

### Reviewer 1:

“The Marine Isotope Stage 7: A relic of the 41-ka world?” by Legrain et al provides a valuable compilation and summary of sea surface temperature (SST) spatio-temporal variability during MIS 7, which after revision would make an excellent contribution to *Climate of the Past*. The comparison with the Clark et al (2024) SST compilation is particularly interesting. Below are some details I would like to see clarified in the manuscript before publication.

We thank Reviewer 1 for their positive assessment of the study.

Major comments:

Methods:

Line 151: The LR04 benthic d18O curve is affected both by ice volume and deep water temperature. Due to a lag between temperature and ice volume, benthic d18O is not synchronous with ice volume change. The Spratt & Lisiecki (2016) sea level stack or the new Clark et al sea level reconstruction would be more suitable to use for ice-volume correction.

We thank Reviewer 1 for the careful attention on SST calibration and agree with the idea of using more recent information on ice volume. However, the ice volume correction included in the BAYFOX calibration requires a  $\delta^{18}\text{O}_{\text{ice}}$  value (estimate of global ice volume change in  $\delta^{18}\text{O}$  for seawater) rather than a sea-level value (see Appendix B in Malevich et al., 2019). We therefore decided to retain the LR04 value as the reference value for correcting the ice volume for  $\delta^{18}\text{O}_{\text{p}}$  calibration.

Lines 241-245: Similarity between the results of “selected” and “all” records is not a particularly good argument in favor of using the selected records compilations because the set of all records is acknowledged to be impacted by bias, particularly from the fact that many planktonic d18O records may be impacted by salinity changes. The compilation that excludes all planktonic d18O records produces a noticeably sharper and warmer peak for MIS 7e, especially in the SH stack. Is there anything particular about the spatial distribution of records that have been removed that makes you consider the d18O<sub>p</sub>-excluded compilation inaccurate? Would using the smaller compilation alter any of your main conclusions?

We thank Reviewer 1 for their comment. We agree that the similarity between the “all” and “selected” stacks is not a sufficient argument to justify the use of the selected compilation. The records excluded from the “selected” compilation were removed because they exhibit trends that are markedly inconsistent with regional patterns. From this perspective, we consider their exclusion justified. The similarity between the two approaches is noted to underscore the minimal influence of selecting one stack rather than the other. Regarding the compilation excluding all d<sup>18</sup>O<sub>p</sub>-derived SST records, we acknowledge that it produces a sharper and warmer peak for MIS 7e in the Southern Hemisphere stack. This difference is primarily related to the strong reduction in data coverage in the South Atlantic. When d<sup>18</sup>O<sub>p</sub>-derived SST records are excluded, the number of annual SST records in this region decreases from 15 to only 5. The resulting stack is therefore based on a much more limited spatial sampling, which amplifies the influence of a few individual records and likely explains the sharper and warmer peak observed in the Southern Hemisphere. Using the smaller compilation excluding d<sup>18</sup>O<sub>p</sub> records would not alter our main conclusions regarding the relative structure, timing, and large-scale features of MIS 7e. The differences mainly affect the amplitude and sharpness of the MIS 7e peak in specific regions, rather than the overall MIS 7 evolution or inter-basin phasing. To ensure full transparency and facilitate broader use of the dataset, we will make all three stacking approaches (“all”, “selected”, and d<sup>18</sup>O<sub>p</sub>-excluded) available in the final publication and associated database. This approach will allow readers to explicitly assess the sensitivity of the results to the inclusion or exclusion of specific proxy types and to select the compilation most appropriate for their intended application. A similar strategy was adopted in our recent MIS 9 synthesis (Stevenard et al., 2025), where the three different stacking approaches were also provided.

We have revised the manuscript to clarify these points and to better justify the rationale for retaining the “selected” compilation as the primary reference stack. Now read in the revised manuscript, lines 268-275:

(...) Excluding all  $\delta^{18}O_p$ -based SST records (2) nearly halves the number of available records and reduces the spatial coverage of the synthesis prior to stacking, particularly in the South Atlantic sector of the Southern Hemisphere. The “selected” stack (3) represents a compromise approach. Records were excluded only when they displayed trends markedly inconsistent with regional patterns, while most of the available data were retained. The similarity between the “all records” (1) and “selected” stacks indicates that the main large-scale features, structure, and timing of MIS 7 are robust to the choice of compilation. We therefore use the “selected” stack as our primary reference for description and interpretation, while making all three stacking approaches (“all”, “selected”, and  $\delta^{18}O_p$ -excluded) available to allow explicit assessment of the sensitivity of the results to proxy selection. (...)

