Response to Reviewer 1

The authors are grateful to the reviewer for their valuable input. The recommendations of the reviewer have been carefully incorporated into the revised manuscript, as described in the following (text colored blue is extracted from the reviewer's feedback; text in italic font represents excerpts from the revised manuscript). Additionally, the guidance that we received regarding figure enhancements was extremely helpful, and we have made the adjustments necessary to improve both the clarity and the impact of the figures.

Kobayashi and colleagues present a novel offline simulation conducted with a carbon isotope-enabled biogeochemical model, forced with climate data from a transient MIROC4m simulation of the last deglaciation. Their investigation focuses on the relationship between changes in atmospheric CO_2, stable and radiocarbon, and the varying AMOC during that period. The simulation reveals relatively minor changes in atmospheric CO_2 concentration compared to ice core reconstructions, while demonstrating that changes in water mass ventilation and sourcing align with proxy reconstructions. The authors further analyze the simulated changes in p CO_2, attributing them to physical (temperature and salinity) and biogeochemical (dissolved inorganic carbon and alkalinity) drivers, revealing complex interactions and compensating effects.

The manuscript is well-written and well-illustrated. While similar studies have been previously conducted with intermediate complexity models, this study represents a significant step towards comprehensive transient simulations with an AOGCM, despite not fully achieving this here. The authors transparently acknowledge certain critical factors during the last deglaciation, such as sea-level rise, ice sheets, and Southern Ocean sea surface temperature biases, which were not accounted for in this study. Considering all these processes for a deglacial simulation is very challenging in such a complex model, making it understandable that they are not fully considered here. Therefore, I recommend the manuscript for publication in Climate of the Past after minor revisions. Detailed comments outlining specific areas for improvement are provided below.

Thank you for your understanding of the key aspects of our research. We validated the calculated carbon isotope ratios by comparing them with sediment core records, and we assessed the current advances and limitations regarding simulation of the deglacial changes in the carbon cycle.

Comments:

L4: Better introduce the abbreviation AMOC already here than in L7.

As recommended, we have defined the meaning of the acronym "AMOC" at the point at which it is first used. In response to feedback from reviewer #2, we have completely revised the structure of the abstract.

L5: Here and in the following, I would replace "atmospheric partial pressure of carbon dioxide" just with "atmospheric concentration of CO_2".

We agree with this suggestion. The term *"atmospheric partial pressure of carbon dioxide"* has been replaced with *"atmospheric concentration of carbon dioxide (pCO_2)"*.

L30: deglacial period not deglaciation period.

The term *"deglaciation period"* has been replaced with *"deglacial period."*

L43: investigate not inter.

The typographical error *"inter"* has been corrected to *"infer."*

L80: What is meant by vertical one-dimensional distribution in this context? Previous studies have evaluated the 3D distribution of data.

As you correctly mentioned, previous EMIC experiments calculated the three-dimensional distribution of carbon isotope ratios. However, in the context of model–data comparisons, the focus often remains on their horizontally averaged one-dimensional distribution over ocean basins. This paragraph has been carefully revised to present the information clearly.

L87-92: The setup of the model is not fully clear to me. In line 87 it is mentioned that a BGC model was coupled to an ocean model, but in line 90 it is stated that the BGC model was forced with MIROC output. Does this mean that the coupled BGC-ocean model was forced with atmospheric boundary conditions of MIROC? Later on it reads to me as if the BGC model is run entirely in offline mode. Can the authors clarify this in section 2.1?

We conducted offline experiments of the ocean biogeochemical cycle, forced with the output from the AOGCM MIROC. The ocean biogeochemical cycle model was run within the COCO ocean model framework. We have revised Section 2.1 to explain the experimental design more clearly.

L91: Can you briefly mention how the MIROC transient simulation was forced, e.g., freshwater fluxes GHG concentrations etc.?

