Reviewer 3:

The centrepiece of this paper is a careful compilation of SSTs across MIS7. This is an excellent effort, very valuable for the community and presenting some interesting insights. There are always going to be issues with the methodologies of such a compilation (to do with choices of proxies, timescale alignments and averaging methods) but the authors here have generally chosen sensible compromises and have explained well the decisions they took. The rationale for reconstructing MIS7 by itself with the added resolution and number of useable datasets that this brings is well-taken. I am therefore confident that this paper should appear in CP. I do have a lot of comments (reflecting my interest and a wish to see everything explained precisely) and suggestions below but they are mainly minor.

We thank Reviewer 3 for their positive assessment of the study.

There are perhaps two somewhat more overriding issues that the authors should think about (after I completed my review I found that they chimed with comments by rev 2):

1. While I agree that using their methods temperatures are slightly higher in 7c than in 7e at global scale, the difference is within the uncertainty, and is determined only by an increase in the north and equatorial Pacific (Fig 5). This seems quite tenuous to be one of the main conclusions highlighted in both the abstract and the conclusions. I think it should be downplayed somewhat and at least the fact that the two stages are within uncertainty should be mentioned.

We have nuanced the comparison between MIS 7c and MIS 7e in the revised manuscript. Now read, in the revised abstract, lines 26-28: (…) *The warmest phase across MIS 7 occurs during the MIS 7c substage, although the global surface temperature difference compared to MIS 7e is within uncertainty of the reconstruction (around 0.4 ±0.4 °C warmer).* (…) And in the revised conclusion, lines 696-698: (…) *At the global scale, MIS 7c ~~is the warmest period of the MIS 7 sequence~~ appears comparable to, or slightly warmer than, MIS 7e.* (…)

2. I also found section 4.3 difficult, particularly with respect to orbital parameters that have very few degrees of freedom. I will discuss below but I think this section is overdone and should be reconsidered.

Based on the Reviewer 2 and Reviewer 3 comments, we decided to remove the correlation analysis performed in the section 4.3 in the new version of the manuscript.

Provided these and other minor comments below are addressed I would recommend the paper should be published: it will be a very valuable resource for the community. The editor should ensure that the datasets forming various outputs (the stacks shown in Fig 4) are publicly available at the time of publication.

Detailed comments:

Line 27 "The warmest phase across MIS 7 occurs during the MIS 7c substage". This should be caveated with something like "although the difference from the temperature of MIS7e is within uncertainty of the reconstruction, and the warmth is confined to the south and tropical Pacific."

We followed Reviewer 3's recommendation and have nuanced our statement accordingly. However, we chose not to mention the regional aspect in the abstract, as this level of detail seems too specific for that section. Now read in the revised manuscript, lines 26-28:

(…) *The warmest phase across MIS 7 occurs during the MIS 7c substage, although the global surface temperature difference compared to MIS 7e is within uncertainty of the reconstruction (around 0.4 ±0.4 °C warmer).* (…)

Line 72. "MIS 7c may or may not be classified as an interglacial period". I am a bit confused by this. Under what kind of definition would you not classify it when it is (by your own analysis) a little warmer than 7E and separated from it by a -80 m SL stand (in Spratt and Lisiecki at least). I think you are creating a controversy that doesn't really exist here.

We agree that, under a sea-level definition of interglacial, MIS 7c clearly qualifies as an interglacial, being separated from MIS 7e by a substantial (~–80 m) sea-level lowstand and associated with a distinct deglacial transition. Our intention was not to create an artificial controversy, but to acknowledge that its classification can depend on the metric used. In particular, definitions based on CO_2 thresholds (e.g., ≥260 ppm) may exclude MIS 7c, as its CO_2 concentrations remain lower than those of other interglacials (Past Interglacials Working Group of PAGES, 2016). However, under sea-level or benthic $\delta^{18}O$–based definitions, and in most syntheses of late Pleistocene interglacials, MIS 7c is indeed classified as an interglacial.