#### Chronologies:

To what extent is the relative timing of NH SST change and SH SST change set directly by the Greenland and Antarctic alignment targets? To help readers evaluate this, please add the GLT\_syn time series to right panel of Fig. 1 and summarize the assumptions inherent in its construction. Are similar regional shifts in SST timing observed when SST and benthic d18O are compared in individual cores? (The timing of benthic d18O changes can vary by 2-4 kyr during terminations also, which would be useful to also mention.)

We acknowledge that the chronological framework is anchored to Greenland and Antarctic ice core records, which necessarily constrains the timing of major climatic transitions between hemispheres. However, the alignment tie-points are defined at large-scale transitions and do not prescribe the internal evolution of SST within the interglacial interval. The timing of peak SST conditions in each hemisphere therefore represents the marine records themselves rather than being directly imposed by the targets. Importantly, age-depth uncertainties are propagated using a Bayesian model. The inferred NH–SH phasing is thus evaluated across a distribution of plausible chronologies, ensuring that the interhemispheric offset is not an artefact of a single deterministic alignment. This alignment strategy is based on the working assumption of near-synchronous high-latitude air–sea temperature changes, consistent with previous interglacial syntheses (e.g. Capron et al., 2014, Hoffman et al., 2027; Stevenard et al., 2025).

Are cores from the North Pacific aligned to the synthetic Greenland record? If so, the authors should discuss the degree to which North Pacific SST is expected to be synchronous with Greenland air temperature. For example, do they vary synchronously with one another during T1 and the Holocene?

We thank Reviewer 1 for highlighting this important point regarding the alignment strategy, which was insufficiently documented in the previous version of the manuscript. A dedicated paragraph has now been added to the Supplementary Materials to clearly describe the alignment targets for each oceanic basin. Now read in the revised Supplementary Materials, lines 133-143:

(...) Chronologies were established by aligning oceanic basins onto ice core and speleothem archives, that differs among the considered basins. In the North Atlantic and Mediterranean basin, the records are aligned to the Greenland Synthetic Curve (Barker et al., 2011). In the North and Equatorial Pacific basin, the alignment procedure differs slightly between sub-basins. The North- and Equatorial-East Pacific records are aligned to the synthetic Greenland temperature record (GLT\_syn). In contrast, the North- and Equatorial-West Pacific records are aligned to a reference record, itself aligned to the Sanbao speleothem record (Cheng et al., 2016). In the South Atlantic, South Pacific and Indian Ocean basins, the records are aligned onto reference record, themselves aligned to the EPICA Dome C ice isotopes records (Jouzel et al., 2007). Finally, the Caribbean records are aligned through a stepwise procedure. Core MD02-2575 was first aligned to U1385 using benthic  $\delta^{18}O$ , as direct alignment of SST signals with North Atlantic SST records was not feasible due to large discrepancies between the records. Subsequently, ODP-999 and ODP-1002 were aligned to MD02-2575 using both Mg/Ca and  $\delta^{18}O$  SST records. (...)

For Termination I and the Holocene, some studies suggest broadly coherent millennial-scale variability between the North Pacific and Greenland (Praetorius et al., 2014). The relationship at sub-millennial scale remains more elusive. However, our strategy of alignment do not assume perfect synchronicity and rather relies on the reproducibility of major transitions and large-scale variability patterns. To our knowledge, there are no dedicated

studies specifically assessing the phase relationship between North Pacific variability and Greenland air temperature over MIS 7. However, based on our observations for both stages (MIS 7 and 9), the North-East Pacific SST records display variability that broadly mirrors the large-scale structure seen in GLT\_syn, within the limits imposed by marine sediment core resolution and chronological uncertainty.

Fig. S4 – Alignments to core U1429: I have concerns that U1429 is used as an alignment target for other cores given that it has a large gap immediately before (during?) TIII. Could the authors align those cores to a different core? Also, Fig S4 suggests that the age model for U1429 is based on alignment to Sanbao, which isn't mentioned as an age model target in the main text.

