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Peer Review File

Centennial-scale intensifications of the Atlantic Meridional Overturning Circulation during Heinrich Stadial 1

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Review Report for "Centennial-scale overshoots in Atlantic Meridional Overturning Circulation during Heinrich Stadial 1" by Jena et al.

In their manuscript titled "Centennial-scale overshoots in Atlantic Meridional Overturning Circulation during Heinrich Stadial 1," Jena and co-authors present high-resolution benthic-planktic (B-P) radiocarbon ventilation data derived from a sediment core collected from the mid-depth western equatorial Atlantic. The authors utilize these data to identify a three-phase structure of the Atlantic Meridional Overturning Circulation (AMOC) during Heinrich Stadial 1 (HS1), including two notable centennial-scale overshoots. To support their analysis, they employ a simulation from an Earth system model of intermediate complexity (EMIC) equipped with a radiocarbon (14C) module, which indicates elevated B-P ventilation ages during HS1. By integrating proxy data with model simulations, the authors propose a connection between AMOC overshoots and deglacial CO2 release. The manuscript is generally well-written, with a logical structure. However, I have identified several significant caveats related to the methodology, results, and the hypothesized CO2 release mechanism. While detailed comments are provided below, these concerns can be summarized into three major points:
1. Additional details regarding the model simulations are necessary to enable a thorough evaluation of model performance and the comparison between model outputs and proxy data.
2. In the latter part of the Introduction, the authors review the relationships between AMOC, upwelling, and CO2. While this is a critical and necessary component of the study's motivation, it requires reorganization to clarify whether these mechanisms operate synergistically or antagonistically.
3. Greater caution is needed when discussing the AMOC-CO2 relationship in Section 2.4. Although the model simulations show consistent B-P ventilation ages, this does not necessarily indicate a specific pattern of carbon sequestration or release. The question of whether an enhanced or weakened AMOC drives deglacial CO2 release remains a subject of ongoing debate.
As I am not an expert in proxy-based reconstruction, I am unable to provide detailed feedback on the processing and analysis of the sediment materials. However, I have included minor suggestions below regarding the plotting in Figure 1.

Detailed Comments

Lines 79 and onward: This paragraph outlines the connection between CO2 fluctuations and the AMOC, which is essential for establishing the motivation of the study. However, both the structure and content require reorganization for clarity and accuracy.
- First, does a weakened AMOC inherently lead to enhanced deep ocean carbon sequestration? I am skeptical of this assumption. The relationship between AMOC and CO2 is not straightforward, as the North Atlantic is characterized by relatively high nutrient utilization efficiency. A reduction in AMOC strength, and thus a decrease in the volume of water ventilated by North Atlantic-sourced deep water, could reduce the efficiency of the soft tissue pump (e.g., see Hain and Sigman, 2024, "CO2 in Earth's Ice Age Cycles"), potentially increasing atmospheric pCO2.
- Second, from Line 89 onward, the discussion of deglacial CO2 changes is presented in a somewhat confusing manner. A key issue is the lack of clarity in defining "AMOC." Does it refer to the volume of water transported across the equator in the Atlantic, or does it simply denote the extent of North Atlantic-sourced deep water? Additionally, does Southern Ocean

upwelling within the Atlantic sector contribute to the AMOC as defined here? These distinctions are critical and should be addressed. For example, Chen et al. (2015) suggest that deglacial AMOC overshoots facilitate CO_2 release through enhanced upwelling, a mechanism similar to that driven by changes in Southern Hemisphere westerlies, which influence carbon storage by altering upwelling patterns. In contrast, Schmittner and Lund (2015) propose that a weakened (or potentially collapsing) AMOC reduces biological pump efficiency, thereby increasing pCO_2. These mechanisms may operate antagonistically, and high-resolution AMOC records, such as those presented in this study, could help determine which process is the primary driver of the AMOC-CO_2 relationship during HS1. The text should be revised to clearly delineate these processes and their potential interactions.
Figure 1: Although I am not an expert in radiocarbon data, I have questions about the presentation in Figure 1. What do the error bars in Figure 1b represent, and how were they calculated? Additionally, why are error bars absent in Figure 1a? The trend in Figure 1b appears clear when considering only the solid squares (presumably mean values). However, the error bars increase in length during HS1 and the Last Glacial Maximum (LGM), suggesting greater uncertainty. Notably, the upper bounds of the LGM data appear to overlap with HS1 data, which may complicate interpretations of distinct differences between these periods.
Line 175: The authors note "striking similarities" between their B-P ventilation age record and 231Pa/230Th records from the North Atlantic. While the 231Pa/230Th data indicate a consistently weak AMOC during HS1, the B-P ventilation ages in this study appear to decrease over time within HS1. Before proceeding with further analysis, I question whether these two datasets are indeed "strikingly similar," as the temporal trends seem to diverge.
Lines 330-333: The authors suggest that a weakened AMOC is associated with enhanced upwelling, which is physically challenging to reconcile. Enhanced upwelling is typically linked to increased wind-driven circulation or changes in ocean stratification, rather than a direct consequence of a weakened AMOC. This statement requires clarification or supporting evidence.
Deglacial CO_2 Discussion: Another factor to consider in the context of deglacial CO_2 release is the role of Southern Ocean sea surface temperature (SST). Dai et al. (2022, Nature Communications) demonstrate that changes in the solubility pump, driven by Southern Ocean SST, could theoretically account for a 30-40 ppm change in surface pCO_2. Their calculations suggest that this process steadily influences CO_2 release from the ocean during HS1. A rise in Southern Ocean SST may be driven by a weakened AMOC, which reduces northward heat transport, leading to warming in the Southern Hemisphere. This mechanism contrasts with the authors' findings:
- Authors' findings: AMOC overshoots during HS1b and HS1c enhance upwelling, resulting in greater CO_2 release.
- Previous calculation: A weakened AMOC reduces northward heat transport, increasing Southern Ocean SST and driving CO_2 release.

Although both scenarios result in CO_2 release from the Southern Ocean, the underlying mechanisms differ significantly. The manuscript should acknowledge and address this alternative mechanism to provide a more balanced discussion.
Lines 434 and onward: The description of the model methodology is insufficient, making it difficult to evaluate the reliability of the model outputs. The current text provides only a brief overview of the CLIMBER-X model, lacking critical details such as specific boundary conditions and experimental design. Given the study's reliance on both proxy data and model simulations, I recommend providing a more detailed methodology section for the model. Additionally, I suggest including a plot of the 14C pattern for late-Holocene conditions to demonstrate the model's performance and validate its applicability to the study period.

Reviewer #2

(Remarks to the Author)
The paper presents new reconstructions of deep ocean ventilation from the intermediate depth tropical Atlantic Ocean across the deglaciation using 14-C in planktonic and benthic foraminifera. The resulting record is impressive and an important and valuable contribution as it is able to depict variability in intermediate ventilation (ages) at an unusually high resolution and does so in an important region. The newly documented centennial scale variations in interior ventilation have bearing on our understanding of the dynamics of deglacial climate and carbon cycle changes and, specifically, provide new constraints for delimiting the role of ocean circulation. Importantly, the work reveals centennial scale intensifications of ventilation during the Heinrich stadial event that is generally considered a period of suppressed ocean ventilation and weak AMOC. The quality of the new records and the discovery of this centennial variability that appears linked to previously described tropical monsoon variability will be of extra-disciplinary interest and thus merits potential publication in this journal. However, when discussing the results the manuscript could be clearer and do an even better job at highlighting the specific significance, novelty, and importance of the results and their implications for how ocean circulation shaped (or not) deglacial climate and carbon cycle changes. Some specific suggestions and observations are:
Lines 66-78 you put extensive effort into describing the discrepancy about whether HS1 was a singular episode or best divided into 2 or 3 fold structure. Beyond the noted discrepancy between records, it is not clear from reading this why this division matters. Also, although you set up this discrepancy, you do not revisit it explicitly later in the manuscript, thus this introduced theme is not well aligned with your discussion and conclusion. Could you revise this to set up a more relevant question to what you are finding and concluding? As it is, I'm a bit lost as to why this is introduced this way and not returned to. I also do not see a very clear 3 fold division in your own records presented or why the division is made. It looks like you have persistently weak ventilation during the first phase (1a) and then it is interrupted by periods of stronger ventilation, or more ventilation variability, in the second part of HS1. Why isn't this just 2 phases, one of weak ventilation and another of weak but more variabile ventilation? What is the difference between 1b and 1c, each has weak ventilation and a short period of strong ventilation? Anyhow, the divisions, how they are defined and their significance are not very clear to me in the current manuscript.

Ventilation age and AMOC discussion is not clear and precise at various places in the manuscript. A linear relationship is

discussed/inferred but when one looks at supplement Fig. 5 there is a very rapid change in ventilation age over just a couple of Sv AMOC change when AMOC is weak (e.g. between 3 and 5-6 Sv there is a change in ventilation between >800 and <400 years—which covers most of the reconstructed variability). The current way it is discussed does not really take this into account. The paper should acknowledge that most of the signal at the core site (based on the model, but also comparison to hydrography) may be driven by relatively modest changes in AMOC when AMOC is relatively weak. Put another way, ventilation at the site is very sensitive to AMOC changes when AMOC is low, but less sensitive to monitoring changes when AMOC is high. This might also explain how/why you get such rapid apparent shifts in ventilation age when other records have not recorded clear/major variability in AMOC at these times. There are some other issues that could be improved when discussing the model results and AMOC-ventilation relations:
a) What is actually done with the model is not clear as the materials and methods (section 3.4) is pointed to but there are very few details here, just which model and boundary conditions and the statement "the implementation of marine D14C essentially follows ref 69" when one looks that up, a number of things were done in that paper making it very unclear what was used from that implementation here. Interestingly when one looks at that paper they suggest that the best LGM state had a 40% reduction in NADW export while in this paper the LGM state in figure 3 has a very similar AMOC to the Holocene? Are these the same simulations because they do not seem particularly compatible? If a different LGM state was chosen (from the best one presented in ref 69) why—because it matched better the data here? Anyhow, the lack of information makes it difficult to know what was actually done and why and how it compares to previous work/conclusions. In any event, these apparent discrepancies, combined with the observation that the largest changes in ventilation in the model occur over a very small range of AMOC made me react to the simple claim on line 235 that your "radiocarbon ventilation age record from the western equatorial Atlantic is a reliable indicator of AMOC strength". I think a little more nuance and detail in how exactly they are related is required.
b) "overshoots". I admit that this term is popping up in the literature but I find it very unclear and problematic since the term assumes an "expected" target/level which is surpassed but I'm not sure what one should expect for a ventilation/circulation level. For your data the deviations are renewals of circulation more than overshoots from a given level. However, if you insist on using it please define it clearly. I suspect it is simply adopted from the Chen et al (2015) paper since this study is relying and building upon those results and discussion. However, they clearly define it as a circulation/ventilation that is greater than the strong circulation of the Holocene. Looking at your plot, and the error bars, I'm not sure any of your periods of intensified/renewed ventilation clearly (beyond error) exceed the modern/Holocene level. Thus, it is not clear that they truly 'overshoot'. Also, please provide information on what the error bars represent (or how derived) in the plot so we know the significance level (e.g. 2 sigma etc.)
c) You argue that the similarity between the model ventilation age change and ventilation ages to that which you observe, underpins their relationship. Its fairly convincing (NB caveats noted above) when one looks at the whisker plots and covariations in different states. However, the focus of the paper is not about these states, it is about the interpretation of the very brief deviations in ventilation age that interrupt these different modes. These are very short lived, but when I look at the model changes in ventilation age, it is not clear to me if such sudden deviations and recoveries are possible for this model (the model ventilation age curves seem to respond too slowly, but its hard to tell). Thus, how relevant/applicable is the model framework for representing these very short lived deviations? It's a question out of interest and it might be worth mentioning or caveating (if for example the model would not predict the magnitude and rate of the observed ventilation changes in responses to AMOC reduction/intensification). In this context its also important to note that very large ventilation changes are found with only minor changes in AMOC in the model (supplement Fig. 5 and comment above).
D)if ventilation is a reliable indicator of AMOC, why is there an offset in supplement figure 6 between increased ventilation and reduced AMOC (increased Pa/Th)--this appears much too large to be proxy response time related.