We have adapted the text to nuance our idea. Now read in the modified manuscript, lines 76-78:

(…) *Due to its relatively modest CO_2 concentrations, MIS 7c has occasionally been excluded from interglacial lists under stricter CO_2-based definitions (Tzedakis et al., 2009, 2012).. However, under sea-level and benthic $\delta^{18}O$ criteria, it is classified as a distinct interglacial (Past Interglacials Working Group of PAGES, 2016).* (…)

Line 130 and around. I was a bit surprised that you used del18O_P as a temperature proxy, but I appreciate that you do consider removing it later. I wonder if you should add a sentence though explaining that it is considered a proxy of temperature and hydrography (salinity) so should be treated with caution (which is why people go to so much trouble to derive specific T-proxies such as Mg/Ca).

We apply the suggestion of Reviewer 3. Now read, in the revised manuscript lines 159-163:

(…) *We decided to include $\delta^{18}O_p$, although this proxy reflects both temperature and hydrographic changes, notably variations in seawater $\delta^{18}O$ linked to salinity. While we correct $\delta^{18}O_p$ for seawater $\delta^{18}O$, SST estimates derived from this proxy should still be interpreted with caution. We therefore also provide alternative global and regional stacks excluding these records from the synthesis (Fig. 3).* (…)

Same area and Table S1. Generally you treat MAT as seasonal and all others as annual. But then for ODP8982 you give UK37 as seasonal even though it is treated as annual at all other sites. Is this an error? If not you should probably justify it.

We understand that this can be confusing. The seasonal MAT data are treated as seasonal directly from the initial author's description in the original study. As the UK'37 records have been calibrated from the original data with the Tierney and Tingley (2018) BAYSPLINE calibration, we applied all authors recommendations. For the case of ODP-982, it is located above 48°N in the North Atlantic and authors (Tierney and Tingley) suggest that UK'37 measurements in this area reflect a August-September-October signal rather than an annual average.

Now read, in the revised Supplementary Materials, in the caption of Table S1, lines 155-157:

(…) *All UK'_{37} records are considered to reflect annual conditions, except for ODP-982. Due to its specific location above 48°N in the North Atlantic, and following the authors' recommendation, this record is interpreted as reflecting seasonal variations (Tierney and Tingley, 2018)* (…)

Line 173. While I appreciate the point being made about methane I think in general it's been shown that the sharp transitions in methane do correspond rather well to millennial variability and Greenland temperature within a very small time lag. I suggest you show GL-SYN on the same figure as methane so we can judge for ourselves whether GL-SYN is acceptable for this purpose.

We thank Reviewer 3 for their suggestion, and we have now updated Fig. 1 with the GL-SYN curve in the new version of the manuscript.

Line 194. "the randomly gridded SSTs (2-5°) and then the randomly latitudinal SSTs (2.5-10°)". I don't understand what this means, please explain what you have done.

We thank Reviewer 3 for pointing out the lack of clarity. We have revised the sentence to explicitly describe the three-step averaging procedure used in each Monte Carlo iteration. Briefly, for each 500-year time bin: (1) all SST ensemble values available at a given site are averaged (independent of proxy type), (2) site averages are grouped into a spatial grid whose resolution is randomly selected between 2° and 5°, and averaged within each grid cell, and (3) grid-cell averages are further averaged within latitudinal bands whose width is randomly selected between

2.5° and 10°. This hierarchical averaging procedure limits the influence of proxy replication at individual sites and uneven spatial site distribution.

Now read in the new version of the manuscript, lines 210-214:

(…) *To reduce spatial bias, we apply a three-step averaging procedure within each Monte Carlo iteration and for each 500-year time bin: (1) we first average all SST ensemble values available at each site, regardless the proxy type; (2) we then average these site means within a randomly defined spatial grid (grid size randomly selected between 2° and 5°); and (3) we finally average the gridded values within randomly defined latitudinal bands (band width randomly selected between 2.5° and 10°).* (…)

Line 196 "weighted mean". Do you mean that they were weighted by area or by uncertainty? I assume the former but you should explain. For completeness. should you also mention that there are no sites north or south of 60 degrees so polar amplification is only represented in the derived GMST record, while the GSST records are only extrapolar averages.