The MIROC 4m experiment focusing on the last deglaciation was performed according to the PMIP protocol (Ivanovic et al., 2016, GMD) with respect to the changes in orbital parameters and greenhouse gases throughout this period (Obase and Abe-Ouchi, 2019). In that earlier work, they fixed the ice sheet to the 21 ka BP state of the ICE-5G reconstruction. Freshwater inputs from the Northern Hemisphere ice sheets deviated from the PMIP protocol after the latter half of Heinrich Stadial 1, but we never stopped the freshwater input at any time (details in Obase and Abe-Ouchi). This procedure was intended to synchronize the simulated AMOC variations with those reconstructed from sediment core records and the associated climatic changes that occurred during the Bølling–Allerød and Younger Dryas periods.

The information on the MIROC 4m experimental design is explained in Section 2.2 of the revised manuscript.

L110: Why were pre-industrial and not LGM values used to initialize these atmospheric values?

As you noted, atmospheric CO_2, $\delta^{13}C$, and $\Delta\Delta^{14}C$ values were all initialized to preindustrial levels for the spin-up under the 21 ka BP forcing. This is consistent with the method used by Kobayashi et al. (2021, *Sci. Adv.*; K21). This approach might increase discrepancies between the model and the observational data, especially during the early deglaciation. However, even if the spin-up occurs using LGM atmospheric values, the discrepancy between the model and the data would become larger in the latter half of the deglaciation.

L138: Can you give numbers for the AMOC strengths during the LGM and the Holocene?

The strength of the AMOC is defined as the maximum meridional volume transport between 30°N and 90°N at depths below 500 m. The calculated strengths of the AMOC at 21 and 11 ka BP are 9.0 and 17.4 Sv, respectively. The derivation of these values has been described in the revised manuscript. Additionally, the definition of the AMOC has been added to the footnote of Fig. 1.

L146: According to Fig. 2j the Pacific appears to me simulated in rather good agreement with the data. Certainly, much better than the Southern Ocean.

As you mentioned, the calculated changes are closer to the data in the Pacific Ocean than in the Southern Ocean. The text has been revised to focus on the Southern Ocean.

L151: Maybe it is worth mentioning, that indeed the agreement of Kobayashi2021 is much better in the deep Southern Ocean, but quite a bit worse for the mid-depths. It therefore appears to me that it is not as simple as including these processes.

We agree with your comment. The reproduction of carbon isotopes of the Southern Ocean in K21 is notably more accurate than in this study; however, changes appear somewhat exaggerated in other regions, e.g., the Pacific Ocean. It is currently difficult to achieve a consistent scenario that explains all the changes in the global ocean. We have added a description in the revised manuscript outlining this difficulty:

"A comparison between the two studies highlights the advances made by Kobayashi et al. (2021) in capturing the dynamics of the Southern Ocean, suggesting that incorporating the processes considered in their research could improve model-data agreement. However, the challenge remains that their LGM simulation slightly overestimates changes in the glacial Pacific. These discrepancies highlight the difficulty of achieving consistent scenarios that account for all changes in the global ocean within a model."

L160: Can you give a reason why the SST difference between the LGM and Holocene is so small compared to observations?

In the MIROC LGM experiment, the AMOC oscillates with a very long period. The initial state of this study corresponds to the physical field long after the AMOC has weakened. Consequently, the meridional heat transport weakens, and both the deep sea and the Southern Ocean tend to warm. Furthermore, although the climate sensitivity of the MIROC model is not small at 3.9°C, the LGM SST in the Southern Ocean tends to be low (Obase et al., 2023, Clim. Past. discuss). With respect to this factor, Obase et al. (2023, CPD) discussed how the weak LGM AMOC in the MIROC, as well as ice sheets and cloud radiation, influence the LGM SST.

