

Reply to reviewer 1

Thank you very much for taking the time to review the manuscript carefully. Below, we indicate the reviewer's comment in *italic* form and our reply in **bold** form.

This study presents investigation into the role of cloud phase parameterization in simulating the Arctic climate during LIG. The topic is highly relevant, given the persistent model-data discrepancy regarding the magnitude of Arctic warming and summer sea ice extent during this period. The authors convincingly demonstrate that a parameter set allowing for more supercooled liquid water can lead to a significantly warmer Arctic and a closer match to proxy-based summer sea ice reconstructions. While the findings are intriguing and the experimental design is robust in its sensitivity framework, several key aspects of the argumentation require clarification and a more nuanced discussion to strengthen the paper's conclusions.

1 Impact of Model Biases and the Validity of Proxy Comparison

It seems there are significant model biases in the PI control simulations, notably a warm bias over North America and an unrealistic sea ice thickness distribution near Canada and Greenland. If the model's description of the modern Arctic climate is flawed, the feedback processes it simulates (e.g., sea ice albedo, sea ice clouds) may also be inaccurate. The fact that the PI simulations using parameter set A (Figure 6a) appear to be closer to observed sea ice concentrations. The authors should clearly explain how the modern biases may affect their interpretation of the LIG results and the generalizability of their findings. In addition, the authors admit they cannot claim which model versions is more realistic. Then is it necessary to compare the LIGvC and LIGvA with the proxy reconstruction?

It is necessary to discuss whether the improved agreement with the LIG proxy parameters stems from more realistic physical processes or from accidental compensation of model errors.

Thank you for raising this point. As the reviewer pointed out and as we described in the text, we believe these biases are not negligible; therefore, we agree that care must be exercised when interpreting and drawing implications from our results. It is admittedly tricky to clearly distinguish the effects of model bias and cloud feedback because they are interconnected in coupled models, and we have added a substantial discussion of this issue in the revised manuscript:

“The significant warm bias over North America and insufficient sea ice along the North American coast are concerns in attributing the simulated results. It is admittedly tricky to disentangle the effects of model bias in modern climate and cloud feedback induced by LIG forcing because they are interconnected. Therefore, care must be exercised when interpreting and drawing implications from our results. Here, we review the models’ performance and clarify our intent and message. Sea ice concentration in September, when sea ice is at its minimum in LIG simulations, improves in PivL relative to PivS, notably in the Barents and Kara Seas. The most significant difference in SAT between PivL and PivS occurs in November, and the magnitude of the biases over the Arctic is comparable, with opposite signs. The difference in low-level cloud cover between PivL and PivS is generally smaller than their model biases, indicating room for improvement in cloud variables. While the cloud distribution was examined in Sherriff-Tadano et al. (2023), the absence of a COSP simulator (Bodas-Salcedo et al. 2011) in this model version prevents a rigorous comparison of cloud phases with satellite observations. The summer SAT anomaly of LIG simulations relative to PI simulations in both models exhibits almost the same root mean squared errors when compared with the limited available proxy reconstructions. The annual mean SAT anomaly in LIG simulations relative to PI simulations indicates improved performance for cloud parameter set L. However, the assumption made about the proxy compilation precludes a confident judgment of superiority. Nevertheless, it remains a valid concern that both models may substantially underestimate the annual mean response to LIG forcing.”

“We point out that simulated modern sea ice thickness has not yet received sufficient attention in interpreting paleoclimate simulations, perhaps due to the lack of a long-term observational dataset of sea ice thickness. Our models generally fail to reproduce observed thicker Arctic ice near the Canadian Archipelago and Greenland because of the zonal distribution induced by polar Fourier filtering. The reduction in sea ice cover increases the moisture supply to the atmosphere, thereby increasing low-level cloud cover and the LW cloud radiative effect. The thinner baseline sea ice at PI may lead to greater sea ice melt and consequently to enhanced LW cloud feedback at LIG. On the other hand, already insufficient sea ice along the North American coast at the PI baseline may have only a minor local impact on subsequent changes in sea ice cover and LW cloud feedback at LIG. Because the melting temperature of ice is virtually constant, we cannot rule out that model biases partly promote the simulated ice-free Arctic summer. The relationship among sea ice extent, sea ice thickness, and cloud feedback warrants further investigation and is likely to be valuable with newer versions of the model that do not require polar filtering, as well as in a multi-model context.”

“Given model biases, our intention is not to declare that either of our simulations is successful, but rather to present cases in which the representation of the temperature dependency of cloud

phase substantially affects the LIG Arctic simulations and can also be a contributing factor, or at least a factor to be concerned about, to the existing and unexplained large model spread. This underscores the need for multi-model analysis of cloud-phase feedback. There are a couple of other reasons why we did not claim which model version is more correct: 1) While the temperature dependency of cloud phase in the parameter set L is designed to be closer to modern satellite observations in the previous study (Sherriff-Tadano et al., 2023), there is no guarantee that the same relation holds at the LIG environment, when the aerosol distribution is likely different from that of the present-day. Thus, we intended to emphasize that the aim of the study has a nature of sensitivity experiments; 2) the agreement of annual mean SAT difference of LIG and PI between the model and proxies is still insufficient or the disagreement cannot be rejected, as commonly seen in other models; and 3) while progress has been made, summer sea ice cover at LIG still has uncertainty due to the limited number of well-dated reliable Arctic sea-ice records.”

2 Initial Condition Sensitivity

The central finding—that a warmer, thinner sea-ice initial state (PIvC) leads to an ice-free LIG Arctic—raises the critical question of whether the cloud-phase process would be sufficiently powerful to generate a similar outcome if the model were initialized from a more realistic, colder glacial state. Is the result demonstrating a unique, enhanced physical response to LIG forcing, or is it primarily a consequence of sensitivity to the initial state? The manuscript does not sufficiently disentangle these two effects.

Thank you for drawing our attention to this viewpoint. The experiment protocol follows the PMIP4 LIG experiment, and the model was integrated for at least 2,000 years, a sufficiently long period to reach a quasi-equilibrium. Therefore, the results are insensitive to the choice of imposed initial conditions, as our study does not focus on the transient response (although we cannot entirely exclude the possibility of multiple equilibria). In the real world, the LIG does not represent the equilibrium climate state, and the complications arising from its transient nature require investigation in separate studies, including the effect of meltwater fluxes from ice sheets. We clarify that cloud-phase feedback is not a forcing that changes colder glacial to warmer interglacial climates; it operates in addition to insolation forcing. The Arctic climate appears to respond to insolation forcing quickly (<100 years; as described in Sime et al., 2025), so a short integration is adopted for the CMIP7 Fast Track protocol. From our simulations, we expect that feedback, such as cloud-phase feedback, operates effectively once the climate enters a warm interglacial in response to the astronomical forcing, if not before. Therefore, we did not consider

the initial conditions a critical factor in our conclusion.

3 Overemphasis on Annual Mean Temperature

The analysis of surface air temperature changes focuses heavily on annual means (e.g., Fig. 2). However, the crucial processes for sea ice evolution—melting in summer and freezing in autumn—are inherently seasonal. The key mechanism proposed, the liquid cloud longwave feedback, is explicitly shown to be a autumn/winter phenomenon. Therefore, the annual mean temperature is a less informative metric for the core thesis than a detailed seasonal analysis.

The authors should place greater emphasis on the seasonal progression of temperature and energy budget changes when discussing the causes of sea ice loss. Seasonal (e.g., JJA, SON) temperature and radiation anomaly maps could be prioritized in the main text to directly link the forcing to the response.

