Review of “Assessing transient changes in the ocean carbon cycle during the last deglaciation through carbon isotope modeling” by Kobayashi et al.

The manuscript presents results of a transient simulation of the last deglaciation with an ocean carbon model forced with MIROC4m outputs (from Obase and Abe-Ouchi, 2019). The authors assess the simulated pCO_2, $\delta^{13}C$ and $\Delta^{14}C$ variations between 21 and 11 ka, in response to AMOC changes – in particular an abrupt increase at the onset of the Bølling-Allerød and Holocene and an abrupt decrease for the Younger Dryas. Using model-data comparison for both $\delta^{13}C$ and $\Delta^{14}C$ and a decomposition analysis of the pCO_2 changes, they are able to discuss to some extent the processes behind the large glacial-interglacial CO_2 variations recorded in ice cores, also building on Kobayashi et al. (2021) results. The observed model-data (mis)matches for $\delta^{13}C$ and $\Delta^{14}C$, which can differ, are informative in terms of processes and could pave the way for further modelling efforts targeting the last deglaciation.

I think that this study is well-suited for Climate of the Past and that the simulation, results, and analysis presented in the article are all worthy of publication. Overall, the manuscript is well-written, although there are places where the writing style and flow could be improved for the reader to follow more easily the scientific reasoning of the authors. To give this study more weight, I also think that the authors should not shy away from underlining more its strengths in a number of instances, explicitly connecting an important research question to a demonstrated knowledge gap ; as well as its weaknesses, hopefully providing for a clearer path forward for modelers interested in this question. I am providing below a number of points to help guide the authors in this direction. Since most of my comments are suggestions of improvement of the writing style/flow/clarifications, I recommend publication after minor revisions.

General comments

1. Abstract structure : The abstract starts out very abruptly with some of the methods (L1) and technical details (L2). I think that delineating the overall subject, why it matters, knowledge gaps and an explicit scientific question should come first for the reader to clearly understand the scientific reasoning behind the study. What is the scientific problem ? Why is it chosen ? Which methods are (thus) proposed to tackle it ? Some knowledge gap can be found right at the end of the abstract (L22-23), introduction (L47-51) or in the discussion (L273-277). Methodological elements are scattered in the abstract (L1-3, L7, L11, L19). I would recommend rearranging all of these elements so that the reader is not given the impression of a list of results, but of a logical approach to tackle an outstanding problem.

2. Introduction and study originality : Although the introduction has a clearer structure, I think that it lacks – like the abstract: (1) a clear explanation of the stakes (why is understanding these processes key?) ; (2) explicit connections between the limitations of previous studies (defining a knowledge gap), a scientific question, and the methods therefore chosen to tackle it. Without these elements, it is difficult for the reader to see the originality (asset, novelty) of this study.

3. Transitions : throughout the manuscript, there is a lack of transitions in-between paragraphs. This slows down reading as the reader has to stop and think about how the new idea is related to the previous one. There are parts (e.g. Discussion) where this absence of transition (and therefore of clear structure) makes the reader a bit lost. I would recommend using more link words (of other types of explicit connections) to make the reasoning more visible (and therefore easier to follow). I have underlined a few examples in the specific comments for guidance.

4. Limitations : Neither the consequences of using a fixed ocean volume (L123-125) and restoring term for carbon isotopes (L126-129) are discussed in Sect 4.3. Such a discussion would be welcomed. Indeed, Sect. 4.3 “Implications for future improvements to the model and experimental design” discusses in length potential model developments, but not much the improvements which could be made to the experimental design. However, both Snoll et al. (in review, 2023) and Bouttes et al. (2023) have demonstrated the influence of the choice of forcings (respectively, freshwater fluxes and interactive bathymetry) on model results. Perhaps describing potential improvements in terms of experiment design as well could help identify a clearer way forward (I mean some kind of shorter term strategy, as Sect. 4.3 and the conclusion are both sending off a rather vague “we need to improve models” message – which is relevant, but in large part a long term endeavor).

5. Tense : I think that the past (e.g. L3, L8, L9… and throughout the manuscript) and past perfect (e.g. L20, L22…) tense tend to make statements less effective than present tense.