We thank Reviewer 1 for pointing out this omission. We have now mentioned the Sanbao  $\delta^{18}\text{O}$  stack as an age model target in the main text. Now read in the revised manuscript, lines 183-188:

*(...) At Site U1429 (the reference for Northwest Pacific), we synchronized the planktic  $\delta^{18}\text{O}$ \_notched record of *G. ruber*, which is the  $\delta^{18}\text{O}$  signal with eccentricity- and obliquity-band components removed (Clemens et al., 2018) interpreted as a proxy for East Asian monsoon variability, with the  $\delta^{18}\text{O}_{\text{calcite}}$  stack from the Sanbao Cave (Cheng et al., 2016). The Sanbao  $\delta^{18}\text{O}_{\text{calcite}}$  series was already been placed on the AICC2023 timescale by linear interpolation between the tie points defined by Bouchet et al. (2023). (...)*

Regarding the data gap around ~245–248 ka, no tie points are defined within this interval. Thus, this gap does not affect the alignment strategy, as tie points are placed outside this interval and the alignment is constrained by surrounding sections.

Interpretation:

Line 446: Is the time resolution of the data sufficient to evaluate the SST response to a 2-kyr CO<sub>2</sub> overshoot? Low temporal resolution in many records and the smoothing effect of bioturbation may prevent you from observing a short-lived warming response to the CO<sub>2</sub> overshoot.

We agree that limited temporal resolution and bioturbation may attenuate short-lived (~2-4 kyr) signals in some marine records. However, in the highest-resolution SST records (<1 kyr resolution), we do not observe a distinct temperature overshoot synchronous with the CO<sub>2</sub> peak (Figs. S1, S2 and S3). While some smoothing cannot be excluded, the absence of a clear warming excursion in the best-resolved records suggests that a pronounced and widespread SST overshoot is unlikely.

Lines 580-585: The manuscript discusses whether the mode of glacial climate dynamics shifted during MIS 7 due to the particularly strong obliquity forcing at the time. One way to evaluate whether the double peak in MIS 7 actually implies a change in dynamics is to analyze the outputs of simple models that fit the pattern of glacial cycles using a single set of equations and parameter values for the entire late Pleistocene. For example, is a double peak for MIS 7 but not other glacial cycles predicted by the Parrenin & Paillard (2012) model or others? It might also be useful to compare the performance of models that are forced by 65N summer insolation with those that separately optimize sensitivity to precession and obliquity orbital parameters.

We thank Reviewer 1 for this suggestion. Literature provides mixed results on this question. Some conceptual models using a single set of equations and parameters for the late Pleistocene (e.g., Leloup et al., 2021; Imbrie et al., 2011) are able to generate complex or multi-peaked interglacials under particular orbital configurations, suggesting that MIS 7-like double peaks can arise without a fundamental shift in glacial dynamics. However, other studies (e.g. Parrenin and Paillard, 2012, Legrain et al., 2023) find MIS 7 more difficult to reproduce and emphasize its strong obliquity forcing as potentially requiring enhanced sensitivity to obliquity or modified feedback strengths. We understand that the comment points toward a model–data comparison. However, such an analysis would require a dedicated framework and a more detailed treatment of the model outputs, which is beyond the scope of the present study.

Minor comments:

Line 206: Rather than “inverse” (which often means 1/x), it would be better to say “multiplied by -1”

We applied the suggestion of Reviewer 1.

Line 256: awkward phrase – “display a very pronounced regional variability of the MIS 7”

Thanks for pointing this out. Now read in the revised manuscript, lines 286-289:

*(...) The regional  $\Delta$ SST stacks (Fig. 4e) of the different oceanic basins ~~display a very pronounced regional variability of the MIS-7~~ show marked differences in the timing and magnitude of MIS 7 at regional scale: their structure ~~is~~ can vary from a structure associated to three temperature peaks clearly identified as the warm substages MIS 7e, 7c and 7a (e.g. North Atlantic) to a weakly amplified signal (e.g. Equatorial Pacific). (...)*

Line 258: I think “low amplitude” would be clearer than “weakly amplified”

We applied the suggestion of Reviewer 1.