To me the stated relation between weak ventilation and intense IRD intervals is not so clear as presented in the text. From the figure it appears that this is sometimes true and other times not true (well ventilated period in 1b is associated also with high or rising IRD).

Lines 309-316; you discuss flushing of the tropical thermocline might be driven by the same type of processes as rapid ventilation in the subpolar/drake passage region? The wording is quite vague here making it hard to sort out what exactly is happening in the two areas (and the potential relationship), but I'd be surprised if they were identical processes in these very different hydrographic settings/regimes. Can you please be more specific about your inferences of what processes are going on and how these are or are not related? Given the difference in timing between the events in the two locations, why do you infer they have a common source/cause/dynamic? Why are some important for CO2 and some not? This latter point is critical to answer given the end focus on the implications for co2.

Section 2.4 (discussion of relation between ventilation and atmospheric co2).
I found this section slightly too vague making it hard to come away with clear conclusions or identify what we have learned exactly from the new data. While many of the processes previously discussed in the literature are invoked or repeated, it is done quite generally and it is hard to identify explicitly exactly how the new data are used to test (confirm/refute) these ideas and how they modify our previous picture. Yet, I think the data have a lot to say here! In addition, key processes discussed previously are not fully discussed regarding their compatibility with the new results. For example why not mention tropical terrestrial carbon reservoir changes (e.g. see Nielsen et al 2019 and refs therein/subsequent; https://doi.org/10.1029/2018PA003481) since you show that your ocean circulation changes correspond to changes in tropical rainfall/monsoon—wouldn't this support a role for terrestrial carbon cycle changes? After reading this section I was left with many questions but few answers (sorry). A few outstanding questions/problems I had after reading it which I think a revision with some more specifics could likely answer are:
A) You discuss the sudden rises in co2 as being driven by your ventilation renewal events via flushing of co2 from the interior. If this is the case, why isn't there a co2 rise/jump for every event (e.g. 1c)…also you say that the 14.8 jump is coeval with your renewed ventilation, but your full ventilation renewal seems to occur prior to the beginning of co2 rise?

B) if (respired) stored carbon is flushed from the intermediate ocean as you suggest then why does d13C stay low throughout HS1 (Supplement figure 6)—I'd expect it to respond quite clearly to changes in respired carbon given the previous literature discussions of d13C and B/Ca tracing respired carbon buildup during HS events?
C) What is the buildup timescale of carbon and how quickly can it be flushed out? E.g if you flush regularly (as your data suggest, what does this mean for the co2 capacitance and contribution to the sudden atmospheric jumps?).
D) Lines 359-362 you mention the other possibilities for co2 rise but fail to mention reduced biological pump or terrestrial carbon reservoir changes (see e.g. Nielsen et al., 2019; Lacerra et al 2017).

Stepping back, I'm struck that you see such intermittent/variable ventilation changes during the Heinrich event. Doesn't this influence how we think about ocean circulation and dynamics and our ability to model them? You end the paper mentioning the challenge of modelling the brief co2 pulses, but doesn't it also pose challenges for modelling circulation dynamics as well? How can circulation return to full strength briefly and then be curtailed again—flickering on during a period of suppression. Is this a forced (relaxed forcing) behavior or is it a dynamic system jumping between attractors? Do any models see such rapid state changes (see earlier comment here as well about model representation of short ventilation events)?

Finally, you seem to rule out AAIW changes, yet your core site sits hydrographically between upper NADW and AAIW and seems to have properties of a mixture of the two—I'd acknowledge this as you still have good arguments that the variations follow along with those observed in deeper and N.Atl. regions thus representing NA source changes/ventilation?

In summary, this is a great new data set and wonderful new constraints and very interesting paper for many groups. However, I do not feel like the specific constraints that this data provides (nor their limitations) are fully explored/explained and the discussion largely restates previous arguments without clearly and explicitly explaining how the new results test or alter this picture.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
During this round of revision, the authors have included more description of the model, incorporated extensive discussion of the AMOC-CO2 relationship, and made some re-arrangements of implications. Overall, the quality of the manuscript has improved. However, several caveats or concerns remain that should be addressed. Please see the enclosed document for the details.

Reviewer #3

(Remarks to the Author)
I thank the authors for revising the manuscript in light of Reviewer #2's insightful comments. As the third reviewer, I acknowledge that the authors present a valuable new high-resolution dataset, provide important constraints, and offer a study of broad interest. While many suggestions have been addressed, some critical concerns remain insufficiently answered and are dismissed as being outside the study's scope. In this reviewer's opinion, addressing these concerns is essential for publication in Nature Communications, and the authors appear to have the necessary modeling data to do so. Therefore, while I appreciate the authors' extensive efforts, I recommend another major revision before the manuscript can be considered for publication. I remain optimistic that the authors can successfully address the outstanding issues.
Reviewer #2 Comment:
Ventilation age and AMOC discussion is not clear and precise at various places in the manuscript. A linear relationship is discussed/inferred but when one looks at supplement Fig. 5 there is a very rapid change in ventilation age over just a couple of Sv AMOC change when AMOC is weak (e.g. between 3 and 5-6 Sv there is a change in ventilation between >800 and <400 years—which covers most of the reconstructed variability). The current way it is discussed does not really take this into account. The paper should acknowledge that most of the signal at the core site (based on the model, but also comparison to hydrography) may be driven by relatively modest changes in AMOC when AMOC is relatively weak. Put another way, ventilation at the site is very sensitive to AMOC changes when AMOC is low, but less sensitive to monitoring changes when AMOC is high. This might also explain how/why you get such rapid apparent shifts in ventilation age when other records have not recorded clear/major variability in AMOC at these times.
In response to this the revised manuscript has categorized the data points (Supplementary Fig. 5) into two groups: (i) data points with AMOC < 10 Sv; and (ii) data points with AMOC > 10 Sv. They show a clear distinction emerges between the two groups, with the weak AMOC phase displaying a stronger correlation between AMOC and radiocarbon ventilation age (R^2 = 0.67) versus the correlation between the strong AMOC and radiocarbon ventilation ages (R^2 = 0). However, this raises another question that during strong AMOC the authors find no linear relationship. They should explain this in detail if they want to support their finding that only weak AMOC phase display a stronger correlation between AMOC and radiocarbon ventilation age. And if so, why?
Reviewer #2 Comment:
There are some other issues that could be improved when discussing the model results and AMOC-ventilation relations:
(a) What is actually done with the model is not clear as the materials and methods (section 3.4) is pointed to but there are very few details here, just which model and boundary conditions and the statement "the implementation of marine D14C essentially follows ref 69" when one looks that up, a number of things were done in that paper making it very unclear what was used from that implementation here. Interestingly when one looks at that paper they suggest that the best LGM state had

a 40% reduction in NADW export while in this paper the LGM state in figure 3 has a very similar AMOC to the Holocene? The revised manuscript should provide more detail on the model boundary conditions for clarity. The model is forced with insolation, greenhouse gases, continental ice sheets, and atmospheric $\Delta^{14}C$. However, it remains unclear how AMOC changes are simulated. For example, most model experiments (including TraCE-21ka) simulate AMOC weakening through explicit freshwater forcing. Does the employed model include meltwater flux from continental ice-sheet melting? The authors argue that AMOC weakening and increased ventilation age coincide with IRD records, so they must explain how AMOC changes are generated in their experiment.

In addition, the simulated AMOC during the Bølling–Allerød (BA) and late Holocene appears equally strong, despite the BA being substantially warmer and previous transient simulations (e.g., TraCE-21ka) showing AMOC overshoot during the BA. This reviewer finds AMOC shoaling during the LGM and stronger AMOC in the late Holocene compared to the BA, yet maximum AMOC strength appears similar across the LGM, BA, and late Holocene. These discrepancies should be clearly discussed and illustrated (e.g., in supplementary figures).

The definition of simulated AMOC strength must also be specified (e.g., maximum streamfunction below 500 m between 20°N–60°N). Similarly, since the model is prescribed with both $\Delta^{14}C$ and greenhouse gases (please clarify whether this includes CO_2, CH_4, etc.), the authors should explain how simulated ventilation age changes can be linked to atmospheric CO_2. Thus, the authors should explain this in more detail and whether a fully coupled climate–carbon cycle model is required to substantiate the proxy–model comparison?

Finally, clarification is needed on how land–sea boundaries evolve in the simulation. Changes in the North Atlantic land–sea boundary could influence convection sites in the Nordic Seas and, in turn, affect AMOC strength.