Specific comments

L5 and L7 : “increased”, “decreasing trend”. By how much? More frequent quantification would be welcomed.

L12-13 : “We found that…”. This statement seems to be contradictory with Fig. 2, unless “after the onset of the BA” is specified.

L17-18 : “smaller atmospheric pCO_2 changes than ice core data”. Are you referring to changes during HS1 or during the whole deglaciation?

L26-27 : the “which is” proposition interrupts the “from … to…” statement, giving a jerky rhythm to the sentence.

L31 : An example of where a transition (e.g. “To decipher the reasons behind those changes,…”) would be welcomed. Same for L34 (e.g. “In particular,…”, with no line jump).

L37 : “Therefore” provides for an incomplete argument for different $\delta^{13}C$ in water masses, for only the impact of fractionation during photosynthesis is described before – not including the impact of ventilation on the isotopic signal.

L54 : Are you using the plural form to designate both the soft tissue and the carbonate pumps combined? According to Kohfeld and Rigwell (2009), strictly speaking, the biological pump (singular) encompasses both.

L55-59 : I find the explanation of the limitations of the use of steady-state differences (and therefore, of the asset of using transient simulations) to be incomplete and therefore only partially convincing. Could you elaborate ?

L59 : The change of tense (“will improve” to “have been conducted”) makes it confusing for the reader to understand what exactly is the knowledge gap, and what is new in this study with respect to previous studies.

L66 and L71 : the lack of transition with “other related studies”, “several related studies” makes it difficult to follow the reasoning and understand where this paragraph is going. I would recommend connecting ideas rather than juxtaposing them.

L71-75 : this sentence contains many processes and seem therefore very long. Could it be divided into two? As for the references L75-76, it is unclear whether they refer to either one, or all, of the mentioned processes.

L78-80 : it seems unclear to me (1) what were the conclusions of those previous studies, (1) what is their limitation you are mentioning, and therefore (3) the novelty brought by your study. Could you elaborate ?

L90 : “boundary conditions” is a rather technical jargon which, depending on the model setup, can designate various things. I would prefer using “was forced with” instead.

Sect. 2.2 : A little surprisingly, the PMIP4 protocol (Ivanovic et al., 2016) is not mentioned. It could be worth noting whether the AOGCM simulation by OA19 followed the PMIP4 protocol for deglacial simulations, and if not, where it differed.

L106 : Without reading the quoted paper, these values and how they were chosen seem a little mysterious.

L115 : Please give out here a quantified value of the pCO_2 simulated in the 2021 paper. Same in L162. The authors could also consider adding a triangle for this value in Fig. 2k.

L124 : Ocean volume change would also induce changes in alkalinity and nutrients, not only dissolved matter concentration.

L142 : “underestimates”. As the simulated values are less negative than the reconstructed ones, I am unsure whether “underestimates (how low these values are)” or “overestimates” should be used. This English vocabulary should be checked.

L155 : “approximately 80 ppm”. The value here is lower than the one chosen L28. It is best to use consistent numbers.

L156 and L161 : “One possible explanation” / “Another possible explanation”. This vocabulary may give the reader the impression that theses reasons are mutually exclusive, which is not the case.

L172 : “at times”. Could you specify which times? and why?

L175-178 : This sentence brings very little new elements with respect to L174-175 (except the numerical value of -200 permil).

L180-181 : The link between the AMOC and the deep Pacific ventilation is unclear.

L183 : “do not provide clear indication of the intrusion of young water masses”. Where is this evidence? Please quote a figure number.

L184-185 : “whereas the model experiment does not show such pronounce change [during HS1]”. I find this statement to be slightly misleading, for I would expect that the absence of a large increase of simulated $\Delta^{14}C$ to be related not to a model error during HS1, but to the very high values (wrt. reconstruction) inherited from the initial state (i.e. not enough carbon sequestration in the ocean at the LGM).

L188 : “ $\Delta^{14}C$-CO2 (Fig. 2a)”. Please explain this choice of plot.

L190-195 : This paragraph feels disconnected from the previous descriptions. A transition would probably help integrate it explicitly in the reasoning.

