Response to David Fastovich

GENERAL COMMENTS

Hollstein and colleagues present a manuscript that pairs an impressive new proxy SST compilation for the tropical Pacific and Indian Oceans with CESM simulations across several interglacial time slices. In my view the three most valuable contributions are (1) the compilation itself, (2) the finding that land surface feedbacks influence the simulated tropical response, and (3) the extension of the "Holocene Conundrum" beyond the Holocene. The data-side work is careful and the model component is a genuine strength of the study. However, the modeling strength is currently underused, and several of the paper's central mechanistic claims are asserted rather than demonstrated. My comments below are organized into major comments, figure comments, and line-by-line comments. I hope the authors find these comments helpful.

We thank David Fastovich for the positive evaluation of our manuscript and for his thoughtful and constructive comments. We will address his comments as outlined below and include the outlined changes in the revised version of our manuscript.

1. The model simulations are underused, particularly with respect to the proposed circulation mechanisms.

The AMOC is introduced as a control on L228–L259 but is supported with just a few papers but no model analysis. The same is true of the weakening of the Pacific Walker Circulation (PWC), which is raised repeatedly through the manuscript and culminates in a proposed connection to the AMOC on L258–L267, where it is left as a hypothesis rather than evaluated against the simulations at hand. L345 illustrates the problem well: "The different SST anomalies lead to contrasting inferences about the atmospheric circulation." Why infer when the raw model output is available? At present the model component is limited to temperature comparisons, essentially plotted SST fields, when the authors have the opportunity to look under the hood and test the AMOC hypothesis directly. This is the single largest missed opportunity in the paper. In its current form the mechanistic discussion reads as proposed rather than tested.

We thank the reviewer for this insightful critique and for pointing out the opportunity to further strengthen the mechanistic discussion. We agree that a direct evaluation of the ocean dynamics, specifically the large-scale overturning and its linkage to the Pacific Walker Circulation (PWC), would be ideal if the models were capable of resolving these processes internally. However, as outlined in the manuscript, reconstructed and simulated SST anomalies are mostly of opposite sign. This makes it impossible to use the model to test the mechanisms that are proposed on the basis of the proxy data.

Moreover, as discussed in the literature (e.g., Toggweiler et al., 2019), state-of-the-art climate models, such as CESM used in our study, fundamentally struggle to simulate the correct extra upwelling (“upshift”) of subantarctic mode and intermediate waters into the tropical surface layer, which is linked to the AMOC’s return flow. Because these models underestimate this large-scale volumetric transport of relatively cold waters, they inherently suffer from a widespread warm tropical SST bias as demonstrated by Toggweiler et al. (2019). When a model produces SST anomalies with incorrect structures, any diagnostic evaluation of the atmospheric response, such as the Walker Circulation, would reflect the model's structural bias rather than a true physical test of the mechanism.

Therefore, we follow the diagnostic framework of Toggweiler et al. (2019) and use the modelled SST warm-bias pattern as a robust fingerprint of the missing upshift mechanism. To clarify this limitation and strengthen the manuscript, we will revise the text accordingly and add a new figure that shows the tropical warm-bias pattern. We believe that presenting the SST bias field provides the most honest and rigorous assessment of what the model output can constrain in its current generation. To this end, we calculated the deviation between the SST fields of the CESM control simulation (PI) and COBE and find that the model does not accurately capture the cool SSTs that arise from eastern Pacific upwelling/upshift, i.e., the simulated SSTs offshore the upwelling areas are too warm (Fig. 1). This can be interpreted as indicator for the inaccurate representation of the AMOC-induced upshifts in CESM. We will include this point in the revised version of the manuscript.

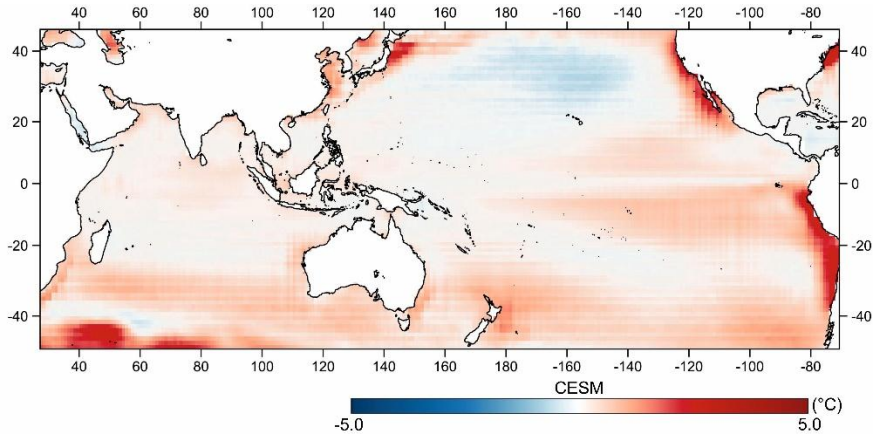


Fig. 1. Deviations of the modelled (CESM) mean annual SST during the PI from COBE SST. Colors indicate colder (blue) and warmer (red) modelled SSTs compared to COBE.

