, or “is likely to”. Furthermore, we will add a cautionary note “It is also noted that our corals provide snapshots of less than 10-year-long time series for selected glacial periods and the actual SST estimates could be potentially changed by interannual and decadal SST variability” in the Discussion [Line XXX-XXX of the revised manuscript with highlighted].

Major comment (#2-4) *“Seasonality analysis:*

Coral skeletons are valuable archives for reconstructing seasonal variability. However, the discussion of seasonality requires more careful treatment, including error estimates and evaluation of whether differences are statistically significant (e.g., P13 L4–L6, discussion of 148 ka and 153 ka records).”

----- Following the comment, we will include respective errors to the seasonality estimates in the Discussion. For example, we will correct the sentence to “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].

Major comment (#2-5) *“The relationship between SPCZ and ENSO:*

The discussion on SPCZ migration would benefit from integration with previous work on ITCZ and ENSO. Relevant references should be added, and the relationship between salinity front, ITCZ position, and ENSO should be clarified (e.g., P11 L250, P13 L278).”

----- Following the comment, we will add some explanations with relevant previous studies in the Discussion as follows;

“It is noted that the salinity front could have changed on interannual and decadal time scales associated with thermal and hydrological variations due

to the ENSO and the Pacific Decadal Oscillation (Delcroix and McPhaden, 2002; Gouriou and Delcroix, 2002; Delcroix et al., 2007)” in this paragraph [Line XXX-XXX of the revised manuscript with highlighted],

“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],

“Paleoclimate records and simulations indicate less frequent and weaker ENSO variability during the LGM relative to the present (e.g., Ford et al., 2015; Thirumalai et al., 2024). 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 could 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].

Specific comments

Comment (#2-6) *“Fig. 2: For 9B17R1 (50–55 cm), was age dating performed? If so, please indicate the ages in the figure; if not, provide a reason.”*

----- We apologize for confusing you. To avoid misunderstanding for readers, we will add the explanation “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)” in the method [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#2-7) *“Fig. 3 and Supplement, Section 2.2 Mineral screening :*

Based on the XRD and SEM observations, you state that only well-preserved skeletal portions were used for analysis. My understanding is that the analyzed areas correspond only to the red-lined segments in Fig. 3. However, other portions also appear well

preserved. For example, in 9D25R1_65–75 cm, Table S1 suggests that segments 1–8 are all well preserved, yet only segments 2–5 were analyzed. Similarly, for 9D25R2_43–51 cm (segments 1–3) and 51–57 cm (segments 1–4), Table S1 indicates they are suitable for analysis. Beyond calcite content and the presence/absence of secondary aragonite cement noted in Table S1 and SEM observations, were there any additional criteria used to decide which portions were selected for analysis?”

----- Thank you for your insightful comment. Following the comment, we will add the explanation “The geochemical profiles (shown as the red-lined segments in Fig. 3) for the use of paleoclimate reconstructions are actually shorter than the criteria based on the XRD analyses and SEM observations (Table S1) because inappropriate skeletal portions were additionally rejected due to irregular skeletal growth and randomly scattered aragonite cements at micro-scale that were confirmed by the first screening $\delta^{18}\text{O}$ analyses” in the method section [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#2-8) *“Fig. 4: Please clarify whether Sr/Ca data have been corrected for seawater Sr/Ca ratio (P10 L197–201). Also, in the caption, “Horizontal bars represent analytical errors” appears to be a typo; should this be “Vertical bars”?”*

----- Following the comment, we will add the explanation “without any corrections” in the caption of Fig. 4. Sorry, this is a typo. We will correct “Horizontal bars” to “Vertical bars” accordingly.

Comment (#2-9) *“P6 L138–140 : It is not clear how the averaging effects were specifically calculated.”*

----- We followed the method of Asami et al. (2020, GRL) clearly showing the evaluation. Following the comment, we will add the supplementary Table (Table S3) representing the evaluation results in our study.

Comment (#2-10) *“P6 L142 : Regarding “the offset in SST seasonality,” is this offset corrected for in the subsequent discussion?”*

----- We apologize for confusing you. To avoid misunderstanding for readers, we will add the explanation “... and the slight difference was not used

for the correction in this study” into this sentence [Line XXX of the revised manuscript with highlighted].

Comment (#2-11) “P7 L155–156: $\delta^{13}\text{C}$ is not discussed in the main text; either move it to the Supplement or add a brief summary in the main text.”

----- Following the comment, we will add a brief summary on $\delta^{13}\text{C}$ in the main text [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#2-12) “P7 L163–165: Coral growth rates are relatively low (<5 mm/yr). Please clarify whether potential vital effects on $\delta^{18}\text{O}$ were corrected, citing relevant studies (e.g., Hayashi et al., 2013, Hirabayashi et al., 2013).”