L200 : Could we perhaps see a figure of biological production as well in Supplementary?

L199-201 : How is a reduced vertically gradient in response to a stronger AMOC related to the sensitivity of $\delta^{13}C$ to climate change?

L204-206 : The reasoning seems incomplete. How should we interpret this different model-data agreement for the North Atlantic / Southern Ocean?

L207: “the AMOC is intensified”. Add “and deepens”.

Sect 3.3.2 and 3.3.3 : Like in Sect 3.3.1, I would welcome here a quantification of the atmospheric pCO_2 changes occurring during the BA and YD.

L260 : “the AMOC resumes over time”. This feels like an inaccurate description of the AMOC variations, as Fig. 1 rather shows a stabilization during the BA after an overshoot at the onset of the BA.

L280 : “after the BA transition are generally consistent”. Starting out this paragraph like this is a bit surprising (in terms of chronology), as this statement does not acknowledge the large model-data gap before the BA transition.

L193 : “a longer period”. Please quantify the difference. The forcing in terms of meltwater fluxes also seem to differ between the two studies.

L297-300 : Although this seems like conclusive remarks, I am not coming out of the paragraph with a clear idea of what the comparison to the Pöppelmeier et al. (2023) results actually brought to the table. Clarifying the transition (L286, L301) could help.

L303 : “to the deep ocean”. Do you mean deep Atlantic ocean? This mismatch seems larger in the Atlantic than Southern Ocean.

L312-313 : Could you specify? As such, this is an underwhelming statement.

L321 : “AMOC weakening” could be changed to “AMOC weak state”, since the actual weakening occurs at the onset of these events.

L323 : Shouldn’t the contribution of SST during YD and HS1 also be mentioned?

L325 : Could we perhaps see a figure with the vertical gradients in Supplementary?

L327 : “increased SST and reduced surface ocean alkalinity”. During which part(s) of the deglaciation specifically? Or do you mean the whole deglaciation?

L329 : The absence of transition is more notable as we go back in time (reversed chronology here).

L332 : “In contrast” seems like the wrong link word here.

L350-351 : “AOGCM of MIROC”, “EMIC of iLOVECLIM”. The model classification is irrelevant here. The difference in model resolution could be mentioned if the authors would like to propose it as a potential cause for the observed model difference (although difference in forcings could also play a role).

L420 : Add something like “at the BA and YD transition respectively”

L422c : “relatively modest”. Please quantify again.

Fig. 1 : (a) Please specify in the legend what the metric used for the AMOC strength (max at 26°N?). Also, you could consider adding triangles for the Kobayashi et al. (2021) values as in Fig. 2.

Fig. 2 : The “PI_sed” and “LGM_all” simulations could be (briefly) described in the legend so that the reader knows in which way they differ from the transient run initial state without having to look for the simulation description in the 2021 paper.

Fig. 3 : Please specify which ocean basin is where on the plots (Atl = left, Pac = right).

Fig. 4 : The colorbar seems to saturate for very negative $\delta^{13}C$ values in marine core data. Also, the contour interval is too dark and narrow to see to subsurface values. A few adjustments (and larger plots) could improve visibility.

Fig. 3 and 4 : Please consider calculating the RMSE for both proxies and all periods.

Fig. 5 : The thin gray line is not very visible on all panels. I would suggest finding an adjustment (e.g. lowering opacity for superimposed curves) to improve visibility.

Technical comments

L2 : “the effects” on what ? Please specify.

L6 : Introduce “(BA)” abbreviation here.

L10 : “Meanwhile” or “in the meantime”

L26 : “has transitioned” → “transitioned”

L43 : typo “infer”

L52 : “those” → “the”

L119 : “ocean biogeochemical cycle” → add “carbon”

L230 : “of” → “the”

L234 : “those” → “the”

L292 : “almost completely” → drastically (or synonyms)

L317 : Add “… to several factors. First, …” ; L319 : “Moreover” → “Second”

L359 : “are synchronized”

L390 : typo “lowering”

L397 : plural “developments”

L436 : “what” → “which”

Most figures, especially Fig. 3 and 4, would gain to be larger.