Our intention is not to emphasize the annual mean variables. The seasonal summary of temperature, as well as surface radiative and heat fluxes in the Arctic region, is presented as concise bar graphs throughout the paper. They are converted and presented in temperature units. As the reviewer wonders, we agree that the mechanism was understated in the original manuscript. In the revised manuscript, we added in Sect. 5.3: “The net decrease in subsurface heat uptake (positive Q_s term) and the net increase in upward surface latent and sensible heat fluxes (negative LH and SH terms), which are respectively tagged with surface warming and cooling from October to December, imply atmospheric warming via heat transfer from the ocean to the atmosphere. The mechanism of warming involving these processes is consistent with many previous studies: stronger insolation at LIG reduces sea ice cover, and the ocean mixed layer absorbs more heat in summer and releases it to the atmosphere in autumn-winter (e.g., O’ishi et al., 2021; Sicard et al., 2022; Hirose et al., 2025).” We also added in Sect. 5.4: “The net increase in subsurface ocean heat content in summer (negative Q_s term), along with the albedo feedback (positive A term), and the net decrease in subsurface ocean heat content in winter (positive Q_s term), indicate enhanced heat exchange between the ocean and the atmosphere with reduced sea ice cover.” We also added polarized seasonal maps of Fig. 2 in the supplementary materials in response to the referee’s suggestion.

4 Oversimplified Explanation for Polar Asymmetry

The explanation for the asymmetric temperature response between the Arctic (warming) and Antarctic (cooling) to the cloud parameter change is attributed solely to the difference in summer climatological

temperatures. This explanation is plausible but likely incomplete. The discussion of this asymmetry should be expanded to acknowledge the potential role of these large-scale dynamical factors, even if a full analysis is beyond the paper's scope. atmospheric circulation, moisture transport,

We agree that the complete response of Southern Ocean cooling near Antarctica is subject to feedback through changes in atmospheric circulation, moisture transport, and other factors. However, we emphasize that: a) the difference between the two model versions in PI simulations is the cloud parameters, and thus the cloud parameterization is the cause of the difference (though not necessarily the dominant contributor to the complete response); and more importantly b) the energy balance analysis at the surface clearly shows year-round cloud longwave warming and the dominant cloud shortwave cooling effect during summer near the Antarctica, compared to other terms. We added the following clarification in the revised manuscript: “The cause of the difference is the cloud parameters, but it is essential to recognize that the equilibrium climate state is determined as a result of feedback through changes in atmospheric circulation, moisture transport, and other factors. Their details are, however, beyond the scope of this paper.”

5 The attribution of autumn/winter warming

The attribution of autumn/winter warming primarily to the cloud parameterization requires refinement. A compelling alternative explanation is the summer remnant effect whereby the larger summer sea-ice loss in LIGvC creates a warmer, moister autumn lower atmosphere, passively leading to more clouds and warming. While the AGCM experiments provide some evidence for an active role of the cloud phase, the coupled model result is likely a combination of this passive response and the active cloud-phase amplification. The authors should clearly distinguish between these two effects and present the autumn/winter warming as a result of a positive feedback loop where the cloud parameterization acts as a powerful amplifier of the conditions created by prior sea-ice loss.

We agree with the referee’s comment, and it is another important point to be carefully delivered in the text. Our interpretation, as stated in Section 5.4 of the original manuscript, is: “As discussed above, the LWP and low-level cloud amount increases in LIG simulations are likely associated with decreased sea ice cover from PI simulations. As the reduction in sea ice cover is much larger in Δ LIGvL than in Δ LIGvS, a larger cloud response occurs in Δ LIGvL than in Δ LIGvS.” In the revised manuscript, we added Fig. 12, which includes ocean temperature response, and a detailed description with clarification: “The description above is consistent with the seasonal evolution of atmospheric and upper-ocean temperature anomalies shown in Fig.

12b, where anomalous ocean heat content increases in summer and decreases in autumn. The contrasting weaker summer and stronger winter signals in the ocean temperature in Fig. 12b, relative to Fig. 12a, are striking. The warming signal is also evident in the winter atmosphere in Fig. 12b. These signatures may be related to the cloud-phase feedback, which helps sustain the positive ocean temperature anomaly through winter and thereby potentially impacts sea ice growth. Both a larger reduction in sea ice cover and stronger cloud feedback in the model with cloud parameter set L functionally contribute to warmer conditions relative to the model with cloud parameter set S. However, given the limitations in disentangling both effects in coupled atmosphere-ocean model simulations, each effect, particularly on sea ice growth and loss, is not quantified separately in the current study. The model capable of ice mass tendency diagnostics, as suggested by Keen et al. (2021) and Sime et al. (2025), would help explore this aspect.”

Reply to reviewer 2

Thank you very much for taking the time to review the manuscript carefully and for your many valuable comments, including information about additional literature. Below, we indicate the reviewer's comment in *italic* form and our reply in **bold** form.

Impact of the temperature-cloud phase relationship on the simulated Arctic warming during the last interglacial by Nozomi Arima et al.

Summary:

This manuscript explores how cloud phase representation affects Arctic climate simulations of the Preindustrial and Last Interglacial. The study finds that models allowing more supercooled liquid water simulate stronger warming – particularly in autumn – and reduced sea-ice cover.

The manuscript also shows that the inclusion of dynamic vegetation increases warming and reduces LIG sea ice in the Arctic. The paper is mostly well written – it is clear and well structured. I enjoyed reading it. There are, however, several significant issues, some key references are missing, and the use of both present-day and Last Interglacial observations needs improvement.

Major comments

Cloud parameterisations

Overall, the two cloud parametrisations clearly have substantially different impacts on clouds and temperature in the Arctic over the Preindustrial and Historical periods. This should be dealt with much more clearly prior to their use for the lig127k. Results for Historical simulations should be shown against present-day cloud observations, Arctic temperature, and Arctic sea ice. This does not preclude the main point of this manuscript – that lig127k Arctic temperatures and SIA are sensitive to cloud parameterisation – but it should make the presentation and discussion of the meaning of the results much clearer. Prior to the use of the two parametrisations for the LIG, they should be shown against present-day cloud observations, Arctic temperature, and Arctic sea ice. This could be done in a second Appendix, if the authors prefer not to put this in the main text.

We have now added a new Appendix A on model performance, which discusses model biases in surface air temperature, sea ice cover, and cloud cover. Historical simulations are not available for all model versions, as is sometimes the case for the designated paleoclimate simulations; however, we attempted to discuss the potential effects of differences between historical and preindustrial simulations using the available data. We also added a summary of model bias and

a substantial discussion on its implications for interpreting our results in the Discussion:

“The significant warm bias over North America and insufficient sea ice along the North American coast are concerns in attributing the simulated results. It is admittedly tricky to disentangle the effects of model bias in modern climate and cloud feedback induced by LIG forcing because they are interconnected. Therefore, care must be exercised when interpreting and drawing implications from our results. Here, we review the models’ performance and clarify our intent and message. Sea ice concentration in September, when sea ice is at its minimum in LIG simulations, improves in PivL relative to PivS, notably in the Barents and Kara Seas. The most significant difference in SAT between PivL and PivS occurs in November, and the magnitude of the biases over the Arctic is comparable, with opposite signs. The difference in low-level cloud cover between PivL and PivS is generally smaller than their model biases, indicating room for improvement in cloud variables. While the cloud distribution was examined in Sherriff-Tadano et al. (2023), the absence of a COSP simulator (Bodas-Salcedo et al. 2011) in this model version prevents a rigorous comparison of cloud phases with satellite observations. The summer SAT anomaly of LIG simulations relative to PI simulations in both models exhibits almost the same root mean squared errors when compared with the limited available proxy reconstructions. The annual mean SAT anomaly in LIG simulations relative to PI simulations indicates improved performance for cloud parameter set L. However, the assumption made about the proxy compilation precludes a confident judgment of superiority. Nevertheless, it remains a valid concern that both models may substantially underestimate the annual mean response to LIG forcing.