Reviewer #2 Comment:

a) You argue that the similarity between the model ventilation age change and ventilation ages to that which you observe, underpins their relationship. Its fairly convincing (NB caveats noted above) when one looks at the whisker plots and covariations in different states. However, the focus of the paper is not about these states, it is about the interpretation of the very brief deviations in ventilation age that interrupt these different modes. These are very short lived, but when I look at the model changes in ventilation age, it is not clear to me if such sudden deviations and recoveries are possible for this model (the model ventilation age curves seem to respond too slowly, but its hard to tell). Thus, how relevant/applicable is the model framework for representing these very short lived deviations? It's a question out of interest and it might be worth mentioning or caveating (if for example the model would not predict the magnitude and rate of the observed ventilation changes in responses to AMOC reduction/intensification). In this context its also important to note that very large ventilation changes are found with only minor changes in AMOC in the model (supplement Fig. 5 and comment above).

The authors responded that their model output is intended to demonstrate that the observed changes in ventilation age at the core site largely reflect variations in AMOC strength, particularly during periods of weaker AMOC, such as during HS1, which is the focus of their study. The model is not intended to resolve short-term (centennial-scale) intensifications within broader millennial-scale AMOC slowdowns like HS1. Firstly, they should mention this simulation caveat in their manuscript. Secondly, the authors should be careful since they repeatedly associated simulated AMOC and ventilation ages to explain their first-of-its kind centennial AMOC intensification ~16.3ka and ~15.4ka.

Reviewer #2 Comment:

D)if ventilation is a reliable indicator of AMOC, why is there an offset in supplement figure 6 between increased ventilation and reduced AMOC (increased Pa/Th)--this appears much too large to be proxy response time related.

This is a very important point. This reviewer agrees with the authors that the relatively low resolution of the Pa/Th data (Voigt et al., 2017) fails to capture the centennial change in AMOC during HS1. In my opinion, the authors may say something like that their proxy study strength is higher resolution record which is not seen in previous proxy records.

Reviewer #2 Comment:

To me the stated relation between weak ventilation and intense IRD intervals is not so clear as presented in the text. From the figure it appears that this is sometimes true and other times not true (well ventilated period in 1b is associated also with high or rising IRD).

This is a very important point. The authors have explained their low AMOC/high ventilation ages matches with the IRD events therefore providing the possible cause of causes of centennial-scale AMOC intensifications. While, sometimes this relationship is not seen therefore this reviewer strictly feel that this point must be answered in the manuscript. Since the authors include simulated AMOC strength, it is within their domain to try to explain the possible causes of these simulated changes in AMOC and ventilation ages.

Reviewer #2 Comment:

Lines 309-316; you discuss flushing of the tropical thermocline might be driven by the same type of processes as rapid ventilation in the subpolar/drake passage region? The wording is quite vague here making it hard to sort out what exactly is happening in the two areas (and the potential relationship), but I'd be surprised if they were identical processes in these very different hydrographic settings/regimes. Can you please be more specific about your inferences of what processes are going on and how these are or are not related? Given the difference in timing between the events in the two locations, why do you infer they have a common source/cause/dynamic? Why are some important for CO2 and some not? This latter point is critical to answer given the end focus on the implications for co2.

This reviewer is of the opinion that addressing these additional events lies within the scope of this present study and therefore must be addressed. Further, the authors could also use their model simulations to show does the simulated centennial scale AMOC strengthening actually enhance northward oceanic heat transport resulting in ITCZ shift and reduced precipitation changes over northeast Brazil. Meanwhile, they could also see the simulated Southern Ocean upwelling to explore its role during centennial scale AMOC change and potential carbon dioxide degassing.

Reviewer #2 Comment:

Section 2.4 (discussion of relation between ventilation and atmospheric co2).

This reviewer acknowledges and appreciates the authors for improving the understanding of this section as per the comments. However, the authors should make it clearer and more concise for better understanding.

Reviewer #2 Comment:
Stepping back, I’m struck that you see such intermittent/variable ventilation changes during the Heinrich event. Doesn’t this influence how we think about ocean circulation and dynamics and our ability to model them? You end the paper mentioning the challenge of modelling the brief co2 pulses, but doesn’t it also pose challenges for modelling circulation dynamics as well? How can circulation return to full strength briefly and then be curtailed again—flickering on during a period of suppression. Is this a forced (relaxed forcing) behavior or is it a dynamic system jumping between attractors? Do any models see such rapid state changes (see earlier comment here as well about model representation of short ventilation events)? This reviewer still believes that an evaluation on the causes of such events is within the scope of this study as it solidifies their attempt to publish at the prestigious Nature Communications journal.

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
During this round of revision, the authors have addressed most of my concerns. The section on the AMOC-CO2 relationship is now sufficiently clear and coherent, although, as the authors acknowledge, a more accurate or realistic investigation would require the use of biogeochemical models. I have only a few minor and specific comments and suggestions. Please see the attached.

Reviewer #3

(Remarks to the Author)
Lines 185–189 of the revised manuscript state that the PaleoMIST ice-sheet reconstruction has a temporal resolution of 2500 years and that the model interpolates conditions between these time steps. The authors argue that, as a result, the model is not expected to capture variability within HS1, and that their simulations are used only to examine millennial-scale relationships between AMOC and B–P ventilation, whereas proxies are used to infer centennial-scale variability. However, this argument is inconsistent with the modelling approach and its application in the manuscript. Moreover, although the model setup closely resembles the PaleoMIST_full experiment, the AMOC strength differs substantially (e.g., ~0 Sv at the HS1 minimum in this study compared to ~10 Sv in PaleoMIST_full), which substantially limits the interpretability and applicability of the simulations presented here.

The authors repeatedly state that their AMOC results should not be interpreted on centennial timescales. Yet this position is difficult to reconcile with the fact that the manuscript draws heavily on model output to support its central conclusions. It is also concerning that many of the mechanistic questions raised by Reviewer #2 and Reviewer #3 in earlier rounds—questions fundamental to evaluating the validity of the modeled responses—have been dismissed by the authors as requiring a “stand-alone manuscript.” This rationale is not scientifically convincing: if key mechanisms underlying the model–proxy comparison cannot be articulated, then the manuscript is not conceptually complete.

The authors assert that the processes in question have been “extensively investigated” in Willeit et al. (2024). If so, the authors should at minimum cite this work and clearly explain the relevant mechanisms in the present manuscript. Furthermore, their explanation regarding the absence of a linear relationship during strong AMOC conditions must transparently acknowledge that this is largely an artifact of the study location.

Given the continued absence of mechanistic interpretation after two rounds of revision, the manuscript’s core conclusions remain descriptive rather than mechanistically supported. The lack of substantive improvement in addressing these scientific concerns leads this reviewer to conclude that the manuscript is not suitable for publication in Nature Communications in its present form. I therefore recommend rejection at this stage, with the possibility of resubmission should a rigorous mechanistic framework be provided.

Version 3:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The authors have addressed my remaining minor comments from the previous round. The addition of Supplementary Figure 9 showing the meltwater discharge is also appreciated, as it allows the reader to evaluate the freshwater forcing applied. Regarding my comment on L216, the authors now acknowledge that B-P ventilation ages at this site respond most strongly during periods of reduced AMOC (L199, 226), and that the absence of a linear relationship under strong AMOC conditions is site-specific (L210-211). These are appropriate clarifications. However, the request to diagnose factors (ii) and (iii) from the model was addressed only through proxy-based arguments in Supplementary Note 2, rather than through model diagnostics. While I accept this as a practical limitation, it does leave the question of what controls B-P ages at this site less fully constrained than it could be.

I have also carefully read the authors' responses to Reviewer #3. The main changes in this round are interpretational: the authors have revised the abstract, introduction (L118 and on), and discussion (L188-193) to reframe the model as a benchmark for the AMOC-ventilation age relationship, and have explicitly stated that the conclusions are based on proxy data. They have also cited the convective instability mechanism from Willeit2024 to provide a physically plausible framework for centennial-scale AMOC change. These textual changes improve the clarity of the manuscript.
That said, I note that the revisions are primarily changes in framing and language rather than new analyses. The substantive issues raised by Reviewer #3 have not been resolved through additional modeling work, particularly regarding the large discrepancy in AMOC magnitude between this study (~0 Sv at HS1 minimum) and the comparable PaleoMIST_full experiment (~10 Sv), and the lack of centennial-scale variability in the model during HS1. The authors attribute the AMOC magnitude difference to a newer model version with updated parameterization, which is a plausible explanation but remains untested. The mechanistic understanding of the centennial-scale AMOC intensifications still relies on citing external work rather than being demonstrated with the authors' own simulations.
In my view, the proxy record remains the principal strength and contribution of this manuscript. The high-resolution B-P ventilation age data from the western equatorial Atlantic represent a genuinely valuable dataset, and the reconstruction of centennial-scale AMOC variability during HS1, including the identification of two intensification episodes, is well-supported by multiple independent proxy records. The model component, while useful as a first-order consistency check, does not add substantial independent evidence beyond confirming that AMOC and B-P ages are correlated at this location.
The concerns raised by Reviewer #3 regarding the model are scientifically valid. The manuscript would be strengthened by a more thorough treatment of these issues. However, given that the conclusions are derived from the proxy data and the model plays a supporting role, I do not consider the modeling limitations to be disqualifying. I leave it to the editor to weigh whether the current level of model-proxy integration meets NCOMM's standard.

Reviewer #3

(Remarks to the Author)
Review of Revised Manuscript

During this round of revision, the authors have addressed the majority of my previous concerns, and I appreciate the substantial effort invested in improving the manuscript. The overall quality, clarity, and presentation have significantly improved.

However, I have one remaining point that requires clarification. The authors have added Supplementary Fig. 9 in this revised version showing the temporal evolution of freshwater runoff flux and simulated AMOC strength. While this addition is useful and necessary, it should be clearly specified whether the freshwater flux shown represents only surface runoff or whether it also includes meltwater discharge from continental ice sheets, as prescribed by the PaleoMIST reconstruction referenced in the manuscript. For clarity and completeness, I suggest that the authors explicitly describe this in the text and figure caption in Supplementary Fig. 9.

More generally, for transparency, it would be helpful to include the temporal evolution of all imposed model forcings—namely, insolation, greenhouse gas concentrations (CO_2), continental ice sheet boundary conditions, and associated meltwater discharge—in Supplementary Fig. 9. This would greatly enhance reproducibility and clarity for readers.

Subject to these minor clarifications, I agree with the authors that the manuscript has been substantially improved, and I recommend it for publication in Nature Communications following another round of minor revision. I have also included some minor comments based on the revisions made in the current manuscript version.

Minor Comments:

Line 34 (Abstract): Please specify the timescale of Heinrich Stadial 1 (HS1), i.e., HS1 (17.8–14.8 ka), consistent with the description provided in line 64.

Lines 181–182: Please confirm whether AMOC strength was calculated as the maximum streamfunction value northward of ~30°N (or ~30°S, if applicable).