By "weighted mean", we refer to a latitude-weighted mean, not a weighting by uncertainty. Each latitudinal band is weighted according to its proportional surface area (i.e., accounting for the cosine of latitude), such that, for example, the 60–55°S band represents a much smaller surface area than the 0–5° band. All regional and global stacks are calculated using this latitude-based area weighting. Now read, in the revised manuscript, lines 216-217:

(…) *All stacks were defined as the weighted mean of all latitudinal band averages, with each band weighted according to its proportional surface area (i.e., cosine of latitude).* (…)

We also agree that it is important to specify the latitudinal coverage of the dataset. Now read in the revised manuscript, lines 217-222:

(…) *The global mean surface air temperature (ΔGMST) is defined as the global SST anomaly (ΔGSST) using a random factor (1.5-2.3) derived from climate model comparisons (Osman et al., 2021; Snyder, 2016; Stevenard et al., 2025). As no sites are located north or south of 60°, polar amplification is not directly represented in the ΔGSST stacks, which therefore reflect extrapolar mean conditions, whereas it is only accounted for in the derived ΔGMST reconstruction.* (…)

Line 207-8. I think this is not quite expressed right. It is not impossible for one of your stacks to be negatively correlated with an orbital parameter (for example one can imagine that SH temperature could be negatively correlated with NH summer insolation). But of course it's interesting to ee whether the correlation is positive or negative so this is the reason to show us r and r^2.

Based on the Reviewer 2 and Reviewer 3 comments, we decided to remove the correlation analysis performed in the section 4.3 in the new version of the manuscript.

Page 10. Just a presentational point but I would have found it helpful to be pointed at sectionns of Fig 4 regularly on this page, while at the top of the page the much less interesting Fig 5 is pointed at.

We have updated these paragraphs with references to Fig. 4.

Line 281. "TIIIa is characterized by a more gradual". Is it? It doesn't look more gradual to me, it's certainly smaller in amplitude but the gradient with time looks if anything steeper in TIIIa than in the early parts of TIII. Either give numbers to back this or alter the text.

Following the comment of Reviewer 3, we have removed the adjective gradual and only kept the mention of the amplitude. Now read in the revised manuscript, lines 312-313:

(…) *In general, TIIIa is characterized by a ~~more gradual and~~ less pronounced warming than TIII* (…)

Line 310: Again point to Fig 4?

We applied the suggestion of Reviewer 3.

Line 315 More precise would be "with the Northern Hemisphere starting to warm last". The tropics actually continue to warm after the others reach their peak.

We applied the suggestion of Reviewer 3.

Line 338. "MIS 7c stands out as the warmest interval over the MIS 7 sequence". Again needs a caveat about the uncertainty.

We applied the suggestion of Reviewer 3. Now read in the revised manuscript, lines 369-370:

(…) *MIS 7c ~~stands out as the warmest interval over the MIS 7 sequence~~ appears comparable to, or slightly warmer than, MIS 7e (about 0.4 ± 0.4°C warmer),* (…)

Line 348. "MIS 7c emerges as the warmest period of MIS 7 (+0.4 ±0.4°C and +0.2 ±0.2°C, respectively)". You need to clarify that those numbers are the difference between 7c and 7e not the absolute temperatures.

We applied the suggestion of Reviewer 3. Now read in the revised manuscript, lines 378-380:

(…) *In the GMST and GSST stacks, MIS 7c is of similar intensity or slightly warmer than MIS 7e (with 0.4 ±0.4°C and 0.2 ±0.2°C ΔGMST and ΔGSST positive anomaly compared to MIS7e)..* (…)

Line 425. "sea-level reconstructions suggest higher stands during MIS 7c than during MIS 7e". I agree, but this is perhaps a slightly different case compared to the temperature reconstructions, because one would expect radiative forcing or temperature to lead to loss of ice, and because we start from a higher glacial SL stand in 7d than in 8, the same radiative forcing would of course lead to a higher SL stand in 7c than 7e. I don't think this is surprising.