We point out that simulated modern sea ice thickness has not yet received sufficient attention in interpreting paleoclimate simulations, perhaps due to the lack of a long-term observational dataset of sea ice thickness. Our models generally fail to reproduce observed thicker Arctic ice near the Canadian Archipelago and Greenland because of the zonal distribution induced by polar Fourier filtering. The reduction in sea ice cover increases the moisture supply to the atmosphere, thereby increasing low-level cloud cover and the LW cloud radiative effect. The thinner baseline sea ice at PI may lead to greater sea ice melt and consequently to enhanced LW cloud feedback at LIG. On the other hand, already insufficient sea ice along the North American coast at the PI baseline may have only a minor local impact on subsequent changes in sea ice cover and LW cloud feedback at LIG. Because the melting temperature of ice is virtually constant, we cannot rule out that model biases partly promote the simulated ice-free Arctic summer. The relationship among sea ice extent, sea ice thickness, and cloud feedback warrants further investigation and is likely to be valuable with newer versions of the model that do not require polar filtering, as well as in a multi-model context.

Given model biases, our intention is not to declare that either of our simulations is successful,

but rather to present cases in which the representation of the temperature dependency of cloud phase substantially affects the LIG Arctic simulations and can also be a contributing factor, or at least a factor to be concerned about, to the existing and unexplained large model spread. This underscores the need for multi-model analysis of cloud-phase feedback. There are a couple of other reasons why we did not claim which model version is more correct: 1) While the temperature dependency of cloud phase in the parameter set L is designed to be closer to modern satellite observations in the previous study (Sherriff-Tadano et al., 2023), there is no guarantee that the same relation holds at the LIG environment, when the aerosol distribution is likely different from that of the present-day. Thus, we intended to emphasize that the aim of the study has a nature of sensitivity experiments; 2) the agreement of annual mean SAT difference of LIG and PI between the model and proxies is still insufficient or the disagreement cannot be rejected, as commonly seen in other models; and 3) while progress has been made, summer sea ice cover at LIG still has uncertainty due to the limited number of well-dated reliable Arctic sea-ice records.”

Fixed-angle calendar

On the use of a fixed-angle calendar, this paper is largely focused on the correct calculation of energy (and mass) budgets. The re-interpolation from fixed-day to a fixed-angle calendar will lead to incorrect budgets for ice and energy (Sime et al., 2025b). If the authors do include comparison with summer air temperatures in the Arctic, I agree that using fixed-angle JJA averages may be helpful. However, I would like to see a more careful consideration of the impacts of incorrect month lengths on all other averaging – and budgets.

We recognize that there is no perfect or universal way to adjust the calendar for comparison between past and present, and we agree that the paleoclimate modeling community needs to establish several robust methodologies for specific purposes. Our feedback analysis aims to explain the differences in surface temperature for each month, independently of the others, using the calendar definition used in previous studies. The time integration of all months under the fixed-angle calendar is not equal to the annual average. We note that no calendar adjustments are applied before computing the annual mean values, and this fact is now clearly stated. In response to the reviewer’s concern, we examined an alternative calendar definition in which the length of each month is the same between PI and LIG, taking into account the effect of precession on the ecliptic longitude. The results are now presented in the Supplementary Materials. The results are relatively insensitive to the choice of two calendars (conventional adjustments vs. new alternative adjustments). The main conclusion remains unchanged.

Observations

On observations, the use of the Turney and Jones (2010) dataset is not recommended by the PMIP community because of the weak dating constraints. See various papers from Capron et al. If the authors wish to use summer air temperature data for comparison, please use instead the database of CAPE et al. (2006), later updated by Otto-Bliesner for the IPCC, then Guarino et al. (2020), and Sime et al. (2023). The data reference Guarino, M. V., & Sime, L. (2022) is provided below.

Thank you very much for suggesting the specific summer reconstruction dataset. This is very helpful for further comparison. We now use this dataset for the model-data comparison. Please see below for the specific reply.

Marine core data

On marine core data, Vermassen et al. (2023) suggest that the Arctic was likely seasonally sea-ice-free during the LIG; however, their Arctic age models are currently characterised by significant uncertainty (Razmjooei et al., 2023). For similar reasons, all but one of the Kageyama et al. (2021) Arctic marine core datapoints are marked as being unreliable with respect to dating. Please address this as suggested below.

Thank you very much for clarifying the reliability of published data. We also appreciate your guidelines for describing the current understanding of the LIG Arctic sea ice state. The descriptions provided in the comment regarding the latest interpretation are invaluable. Please see below for specific responses.

Section and Line Comments:

L31-42 The Introduction should be modified to better reflect our current understanding of Arctic sea ice and summer temperatures. Please include that Guarino et al. (2020) pointed out that the Arctic during the Last Interglacial was likely sea-ice-free. (Prior to this, Malmierca Vallet et al. (2018) also noted that the Greenland Last Interglacial ice-core water isotope values were most easily explained by a sea-ice-free summer in the Arctic.) Sime et al. (2023) updated this work, estimating the Arctic-wide summer surface air temperature warming at 127 ka to be 3.7 ± 1.5 K, and that the LIG climatological minimum SIA was most likely 1.3 to 1.5×10^6 km², which is rather close to the definition of a practically summer ice-free Arctic (a maximum sea-ice extent of less than 1×10^6 km²). Read and reference Sime et al. (2025b)

A sea ice free Arctic:

Assessment Fast Track abrupt-127k experimental protocol and motivation. L42 This is not correct.

See Sime et al. (2023) and Sime et al. (2025b). Six models are occasionally practically sea-ice-free in their first 100 years of lig127k forcing.

We enhanced and corrected the sentences in the revised manuscript: “While the proxy reconstruction is subject to substantial dating uncertainty and may not necessarily represent LIG conditions (Razmjooei et al., 2024), isotope-enabled modelling studies support a summer-ice-free Arctic: the higher oxygen isotope ratio in Greenland ice cores at LIG is well explained by the absence of sea ice and warmer summer sea-surface temperatures in the Arctic (Malmierca-Vallet et al., 2018). Additional support for the nearly ice-free summer Arctic at LIG is provided by Sime et al. (2023), who applied the emergent relationship between summer temperature increases and sea ice area reductions across models to a reconstructed LIG warming. Nevertheless, only one of the 12 models analyzed by Kageyama et al. (2021) simulated climatologically (long-term mean) ice-free conditions, and a few models occasionally produced ice-free summers (Sime et al., 2023, Figure 4; Sime et al., 2025, Figure 2).”

L44-47 Better to read and reference also Sime et al. (2025b), and make it clearer that albedo feedbacks are crucial, and that the inclusion of advanced ice physics such as explicit melt ponds can lead to a better representation of sea ice in models including CESM2 and HadGEM3 (Guarino et al., 2020; Diamond et al., 2024).

We rewrote the sentence to: “Guarino et al. (2020) and Diamond et al. (2021) reported that the explicit representation of melt ponds and the consequent improvement of the surface albedo feedback process, leading to better present-day sea ice simulations, play an essential role in ‘successfully’ simulating the ice-free Arctic summer.”

L49-70 I like this section, but find myself also wanting to know what the usual cloud parameterisation schemes used in CMIP6/7 models are. A little background to say which form of parameterisation in cloud scheme A and C are relevant to the CMIP Fast Track community would be very helpful. Additionally, can you make the relationship between this manuscript and the energy budget findings in Kageyama et al. (2021) clearer? And also relate it to the energy-budget aims of A sea ice free Arctic: Assessment Fast Track abrupt-127k.