MIROC exhibits small SST differences between the LGM and the Holocene, particularly in the Southern Ocean. In fact, a recent six-model intercomparison of the last deglaciation shows that MIROC tends to have smaller SST changes than other models, although the equilibrium climate sensitivity of the MIROC model is not small at 3.9°C (Fig. S4 of Obase et al., 2023, *Clim. Past. Discuss*). One possible explanation is the state of the AMOC during the LGM. The initial state in this study is derived from the physical field long after the AMOC has weakened. It is important to note that the AMOC oscillates on millennial timescales in the MIROC under the LGM condition. Consequently, the weakened meridional heat transport tends to warm the Southern Ocean more than in other models. Furthermore, Obase et al. (2023, *Clim. Past. Discuss*) discussed that the asymmetric responses to warming and cooling, associated with LGM ice sheets and cloud radiation, might contribute to the smaller LGM SST changes despite the relatively high climate sensitivity.

L178: There appears to be very little change during the transition from the LGM to HS1, which makes sense, since the AMOC also remains virtually constant. Maybe this needs to be hence slightly rephrased.

As you correctly noted, weakening of the AMOC during Heinrich Stadial 1, as suggested by $^{231}Pa/^{230}Th$, is very small in this study. This statement has been changed to refer to the period after the Bølling–Allerød transition.

L203: As mentioned before, the difference between the LGM and HS1 seems very small in the simulation, because the AMOC also changes very little. In contrast, a larger decrease is observed in the data.

As you mentioned, the changes in $\delta^{13}C$ during Heinrich Stadial 1 are minimal compared to the data, which can be attributed to the small changes in the AMOC in the MIROC. The following sentences have been added in the revised manuscript:

"However, the observed $\delta^{13}C$ change is relatively small compared to the sediment core data because the AMOC change is less pronounced than expected from the $^{231}Pa/^{230}Th$ reconstruction (McManus et al., 2004; Ng et al., 2018)."

L209: Mention that this is most pronounced in the Atlantic.

In accordance with your suggestion, the following sentence has been added in the revised manuscript: *"This change is most pronounced in the Atlantic."*

L210: It is important to note, that two different things are compared here. The model output has an annual resolution and therefore shows the "true" perturbation magnitude. On the other hand, the reconstructions from marine sediments are smoothed out by processes such as bioturbation, coring artifacts, etc. It is therefore expected that the signal amplitude is bigger in the model than the data for such short perturbations like the YD.

We appreciate your insightful advice on the comparison between the model output and the sediment core data. We have included this note in the Discussion section regarding the explanation of the differences in temporal resolution between the model data and the sediment core data.

L211: Maybe explicitly mention that this is for the reconstructions.

This comment appears to refer to the point regarding L210. Building on the previous response, we have included discussion of the inaccuracies in sediment dating and temporal resolution issues associated with the sediment core data.

L224: This is rather surprising to me, as the AMOC strength actually doesn't change much, but the change in carbon export is rather large in the South Pacific. I'm therefore wondering whether this can really be attributed to an AMOC weakening, or whether other processes dominate this effect? From the pattern and the fact that this negative anomaly persists throughout the deglaciation independent of the AMOC strength, suggest to me that this is primarily a signal of increased iron limitation, which is mentioned in the text.

During Heinrich Stadial 1, the AMOC weakens slightly, resulting in nutrient accumulation in the lower cells of the meridional overturning circulation and in reduced nutrient supply to the South Pacific gyre. Furthermore, we hypothesize that increased iron limitation also contributes to the reduction in carbon export in the South Pacific. For more quantitative understanding, comparison with sensitivity experiments that fix the atmospheric iron supply at the LGM state is necessary. Additionally, multi-model intercomparisons using different iron cycle models and ocean models would be valuable to confirm the validity of the proposed mechanisms.

L227: By which mechanism propagate these changes to the North Pacific?

When the AMOC is strengthened during the Bølling–Allerød period, more nutrients are redistributed to the upper cells of the AMOC. These increased nutrients are outcropped in the lower latitudes of the Southern Ocean. Consequently, the transport of nutrients by surface and intermediate waters from the Southern Ocean to the North Pacific increases.

L304-310: It could also be that the AMOC weakening is too strong in the model.