Toggweiler, J. R., Druffel, E. R. M., Key, R. M., & Galbraith, E. D. (2019). Upwelling in the Ocean Basins North of the ACC: 1. On the Upwelling Exposed by the Surface Distribution of Δ14C. Journal of Geophysical Research: Oceans, 124(4), 2591–2608. https://doi.org/10.1029/2018jc014794

2. The equivalence between the zonal SST gradient and PWC strength is assumed rather than established.

Throughout the manuscript, a reduced zonal SST gradient is treated as equivalent to a weakened PWC (see L214–L217, L435). The logical step connecting the two is never presented, and it is needed to support claims about the PWC based solely on SST records. Either the link should be substantiated (ideally with the model output, which would resolve this directly), or the language should be softened consistently: without a direct measure of the PWC, a reduced zonal SST gradient suggests rather than indicates a weakening, since "indicates" implies direct evidence.

We agree that the connection between the zonal SST gradient and the PWC should be clear. The zonal SST gradient across the equatorial Pacific is mutually connected to the PWC. This coupling is the core of the Bjerknes feedback. We will add a short paragraph about this coupling to the discussion section of the article. In line with the referee's suggestion, we will also soften the language wherever necessary.

3. Age model: BIGSTACK should replace LR04.

At L70 and L108–L109, the new BIGSTACK record is a direct update to LR04 and should be used here, particularly because it incorporates age uncertainties explicitly, which fits naturally with the Monte Carlo framework already adopted (Zhou et al., 2026, Geochronology, https://doi.org/10.5194/gchron-8-85-2026).

We thank David Fastovich for this suggestion. Our compilation is based on the PALMOD SST database (Jonkers et al., 2026), which uses the LS16 benthic stack to provide a common framework for the chronologies of all cores. Because the PalMod age-depths models are restricted to the last 130 kyr, we used the LR04 benthic stack as reference record to align the δ18O records during MIS 11. Please note that we do include age uncertainty in the LS16 and LR04 stacks.

The LS16 benthic stack was also used as tuning target for BIGSTACK and therefore, the two stacks are consistent (see Fig. S2 in Zhou et al., 2026). Replacing LR04 by BIGSTACK would only affect the MIS 11c anomalies. During MIS 11c, the new BIGSTACK record is slightly younger than the LR04 stack with a δ18O minimum at 401 ka as compared to 405 ka. Notably, BIGSTACK has a lower resolution than the LR04 stack, which would make the alignment more difficult.

Please also note that BIGSTACK was published after we had completed our proxy data compilation. For all these reasons, we prefer to keep our original age models, but we consider an update to BIGSTACK an interesting suggestion for our future work.

Jonkers, L., Hollstein, M., Siccha, M., & Kucera, M. (2026). The PALMOD 130k marine palaeoclimate data synthesis version 2. Earth System Science Data.

Zhou, Y., Lisiecki, L. E., Meyers, S. R., Lee, T., & Lawrence, C. (2026). Global and regional Pleistocene benthic δ18O stacks with a comparison of different age modeling strategies. Geochronology, 8(1), 85–107. https://doi.org/10.5194/gchron-8-85-2026

4. Uncertainty treatment

Three related points. First, I commend the authors for propagating SST uncertainties, but is 1 °C (1σ) appropriate given the uncertainties BAYSPLINE produces for these records? Second, if a Monte Carlo framework is used to propagate temporal and reconstruction uncertainties throughout, it is not clear why Figure 4 uses bars rather than the box-and-whisker plots of Figures 3 and 5.

We thank David Fastovich for the recognition of the uncertainty propagations.