Line 212: The author may consider changing the typography from “artefact” to “artifact.”

Response to reviews on the manuscript titled "Centennial-scale intensifications of the Atlantic Meridional Overturning Circulation during Heinrich Stadial 1" (NCOMMS-25-07736)

We sincerely thank the reviewers for their valuable and helpful inputs. We believe that the revised version of the manuscript improved substantially thanks to the feedbacks. In the following point-by-point response, the Reviewers' comments are given in black, while our answers are provided in blue. Stated line numbers refer to the revised versions of the manuscript and the supplement with "tracked changes". Additionally, we are also providing a "clean" versions of both documents. Citations from the revised manuscript are marked by " ", combined with its respective line reference. Wherever necessary, we have subdivided the Reviewer's comments into individual points in order to clearly address and refer to related issues. As some comments from the Reviewers partly overlap, we respectfully refer to our responses to similar questions raised by other Reviewers wherever applicable.

We hope that our revisions and clarifications will fulfil the Editor's as well as the Reviewers' requirements and expectations.

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

Review Report for "Centennial-scale overshoots in Atlantic Meridional Overturning Circulation during Heinrich Stadial 1" by Jena et al.
In their manuscript titled "Centennial-scale overshoots in Atlantic Meridional Overturning Circulation during Heinrich Stadial 1," Jena and co-authors present high-resolution benthic-planktic (B-P) radiocarbon ventilation data derived from a sediment core collected from the mid-depth western equatorial Atlantic. The authors utilize these data to identify a three-phase structure of the Atlantic Meridional Overturning Circulation (AMOC) during Heinrich Stadial 1 (HS1), including two notable centennial-scale overshoots. To support their analysis, they employ a simulation from an Earth system model of intermediate complexity (EMIC) equipped with a radiocarbon (14C) module, which indicates elevated B-P ventilation ages during HS1. By integrating proxy data with model simulations, the authors propose a connection between AMOC overshoots and deglacial CO2 release. The manuscript is generally well-written, with a logical structure. However, I have identified several significant caveats related to the methodology, results, and the hypothesized CO2 release mechanism.

Response: We sincerely appreciate the thorough evaluation and valuable feedback from Reviewer #1. We believe these insights have substantially improved the manuscript. We carefully considered each comment and made the necessary revisions accordingly. Additionally, we have provided detailed responses to each comment, outlining the specific changes made wherever applicable.

While detailed comments are provided below, these concerns can be summarized into three major points:
1. Additional details regarding the model simulations are necessary to enable a thorough evaluation of model performance and the comparison between model outputs and proxy data.

Response: Additional model and simulation details are provided in the revised manuscript, please see our detailed reply to the detailed comment of Reviewer #1 further below.

2. In the latter part of the Introduction, the authors review the relationships between AMOC, upwelling, and CO2. While this is a critical and necessary component of the study's motivation, it requires reorganization to clarify whether these mechanisms operate synergistically or antagonistically.

Response: We sincerely appreciate the insightful suggestion from Reviewer #1. Accordingly, we have made the necessary revisions to the introduction, including a clear definition of the Atlantic Meridional Overturning Circulation (AMOC) (*lines 84-85*). In our study, we have defined AMOC as deepwater formation in the North Atlantic and its subsequent southward flow. We treat this circulation distinctly from Southern Ocean upwelling, which primarily refers to the vertical movement of deep waters to the surface driven by surface wind stress and Ekman divergence in the Southern Ocean. This separation reflects the fact that different mechanisms govern these processes. AMOC is mainly influenced by surface water density changes due to heat and freshwater fluxes in the North Atlantic (Duplessy et al., 1992; Rahmstorf, 2002). While Southern Ocean upwelling is primarily driven by the strength and position of the Southern Hemisphere westerly winds (Anderson et al., 2009; Talley, 2013). Based on the latest research, our understanding is that AMOC and Southern Ocean upwelling operate antagonistically (Baker et al., 2024). Lateral flushing of CO_2-rich waters occurred during periods of enhanced AMOC, whereas strong upwelling was concurrent with weakened AMOC (as seen during HS1 and YD) (Anderson et al., 2009; Skinner et al., 2021). This has been made clear in the introduction (*lines 99-165*).

3. Greater caution is needed when discussing the AMOC-CO2 relationship in Section 2.4. Although the model simulations show consistent B-P ventilation ages, this does not necessarily indicate a specific pattern of carbon sequestration or release. The question of whether an enhanced or weakened AMOC drives deglacial CO2 release remains a subject of ongoing debate.

Response: We sincerely thank Reviewer #1 for their insightful suggestion. We recognize that the interplay between AMOC and atmospheric CO_2 is complex and modulated by various contributing factors. Following the suggestion from Reviewer #1, the revised manuscript now presents an expanded discussion encompassing the most relevant AMOC-driven processes that may influence atmospheric CO_2 concentrations. These include AMOC-induced flushing (advection of CO_2 by south-moving moving watermasses), Southern Ocean upwelling (Anderson et al., 2009), the marine biological pump (Schmittner & Lund, 2015), the solubility pump (Dai et al., 2022), and terrestrial carbon fluxes (Nielsen et al., 2019). Furthermore, we added a critical assessment of the influence each of these mechanisms exerts on atmospheric CO_2 variability over centennial timescales. A detailed exploration of these topics is now provided in *Lines 476-512* of the revised manuscript.

As I am not an expert in proxy-based reconstruction, I am unable to provide detailed feedback on the processing and analysis of the sediment materials. However, I have included minor suggestions below regarding the plotting in Figure 1.

Response: Once again, we sincerely appreciate the comments from Reviewer #1. The suggestion regarding Figure 1 has been carefully addressed and is discussed in detail in the relevant section below.

Detailed Comments

Lines 79 and onward: This paragraph outlines the connection between CO2 fluctuations and the AMOC, which is essential for establishing the motivation of the study. However, both the structure and content require reorganization for clarity and accuracy.

- First, does a weakened AMOC inherently lead to enhanced deep ocean carbon sequestration? I am skeptical of this assumption. The relationship between AMOC and CO2 is not straightforward, as the North Atlantic is characterized by relatively high nutrient utilization efficiency. A reduction in AMOC strength, and thus a decrease in the volume of water ventilated by North Atlantic-sourced deep water, could reduce the efficiency of the soft tissue pump (e.g., see Hain and Sigman, 2024, "CO2 in Earth's Ice Age Cycles"), potentially increasing atmospheric pCO2.

Response: We greatly appreciate the thoughtful comment from Reviewer #1 and acknowledge the complex interplay between AMOC variability and atmospheric CO_2 levels. A weakened AMOC reduces ocean ventilation, influencing nutrient transport and amplifying the efficiency of the soft tissue pump. Concurrently, reduced deep-ocean flushing facilitates carbon sequestration in the deep Atlantic (Skinner et al., 2010). While our initial manuscript emphasized CO_2 removal via deepwater flushing, we concur that other mechanisms deserve equal consideration. Accordingly, the revised version of the manuscript broadens the discussion to encompass additional contributors, including the biological pump, temperature- and solubility-driven changes, physical flushing of CO_2-rich waters, and terrestrial carbon inputs (Anderson et al., 2009; Dai et al., 2022; Nielsen et al., 2019; Schmittner & Lund, 2015). *Lines 99-165* in the revised manuscript discuss these processes.

- Second, from Line 89 onward, the discussion of deglacial CO2 changes is presented in a somewhat confusing manner. A key issue is the lack of clarity in defining "AMOC." Does it refer to the volume of water transported across the equator in the Atlantic, or does it simply denote the extent of North Atlantic-sourced deep water? Additionally, does Southern Ocean upwelling within the Atlantic sector contribute to the AMOC as defined here? These distinctions are critical and should be addressed. For example, Chen et al. (2015) suggest that deglacial AMOC overshoots facilitate CO2 release through enhanced upwelling, a mechanism similar to that driven by changes in Southern Hemisphere westerlies, which influence carbon storage by altering upwelling patterns. In contrast, Schmittner and Lund (2015) propose that a weakened (or potentially collapsing) AMOC reduces biological pump efficiency, thereby increasing pCO2. These mechanisms may operate antagonistically, and high-resolution AMOC records, such as those presented in this study, could help determine which process is the primary driver of the AMOC-CO2 relationship during HS1. The text should be revised to clearly delineate these processes and their potential interactions.

Response: We appreciate the thoughtful comment from Reviewer #1 and acknowledge that our previous description of deglacial CO_2 changes was not enough clear. To address this, we have thoroughly revised the introduction (particularly in *lines 99-165*) for improved clarity. Additionally, we now properly define the AMOC in the introduction, specifying that, according to our definition, it does not include Southern Ocean upwelling but rather encompasses deepwater formation in the North Atlantic followed by its lateral southward transport (*line 84)*

We agree that multiple processes influence the carbon cycle, and the existing uncertainties underscore the significance of studies like the present one. Accordingly, we have refined our introduction to clearly articulate our motivation for this research.

Figure 1: Although I am not an expert in radiocarbon data, I have questions about the presentation in Figure 1. What do the error bars in Figure 1b represent, and how were they calculated? Additionally, why are error bars absent in Figure 1a? The trend in Figure 1b appears clear when considering only the solid squares (presumably mean values). However, the error bars increase in length during HS1 and the Last Glacial Maximum (LGM), suggesting greater uncertainty. Notably, the upper bounds of the LGM data appear to overlap with HS1 data, which may complicate interpretations of distinct differences between these periods.

Response: We appreciate the feedback from Reviewer #1 on the representation of radiocarbon ventilation age data. In Figure 1b, the errors in B-P ventilation age stem from the propagation of uncertainties in benthic and planktic radiocarbon measurements, represented as 1σ error. In Figure 1a, error bars appear absent because individual measurement uncertainties are smaller than symbol size (i.e., squares), a point now explicitly noted in the figure caption (*lines 197-200).*

The error bars increase with sample age due to growing uncertainties in individual radiocarbon measurements, primarily caused by the decreasing ^{14}C counts in older samples, which affect accelerator mass spectrometer (AMS) counting statistics. As noted by Reviewer #1, variability is more visible in a plot without error bars, and specific values from the Last Glacial Maximum (LGM) overlap some values of HS1 when the errors are considered. While this observation is valid, it reinforces our main message of substantial intensifications of AMOC strength within HS1. Moreover, at the LGM–HS1 boundary, we observe distinct shifts that remain clearly identifiable, suggesting that a significant transition occurred during this period.