Based on our limited survey, many recent models do not diagnose cloud-phase partitioning by temperature and instead use more complex procedures (this is also true for newer versions of MIROC models). Given the extremely large spread in the temperature-cloud phase relationship in CMIP5 and CMIP6 models, we think the sensitivity experiments in this paper with both cloud

parameter sets are relevant, even for the CMIP7-Fast Track community. We benefited from our parameterization of explicit temperature dependence because sensitivity experiments are only possible with such a simple scheme, although we must acknowledge limitations arising from numerical polar filtering and the absence of COSP cloud simulators in this non-latest model for assessment. We updated the sentence: “Furthermore, it is essential to note that the temperature-cloud phase relationship is extremely diverse among models (McCoy et al., 2015), and even in the recent CMIP6 models (Tan et al., 2025).” We also add: “In contrast to the previous work by Kageyama et al. (2021), which emphasized the importance of shortwave (SW) radiative feedback, we highlight LW radiative feedback as an active player.”

L115 The meaning of the comment on which parameterisation is more accurate is very unclear. The two parametrisations clearly have substantially different impacts on clouds and temperature in the Arctic over the Preindustrial and Historical periods. This should be dealt with much more clearly. Prior to the use of the two parametrisations for the LIG, they should be shown against present-day cloud observations, Arctic temperature, and Arctic sea ice.

As in the reply above, we have now added a new Appendix A on model performance, which discusses model biases in surface air temperature, sea ice cover, and cloud cover. We also attempted to discuss the potential effects of differences between historical and preindustrial simulations using the available data.

L175-182 Because the Earth moves fastest near perihelion (when Earth is closest to the sun) and slowest near aphelion (when Earth is farthest from the sun), the use of a fixed-angle calendar causes problems in the calculation of energy budgets (e.g. Otto-Bliesner et al., 2017; Bartlein and Shafer, 2019). This is because the use of a fixed-angle calendar results in months and seasons of unequal day lengths. Given that this paper is largely focused on the correct calculation of energy (and mass) budgets, the re-interpolation from fixed-day to a fixed-angle calendar will lead to incorrect budgets for ice and energy (Sime et al., 2025b).

The authors are correct that switching to a fixed-angle calendar can be helpful for comparing seasonal observations across different time periods. However, in the analysis that follows this is not done (see also below). I can see that there are ‘monthly’ comparisons used for the lig127k output. I’d like to see a more careful consideration of the impacts of incorrect month lengths in this ‘monthly’ averaging (that it is not possible to compare months in a truly meaningful way between the PI and lig127k), and ideally instead the use of fixed-length output averaging –perhaps centred on their season/date of interest. For example, using the 15 days either side of the solstice or equinox, rather than ‘September’ or ‘March’ averages. This prevents the temporal stretching and compression which introduces

artefacts into energy budgets that are otherwise associated with re-interpolating to a fixed-angle calendar. It would also be helpful for the authors to confirm that they produce any annual averages without angle-based interpolation –i.e. that these are standard (correct) CMIP fixed-length type averages.

As in the reply above, we examined the alternative calendar definition as suggested. We added: “While our energy balance analysis, described in the next section, is applied independently for each month and the interpretation should not be affected by differences in month length, we also examined an alternative calendar definition: the mid-month date is defined according to the fixed-angle calendar, and the month length is fixed for 31 days, with 15 days before and after the mid-month date. The results change little and are presented in the Supplementary Materials.” Please see also Section 3 of Supplementary Materials.

L212, and below Such large temperature differences between the A and C simulations imply that there should be a preference for one or the other parameterisation on the basis of its match to present-day observations. For example, one can say whether 2.8 °C warmer in November in the PI/present-day is an improvement or not.

In the revised manuscript, we clarified this obscure statement: “When focused on the November SAT in the region north of 70°N, covering most of the Arctic Ocean, PIVL (-17.43°C) appears to be a better match to the ECMWF ERA5 reanalysis dataset (-17.62°C for 1980-1999, Hersbach et al., 2020) than PIVS (-19.89°C). However, as Sime et al. (2025) stress, it is important to consider the differences between present-day and preindustrial conditions. The difference between the 1980-1999 average and the 1850-1899 average is 1.05°C in the long-term NOAA 20CRv3 reanalysis dataset (Slivinski et al., 2021). Thus, PIVL and PIVS may contain a similar magnitude of errors of about 1°C with opposite signs. Note that the 1980-1999 average differs by 1.29°C between ERA5 and 20CRv3 reanalyses, which is comparable to, or slightly larger than, the estimated model biases.”

Figure 6, and other places If removing the attempt to compare ‘months’, the month (or 30-day period) with the minimum SIC/SIA is otherwise used in most of the papers below – rather than ‘September’.

Both sea ice area and extent are at their minimum in September (after calendar adjustment) in the LIG simulations. While the minimum in sea ice area occurs in August (extent is comparable between August and September), we retain the figure to be consistent with LIG and PI simulations. We added this information in the revised manuscript.

5.2 *The use of the Turney and Jones (2010) dataset is not recommended by PMIP because of the weak dating constraints used (see various papers from Capron et al.). If the authors wish to use summer air temperature data for comparison, please use instead the database of CAPE et al. (2006), later updated by Otto-Bliesner for the IPCC, then Guarino et al. (2020), and Sime et al. (2023). The data reference Guarino, M. V., & Sime, L. (2022) is provided below. Marine core data Vermassen et al. (2023) suggest that the Arctic was likely seasonally sea-ice-free during the LIG; however, their Arctic age models are currently characterised by significant uncertainty (Razmjooei et al., 2023). For similar reasons, all but one of the Kageyama et al. (2021) Arctic marine core datapoints are marked as being unreliable with respect to dating. For this reason, it is good practice to either (i) not show the Kageyama et al. (2021) datapoints which cannot be reliably dated to the Last Interglacial, or (ii) mark them as 'date unknown' (e.g. see Figure 3 in Sime et al., 2023, and the figures in Kageyama et al., 2021). To show them as dated to the Last Interglacial is misleading.*

Thank you very much for the latest summer surface air temperature reconstruction dataset. We added the comparison with the summer proxy reconstruction in Figs. 8a and 8b in the revised manuscript: “Figures 8a and 8b show the difference in summer SAT between the LIG and PI simulations. The estimate from proxy records (Guarino and Sime, 2022) is also presented. Note that this family of proxy datasets has been used previously and updated for the model-data comparison (CAPE, 2006; Guarino et al., 2020; Sime et al., 2023). Both ΔLIGvS and ΔLIGvL generally capture the strong summer warming at LIG, with some overestimation over Alaska and underestimation over Greenland. The root mean squared error is 1.8 °C for ΔLIGvS and 1.9 °C for ΔLIGvL , and they are comparable. It is difficult to draw a definitive conclusion about the superiority, given the uncertainty in proxy reconstructions and the limited number of sample locations.”

We now replace the figure in the main text comparing simulated data with the annual mean temperature reconstructions of Turney and Jones (2010) and Capron et al. (2017) with a summer temperature reconstruction. Nevertheless, we retain the original figure in the supplementary for two reasons: “Nevertheless, the comparison is referenced here because the impact of cloud phase representation on SAT is more critical in winter, rather than summer, and it is important to point out that the discrepancy of simulation with this dataset, shown by O’ishi et al. (2021), remains substantial even with the alternative cloud phase representation. We must wait for a well-dated dataset for a more quantitatively solid comparison.”

Section 5.4, and other places A main feedback on clouds in the Arctic is the release of heat in autumn that is stored in the Arctic mixed layer (upper ocean). Less sea ice in summer → more heat absorbed

and stored in the mixed layer → released to the atmosphere in autumn, with slower freeze-up and cloud feedbacks. This process is not currently adequately covered in 5.4 or the rest of the paper. The process and its consequences need consideration.