We agree with the comment of Reviewer 3 and have nuanced this section of the text. Now read in the revised manuscript, lines 458-461:

(...) *Furthermore, sea-level reconstructions suggest higher stands during MIS 7c than during MIS 7e (Berends et al., 2021b; Spratt and Lisiecki, 2016) (Fig. 1h), reinforcing the interpretation that MIS 7c was not only warmer globally but also associated with a reduced global ice volume. However, this higher peak may also reflect the fact that MIS 7c followed a higher glacial sea-level baseline than MIS 7e (Fig. 1h).* (…)

Page 16. I didn't find this discussion convincing. Earlier on you explain that the CO2 high in 7e and 9 is transient, with the very high concentrations lasting only centuries. While your data have an average resolution of 1.7 ka. Isn't it possible that you just aren't seeing rapid SST excursions, or that the CO2 increase was too short for the SSTs to reach any equilibrium.? Your explanation involving sea ice and obliquity seems very tenuous and speculative.

We thank Reviewer 3 for this comment. The atmospheric CO_2 overshoots discussed here last approximately ~2 kyr (MIS 7e) and ~4 kyr (MIS 9), which is longer than the mean temporal resolution of our global SST dataset (~1.7 kyr). Moreover, in the highest-resolution records (<1 kyr resolution), no clear SST overshoot synchronous with the CO_2 peak is observed, suggesting that the absence of a pronounced SST response is not solely an artefact of temporal smoothing. We nevertheless agree that the short-lived nature of the CO_2 increase raises the possibility that the climate system did not reach equilibrium.

We agree that the highest CO_2 concentration reported for MIS 7e corresponds to a short-lived overshoot rather than sustained interglacial background levels. Figure 7 explicitly addresses this issue by testing alternative 3-kyr averaging windows for MIS 7e (and MIS 9e), either including or excluding the overshoot (panel A). When the overshoot is included (yellow symbols in panel C), MIS 7e and MIS 9e clearly deviate from the linear GMST–CO_2 relationship defined by the other interglacials. This offset reflects transient decoupling between atmospheric CO_2 and global surface temperature: CO_2 reaches very high values without a proportional increase in GMST. However, when the overshoot is excluded (light-blue symbols), the window-averaged values for MIS 7e fall within a range comparable to MIS 7c and align closely with the regression defined by the other interglacials. This indicates that the decoupling does not characterize the sustained interglacial state, but rather the brief overshoot phase. In other words, the decoupling exists but it is confined to the overshoot interval and not to the whole interglacial. It does not imply a fundamentally different equilibrium relationship between radiative forcing and temperature during MIS 7e, but only during the overshoot, also occurring at the onset of the MIS 9e. We revised the manuscript to clarify that the mentioned radiative decoupling relates to the interval during which the overshoot peak occurs and to better distinguish between transient peak values and sustained mean interglacial conditions.

Now read in the revised manuscript, lines 462-470:

(…) *From a radiative forcing perspective, the greenhouse gas difference between MIS 7e (~275 ppm CO_2, ~700 ppb CH_4) and MIS 7c (~245 ppm CO_2, ~600 ppb CH_4) corresponds to an estimated radiative forcing of ~1 W/m², which would produce a global temperature change of ~0.8°C (Hansen et al., 2011; Past Interglacials Working Group of PAGES, 2016). Given that MIS 7e was cooler than MIS 7c by 0.4°C despite higher GHG concentrations, this implies an anomalous offset of ~1.2°C between the two periods, suggesting a decoupling between radiative forcing and global temperature response. However, it is important to note that the high CO_2 concentrations observed during MIS 7e are confined to its very beginning (approximately 2 kyr), forming a brief overshoot. After this overshoot, CO_2 concentrations no longer exceed those recorded during MIS 7c (Legrain et al., 2024).*