Line 175: The authors note "striking similarities" between their B-P ventilation age record and 231Pa/230Th records from the North Atlantic. While the 231Pa/230Th data indicate a consistently weak AMOC during HS1, the B-P ventilation ages in this study appear to decrease over time within HS1. Before proceeding with further analysis, I question whether these two datasets are indeed "strikingly similar," as the temporal trends seem to diverge.

Response: We appreciate the valuable insight on this point. While we acknowledge that the two records exhibit distinct features within HS1, our original intent was to highlight their overall similarity on millennial timescales. Recognizing the ambiguity in our wording in the first version of the manuscript, we have revised the statement as follows (*line 255)*:

"On millennial timescales, notable similarities between our B-P ventilation age record and $^{231}Pa/^{230}Th$ records from the North Atlantic (Refs. ##; Fig. 2 a and h) suggest that the rate of NADW formation and lateral transport to our study site was the primary factor controlling B-P ventilation age variations."

Lines 330-333: The authors suggest that a weakened AMOC is associated with enhanced upwelling, which is physically challenging to reconcile. Enhanced upwelling is typically linked to increased wind-driven circulation or changes in ocean stratification, rather than a direct consequence of a weakened AMOC. This statement requires clarification or supporting evidence.

Response: We appreciate the insightful comment from Reviewer #1. We acknowledge that while a weakened AMOC and increased Southern Ocean upwelling occur concurrently during HS1, they are not necessarily dependent on each other. In fact, studies suggest that enhanced upwelling in the Southern Ocean is responsible to some degree for continuing overturning in the North Atlantic (e.g., Baker et al., 2024). We have carefully considered the feedback from Reviewer #1 in parts of the manuscript where AMOC and Southern Ocean upwelling are discussed, and rephrased the text accordingly (*lines 415-431*).

Deglacial CO2 Discussion: Another factor to consider in the context of deglacial CO2 release is the role of Southern Ocean sea surface temperature (SST). Dai et al. (2022, Nature

Communications) demonstrate that changes in the solubility pump, driven by Southern Ocean SST, could theoretically account for a 30-40 ppm change in surface pCO2. Their calculations suggest that this process steadily influences CO2 release from the ocean during HS1. A rise in Southern Ocean SST may be driven by a weakened AMOC, which reduces northward heat transport, leading to warming in the Southern Hemisphere. This mechanism contrasts with the authors' findings:
- Authors' findings: AMOC overshoots during HS1b and HS1c enhance upwelling, resulting in greater CO2 release.
- Previous calculation: A weakened AMOC reduces northward heat transport, increasing Southern Ocean SST and driving CO2 release.
Although both scenarios result in CO2 release from the Southern Ocean, the underlying mechanisms differ significantly. The manuscript should acknowledge and address this alternative mechanism to provide a more balanced discussion.

Response: We agree that the solubility pump plays a significant role in modulating carbon cycle dynamics, a point that is now explicitly addressed in the revised manuscript. Notably, during HS1, it substantially contributed to the rise in atmospheric CO_2. The possible role of this process in driving atmospheric CO_2 is now thoroughly discussed in *lines 415-431 and 485-486.*

Lines 434 and onward: The description of the model methodology is insufficient, making it difficult to evaluate the reliability of the model outputs. The current text provides only a brief overview of the CLIMBER-X model, lacking critical details such as specific boundary conditions and experimental design. Given the study's reliance on both proxy data and model simulations, I recommend providing a more detailed methodology section for the model. Additionally, I suggest including a plot of the 14C pattern for late-Holocene conditions to demonstrate the model's performance and validate its applicability to the study period.
Response: We appreciate the insightful comment from Reviewer #1. We now provide a more detailed description of the model methodology in the "Material and Methods" Subsection 3.4. Regarding the specific boundary conditions, we now explicitly mention that we consider the PaleoMIST ice sheet reconstruction (Gowan et al., 2021) implemented into CLIMBER-X by (Masoum et al., 2024) (who also discuss the implementation of a second ice sheet reconstruction in their paper, cf. *lines 590-591*). We are also more specific regarding the implementation of marine ^{14}C into our model, where we replaced the misleading reference to the paper by Butzin et al. 2005 with an explicit explanation of our modelling approach including its underlying approximations, parametrizations and uncertainties (*lines 598-599*) as well as a description of when and how modelled ^{14}C values are converted to ventilation ages (*lines 605-607*). Regarding the experimental design, we now describe the sequence of simulations from model spin-up with constant boundary conditions to subsequent transient simulations (initially with transient climate forcing but constant ^{14}C boundary conditions, and finally with both transient climate and ^{14}C forcing; see *lines 600-605*). Last not least, we compare simulated and observed / reconstructed ^{14}C distributions in the western Atlantic in

1950 CE (*lines 600-602* and Supplementary Figs. 7 and 8).

Reviewer #2 (Remarks to the Author):

The paper presents new reconstructions of deep ocean ventilation from the intermediate depth tropical Atlantic Ocean across the deglaciation using 14-C in planktonic and benthic foraminifera. The resulting record is impressive and an important and valuable contribution as it is able to depict variability in intermediate ventilation (ages) at an unusually high resolution and does so in an important region. The newly documented centennial scale variations in interior ventilation have bearing on our understanding of the dynamics of deglacial climate and carbon cycle changes and, specifically, provide new co-nstraints for delimiting the role of ocean circulation. Importantly, the work reveals centennial scale intensifications of ventilation during the Heinrich stadial event that is generally considered a period of suppressed ocean ventilation and weak AMOC. The quality of the new records and the discovery of this centennial variability that appears linked to previously described tropical monsoon variability will be of extra-disciplinary interest and thus merits potential publication in this journal. However, when discussing the results the manuscript could be clearer and do an even better job at highlighting the specific significance, novelty, and importance of the results and their implications for how ocean circulation shaped (or not) deglacial climate and carbon cycle changes.

Response: We greatly appreciate the meticulous assessment and insightful feedback from Reviewer #2, which have significantly strengthened the revised version of the manuscript. Each comment has been carefully evaluated, and we have made the necessary revisions accordingly. Furthermore, we have provided detailed responses to clarify the implemented changes.

Some specific suggestions and observations are:
Lines 66-78 you put extensive effort into describing the discrepancy about whether HS1 was a singular episode or best divided into 2 or 3 fold structure. Beyond the noted discrepancy between records, it is not clear from reading this why this division matters. Also, although you set up this discrepancy, you do not revisit it explicitly later in the manuscript, thus this introduced theme is not well aligned with your discussion and conclusion. Could you revise this to set up a more relevant question to what you are finding and concluding? As it is, I'm a bit lost as to why this is introduced this way and not returned to. I also do not see a very clear 3 fold division in your own records presented or why the division is made. It looks like you have persistently weak ventilation during the first phase (1a) and then it is interrupted by periods of stronger ventilation, or more ventilation variability, in the second part of HS1.

Why isn't this just 2 phases, one of weak ventilation and another of weak but more variabile ventilation? What is the difference between 1b and 1c, each has weak ventilation and a short period of strong ventilation? Anyhow, the divisions, how they are defined and their significance are not very clear to me in the current manuscript.

Response: We appreciate the insightful comment from Reviewer #2. We acknowledge that determining the structure of HS1 is not the focus of this manuscript; rather, our study aims at a detailed characterization of the centennial-scale AMOC intensifications within HS1 and examining the consequences to atmospheric CO_2 and tropical hydroclimate. Accordingly, we refined the introduction to better articulate the motivation of our study and the research findings (*lines 99-160*).

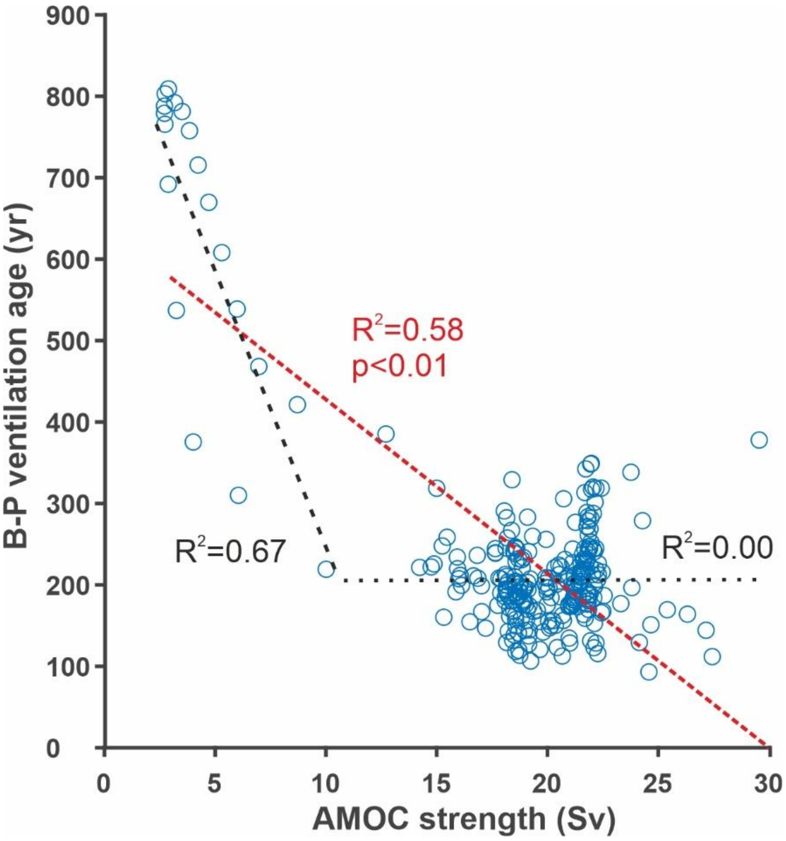
Upon reevaluating AMOC variations during HS1, we concur with Reviewer #2 that the fluctuations in ventilation age do not necessarily indicate a threefold structure. Consequently, we revised our discussion accordingly (*lines 323-326*). Early HS1 represents a period of consistently weakened AMOC, whereas late HS1 denotes a phase of weakened yet variable AMOC.