Line 307-308: “North of 23N” or “23-90N” would be clearer than “North to 23N” (same for south)

We applied the suggestion of Reviewer 1.

Line 309: This should be “Southern Hemisphere stack”

Thanks for pointing this out, we modified accordingly the manuscript.

Line 564-565: Missing words? “a global synthesis evidence warmer deep ocean temperature during MIS 7C, similar to what observed during Holocene”

Thanks for pointing this out. We modified the sentence accordingly. Now read in the revised manuscript, lines 617-619:

*(...) However, a global synthesis indicates that deep-ocean temperatures were warmer during MIS 7C, comparable to those observed during the Holocene. (...)*

Line 565: Insert a paragraph break before “Regarding termination mechanisms...”

We applied the suggestion of Reviewer 1.

Line 614-624: English usage needs revision

We rephrased the paragraph that now reads (lines 670-679):

*(...) In the Clark et al. (2024) synthesis, all terminations of the past 500 ka registered an early warming in the Southern Hemisphere compared to the Northern Hemisphere (Fig. 9b). In contrast, our results for TIIIa, which leads into MIS 7c, are marked by a near-synchronous SST rise across all latitudes, with no detectable interhemispheric phasing at the onset of warming (Fig. 9b). This globally uniform warming signal differs from the youngest terminations and suggests the involvement of other underlying dynamics, such as the role of the Agulhas Leakage (Stevenard et al., 2025; Fig. 9b). However, the analysis of recently published hemispheric SST stacks spanning the entire Pleistocene (Clark et al., 2024) reveals that a warming sequence characterized by an early Southern Hemisphere warming also appears to be a dominant feature of the 41-ka world. Some exceptions may occur during MIS 53 and MIS 41 (Fig. 9a), but chronological uncertainties for this interval remain large, and the analyses rely on only a small number of records (Clark et al., 2024). (...)*

Reviewer 2:

The manuscript by Legrain et al. provides a comprehensive and valuable synthesis of sea-surface temperature changes during the relatively understudied interglacial Marine Isotope Stage 7 (MIS 7). I find this to be an impressive piece of work, which makes a significant contribution to our understanding of climate variability during MIS 7. The study has some limitations that the authors acknowledge, but the main conclusions remain supported by the results. Most of my comments are therefore minor and relate to some points that in my opinion require some clarification.

We thank Reviewer 2 for their positive assessment of the study.

Specific comments:

L.350: “In contrast, the atmospheric CO<sub>2</sub> record shows a higher concentration during MIS 7e (275 ppm) than during MIS 7c (245 ppm), highlighting a partial decoupling between radiative forcing and global surface temperature variations.” Yes, but the highest CO<sub>2</sub> concentrations during MIS 7e only correspond to the overshoot. Mean CO<sub>2</sub> concentrations during MIS 7e are not higher than during MIS 7c but fall within a similar range.

Caption Fig. 7: CO<sub>2</sub> values correspond to maximum concentrations. How is the “climate optimum” defined? Does it correspond to the time of maximum CO<sub>2</sub> or maximum temperature, even if these are not synchronous? Would MIS 7e and MIS 9e align more closely with other interglacials if mean values were considered instead of peak overshoot values?

We agree that the highest CO<sub>2</sub> 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<sub>2</sub> relationship defined by the other interglacials. This offset reflects transient decoupling between atmospheric CO<sub>2</sub> and global surface temperature: CO<sub>2</sub> 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.