Comparison of the model and the sediment data for $\Delta\Delta^{14}C$ and $\delta^{13}C$ supports the suggestion that weakening of the AMOC during the Younger Dryas period might be overly pronounced in the model. The relevant sentences have been amended in the revised manuscript as follows:

"Another important factor is the weakening of the simulated AMOC during the YD. The comparison of the model and sediment data for $\Delta\Delta^{14}C$ and $\delta^{13}C$ *suggests that the weakening of the AMOC during the YD may be overly pronounced in the model."*

L334-344: The terrestrial biosphere plays an important role for atmospheric δ13C, which I think should be mentioned here as well (see e.g., Jeltsch-Thoemmes et al., 2019, doi:10.5194/cp-15-849-2019).

As you correctly noted, changes in vegetation also contribute to the deglacial changes in atmospheric $\delta^{13}C$–CO_2. We have added the reference that you suggested, reorganized the text in the Discussion section, and summarized the description of terrestrial carbon reservoirs in Section 4.3.

Fig. 1: I understand the intention to make the model and data timeseries overlap for better comparability. However, I find this in panel c somewhat misleading, as there is a factor of more than two between both y-axes. I would like to see the same increment for both axes as it is done in panel b. Further, can a panel showing global mean surface temperature be added, for instance compared to the data assimilation by Osman et al., 2021 (doi:10.1038/s41586-021-03984-4)?

Thank you for your feedback. Panels (b) and (c) in Fig. 1 have been adjusted to have the same scale for the vertical axis.

Additionally, the global mean surface temperature (GMST) changes are shown below. Because the ice sheet is fixed at the state of the LGM, the GMST has increased only by approximately 2.5° C.

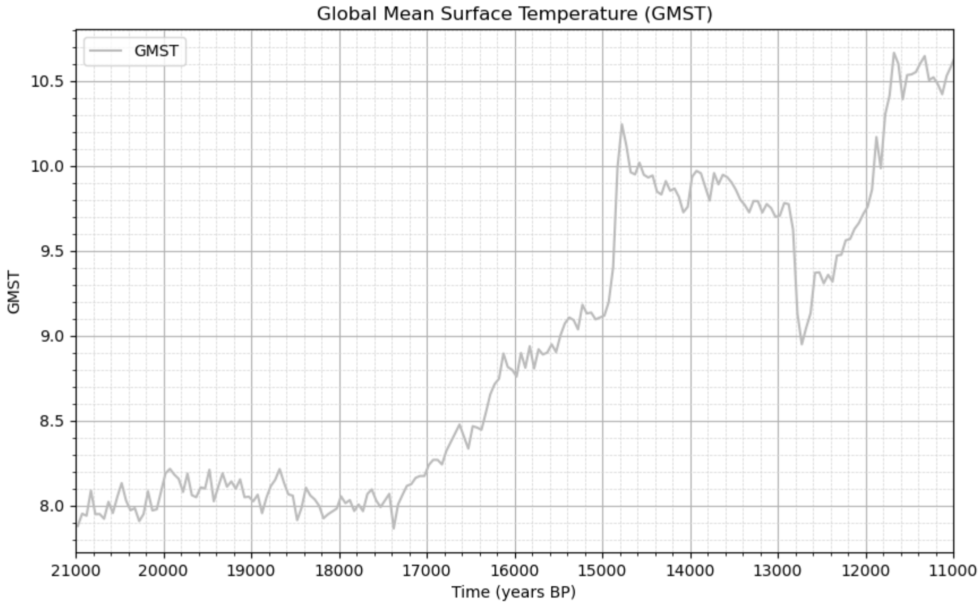

Fig. 3 and 4: Can you add a similar figure like these two, but for DIC (anomaly?), to better illustrate where the carbon was stored in the course of the deglaciation?

A figure showing the temporal changes in DIC was added to the Supplementary Figures.

Figs. S4 and S5: The panels are very small and hence hard to read. Can they be made larger, splitting the panels into different rows or columns?

The panels in Figs. S4 and S5 have been enlarged and reorganized into different rows to improve readability based on the feedback received.

Fig. S7 to S9. I find both these figures very instructive and if possible would like to see them in the main text.

Figures S7–S9 have been incorporated into the main text as Figs. 6–8, respectively.