The modified manuscript in *lines 323-326* read as-

"*HS1 is subdivided into two intervals (**Fig. 4a**): an early phase (17.8 to 16.5 ka) marked by a relatively stable but weak AMOC, and a late phase (16.5 to 14.8 ka) characterized by a weak yet variable AMOC. Notably, the late HS1 features two prominent centennial-scale AMOC intensification events centred around 16.3 and 15.4 ka (**Fig. 4a**).*"

Ventilation age and AMOC discussion is not clear and precise at various places in the manuscript. A linear relationship is discussed/inferred but when one looks at supplement Fig. 5 there is a very rapid change in ventilation age over just a couple of Sv AMOC change when AMOC is weak (e.g. between 3 and 5-6 Sv there is a change in ventilation between >800 and <400 years—which covers most of the reconstructed variability). The current way it is discussed does not really take this into account. The paper should acknowledge that most of the signal at the core site (based on the model, but also comparison to hydrography) may be driven by relatively modest changes in AMOC when AMOC is relatively weak. Put another way, ventilation at the site is very sensitive to AMOC changes when AMOC is low, but less sensitive to monitoring changes when AMOC is high. This might also explain how/why you get such rapid apparent shifts in ventilation age when other records have not recorded clear/major variability in AMOC at these times.

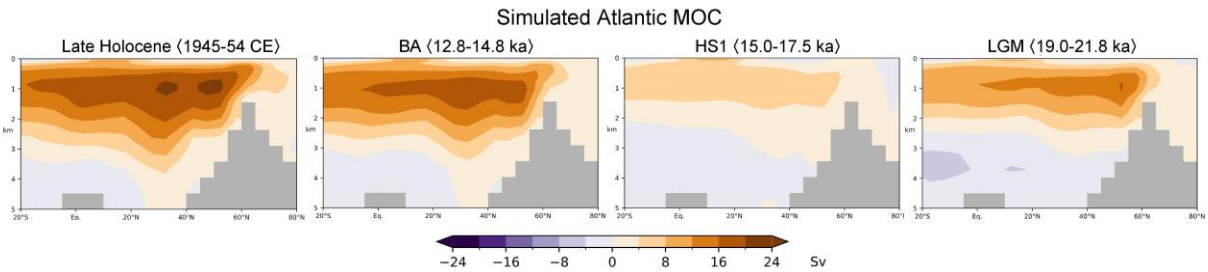
Response: We appreciate the insightful comment from Reviewer #2. We concur that, at our study location, ventilation exhibits greater sensitivity to AMOC when AMOC is weak, whereas it is less responsive under strong AMOC conditions. This pattern is evident in the revised Supplementary Fig. 5 (also shown below), where we have categorized the data points into two groups: (i) data points with AMOC < 10 Sv; and (ii) data points with AMOC > 10 Sv. A clear distinction emerges between the two groups, with the weak AMOC phase displaying a stronger correlation between AMOC and radiocarbon ventilation age ($R^2 = 0.67$) versus the correlation between the strong AMOC and radiocarbon ventilation ages ($R^2 = 0$).



There are some other issues that could be improved when discussing the model results and AMOC-ventilation relations:

a) What is actually done with the model is not clear as the materials and methods (section 3.4) is pointed to but there are very few details here, just which model and boundary conditions and the statement "the implementation of marine D14C essentially follows ref 69" when one looks that up, a number of things were done in that paper making it very unclear what was used from that implementation here. Interestingly when one looks at that paper they suggest that the best LGM state had a 40% reduction in NADW export while in this paper the LGM state in figure 3 has a very similar AMOC to the Holocene? Are these the same simulations because they do not seem particularly compatible? If a different LGM state was chosen (from the best one presented in ref 69) why—because it matched better the data here? Anyhow, the lack of information makes it difficult to know what was actually done and why and how it compares to previous work/conclusions. In any event, these apparent discrepancies, combined with the observation that the largest changes in ventilation in the model occur over a very small range of AMOC made me react to the simple claim on line 235 that your "radiocarbon ventilation age record from the western equatorial Atlantic is a reliable indicator of AMOC strength". I think a little more nuance and detail in how exactly they are related is required.

Response: We have realized that the reference to ref. 69 (citer in the earlier version) is obviously misleading. While we targeted the technical appendix of that paper (providing a description of how to implement $\Delta^{14}C$ as a single abiotic tracer), the main text of ref. 69 discusses sensitivity experiments that were carried out for certain time slices with another model in a dated and uncoupled modelling framework. Those simulations have nothing to do with the fully transient simulation considered here. Here, we do not choose a specific AMOC state for the LGM (or any other period) but the AMOC is a model result in response to the applied external climate forcing. In fact, as can be seen below, our study yields quite substantial AMOC changes during the deglaciation which are not fully visible in Fig. 3:



We therefore decided to remove the misleading reference to ref. 69 and to replace it with more specific information about the model setup in this study which can be found in the "Material Methods" Subsection 3.4 (*cf. lines 584-611*). This section now includes an explicit description of our modelling approach including the underlying approximations, parametrizations and uncertainties regarding the implementation of marine ^{14}C. We also clarified our simulation design there and provide a brief model – data comparison for marine ^{14}C in the Supporting Document (Supplementary Figs. 7 and 8).

b) "overshoots". I admit that this term is popping up in the literature but I find it very unclear and problematic since the term assumes an "expected" target/level which is surpassed but I'm not sure what one should expect for a ventilation/circulation level. For your data the deviations are renewals of circulation more than overshoots from a given level. However, if you insist on using it please define it clearly. I suspect it is simply adopted from the Chen et al (2015) paper since this study is relying and building upon those results and discussion. However, they clearly define it as a circulation/ventilation that is greater than the strong circulation of the Holocene. Looking at your plot, and the error bars, I'm not sure any of your periods of intensified/renewed ventilation clearly (beyond error) exceed the modern/Holocene level. Thus, it is not clear that they truly 'overshoot'. Also, please provide information on what the error bars represent (or how derived) in the plot so we know the significance level (e.g. 2 sigma etc.)

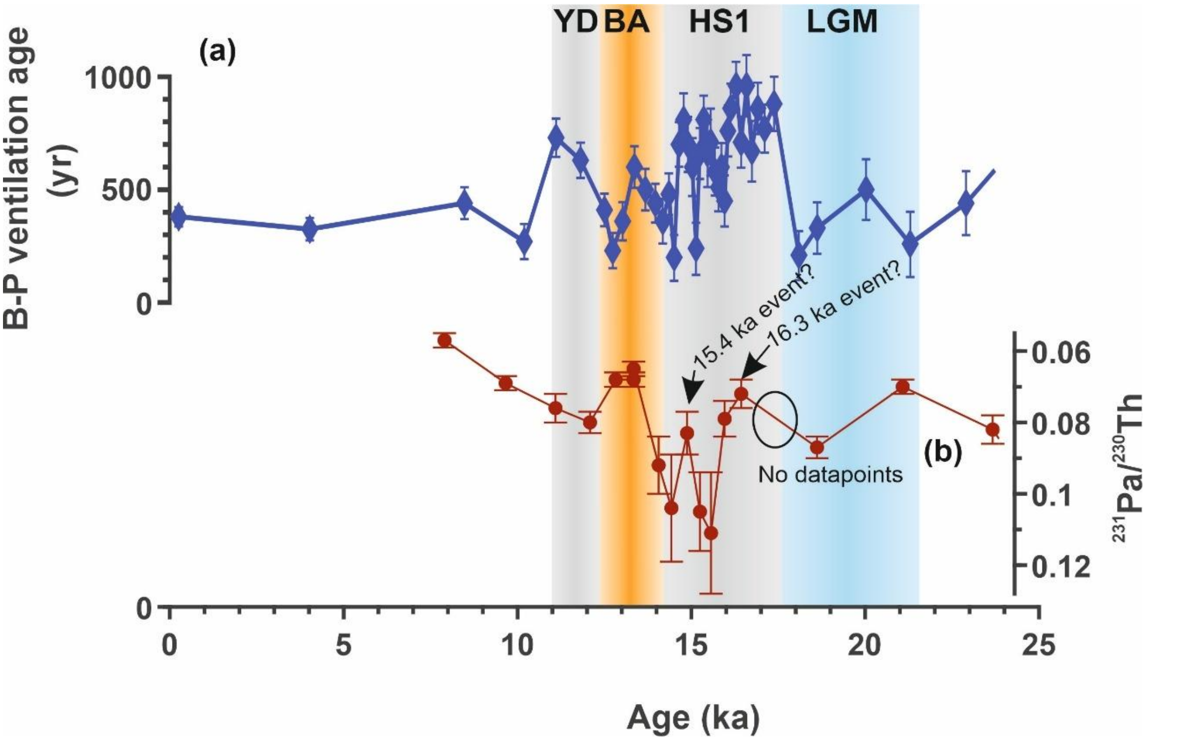
Response: We acknowledge the feedback from Reviewer #2 and have replaced the term "overshoot" with "intensification." Additionally, we have provided details of the error calculations in the figure captions of figures 1, 2 and 4.

c) You argue that the similarity between the model ventilation age change and ventilation ages to that which you observe, underpins their relationship. Its fairly convincing (NB caveats noted above) when one looks at the whisker plots and covariations in different states. However, the focus of the paper is not about these states, it is about the interpretation of the very brief deviations in ventilation age that interrupt these different modes. These are very short lived, but when I look at the model changes in ventilation age, it is not clear to me if such sudden deviations and recoveries are possible for this model (the model ventilation age curves seem to respond too slowly, but its hard to tell). Thus, how relevant/applicable is the model framework for representing these very short lived deviations? It's a question out of interest and it might be worth mentioning or caveating (if for example the model would not predict the magnitude and rate of the observed ventilation changes in responses to AMOC reduction/intensification). In this context its also important to note that very large ventilation changes are found with only minor changes in AMOC in the model (supplement Fig. 5 and comment above).

Response: We appreciate the feedback from Reviewer #2. We agree that the model does not fully capture the centennial-scale variations observed in the ventilation age record. This limitation is partly due to constraints related to the age–depth model and the temporal resolution of various datasets used in the simulations. However, the model performs well on millennial timescales, not only reproducing the overall pattern of variability but also producing comparable values. In our manuscript, the model output is intended to demonstrate that the observed changes in ventilation age at our core site largely reflect variations in AMOC strength, particularly during periods of weaker AMOC, such as during HS1, which is the focus of our study. The model is not intended to resolve short-term (centennial-scale) intensifications within broader millennial-scale AMOC slowdowns like HS1.

D)if ventilation is a reliable indicator of AMOC, why is there an offset in supplement figure 6 between increased ventilation and reduced AMOC (increased Pa/Th)--this appears much too large to be proxy response time related.

Response: We appreciate the feedback from Reviewer #2 and acknowledge the delayed onset of HS1 in the Pa/Th record (Voigt et al., 2017). However, this delay is most probably influenced by the low resolution of sampling for Pa/Th. As shown in the figure below, there are no Pa/Th data points between 18.8 and 16.6 ka, which prevents the record from capturing the expected high Pa/Th values at the beginning of HS1(Supplementary Fig. 6). Indeed, the low Pa/Th values observed at 16.6 ka likely coincide with our first AMOC intensification event, as indicated by radiocarbon data. Similarly, another decline in the Pa/Th record between 15.5 and 15.1 ka corresponds well with our second AMOC intensification event. This further supports our main conclusions, but given the relatively low resolution of the Pa/Th data (Voigt et al., 2017) we preferred not to mention it in the text.