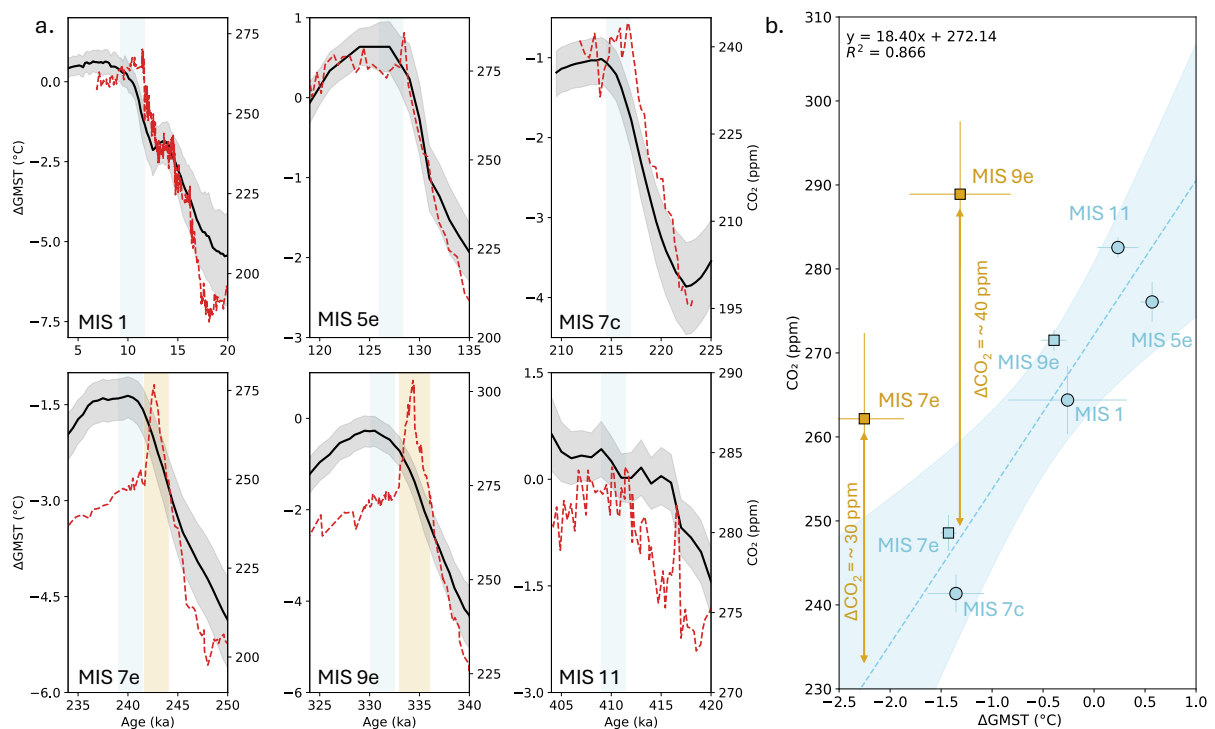
Now read in the revised manuscript, lines 462-489:

*(...) From a radiative forcing perspective, the greenhouse gas difference between MIS 7e (~275 ppm CO<sub>2</sub>, ~700 ppb CH<sub>4</sub>) and MIS 7c (~245 ppm CO<sub>2</sub>, ~600 ppb CH<sub>4</sub>) corresponds to an estimated radiative forcing of ~1 W/m<sup>2</sup>, 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<sub>2</sub> concentrations observed during MIS 7e are confined to its very beginning (approximately 2 kyr), forming a brief overshoot. After this overshoot, CO<sub>2</sub> concentrations no longer exceed those recorded during MIS 7c (Legrain et al., 2024).*

*High-resolution records of CO<sub>2</sub> 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<sub>2</sub> concentrations period (3 kyr window) with GMST over the same period, it reveals that for MIS 7c, the atmospheric CO<sub>2</sub> concentrations are within the range of expected values considering the GMST over this period and the CO<sub>2</sub>–GMST relationship of the past interglacials (Fig. 7). However, both MIS 7e and MIS 9 evidence anomalously high CO<sub>2</sub> concentrations of 31 ±14ppm and 41 ±13ppm (1σ), respectively,*

compared to the expected CO<sub>2</sub> concentrations value based on their GMST (Fig. 7b). However, these two interglacials are characterized by the most pronounced overshoots of atmospheric CO<sub>2</sub> concentration of the past 500 ka, which consist in an abrupt increase followed by a similarly abrupt decrease of atmospheric CO<sub>2</sub> 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<sub>2</sub> 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<sub>2</sub> overshoot, once CO<sub>2</sub> 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<sub>2</sub> concentrations, leading to a transient radiative decoupling. However, once the overshoot has subsided and CO<sub>2</sub> 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<sub>2</sub> across the interglacials of the past 450 ka** (a) Global mean surface temperature (GMST; black line with 1 $\sigma$  grey envelope) and atmospheric CO<sub>2</sub> 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<sub>2</sub> 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<sub>2</sub> concentrations peak. For MIS 9e and MIS 7e, blue/yellow bands denote alternative 3kyr windows including/excluding the CO<sub>2</sub> 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<sub>2</sub> 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<sub>2</sub> overshoot for MIS 7e and MIS 9e and are excluded from the regression.