High-resolution records of CO_2 concentrations and GMST estimates are now available over MIS 1 (Bauska et al., 2021; Osman et al., 2021), MIS 5 (Hoffman et al., 2017; Landais et al., 2013), MIS 7 (Legrain et al., 2024; this study), MIS 9 and MIS 11 (Clark et al., 2024; Nehrbass-Ahles et al., 2020; Stevenard et al., 2025). When comparing the average maximum CO_2 concentrations period (3 kyr window following the maximum CO2 value) with GMST over the same period, it reveals that for MIS 7c, the atmospheric CO_2 concentrations are within the range of expected values considering the GMST over this period and the CO_2 – GMST relationship of the past interglacials (Fig. 7). However, both MIS 7e and MIS 9 evidence anomalously high CO_2 concentrations of 31 ±14ppm and 41 ±13ppm (1σ), respectively, compared to the expected CO_2 concentrations value based on their GMST (Fig. 7b). However, these two interglacials are characterized by the most pronounced overshoots of atmospheric CO_2 concentration of the past 500 ka, which consist in an abrupt increase followed by a similarly abrupt decrease of atmospheric CO_2 concentrations at the end of a Termination (Bereiter et al., 2015; Legrain et al., 2024; Nehrbass-Ahles et al., 2020). Over MIS 7e, atmospheric CO_2 concentrations increase and drop by 30 ppm in 2,000 years, while during MIS 9 concentrations fluctuate by 35 ppm in 4,000 years. In contrast, when considering a 3 kyr window immediately following the CO_2 overshoot, once CO_2 levels have stabilized at typical interglacial equilibrium values, MIS 7e and MIS 9e fall in line with the other interglacials. These results suggest that during the overshoot phase, the global climate does not fully equilibrate with CO_2 concentrations, leading to a transient radiative decoupling. However, once the overshoot has subsided and CO_2 follows a more stable interglacial trajectory, this decoupling disappears. (…)

Finally, based on these new analyses, we decided to remove the final discussion paragraph of this section regarding the hypothesis of the role of sea ice to explain this decoupling.