To me the stated relation between weak ventilation and intense IRD intervals is not so clear as presented in the text. From the figure it appears that this is sometimes true and other times not true (well ventilated period in 1b is associated also with high or rising IRD).

Response: We appreciate the feedback from Reviewer #2 and recognize that the lowest magnetic susceptibility values from core SU8118 (Thouveny et al., 2000) exhibit slight offsets (ca. ~300 yr) relative to our observed ventilation age drop (Figure 4a and d). However, we are not surprised by these slight offsets, given the relatively low sampling resolution and the radiocarbon-based age model for core SU8118 (Thouveny et al., 2000). Still, as the objective of our study is not to determine the causes of centennial-scale AMOC intensifications, we refrained from driving further interpretations on it and strictly focused on finding the consequences of these changes.

Lines 309-316; you discuss flushing of the tropical thermocline might be driven by the same type of processes as rapid ventilation in the subpolar/drake passage region? The wording is quite vague here making it hard to sort out what exactly is happening in the two areas (and the potential relationship), but I'd be surprised if they were identical processes in these very different hydrographic settings/regimes. Can you please be more specific about your inferences of what processes are going on and how these are or are not related? Given the difference in timing between the events in the two locations, why do you infer they have a common source/cause/dynamic? Why are some important for CO2 and some not? This latter point is critical to answer given the end focus on the implications for co2.

Response: We thank Reviewer #2 for their insightful feedback. At present, it is difficult to determine whether the events in question are fully synchronous or asynchronous, particularly due to the lack of high-resolution B-P ventilation age data around 11.7 ka in our record, one of the two events reported by Chen et al. (2015). The 14.3 ka event of Chen et al., (2015) appears to be captured in our record between ~14.2 to 14.9 ka. A common mechanism potentially driving changes at both sites is substantial bottom water flushing, though the underlying processes may vary. In our case, this may have been facilitated by an intensified supply of North Atlantic Deep Water (NADW).

However, addressing these additional events requires further attention and likely a dedicated future study. Since the primary aim of this manuscript is to focus on the centennial-scale HS1 event, we have limited our discussion to the ~16.3 and 15.4 ka events and have decided to removed discussion of other events.

Section 2.4 (discussion of relation between ventilation and atmospheric co2).
I found this section slightly too vague making it hard to come away with clear conclusions or identify what we have learned exactly from the new data. While many of the processes previously discussed in the literature are invoked or repeated, it is done quite generally and it is hard to identify explicitly exactly how the new data are used to test (confirm/refute) these ideas and how they modify our previous picture. Yet, I think the data have a lot to say here! In addition, key processes discussed previously are not fully discussed regarding their compatibility with the new results. For example why not mention tropical terrestrial carbon reservoir changes (e.g. see Nielsen et al 2019 and refs therein/subsequent; https://doi.org/10.1029/2018PA003481) since you show that your ocean circulation changes correspond to changes in tropical rainfall/monsoon—wouldn't this support a role for terrestrial carbon cycle changes? After reading this section I was left with many questions but few answers (sorry). A few outstanding questions/problems I had after reading it which I think a revision with some more specifics could likely answer are:
A) You discuss the sudden rises in co2 as being driven by your ventilation renewal events via flushing of co2 from the interior. If this is the case, why isn't there a co2 rise/jump for every event (e.g. 1c)…also you say that the 14.8 jump is coeval with your renewed ventilation, but your full ventilation renewal seems to occur prior to the beginning of co2 rise?
B) if (respired) stored carbon is flushed from the intermediate ocean as you suggest then why does d13C stay low throughout HS1 (Supplement figure 6)—I'd expect it to respond quite clearly to changes in respired carbon given the previous literature discussions of d13C and B/Ca tracing respired carbon buildup during HS events?
C) What is the buildup timescale of carbon and how quickly can it be flushed out? E.g if you flush regularly (as your data suggest, what does this mean for the co2 capacitance and contribution to the sudden atmospheric jumps?).
D) Lines 359-362 you mention the other possibilities for co2 rise but fail to mention reduced biological pump or terrestrial carbon reservoir changes (see e.g. Nielsen et al., 2019; Lacerra et al 2017).

Response: We appreciate the thoughtful feedback from Reviewer #2. In the revised manuscript (*lines 410-510*), we have thoroughly amended the various mechanisms influencing atmospheric CO_2. Guided by the suggestions from Reviewer #2, we expanded the discussion to comprehensively cover the main AMOC-driven processes that may impact atmospheric CO_2 concentrations. These include AMOC-induced flushing, Southern Ocean upwelling, the marine biological pump, the solubility pump, and terrestrial carbon fluxes (Anderson et al., 2009; Chen et al., 2015; Dai et al., 2022; Schmittner & Lund, 2015). Processes occurring in the tropical region have also been carefully considered.

A. All the ventilation age drops observed in our record are concurrent with atmospheric CO_2 rises. However, we observe a modest CO_2 rise at 15.4 ka—significantly smaller than those associated with other events. Here, we accept that we did not explicitly mention it earlier, which we have corrected in the revised version. As noted in *line 454,* of the revised manuscript, "*Along with the millennial-scale periods (i.e., HS1, YD) of gradual CO_2 rise observed during the last deglaciation, a few short-term centennial-scale steps of sudden CO_2 rise have been noted. Among them, the events at 16.3, 14.8 and 11.7 ka are marked by significant atmospheric CO_2 rises (10-15 ppmv increase in atmospheric CO_2 in ~200 yr. Additionally, several minor CO_2 rise events are also observed, which are characterized by an CO_2 increase exceeding 5 ppmv within a 100-year span, one notable instance occurring at 15.4 ka.*"

   Regarding the 14.8 ka event, the CO_2 increase appears more complex. An initial short-lived rise of ~4 ppmv occurs at 14.9 ka (within ~50 years), followed by a more pronounced increase of ~15 ppm around 14.7 ka. The drivers behind this two-phase evolution remain unclear, but including the small rise along with the prominent one, it is certainly concurrent with our record.

B. We agree and also expect the $\delta^{13}C$ signal to reflect such changes. However, our $\delta^{13}C$ record lacks the resolution needed to capture centennial-scale flushing events within HS1 (Voigt et al., 2017).
C. Based on our data, the period between the centennial-scale events of AMOC intensification was long enough to promote the accumulation of respired carbon in the mid-depth Atlantic. Thus, the centennial-scale events of AMOC intensification rapidly flushed the accumulated carbon, ultimately allowing CO_2 leakage to the atmosphere. We propose that this atmospheric release was largely facilitated by vigorous Southern Ocean upwelling, which was a persistent feature during the deglaciation.
D. It has been included in the discussion (lines 392-398).

Stepping back, I'm struck that you see such intermittent/variable ventilation changes during the Heinrich event. Doesn't this influence how we think about ocean circulation and dynamics and our ability to model them? You end the paper mentioning the challenge of modelling the brief co2 pulses, but doesn't it also pose challenges for modelling circulation dynamics as well? How can circulation return to full strength briefly and then be curtailed again—flickering on during a period of suppression. Is this a forced (relaxed forcing) behavior or is it a dynamic system jumping between attractors? Do any models see such rapid state changes (see earlier comment here as well about model representation of short ventilation events)?

Response: We concur that the centennial-scale events of AMOC intensification during HS1 as shown by our data are a striking new finding. In this manuscript, we focus on (i) describing the events, and (ii) discussing its consequences for tropical South American hydroclimate and atmospheric CO_2 concentration. In-depth evaluations on the causes of such events will certainly be the focus of future manuscripts both by our group and other groups.

Finally, you seem to rule out AAIW changes, yet your core site sits hydrographically between upper NADW and AAIW and seems to have properties of a mixture of the two—I'd acknowledge this as you still have good arguments that the variations follow along with those observed in deeper and N.Atl. regions thus representing NA source changes/ventilation?

Response: We thank the reviewer for this suggestion. In in supplementary note 2, we have acknowledged that a mixing between AAIW and NADW is possible. The modified lines read as

"The sediment core under investigation was collected at a water depth of ~1400 m ***(section 3).*** *Considering the boundary between Antarctic Intermediate Water (AAIW) and North Atlantic Deep Water (NADW) in this region lies at ~1200 m*(Talley, 2013)*, we carefully examine the effect of AAIW on our radiocarbon signal.*

……

Therefore, we conclude that while there is a possibility of some AAIW mixing, its contribution appears minimal during HS1, and the ventilation signal is predominantly driven by NADW."

In summary, this is a great new data set and wonderful new constraints and very interesting paper for many groups. However, I do not feel like the specific constraints that this data provides (nor their limitations) are fully explored/explained and the discussion largely restates previous arguments without clearly and explicitly explaining how the new results test or alter this picture.

Response: We are sure that the suggestions of Reviewer #2 resulted in a more focused revised version of the manuscript that clearly show the relevance of the new results.