As outlined in the supporting information, we base our analysis of proxy-derived SST anomalies on the assessments that result from the application of the calibration equations used in the original publications of the records rather than using BAYSPLINE for UK'37 records and MgCaRB for Mg/Ca records. Unfortunately, only some studies include uncertainty estimates for their SST reconstructions. The assumed uncertainty of 1 °C is in the typical range of Mg/Ca-based SST uncertainties (e.g., Hollstein et al., 2018; Mohtadi et al., 2014). Assessing the uncertainty of UK'37-based SST estimates is more difficult, because most studies only provide information about analytical errors or assessments that base on duplicates (and not on replicates). These are very small (<0.1 °C) (e.g., Bova et al., 2015; Lauterbach et al., 2020). Please also note that the provided standard deviations of SSTs derived from the 'traditional' calibrations are smaller than the uncertainty estimates of BAYSPLINE and the assumed uncertainty of 1 °C is in the range of the traditional estimates.

Following the referee's suggestion, we will revise Fig. 4 to include uncertainty estimates (whiskers) of the reconstructed gradient anomalies.

Bova, S. C., Herbert, T., Rosenthal, Y., Kalansky, J., Altabet, M., Chazen, C., Mojarro, A., & Zech, J. (2015). Links between eastern equatorial Pacific stratification and atmospheric CO2 rise during the last deglaciation. Paleoceanography, 30(11), 1407–1424. https://doi.org/10.1002/2015pa002816

Hollstein, M., Mohtadi, M., Rosenthal, Y., Prange, M., Oppo, D. W., Martínez Méndez, G., Tachikawa, K., Moffa Sanchez, P., Steinke, S., & Hebbeln, D. (2018). Variations in Western Pacific Warm Pool surface and thermocline conditions over the past 110,000 years: Forcing mechanisms and implications for the glacial Walker circulation. Quaternary Science Reviews, 201, 429–445. https://doi.org/10.1016/j.quascirev.2018.10.030

Lauterbach, S., Andersen, N., Wang, Y. V., Blanz, T., Larsen, T., & Schneider, R. R. (2020). An ~130 kyr Record of Surface Water Temperature and δ18O From the Northern Bay of Bengal: Investigating the Linkage Between Heinrich Events and Weak Monsoon Intervals in Asia. Paleoceanography and Paleoclimatology, 35(2), e2019PA003646. https://doi.org/https://doi.org/10.1029/2019PA003646

Mohtadi, M., Prange, M., Oppo, D. W., De Pol-Holz, R., Merkel, U., Zhang, X., Steinke, S., & Luckge, A. (2014). North Atlantic forcing of tropical Indian Ocean climate. Nature, 509(7498), 76–80. https://doi.org/https://doi.org/10.1038/nature13196

5. Framing and readability.

The introduction is missing a sentence establishing why interglacials in particular are worth examining, and the logical step from "the IPWP is important" to "we therefore need to study it on long timescales" (L33) is absent. The connection between the IPWP and the Holocene Conundrum is also never made explicitly, even though the extension of that conundrum is one of the paper's main contributions.

We will do as suggested and add the missing information.

More generally, the text is very dense and difficult to follow. Beyond the specific suggestions below, I would encourage the authors to make a dedicated pass for readability. Terminology contributes here. "upshift" (L243 and elsewhere) is not a term I have encountered in the paleoclimate literature, and I suspect "shoaling" is intended. Likewise, "the shallow overturning circulation" (L244, L246) is never defined.

We will do as suggested, make a dedicated pass for readability and clarify the terminology.

6. Figures

General. Each figure should include text identifying the time slice ("mid-Holocene," "MIS 5e," "MIS 11c"). The manuscript alternates between time-slice names and experiment names, and consistent usage across figures and text would help the reader considerably.

We will make sure that all Figures provide the necessary time slice information. We will also ensure a consistent usage of time-slice / experiment names.

Figure 2. A point symbology legend is needed and the points should be larger. If overlap is a concern, experiment with opacity. Font size needs to be increased to match Figures 3 and 5.

We will add a point symbology to include this information within the figure. We will also check the size of the symbols in Fig. 2, as well as the fonts in Figs. 3 and 5.

Figures 3 and 5. The three-panel layout is effective but makes it difficult to compare across regions. Combining these into a single panel and using color to identify the regions would enable that comparison. Currently color identifies the time slices, which the x-axis already accomplishes.

We will do as suggested and organize the figures in a single panel using color to identify the SCS, WPWP and EEP areas (Figs. 2 and 3; which correspond to main text Figures 3 and 5). According to these changes, we will also adjust the color coding in main text Figures 1, 2 and 4.