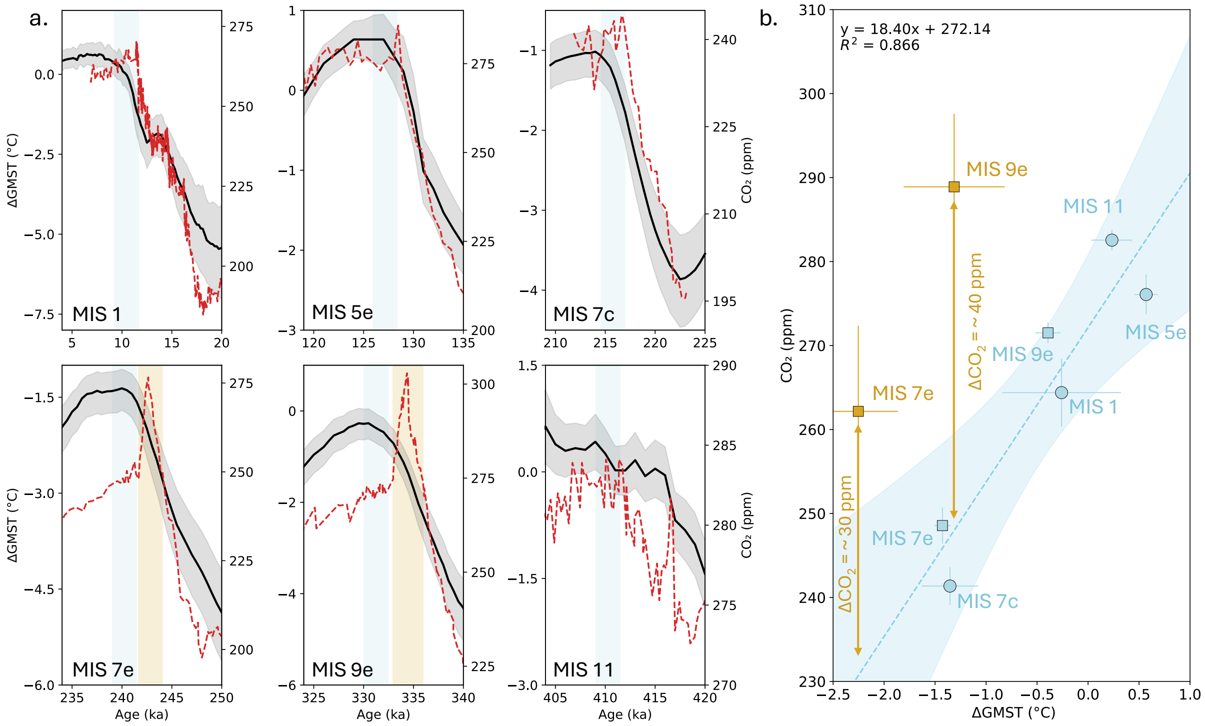

Fig. 7: Relationship between GMST and atmospheric CO_2 across the interglacials of the past 450 ka (a) Global mean surface temperature (GMST; black line with 1σ grey envelope) and atmospheric CO_2 concentrations (red dashed line) for MIS 1 ((Bereiter et al., 2015; Osman et al., 2021) , MIS 5e (Bereiter et al., 2015; Clark et al., 2024), MIS 7c and MIS 7e (this study, Legrain et al., 2024), MIS 9e (Nehrbass-Ahles et al., 2020; Stevenard et al., 2025), and MIS 11 (Clark et al., 2024; Nehrbass-Ahles et al., 2020). The timescale used for atmospheric CO_2 records is the AICC2023 chronology (Bouchet et al., 2023). Shaded vertical bands indicate the time windows used to compute the window-averaged values shown in panel (b). For MIS 1, MIS 5e, MIS 7c and MIS 11, light blue bands correspond to the 3kyr time windows following CO_2 concentrations peak. For MIS 9e and MIS 7e, blue/yellow bands denote alternative 3kyr windows including/excluding the CO_2 overshoot. (b) Dots and square represent window-averaged values derived from the intervals shown in panel (a), with horizontal and vertical error bars indicating the standard deviation of GMST and atmospheric CO_2 concentrations within each window. The dashed line shows the best-fit linear regression, with blue shading indicating the 95 % confidence interval of the predicted mean response. Light blue squares and dots represent interglacials values included in the regression. Yellow squares correspond to values calculated including the CO_2 overshoot for MIS 7e and MIS 9e and are excluded from the regression.

Line 510. "average r-pearson of 0.84 and 0.89". Please explain what you mean by the average, do you mean the average r value for the maximum correlation of each stack? Seems a slightly strange thing to average…

Based on the Reviewer 2 and Reviewer 3 comments, we decided to remove the correlation analysis performed in the section 4.3 in the new version of the manuscript.

Line 513. This says the optimum is at 4100 years but on the figure it shows 3.4 kyr.

Based on the Reviewer 2 and Reviewer 3 comments, we decided to remove the correlation analysis performed in the section 4.3 in the new version of the manuscript.

Line 520 and following paragraph. I am not at all convinced by this comparison with orbital parameters. You get a strong r with eccentricity across all lags and yet it is obvious that this is because ecc changes very slowly and weakly and is essentially just a single sine curve . No-one looking at ecc and the data in Figs 1 and 4 would see a meaningful correlation, and I think the same more or less applies to the other orbital comparisons though it is less clear. I would honestly remove this part completely to avoid diluting the paper with a rather meaningless analysis (but I don't insist as long as it is clarified whether the correlations are actually significant given the very few degrees of freedom in the insolation curves).

Based on the Reviewer 2 and Reviewer 3 comments, we decided to remove the correlation analysis performed in the section 4.3 in the new version of the manuscript.

Page 20. The discussion is OK but I would tend to phrase it differently. Surely in the 100 kyr world we are in a state where the internal timescale of the system is long and it takes a huge amplitude of insolation to overcome that and melt an ice sheet.

It is unclear to us which specific sentences this remark refers to. Therefore, we have not made changes to the revised version at this stage. We would be happy to revise the text accordingly if the reviewer could clarify their point and to which exact passage they refer to.

Line 641. Again you need to caveat the statement about 7c being warmer than 7e.

We applied the suggestion of Reviewer 3. Now read in the revised manuscript, lines 696-698:

(…) -*At the global scale, MIS 7c appears comparable to, or slightly warmer than, MIS 7e.* (…)