This important mechanism was understated in the original manuscript. In the revised manuscript, we added in Sect. 5.3: “The net decrease in subsurface heat uptake (positive Q_s term) and the net increase in upward surface latent and sensible heat fluxes (negative LH and SH terms), which are respectively tagged with surface warming and cooling from October to December, imply atmospheric warming via heat transfer from the ocean to the atmosphere. The mechanism of warming involving these processes is consistent with many previous studies: stronger insolation at LIG reduces sea ice cover, and the ocean mixed layer absorbs more heat in summer and releases it to the atmosphere in autumn-winter (e.g., O’ishi et al., 2021; Sicard et al., 2022; Hirose et al., 2025).” We also added in Sect. 5.4: “The net increase in subsurface ocean heat content in summer (negative Q_s term), along with the albedo feedback (positive A term), and the net decrease in subsurface ocean heat content in winter (positive Q_s term), indicate enhanced heat exchange between the ocean and the atmosphere with reduced sea ice cover.”

In the revised manuscript, we added Fig. 12, which includes ocean temperature response, and a detailed description with clarification: “The description above is consistent with the seasonal evolution of atmospheric and upper-ocean temperature anomalies shown in Fig. 12b, where anomalous ocean heat content increases in summer and decreases in autumn. The contrasting weaker summer and stronger winter signals in the ocean temperature in Fig. 12b, relative to Fig. 12a, are striking. The warming signal is also evident in the winter atmosphere in Fig. 12b. These signatures may be related to the cloud-phase feedback, which helps sustain the positive ocean temperature anomaly through winter and thereby potentially impacts sea ice growth. Both a larger reduction in sea ice cover and stronger cloud feedback in the model with cloud parameter set L functionally contribute to warmer conditions relative to the model with cloud parameter set S. However, given the limitations in disentangling both effects in coupled atmosphere-ocean model simulations, each effect, particularly on sea ice growth and loss, is not quantified separately in the current study. The model capable of ice mass tendency diagnostics, as suggested by Keen et al. (2021) and Sime et al. (2025), would help explore this aspect.”

~L379 referencing etc Do refer to the energy budget work done in the various Guarino (2020), Diamond (2021/4), Kageyama (2021), and Sime (2025b) papers here too, and rewrite as necessary.

Thank you for the suggestion. The original sentence was somewhat confusing, and thus we rewrote it: “The melt-pond feedback accelerates summer ice melt through the albedo effect.

Excessive heat is stored in the ocean mixed layer, amplifying the autumn-winter warming through its release (Diamond et al., 2021, 2024; Guarino et al., 2020; Kageyama et al., 2021; Sime et al., 2025). As cloud-phase feedback is enhanced by reduced sea ice cover and amplifies surface warming via the greenhouse effect during a relatively cold season, both feedbacks may act constructively to amplify Arctic warming at LIG.”

L386 and thereafter Strangely phrased here. Perhaps start with the obvious – that the differences between cloud parameterisation A and C have an impact in a particular temperature range. This therefore means that impacts will clearly be different between the Southern Ocean and the Arctic.

Although the Southern Ocean is not the focus of this study, we included this paragraph because we were asked this somewhat “obvious” question several times in the past. Following the reviewer's suggestion, we revised the beginning of the section: “The cloud parameter sets S and L determine the cloud phase for a particular temperature range. Therefore, their impacts on PI simulations are different between the Southern Ocean and the Arctic. Although the Southern Ocean is clearly not the focus of this study, we briefly discuss this asymmetric response between the hemispheres.”

L401 Again, odd claim that you can't tell whether a 3 °C warmer Arctic in November (and other temperature and cloud changes) in the PI/present-day is better or not. Best to rewrite these sections once this is clearer to the authors.

We rewrote this section. Please see the response to the comment regarding “L212” above.

Reply to reviewer 3

Thank you very much for taking the time to review the manuscript carefully. Thank you also for the support. Below, we indicate the reviewer's comment in *italic* form and our reply in **bold** form.

Review comment on "Impact of the temperature-cloud phase relationship on the simulated Arctic warming during the last interglacial" by Arima et al.

This manuscript addresses a source of uncertainty in climate simulations for the LIG Arctic by performing in total 4 model simulations using different cloud parameters and models versions with and without dynamic vegetation. This study examines the impact of cloud phase representation and shows an important influence of SLF parameterization on LIG Arctic warming and sea ice cover. A parameter set (socalled C) which permits liquid water at lower temperatures and higher SLF can lead to a warmer PI Arctic, larger LIG Arctic warming, and substantially reduced LIG summer sea ice. This aligns with proxy records suggesting a nearly ice-free LIG Arctic summer, addressing a longstanding gap between models, which often fail to simulate ice-free conditions, and paleo observations. This study also present surface energy balance diagnostics which highlight the importance of LW CLOUD RADIATIVE EFFECT particularly in autumn and winter. Overall I find the manuscript is clear, methodologically thorough and can make a valuable contribution. The following are my suggestions, mostly minor:

1 The introduction outlines cloud phase's general role in Arctic climate, but lacks explicit connection to the LIG's unique orbital-driven insolation pattern. So, why is temperature-cloud phase relationship particularly important for resolving LIG Arctic climate ...than the MH, for example?

Although we have not yet examined the sensitivity of the mid-Holocene (MH) Arctic simulation to the cloud phase representation of the model, we expect the representation to be significant for the LIG, as the summer insolation anomaly from today is much larger than the MH, and the reconstruction suggests a warmer Arctic at the LIG. This is because perihelion was close to the summer solstice under a larger eccentricity at the LIG (which was close to the autumnal equinox at the MH). Because cloud-phase change is feedback, part of which is related to sea ice cover (as discussed in the manuscript), we expect stronger forcing to induce a greater influence. That said, the exact mechanism should apply qualitatively to other interglacial periods, including the MH. To avoid the lengthy introduction, we briefly state in the revised manuscript: "The representation of the cloud-phase change process may be important for simulating the Arctic

climate at LIG, although the same reasoning applies to other interglacial periods.”

2 Table 2 and 3 could be merged together while adding two columns called “model” and “vegetation” (either dynamic or fixed).

The two tables were merged as suggested.

3 Line 94-100, the text explains LPJ-DGVM’s vegetation dynamics but does not say about how dynamic vegetation might interact with cloud phase. So why you choose two models with and without dynamic vegetation?

To demonstrate the effect of differences in cloud-phase representation, it is sufficient to use only the model with dynamic vegetation, as we believe that dynamic-vegetation feedback is vital for realistic simulations. We also examined the sensitivity of cloud-phase representation using the model without dynamic vegetation, because most PMIP models that conducted LIG simulations in previous studies did not include it. There was concern that differences in simulated LIG climate may lead to varying sensitivities to cloud parameterization. Thus, we aim to examine whether the effect of cloud representation on simulated LIG climate is comparable and helps explain part of the spread in simulated LIG climates among many models. As the reviewer notes, we are not motivated to explore the direct interaction between vegetation feedback and cloud-phase feedback. We clarified this by the following sentences: “We also examined the sensitivity of cloud-phase representation using the model without dynamic vegetation, because most models used in previous LIG simulations did not include it. There is concern that differences in simulated LIG climates may lead to varying sensitivities to cloud parameterization. ... Note that this study was not motivated to explore the direct interaction between vegetation and cloud-phase feedback processes.”

4 I think your results can be enhanced by adding a positive feedback between sea ice thickness and cloud. For example, thinner PI sea ice in C may lead to more LIG sea ice melt, which increases surface moisture fluxes, drives more low-level clouds and thus higher LW CRE, and further reduces sea ice.