L. 421: “In our  $\Delta$ GMST reconstruction, MIS 7c emerges as the warmest phase of the MIS 7 sequence, with global mean surface temperatures  $0.4 \pm 0.4^\circ\text{C}$  warmer than during MIS 7e.” Here, the uncertainty is as large as the inferred temperature difference. While I am not calling this result into question, it should be treated more cautiously. Taken together (i.e. CO<sub>2</sub> values excluding overshoot values and uncertainties in the temperature difference

between MIS 7c and 7e), the evidence for a strong decoupling between radiative forcing and temperature appears weaker than stated.

We agree with the comment of Reviewer 2. 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  $0.4 \pm 0.4$  °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. (...)*

Regarding the second part of the comment, which concerns the decoupling between radiative forcing and temperature, we refer Reviewer 2 to our response to the previous comment.

In my opinion Section 4.3 is less convincing than the rest of the manuscript. This section requires clearer explanations of the methodology and rationale behind using the entire MIS 7 interval, given that phasing with each orbital parameter varies greatly within the stage and that the processes leading to TIII and TIIIa are shown to be different. I strongly recommend adding a supplementary figure showing the global and regional SST stacks alongside orbital forcing. This is necessary to assess whether the reported correlations and lags are representative of both MIS 7e and MIS 7c. The method used to calculate the r-Pearson coefficient to estimate the lag must be more detailed in order to allow reproducibility.

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.

L. 510: MIS 7e does occur during declining obliquity as do other interglacials, but its phasing relative to the obliquity maximum differs from most other interglacials. As noted by the Past Interglacials Working Group of PAGES (2016): “The minimum in  $\delta^{18}O$  also always occurs within or just later than a quarter of an obliquity cycle after the obliquity maximum, with the notable exception of MIS 7c and 7e.”

We thank Reviewer 2 for pointing this out. We have now detailed and nuanced this part of the discussion. Now read, in the new version of the manuscript, lines 547-556:

*(...) While MIS 7e occurred during a declining phase of obliquity, consistent with most late Quaternary interglacials, its phasing relative to the obliquity cycle shows a deviation from the canonical pattern. In most cases, peak warmth (or minimum  $\delta^{18}O$ ) occurs within a quarter obliquity cycle following the obliquity maximum (Laskar et al., 2004; Past Interglacials Working Group of PAGES, 2016). MIS 7e appears somewhat later within the declining phase, closer to the subsequent obliquity minimum, but remains broadly aligned with the general pacing of interglacials. In contrast, MIS 7c is distinctly unusual: it is the only interglacial of the past 800 ka that coincides with, or occurs slightly before to, an obliquity maximum, which itself represents the highest obliquity value of the past 2 million years (Laskar et al., 2004; Past Interglacials Working Group of PAGES, 2016). (...)*

L. 520: “Regarding external orbital forcing, these analyses reveal a strong correlation with eccentricity (average r-pearson of 0.62), consistent with MIS 7 position within the 100-ka world.” The way this sentence is written implies that eccentricity is the main driver of the 100 ky cycle, which is inconsistent with the current debate. Eccentricity produces only weak insolation changes, so direct orbital forcing related to eccentricity of glacial-interglacial variability thought to be negligible and to influence glacial cycles mainly through modulation of precession (Raymo et al. 1997, Imbrie et al. 1984, Lisiecki, 2010). The physical significance of SST correlation with eccentricity, especially over a period of than one eccentricity cycle as in the case of MIS 7, may be discussed more cautiously.

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.

L. 536: “A conceptual modelling approach identified both precession and obliquity as crucial to constrain the timing of glacial terminations over the past 800 ka (Parrenin and Paillard, 2012).” Additional key references should be cited in this section, such as Huybers (Science 2006, Nature 2011), Huybers and Wunsch (Nature 2004) and Bajo et al. (Science 2020) for the role of obliquity in driving the glacial-interglacial cycles in the 100-ky and 41-ky worlds and for the timing of termination relative to obliquity and precession forcing in the 100-ky world.