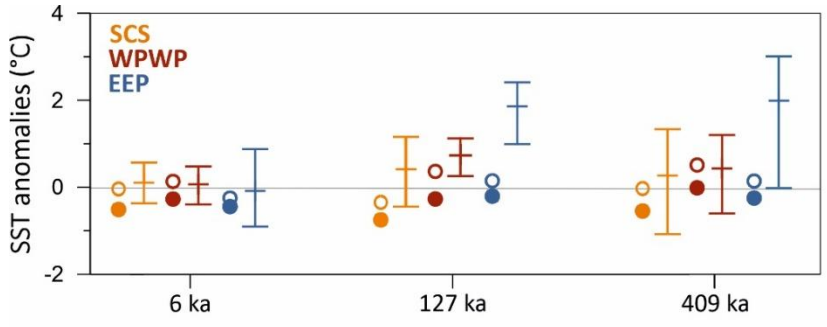


Figure 2. Comparison between interglacial proxy- and model-based SST anomalies in the SCS (orange), the WPWP (red) and the EEP (blue) during (a) the Holocene, (b) MIS 5e and (c) MIS 11c time slices. Bars indicate proxy-based assessments of computed mean SST anomalies and uncertainties (shown as median and 95 % confidence intervals), dots indicate SST anomalies derived from CESM runs without (filled) and with (open) expanded vegetation cover.

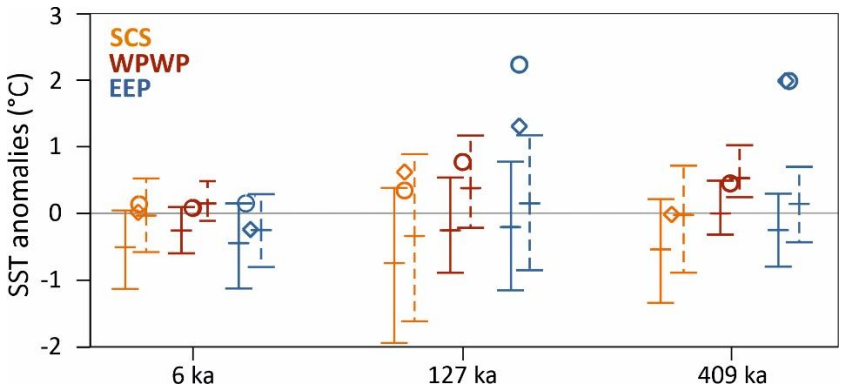


Figure 3. Comparison between reconstructed and simulated SST anomalies in the SCS (orange), WPWP (red) and EEP (blue) during mid-Holocene, MIS 5e and MIS 11c time slices relative to the preindustrial period. Bars indicate mean, minimum and maximum daily SST anomalies in CESM without (solid lines) and with (stippled lines) expanded NH vegetation cover. Symbols represent Mg/Ca- (dots) and UK′37 - (diamonds) based SST anomalies.

Figure 4. See major comment 4 regarding the choice of bars over box-and-whisker plots and the magnitude of the plotted uncertainties.

See our reply to comment 4.

LINE-BY-LINE COMMENTS

L23–L25: For the introduction, I would leverage the primary role the tropical Pacific and Indian Oceans play in global climate.

The referee's suggestion is not clear to us. In fact, we leverage the important role that the Pacific and Indian Oceans, and particularly the IPWP, play in global climate in lines 25-33.

L26: It is not clear what is meant by "most dynamic". please be more specific.

We agree and will omit the term.

L33: The logical step from "the IPWP is important" to "we need to study it on long timescales" is missing and needed.

Despite its importance for global climate, our knowledge about the spatiotemporal SST variability across the low-latitude Indo-Pacific and its underlying forcings is still limited and reconstructions that extent the observational period are therefore needed. To improve projections of regional SSTs during future global warming, a thorough reconstruction of the regional SSTs, their spatiotemporal variability, and their role in climate dynamics during previous warm, i.e., interglacial periods, is thus important. We will add a short statement with this information to the introduction.

L33–L56: These paragraphs appear intended to both motivate the need for more data and provide an overview of the literature, but two issues make them hard to follow. First, the connection between the IPWP and the Holocene Conundrum is not made. Second, seasonal biases and model deficiencies are both raised as causes, but their relationship to the CESM simulations presented here is not drawn.

In this paragraph, we summarize the outcome of global proxy-model compilations that focused on the mid-Holocene, MIS5e or MIS 11c. The mismatch in the evolution of SSTs across the Holocene is well known as Holocene Conundrum and is also apparent across the low-latitude Indo-Pacific Ocean. We submit that this connection exists but should not be artificially inflated.

We introduce that model deficiencies include the insufficient representation of vegetation. We tackle this issue in our simulations and integrate an improved vegetation scheme. We will slightly rephrase our introduction to clarify this relation in the revised version of the manuscript. We will also clarify that we take seasonal and interannual proxy biases into account.