Thank you for highlighting this important process. This was stated briefly but may have been implicit in the text. We added in the Discussion: “Excessive heat is stored in the ocean mixed layer, amplifying the autumn-winter warming through its release (Diamond et al., 2021, 2024; Guarino et al., 2020; Kageyama et al., 2021; Sime et al., 2025). As cloud-phase feedback is enhanced by reduced sea ice cover and amplifies surface warming via the greenhouse effect

during a relatively cold season, both feedbacks may act constructively to amplify Arctic warming at LIG.” Regarding the ice thickness, please see the reply to comment #6 below.

5 Fig 4: should make the caption more detailed. I guess that each colored segment’s vertical length corresponds to its contribution to surface temperature change, with positive contributions above the 0 line and negative ones below. The caption should explicitly state this to avoid guesswork. The same for Fig. 9.

Thank you for pointing this out. We improved the corresponding captions by adding “In the stacked vertical bars, the positive contributions are shown above the zero horizontal line and the negative ones below. The solid black polygonal line denotes simulation, and the dashed blue line indicates the sum of the diagnosed partial temperature differences, i.e., the cumulative value of the vertical bars.”

6 The authors show that both model versions (A and C) exhibit biases, including a warm bias over North America, insufficient Arctic sea ice along the North American coast, and uncertainties in sea ice thickness simulations due to polar Fourier filtering. How these biases might influence the interpretation of LIG results. For example, the warm bias over North America could affect atmospheric circulation patterns that interact with Arctic clouds (e.g., moisture transport to the Arctic). Could this bias amplify or dampen the cloud phase effect on LIG Arctic warming? Might be better to add a discussion on this.

This is a critical viewpoint, which other reviewers also raised. We added the detailed discussion on this uncertainty:

“The significant warm bias over North America and insufficient sea ice along the North American coast are concerns in attributing the simulated results. It is admittedly tricky to disentangle the effects of model bias in modern climate and cloud feedback induced by LIG forcing because they are interconnected. Therefore, care must be exercised when interpreting and drawing implications from our results. Here, we review the models’ performance and clarify our intent and message. Sea ice concentration in September, when sea ice is at its minimum in LIG simulations, improves in PivL relative to PivS, notably in the Barents and Kara Seas. The most significant difference in SAT between PivL and PivS occurs in November, and the magnitude of the biases over the Arctic is comparable, with opposite signs. The difference in low-level cloud cover between PivL and PivS is generally smaller than their model biases, indicating room for improvement in cloud variables. While the cloud distribution was examined in Sherriff-Tadano et al. (2023), the absence of a COSP simulator (Bodas-Salcedo et al. 2011) in this

model version prevents a rigorous comparison of cloud phases with satellite observations. The summer SAT anomaly of LIG simulations relative to PI simulations in both models exhibits almost the same root mean squared errors when compared with the limited available proxy reconstructions. The annual mean SAT anomaly in LIG simulations relative to PI simulations indicates improved performance for cloud parameter set L. However, the assumption made about the proxy compilation precludes a confident judgment of superiority. Nevertheless, it remains a valid concern that both models may substantially underestimate the annual mean response to LIG forcing.

We point out that simulated modern sea ice thickness has not yet received sufficient attention in interpreting paleoclimate simulations, perhaps due to the lack of a long-term observational dataset of sea ice thickness. Our models generally fail to reproduce observed thicker Arctic ice near the Canadian Archipelago and Greenland because of the zonal distribution induced by polar Fourier filtering. The reduction in sea ice cover increases the moisture supply to the atmosphere, thereby increasing low-level cloud cover and the LW cloud radiative effect. The thinner baseline sea ice at PI may lead to greater sea ice melt and consequently to enhanced LW cloud feedback at LIG. On the other hand, already insufficient sea ice along the North American coast at the PI baseline may have only a minor local impact on subsequent changes in sea ice cover and LW cloud feedback at LIG. Because the melting temperature of ice is virtually constant, we cannot rule out that model biases partly promote the simulated ice-free Arctic summer. The relationship among sea ice extent, sea ice thickness, and cloud feedback warrants further investigation and is likely to be valuable with newer versions of the model that do not require polar filtering, as well as in a multi-model context.”

7 Line 341: The phrase "the difference between $\Delta LIGvC (=LIGvC - PIVC)$ and $\Delta LIGvA (=LIGvA - PIVA)$ " is redundant. Try to just simplify to "the difference between $\Delta LIGvC$ and $\Delta LIGvA$ ".

We changed the expression as suggested.

Reply to reviewer 4

Thank you very much for taking the time to review the manuscript carefully. Thank you also for the encouraging comment and many useful suggestions. Below, we indicate the reviewer's comment in *italic* form and our reply in **bold** form.

Review of the manuscript „Impact of the temperature-cloud phase relationship on the simulated Arctic warming during the last interglacial“ by Arima and Yoshimori et al.

Overview

In their study Arima and Yoshimori et al. address the problem that many climate models are too insensitive to the radiative forcing of the Last Interglacial (LIG) to reproduce current inferences on the state of the Arctic Cryosphere. The literature has addressed many hypotheses and studied how retuning parameterizations of climate models optimized for current climate could reduce some of the biases of models with regard to inferences from the geologic record. In their study Arima and Yoshimori et al. shortly refer to previous work that aims at explaining potential causes for limited model sensitivity when simulationg LIG climate (including vegetation dynamics and melt-pond scheme). Based on this motivation the authors focus on another aspect of climate model formulation and demonstrate, how changes in cloud parameter sets, in combination with and without dynamic vegetation, can lead to differences in simulated climates. The authors find that increasing the availability of supercooled liquid water warms the Arctic and reduces LIG summer sea ice via different behavior of low-level clouds.

*The research presented by Arima and Yoshimori et al. presents a prime example of study that a) employs paleoclimate as a laboratory to understand potential reasons for model biases with respect to our inference from the geologic record, b) applies that understanding towards developing a modified climate model, and c) critically evaluates the changes in the context of other uncertainties in the model. I find the presented research both important and generally very well written. I recommend it for publication in *Climate of the Past* after addressing or rebutting a number of comments (mostly minor) that I suggest for consideration by the authors when creating a revised manuscript. Please refer to details below.*

Overarching comments

The relevant mechanism that is the focus of this study is explained in detail in the introduction and mostly supported by referenced relevant literature, contributing to the comprehension of the presented research. Exceptions to this rule are, in my humble opinion, the explanation of the technical details in Section 2. From statements on pages 4 and 5 I interpret that the actual relevant model parameter to change is T_{ice} , and that the other two parameters are merely modified to retune the simulated climate to maintain sufficient model skill for some observed modern climate patterns. Presenting this information further up in Section 2.2 may simplify comprehension on the side of your readership. Highlighting T_{ice} as the relevant change (maybe even in Table 1) would make this even clearer. Lots of parameters are referenced („ α , β , g and C_c “ are constants“; „ V_0 and δ are constants“). Please make clear in the text whether these are the same as those that you specifically target in your simulation, and also describe what the other symbols refer to (unless they are not of interest to this study, which would then pose the question why they are mentioned here).

As the reviewer understand correctly, the target cloud parameter that was motivated to change is only T_{ice} , but two other parameters were also retuned to avoid model drift. It turns out that the effect of these latter two parameters are not critical for our conclusion. For transparency, we retain Eqs. (2) and (3), and their description in the main text, but we now move Eq. (4), which is relevant to Fig. S1, to the supplementary to avoid overwhelming details. In addition, we separate a sentence about α and V_0 into two sentences right after introducing them for clarity: “In the cloud parameter set L, the coefficients associated with autoconversion rate α in Eq. (2) is increased compared to set S (Table 1)”, and “In the cloud parameter set L, the coefficient associated with ice sedimentation rate V_0 in Eq. (3) is increased compared to set S (Table 1).”