We thank Reviewer 2 for their suggestions of additional references. Now read, in the modified manuscript, lines 588-590:

*(...) Conceptual modelling approaches and radiometric dating of glacial terminations identified both precession and obliquity as crucial to constrain the timing of glacial terminations over the past 800 ka (e.g. Bajo et al., 2020; Huybers and Wunsch, 2005; Parrenin and Paillard, 2012). (...)*

---

Typos:

L.145: “Gray and Evans (2019)”

Thanks for pointing this out, we modified the text accordingly.

L.179: “benthic  $\delta^{18}\text{O}$  record”

Thanks for pointing this out, we modified the text accordingly.

L. 198: “ $\Delta\text{GSST}$ ”

Thanks for pointing this out, we modified the text accordingly.

L. 564: “MIS 7c”

Thanks for pointing this out, we modified the text accordingly.

L. 565: “during the Holocene”

Thanks for pointing this out, we modified the text accordingly.

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  $0.4 \pm 0.4$  °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  $0.4 \pm 0.4$  °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<sub>2</sub> thresholds (e.g., ≥260 ppm) may exclude MIS 7c, as its CO<sub>2</sub> concentrations remain lower than those of other interglacials (Past Interglacials Working Group of PAGES, 2016). However, under sea-level or benthic δ<sup>18</sup>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<sub>2</sub> concentrations, MIS 7c has occasionally been excluded from interglacial lists under stricter CO<sub>2</sub>-based definitions (Tzedakis et al., 2009, 2012). However, under sea-level and benthic δ<sup>18</sup>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 δ<sup>18</sup>O<sub>p</sub>, although this proxy reflects both temperature and hydrographic changes, notably variations in seawater δ<sup>18</sup>O linked to salinity. While we correct δ<sup>18</sup>O<sub>p</sub> for seawater δ<sup>18</sup>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 ( $\Delta$ GMST) is defined as the global SST anomaly ( $\Delta$ GSST) 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  $\Delta$ GSST stacks, which therefore reflect extrapolar mean conditions, whereas it is only accounted for in the derived  $\Delta$ GMST 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 see 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 sections 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  $0.4 \pm 0.4^\circ\text{C}$  warmer), (...)*

Line 348. “MIS 7c emerges as the warmest period of MIS 7 ( $+0.4 \pm 0.4^\circ\text{C}$  and  $+0.2 \pm 0.2^\circ\text{C}$ , 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  $0.4 \pm 0.4^\circ\text{C}$  and  $0.2 \pm 0.2^\circ\text{C}$   $\Delta\text{GMST}$  and  $\Delta\text{GSST}$  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 CO<sub>2</sub> 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 CO<sub>2</sub> 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<sub>2</sub> 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<sub>2</sub> 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<sub>2</sub> increase raises the possibility that the climate system did not reach equilibrium.

We agree that the highest CO<sub>2</sub> 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<sub>2</sub> relationship defined by the other interglacials. This offset reflects transient decoupling between atmospheric CO<sub>2</sub> and global surface temperature: CO<sub>2</sub> 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