L115–L118: Rather than a separate sentence on the PMIP alignment, consider appending "to align with Paleoclimate Modelling Intercomparison Project (PMIP) experiments."

Will do

L133: Consider changing "The reconstructed SST anomalies vary from site to site. To get a better idea of regional SST anomalies," to "We investigate regional SST anomalies by averaging proxy records in the Western Pacific Warm Pool (WPWP), the South China Sea (SCS), and the eastern equatorial Pacific (EEP)."

Will do

L174–L181: Beginning the paragraph with caveats limits the impact of the results that follow. If the text is retained here, adding a new preceding section titled "Proxy Compilation Spatial Characteristics" would partly address this. I would instead suggest moving the text elsewhere, for example to Section 3.4.

We will do as suggested and move the paragraph to section 3.4.

L182–L184: Here and elsewhere, giving the proportion of sites showing warming in the Pacific as a parenthetical would be helpful.

We thank the referee for this suggestion and will provide the proportion of sites warming in parenthetical, wherever it is helpful.

L208–L210: This sentence is unclear, different in what respect?

We will specify this and replace “different” by “SST anomalies of different magnitudes”.

L211: "partly very low" reads as contradictory; either remove "partly" or revise the sentence.

We will do as suggested and revise the sentence. We will state: “This should be interpreted with caution due to the very low number of records included in some of the area mean anomalies […]”.

L219–L230: This paragraph presents both a key finding (warm anomalies unrelated to insolation or GHGs) and a key hypothesis analyzed later (the role of the AMOC). Moving "Hence, the warm anomalies are not related to local insolation forcing and varying GHGs, but to the astronomically modulated distribution of insolation and its feedback mechanisms" to after "While local effects might also play a role — such as precession-controlled changes in upwelling intensity (Lückge et al., 2009) — the consistency of surface ocean warming across the entire study area points to common forcings" would make the transition to the AMOC considerably clearer.

Will do

L243: Here and elsewhere, I believe "upshift" should be "shoaling," though I am not certain in the context of this sentence.

We adopted this term from (Toggweiler & Si, 2025). His concept of the AMOC upshift describes how the AMOC’s return flow rises to the surface as it passes through the tropics. In contrast to classical

upwelling, the upshift is not a wind-driven process, but part of the AMOC. We will clarify the term in the revised version of the manuscript.

Toggweiler, J. R., & Si, W. (2025). A New Role for the AMOC During the Ice Ages. Paleoceanography and Paleoclimatology, 40(10), e2024PA004973. https://doi.org/https://doi.org/10.1029/2024PA004973

L244 and L246: "The shallow overturning circulation" is never defined. Is this referring to the subtropical cells?

Yes, the shallow overturning circulation refers to the subtropical cells. We will clarify this in the revised version of the manuscript.

L281: Here and elsewhere, when presenting modeling results, it is unclear which experiment this sentence refers to, and whether the paragraph discusses the "without expanded NH vegetation" or "with expanded NH vegetation" case.

Where necessary, we specified which experiment we refer to (lines 276).

L297: Briefly restating the proxy-based interpretation of SST anomalies here would keep the reader in the text rather than jumping back to earlier sections.

Done (lines 293-294). We now state that “Sensitivity experiments with fixed (PI) GHGs show that varying orbital parameters result in extended areas of positive SST anomalies during the investigated time slices, supporting our proxy-based interpretation that the observed SST anomalies are related to the astronomically modulated distribution of insolation and its feedback mechanisms”.

L345: "The different SST anomalies lead to contrasting inferences about the atmospheric circulation" — the model output could resolve this directly (see major comment 1).

See our reply to major comment 1 of the referee.

L377–L379 and L390–L391: As written, it is unclear which model output is being used. Monthly minima and maxima? Daily values? Seasonal means?

We refer to daily values. We will clarify this in the text, which will then read “we compare the proxy-based mean area SST anomalies of the SCS, WPWP and EEP to the corresponding seasonal SST anomalies, i.e. minimum and maximum daily SST, inferred from CESM (Fig. 5)” (lines 377-379; Figure Caption Fig. 5).

L434: "the regional SSTs" refers back to the earlier regional averages, but by this point the reader will have forgotten them. Please write them out again.

In the revised version of the text, we will explicitly state that “the SSTs of the SCS, WPWP and EEP areas during MIS 5e and MIS 11c were generally higher than during the PI” (lines 437-438).

L435: Without a direct measure of the PWC, reduced zonal SST gradients suggest, rather than indicate, a weakening.

We will do as suggested and change the wording.