Some formulations and text sections remain unclear to me and are (in my personal opinion) overly complicated. Examples: line 103: „We note that the parameter set “B” referred to the parameter set A applied to a different model version in their study as well as in Sherriff-Tadano and Abe-Ouchi (2020), and we retain the names “A” and “C” in this study.“ Here a reader, who does not already know details of the model parameter sets, may feel overwhelmed. Given that the research presented here should be relevant well beyond the MIROC community I suggest to aim for easier comprehension. The whole paragraph seems to be providing very detailed technical information without providing an explanation at a higher flight level.

The authors provide a clear description of the expected impact of changes in selected parameter values (line 140f). I think that highlighting this aspect more, maybe even further up, may simplify reading the various technical details (that are important nonetheless). Ideally, one could add in the discussion section later on whether there were any surprises found in the modelled results; i.e., where there any simulated changes in climate characteristics that differed from what the authors initially expected?

We removed the sentence of “We note that the parameter set “B” referred to the parameter set A applied to a different model version in their study as well as in Sherriff-Tadano and Abe-Ouchi (2020), and we retain the names “A” and “C” in this study.” To simplify further, we moved part of the description (about Eq. (4)) to the supplementary, as in the reply above. Regarding the “surprise”, the basic qualitative behavior follows our expectation, but we added in the conclusion: “It is somewhat surprising that the difference between the two cloud parameter sets induces greater winter warming in both the near-surface atmosphere and the ocean mixed layer than does LIG radiative forcing”.

It may be a personal limitation on my side, but I was sometimes a bit confused on the parameter set names „A“ and „C“. The relationship of the other parameter set, „B“, to „A“, and what its difference and relevance to the work at hand might be, remains unclear. If there is no good reason to keep the names as they are (maybe „A“ and „C“ carry a specific meaning in the MIROC community that must be maintained for clarity) then I would maybe come up with clearer names that aim for simplicity and comprehension within this current manuscript.

In the original manuscript, the names were chosen to be consistent with those in the published papers by Sherriff-Tadano et al. (2023). We removed all irrelevant descriptions about the cloud parameter set “B”, which is not used in this study. We also renamed “A” and “C” to “I” and “L”, respectively, with the explicit reason that “I” refers to more ice cloud water and “L” to more liquid cloud water at the same temperature.

A bit more explanation regarding the model-data comparison and the analysis of contributions of different actors in the energy balance to the overall temperature change would be helpful. While at first sight it appears that the energy balance analysis may be done based on results from the fully coupled atmosphere-ocean simulation including sea surface, some statements in the text (subsurface heat from the soil rather than from the ocean) and the selection of the proxy data base may hint that the focus is on atmosphere model output (standalone atmosphere model, see lines 169ff) alone. If so, what is the motivation? Please clarify the text accordingly.

The energy balance analysis has been performed using the fully coupled model, and the upper-ocean temperature response is now included. To clarify the motivation, we added in the paragraph describing the research purpose: “We are particularly motivated to investigate surface air temperature and sea ice because previous studies show substantial multi-model spread and discrepancies with proxy reconstructions in both fields (Otto-Bliesner et al., 2021;

Kageyama et al., 2021).”

Terminology is not consistent across the manuscript, in particular Table 5 and legends of various figures could be improved in this regard. Very important, please make sure that each and every parameter on pages 7 and 8 is clearly defined and explained, ideally also featuring the respective physical symbol in Table 5. This is important in particular for alpha, since that parameter has different meanings between pages 6/7 (albedo) and 4/5 (autoconversion rate).

Thank you for point these out. We corrected them; please see individual replies below.

When referring to the use of the model in previous studies, I suggest to provide a bit of detail in how far the referenced previous work is specifically relevant to the current work. If you did so, then the relevant statements would be a bit more connected to your work. For example: line 99 „This vegetation-coupled climate model has also been used in previous studies (e.g., Hirose et al., 2025; O’ishi et al., 2021).“ and line 91 „This climate model runs computationally very efficiently and has been used in many previous studies (e.g., Chan and Abe-Ouchi, 2020; Kuniyoshi et al., 2022; Sherriff-Tadano et al., 2023).“ A bit more details may be informative towards judging the specific value of this model for the research at hand.

We now emphasize the relevance by adding: “As stated in the Introduction, Sherriff-Tadano et al. (2023) is particularly relevant, as they demonstrated the importance of cloud parameterization in the LGM climate, which is employed in the current study.”, and also: “As stated in the Introduction, O’ishi et al. (2021) are particularly relevant, as they demonstrated the importance of vegetation feedback for LIG Arctic warming, and Hirose et al. (2025) are relevant, as they identified remnant glacial ice sheets as a critical factor in the diversity of past interglacials.”

For some statements that are based on referenced literature more appropriate references could be chosen. For example, statements on the state of the LIG could be made based on primary literature from multi-model analysis in PMIP or proxy-based inferences, many of which may be cited by Gulev et al. (2021), rather than referring to Gulev et al. (2021) alone (choice is of course subjective and this is merely a suggestion).

We added several publications, although we did not describe each of them to avoid a lengthy introduction.

For the statement „given that the Arctic is currently warming much faster than the rest of the world, and sea ice is decreasing dramatically.“ one could provide a reference to observations or reanalyses (again, subjective point of view). Not sure whether the statement „Arctic amplification reaches its peak“ (line 64) is covered by the reference mentioned in the same line before. If applicable, one could add more specific references here.

We added citations: “...given that the Arctic is currently warming much faster than the rest of the world, and sea ice is decreasing dramatically (e.g., Rantanen et al., 2022; Box et al., 2024).” And “...and Arctic warming is seasonally large (e.g., Screen and Simmonds, 2010).”

Specific comments:

line 41: add a comma after „assemblages“? (not sure)

It is now added.

line 69: capitalize Last Glacial Maximum?

It is now capitalized.

line 72: Please address in your discussion to which extent keeping the number of ice-nucleating particles fixed could impact the results.

First, we now clarify the sentence: “For simplicity, this study assumes a fixed relationship between temperature and cloud phase across climates, whereas in reality, this relationship varies with aerosol distribution”. Second, we added the implied uncertainty in the discussion: “While the temperature dependency of cloud phase in the parameter set L is designed to be closer to modern satellite observations in the previous study (Sherriff-Tadano et al., 2023), there is no guarantee that the same relation holds at the LIG environment, when the aerosol distribution is likely different from that of the present-day”. We did not infer the impact of the assumption because it is difficult assess it from this study alone.

line 73: the meaning of the following statement remains unclear to me, please check: „Additionally, we would like to discuss the magnitude of this effect against a range of model spreads.“

We changed the statement to: “Additionally, we discuss whether the magnitude of this effect is

comparable to the range of multi-model spreads in LIG simulations. By doing so, we aim to determine whether this effect warrants concern in the LIG climate simulation.”

line 96: grid -> grid cell?

Corrected.

line 155: „the most dominant vegetation type simulated by PIVa over the last 100 years for each grid is prescribed in both PIfA and LIGfA experiments“ - was this done on grid cell level or at global scale? Does dominant refer to what is shown in Figure 3? This is not clear to me.