----- We corrected the effects on $\delta^{18}\text{O}$ using the relationship of Felis et al. (2003) (Please see Line 203-205 in the manuscript version 1). This relationship is similar to the two studies you suggested. Therefore, we will add the explanations “... (Felis et al., 2003) that is similar to other studies (Hayashi et al., 2013; Hirabayashi et al., 2013)” in the discussion [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#2-13) “P9 L193–195: This methodological description should be moved to the Methods section.”

----- Following the comment, we will move the sentence to the method section [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#2-14) “P10 L215–216: Since the 30 ka record covers only about 2.5 years, its treatment requires caution. For the 153 ka record, the Sr/Ca data suggest that during the 20–30 mm interval, summer SSTs were comparable to those of 2000–2008, indicating that temperatures were not necessarily 3–4 °C cooler than today. In particular, for the 153 ka Sr/Ca record, both the amplitude (i.e., seasonality) and the mean values appear to differ between 0–15 mm and 20–30 mm. I would encourage the authors to take special care in discussing this aspect.”

----- Following the comments, we will add the sentence “It is also noted that our corals provide snapshots of less than 10-year-long time series for selected

glacial periods and the actual SST estimates could be potentially changed by interannual and decadal SST variability” in this paragraph [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#2-15) *“P13 L4-L6 : Regarding seasonality, what are the associated uncertainties for each record? Do the differences remain significant when errors are taken into account? Both the 153 ka and 148 ka records seem to include years where the seasonality is nearly identical to that of the modern record. For example, in the 148 ka record, the 0–20 mm interval appears to show reduced cyclicity compared to the 20–45 mm interval. Similarly, in the 153 ka record, especially within the 0–20 mm interval, the seasonality appears comparable to that of the modern record.”*

----- 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, which 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 (#2-16) *“P7 L163–165 : The coral skeletal extension rates are relatively low (<5 mm/yr). Could the influence of $\delta^{18}\text{O}$ vital effects be an issue in this case (e.g., Gagan et al., Inoue et al., Hayashi et al., Hirabayashi et al.)? If a correction has been applied, it would be helpful to briefly describe the method in the Materials and Methods section.”*

----- We corrected the effect and will describe the explanation “...for the $\text{d}18\text{O}$ offset caused by difference in annual growth rate between modern and fossil corals using a previously established equation with r^2 value of 0.91 for eleven *Porites* spp. corals with growth rate of 2.0–15.2 mm/year (Felis et al., 2003) that is similar to other studies (Hayashi et al., 2013; Hirabayashi et al., 2013)” [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#2-17) *“L200–201 & L205–206: Slopes of Sr/Ca–SST and $\delta^{18}\text{O}$ –SST conversion equations are given, but the associated uncertainties are not. Please estimate how these propagate into reconstructed SST and SSS.”*

----- The uncertainties of these equations were included for SST and $\delta^{18}\text{O}$ sw estimations. Following the comment, we will correct the descriptions to “...of -0.050 ± 0.002 mmol/mol/°C...” and “...of -0.20 ± 0.02 ‰/°C...” in respective sentences [Line XXX and XXX of the revised manuscript with highlighted].

Comment (#2-18) *“The Discussion focuses on SPCZ displacement — is it possible to also discuss contemporaneous changes in the ITCZ and ENSO? In the Conclusion the authors note that longer coral records would be needed to discuss ENSO; however, are there existing paleoclimate records or model studies that currently allow discussion of ENSO or ITCZ behavior for these time intervals? If so, please cite and discuss those prior findings (from proxy records and/or model simulations) and clarify whether they support or contradict the SPCZ-centered interpretation.”*

----- Unfortunately, we cannot discuss contemporaneous changes in the ITCZ and ENSO because there existing no coral records and model studies on these behaviors at 153-148 ka and 30 ka. However, some 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 recent climate simulation suggests that the equatorial Pacific climate under glacial conditions (LGM) 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 could support our interpretation on the SPCZ displacement. Following the comment, we will add the explanations in the Discussion [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#2-19) *“P11 L250 — The salinity front: how is this feature related to ITCZ position and to ENSO variability?”*

----- Following the comment, we will add the sentence “It is noted that the salinity front could have changed on interannual and decadal time scales associated with thermal and hydrological variations due to the ENSO and the Pacific Decadal Oscillation (Delcroix and McPhaden, 2002; Gouriou and Delcroix, 2002; Delcroix et al., 2007)” in this paragraph [Line XXX-XXX of the revised manuscript with highlighted].

Comment (#2-20) *“P13 L278 — The “stronger zonal SST gradient”: can this be interpreted as being related to ENSO variability?”*

----- Yes, as you pointed out, we also think that is a possibility. So, 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; Thirumalai et al., 2024). 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 could 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].