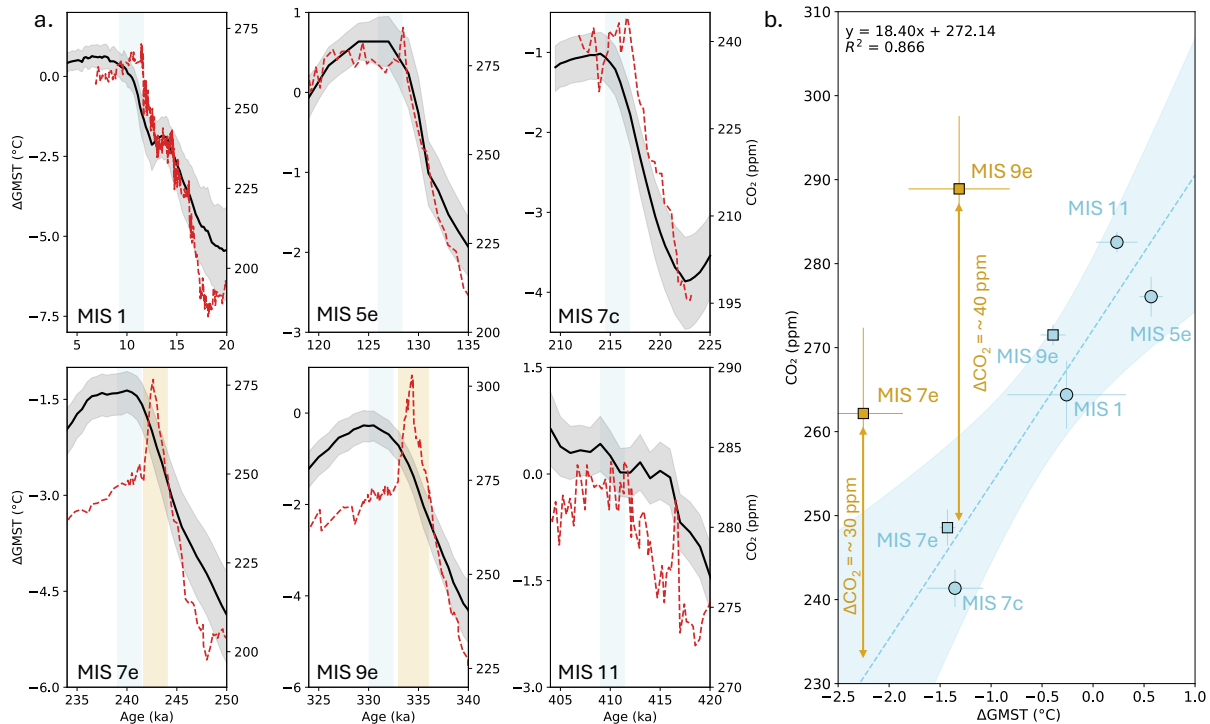
other words, the decoupling exists but it is confined to the overshoot interval and not to the whole interglacial. It does not imply a fundamentally different equilibrium relationship between radiative forcing and temperature during MIS 7e, but only during the overshoot, also occurring at the onset of 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<sub>2</sub>, ~700 ppb CH<sub>4</sub>) and MIS 7c (~245 ppm CO<sub>2</sub>, ~600 ppb CH<sub>4</sub>) corresponds to an estimated radiative forcing of ~1 W/m<sup>2</sup>, 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<sub>2</sub> concentrations observed during MIS 7e are confined to its very beginning (approximately 2 kyr), forming a brief overshoot. After this overshoot, CO<sub>2</sub> concentrations no longer exceed those recorded during MIS 7c (Legrain et al., 2024).*

*High-resolution records of CO<sub>2</sub> 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<sub>2</sub> concentrations period (3 kyr window following the maximum CO<sub>2</sub> value) with GMST over the same period, it reveals that for MIS 7c, the atmospheric CO<sub>2</sub> concentrations are within the range of expected values considering the GMST over this period and the CO<sub>2</sub> – GMST relationship of the past interglacials (Fig. 7). However, both MIS 7e and MIS 9 evidence anomalously high CO<sub>2</sub> concentrations of 31 ±14ppm and 41 ±13ppm (1σ), respectively, compared to the expected CO<sub>2</sub> concentrations value based on their GMST (Fig. 7b). However, these two interglacials are characterized by the most pronounced overshoots of atmospheric CO<sub>2</sub> concentration of the past 500 ka, which consist in an abrupt increase followed by a similarly abrupt decrease of atmospheric CO<sub>2</sub> 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<sub>2</sub> 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<sub>2</sub> overshoot, once CO<sub>2</sub> 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<sub>2</sub> concentrations, leading to a transient radiative decoupling. However, once the overshoot has subsided and CO<sub>2</sub> 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<sub>2</sub> across the interglacials of the past 450 ka** (a) Global mean surface temperature (GMST; black line with 1σ grey envelope) and atmospheric CO<sub>2</sub> 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<sub>2</sub> 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<sub>2</sub> concentrations peak. For MIS 9e and MIS 7e, blue/yellow bands denote alternative 3kyr windows including/excluding the CO<sub>2</sub> 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<sub>2</sub> 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<sub>2</sub> 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. (...)*