It was done on the grid cell level. We now write: ” the dominant (most frequent) vegetation type for each grid cell simulated by PIVS over the last 100 years is prescribed for the corresponding grid cell in both PIfS and LIGfS experiments, and that simulated by PIVL is prescribed in both PIfL and LIGfL experiments. They are described in Sect. 5.1”. We also clarified the caption of Fig. 3 by adding: “These maps show the dominant (most frequent) vegetation type in each grid cell over the last 100 years of the integrations.”

line 185: „whose total sum amounts to the simulated surface temperature change under an excellent approximation“ - Do you maybe mean here: „whose total sum is an excellent approximation to the simulated surface temperature change?“

We rewrote it: “..., whose sum is an excellent approximation to the simulated ST change.”

line 190f: „and Q is the heat storage rate in the subsurface (e.g., heat conduction into the soil layers)“. Does this analysis only refer to the standalone atmosphere simulation that you mention a bit further up? If not, then heat storage in the ocean may be an at least equally relevant process to mention here?

We change it: “heat uptake by the ocean and heat conduction into the soil layers”

line 198: type setting of parameters $evap$, $sens$ and $subsurf$ could be improved (use one letter symbols instead)?

We changed them to LH, SH, and Qs, respectively.

line 280: „It is important to note that the LIG temperature reconstruction by Turney and Jones (2010)

is compiled from the locally warmest time within a wide period of approximately 13,000 years, from 129 to 116 kaBP. Thus, it tends to overestimate the 127 kaBP warming rather than underestimate it“. In the text one nearly overreads that you actually also make a data comparison with Capron et al. (2017). You mention this only in the caption to Fig. 8. I suggest to make the use of two different proxy data sets clearer at this location. Furthermore, you could elaborate on arguments why the comparison to Capron et al. (2017) may be more robust, and you could reflect on differences in the model-data comparison that may arise from using any of those two data sources. Capron et al. (2017) provide some arguments why Turney and Jones (2010) should (maybe) not be used for an evaluation of 127k. If the authors see good reasons for comparing against Turney and Jones (2010) (while they are aware of some of the drawbacks as explicitly stated here), then it would be good to provide at a suitable location of the manuscript a motivation / justification for that decision.

We now replace the figure in the main text comparing simulated data with the annual mean temperature reconstructions of Turney and Jones (2010) and Capron et al. (2017) with a summer temperature reconstruction suggested by Review 2. Nevertheless, we retain the original figure in Supplementary Material for two reasons: “Nevertheless, the comparison is referenced here because the impact of cloud phase representation on SAT is more critical in winter, rather than summer, and it is important to point out that the discrepancy of simulation with this dataset, shown by O’ishi et al. (2021), remains substantial even with the alternative cloud phase representation. We must wait for a well-dated dataset for a more quantitatively solid comparison.” We do not emphasize the comparison with Capron et al. (2017) because only a single data point was provided, but it is now explicitly mentioned in the text.

line 286: add a dot after the bracket; grids-> grid cells?

Corrected.

line 292: delete dot after „Figures.“

Corrected.

line 304: not sure whether the term „grossly“ is the best choice here, it has a very negative conotation.

We removed the term “grossly” because it was an English misuse.

line 314: When reading this I already had forgotten the definition of the abbreviation CRE (line 226).

This term does not appear too often, one could consider to write it out for clarity.

We now spell out this abbreviation.

line 328: capitalize archipelago? (also line 404)

Corrected.

line 329ff: While the argumentation seems plausible, I am asking myself whether the simulation and model output would allow a further test of this statement. Maybe it would be already illustrative to extend Figure 11 by showing more preceding and subsequent months to illustrate when and where the relationship breaks?

A reduction in sea ice cover at these locations is observed in September, October, and November, when low-level clouds increase in LIGvS relative to PIVS, and the relationship appears to be robust in the simulated data. We think that further study of the sensitivity of cloud response to sea ice reduction in the AGCM framework across various times of the year, and the diagnosis of cloud tendency terms, may be useful but are beyond the scope of this paper.

line 344: delete „The“? Use an em-dash (or similar) for the „range“ -sign in October - December?

“the” was deleted. Also, the expression “range” was changed to “from October to December”.

line 348: keep minus sign directly in front of the value to avoid confusion

Adjusted.

line 370: „We added four values of summer (July-September) sea ice area (LIGvA, LIGvC, LIGfA, and LIGfC) to the multi-model data shown in Fig. 13.“ I suggest to refer here to the original publication for clarity.

Added.

line 442: „for the future simulations“- do you refer here to simulations of future climate or to future (i.e. work in progress) simulations of LIG climate?

We clarified it by changing to: “simulations of future climate”.

line 457: „importing the model to“ -> „deploying the model at“?

Changed as suggested.

Figure 1: caption could be more informative. In particular, a description of the meaning of the vertical gray dashed bars (relevant temperature ranges for co-existence of solid and fluid cloud droplets in parameter sets A and C), and a reference to relevant mathematical symbols (e.g. T_{ice}), would make information in the figure more self-contained.

It is now improved: “Fraction of supercooled liquid water in the two cloud-parameter sets S ($T_{ice} = -15^{\circ}\text{C}$) and L ($T_{ice} = -28^{\circ}\text{C}$). Vertical gray dashed lines indicate the temperature ranges within which solid and liquid phases coexist in cloud particles.”

Figure 3: Are these the dominant vegetation types the text speaks of? I.e. are there more vegetation types per grid cell, but you show here only the dominant one? Please clarify the text accordingly.

We clarified: “These maps show the dominant (most frequent) vegetation type in each grid cell over the last 100 years of the integrations. Note that each grid cell is occupied by a single vegetation type, although this may vary over time.”

Figure 4: „Please refer to Table 5 for a description of each component“ - I note that keys in the legend differ from those used in Table 5. This could be harmonized.

They are now harmonized.

Figure 8: I am at a loss what is the difference between this figure and Fig. S4. The modelled temperature anomalies shown for these two figures are obviously different, but figure captions appear identical to me (except for a typo). Could it be that the caption in S4 is wrong and should refer to LIGfA (=LIGfA–PIfA); (b) LIGfC (=LIGfC–PIfC) (replace any „v“ with an „f“).

The reviewer is correct. The caption was wrong, and we corrected it.

Figure 9: increasing font size may lead to easier readability.

It is now improved.

Figure 10: Subfigure b seems to have a partial frame, I guess this is an artifact.

The figure was redrawn.

Figure 11: increase font size

It is now improved.

Figure 12: increase font size

It is now improved.

Figure 13: You use different abbreviations for Northern Hemisphere in x-axis label and caption

We now use the same abbreviations.

Appendix:

Is there a good reason to have a rather short appendix rather than implementing this information directly in the main text?

The appendix has been substantially expanded in response to other reviewers' comments, and we would like to keep it as an appendix so readers can focus on the main storyline.

Figure A1: Increase font size

It is now improved.

Figure A2: Increase font size

It is now improved.

Supplement:

For those Figures that are similar to Figures in the main text it could be made clearer in the captions what is the difference to the Figure in the main text. In one case absence of such clear statements, in

combination with a series of typos („v“ instead of „f“) left me clueless regarding the data shown. Explain the abbreviations (PIvA, PIvC, LIGvA, LIGvC) used in Fig. S1 within the supplement. While you refer to the main text for details, the location where you reference Fig. S1 first (Section 2, line 139) does not directly provide these definitions, which rather follow later in Section 3, thereby leading to unclarity of their meaning at this place.

We added a brief explanation and a reference to the main text in the Supplementary.

Not sure whether there is a more meaningful location for the data availability disclaimer (currently in line 25ff). Please check whether it must be put here rather than in the main manuscript's corresponding data availability and disclaimer sections.

It has now been moved to the main text.

Fig. S4: potentially an error in the caption, see my remark at Fig. 8

It is now corrected.

line 15: remove brackets around the unit? grids -> grid cells?

The brackets were removed, and “grid cells” are used.

line 22: not sure whether „crude“ is the right term here (it is also used at some other locations at the manuscript, please also check there)

It was removed here and was replaced by “not rigorous” in the other place.