REFERENCES:

Anderson, R. F., Ali, S., Bradtmiller, L. I., Nielsen, S. H. H., Fleisher, M. Q., Anderson, B. E., & Burckle, L. H. (2009). Wind-Driven Upwelling in the Southern Ocean and the

Deglacial Rise in Atmospheric CO_2. *Science*, *323*(5920), 1443–1448. https://doi.org/10.1126/science.1167441

Baker, J., Bell, M., Jackson, L., Vallis, G. K., Watson, A., & Wood, R. (2024). Wind-driven upwelling sustains weakened Atlantic overturning under climate extremes. https://doi.org/10.21203/RS.3.RS-4319650/V1

Chen, T., Robinson, L. F., Burke, A., Southon, J., Spooner, P., Morris, P. J., & Ng, H. C. (2015). Synchronous centennial abrupt events in the ocean and atmosphere during the last deglaciation. *Science*, *349*(6255), 1537–1541. https://doi.org/10.1126/science.aac6159

Dai, Y., Yu, J., Ren, H., & Ji, X. (2022). Deglacial Subantarctic CO_2 outgassing driven by a weakened solubility pump. *Nature Communications*, *13*(1), 1–9. https://doi.org/10.1038/S41467-022-32895-9;SUBJMETA=106,2738,4113,47,704;KWRD=CARBON+CYCLE,PALAEOCEANOGRAPHY

Duplessy, J. C., Labeyrie, L., Arnold, M., Paterne, M., Duprat, J., & Van Weering, T. C. E. (1992). Changes in surface salinity of the North Atlantic Ocean during the last deglaciation. *Nature*, *358*(6386), 485–488. https://doi.org/10.1038/358485a0

Gowan, E. J., Zhang, X., Khosravi, S., Rovere, A., Stocchi, P., Hughes, A. L. C., Gyllencreutz, R., Mangerud, J., Svendsen, J. I., & Lohmann, G. (2021). A new global ice sheet reconstruction for the past 80 000 years. *Nature Communications 2021 12:1*, *12*(1), 1–9. https://doi.org/10.1038/s41467-021-21469-w

Masoum, A., Nerger, L., Willeit, M., Ganopolski, A., & Lohmann, G. (2024). Paleoclimate data assimilation with CLIMBER-X: An ensemble Kalman filter for the last deglaciation. *PLOS ONE*, *19*(4), e0300138. https://doi.org/10.1371/JOURNAL.PONE.0300138

Nielsen, S. B., Jochum, M., Pedro, J. B., Eden, C., & Nuterman, R. (2019). Two-Timescale Carbon Cycle Response to an AMOC Collapse. *Paleoceanography and Paleoclimatology*, *34*(4), 511–523. https://doi.org/10.1029/2018PA003481

Rahmstorf, S. (2002). Ocean circulation and climate during the past 120,000 years. *Nature*, *419*(6903), 207–214. https://doi.org/10.1038/nature01090

Schmittner, A., & Lund, D. C. (2015). Early deglacial Atlantic overturning decline and its role in atmospheric CO2 rise inferred from carbon isotopes ($\delta^{13}C$). *Climate of the Past*, *11*(2), 135–152. https://doi.org/10.5194/cp-11-135-2015

Skinner, L. C., Fallon, S., Waelbroeck, C., Michel, E., & Barker, S. (2010). Ventilation of the deep southern ocean and deglacial CO_2 rise. *Science*, *328*(5982), 1147–1151. https://doi.org/10.1126/science.1183627

Skinner, L. C., Freeman, E., Hodell, D., Waelbroeck, C., Vazquez Riveiros, N., & Scrivner, A. E. (2021). Atlantic Ocean Ventilation Changes Across the Last Deglaciation and Their Carbon Cycle Implications. *Paleoceanography and Paleoclimatology*, *36*(2), 1–

21. https://doi.org/10.1029/2020PA004074

Talley, L. D. (2013). Closure of the global overturning circulation through the Indian, Pacific, and southern oceans. *Oceanography*, *26*(1), 80–97. https://doi.org/10.5670/oceanog.2013.07

Thouveny, N., Moreno, E., Delanghe, D., Candon, L., Lancelot, Y., & Shackleton, N. J. (2000). Rock magnetic detection of distal ice-rafted debries: clue for the identification of Heinrich layers on the Portuguese margin. *Earth and Planetary Science Letters*, *180*(1–2), 61–75. https://doi.org/10.1016/S0012-821X(00)00155-2

Voigt, I., Cruz, A. P. S., Mulitza, S., Chiessi, C. M., Mackensen, A., Lippold, J., Antz, B., Zabel, M., Zhang, Y., Barbosa, C. F., & Tisserand, A. A. (2017). Variability in mid-depth ventilation of the western Atlantic Ocean during the last deglaciation. *Paleoceanography*, *32*(9), 948–965. https://doi.org/10.1002/2017PA003095

Response to reviews on the manuscript titled "Centennial-scale intensifications of the Atlantic Meridional Overturning Circulation during Heinrich Stadial 1" (NCOMMS-25-07736)

We sincerely thank the Reviewers for their valuable and constructive feedback. We believe that the manuscript and supplement have improved significantly as a result of their insightful comments. In the following point-by-point response, the Reviewers' comments are presented in **black**, while our replies are shown in **blue**. The cited line numbers in this response refer to the revised versions of the manuscript and the supplement in the version with changes tracked. We also provide marked and clean versions of both the manuscript and the supplement for clarity. Citations from the revised manuscript are indicated by quotation marks (" "), followed by the corresponding line references.

Where appropriate, we have subdivided the Reviewers' comments into individual points to clearly address each issue. In cases where similar comments were raised by both Reviewers, we have cross-referenced our earlier responses to avoid repetition.

We truly hope that our revisions and clarifications satisfactorily address all the concerns and meet the expectations of both the Editor and the Reviewers.

Reviewer #1 (Remarks to the Author):

During this round of revision, the authors have included more description of the model, incorporated extensive discussion of the AMOC-CO2 relationship, and made some re-arrangements of implications. Overall, the quality of the manuscript has improved. However, several caveats or concerns remain that should be addressed. Please see the enclosed document for the details.

Response: We sincerely appreciate that Reviewer #1 considered that the first round of revision improved the quality of our manuscript. We are grateful for the thoughtful evaluation and constructive feedback provided in this second round of revision. We believe these valuable insights have strengthened the manuscript even more. Each comment has been carefully considered, and corresponding revisions have been made in the manuscript as appropriate. Detailed responses are provided below, highlighting the specific changes implemented where applicable.

General Comments

During this round of revision, the authors have included more description of the model, incorporated extensive discussion of the AMOC-CO2 relationship, and made some re-arrangements of implications. Overall, the quality of the manuscript has improved. However, several caveats or concerns remain that should be addressed:

Response: We thank the Reviewer #1 for their valuable feedback. We have carefully addressed all comments, further improving the manuscript. Our point-by-point responses to each comment are presented below.

Model Information: There still lacks some information on the model, such as the fresh water forcing, which is supposedly a major driver of the HS1 weakened AMOC.
Model-Proxy Mismatch: The variability of modeled AMOC seems lower during HS1, while, as noted by the other reviewer, the proxy data appear more sensitive when AMOC is less active.
AMOC-CO2 Relationship: The relationship between AMOC and deglacial pCO2 change is complex and debatable. Some processes in Section 2.4 conflict with existing reconstructions. No ocean carbon cycle-based proxies were included, and no biogeochemical processes were considered in the model. The discussion on the carbon cycle remains vague rather than robust.
Focus of Implications: The authors relate reconstructed B-P ventilation age to both regional climate change and deglacial CO2 increase. While both are important, discussing both may obscure the paper's focus.

Response: We sincerely thank Reviewer #1 for identifying the four major points for further improvement of the manuscript and for providing detailed comments on each. We have carefully considered all the comments and revised the manuscript accordingly.

We have added additional information on the model parameters in Section 3.4 (lines 500-508). The apparent model-proxy mismatch has also been addressed and the limitations of the present model has been stated in lines 185-189. We have included an explanation in lines 199-203 clarifying why our study site shows a consistent relationship between B-P ventilation age and AMOC under weak AMOC conditions.

We acknowledge that, at this stage, we are unable to incorporate carbon cycle model simulations to further support the results. We also recognize that many models, including ours, currently lack the resolution to capture the finer-scale changes observed in our record. While we believe our results are both novel and significant, we agree with Reviewer #1 that future confirmation using carbon cycle models would be valuable. In the revised version, we have strengthened the discussion of the AMOC-CO_2 relationship, incorporating relevant published studies as suggested by Reviewer #1 (lines 391-413). Finally, regarding the focus and implications of our study, we believe that both processes under investigation (i.e., regional climate and deglacial CO_2 increase) are of critical importance and therefore have been discussed in the study. Still, we rephrased specific sentences to clarify the focus and implications (lines 113-120).

Specific Comments

L39 and onward: The statement about CO2 storage change lacks evidence from the data and is speculative. I suggest softening the tone.

Response: We appreciate the valuable feedback from Reviewer #1 and changed the text accordingly. Line 39 now reads: “These episodes of AMOC intensification likely transported substantial volumes of CO_2-rich water from the mid-depth Atlantic to the Southern Ocean, where the CO_2 was rapidly outgassed to the atmosphere.”

L86: Revise the statement to clarify that the Pa/Th data ‘show no significant variability within HS1’ is partly due to material resolution, to justify the aim of this study.

Response: We thank Reviewer #1 for highlighting this important point. Indeed, sample resolution is one of the primary factors that hinders past records to show the variability within HS1. The sentence was modified accordingly. Line 86 now reads: “Adding to the ambiguity, ^{231}Pa/^{230}Th records, a proxy for the rate of deepwater transport and thus AMOC strength, show no significant variability within HS1, likely due to the low temporal resolution that hampers the detection of short-term fluctuations.”

L216: Regarding glacial Atlantic water masses, I question the use of ‘GNADW’ as the major body. Previous studies (e.g., Lippold2012NatGeo, Yu2019NatGeo) and models (e.g., Hain2010GBC, Du2022GRL) align on the narrative of shoaling of interglacial NADW to GNAIW. Ref31, based on Nd isotope, requires caution, as several processes can influence these signals.

Response: We thank Reviewer #1 for raising this point. We agree that the use of specific water mass terms (e.g., GNADW) could be problematic, given the existing uncertainties regarding their characteristics. Therefore, following the suggestion from Reviewer #1, we have modified lines 232-233.The line now reads as “The shallower and vigorously ventilated waters probably had a northern source and spread as far south as 30°S during LGM, and played a key role in the effective ventilation of the mid-depths of the western equatorial Atlantic, while deeper depths (i.e., > 2.5 km) remained isolated”

Fig 4: Fig 4a shows two ventilation peaks (B-P age minima) during Late HS1, but these do not align well with Fig 4e’s Pa/Th record. Pa/Th variation is small during HS1, while B-P ages vary greatly.

While the authors attribute this to Pa/Th sample resolution, short-lived regional processes may also play a role. Can the authors check whether the B-P age decrease is driven more by B or P? If B, the discussion may be valid; if P, the interpretation is more complex.

Response: We sincerely appreciate the insightful comment from Reviewer #1. We agree that interpreting changes in planktic values is more complex. In our results, though minor changes are observed in both benthic and planktic (as expected due to potential reservoir age effects) values during HS1, the two main events (~16.3 and ~15.4 ka) are primarily driven by variations in the benthic values. This means that the changes in ventilation ages are related to lower benthic values (see the plot below). Please note the benthic radiocarbon ages are significantly young in reference to the dashed line during the centennial scale events. The smaller vertical distance (low B-P age) between benthic and planktic radiocarbon ages during AMOC intensification periods are triggered by deviations in benthic ages from the dashed line. Therefore, the dominant factor influencing the B–P values are changes in the radiocarbon concentration of bottom waters, related to changes in bottom water circulation.

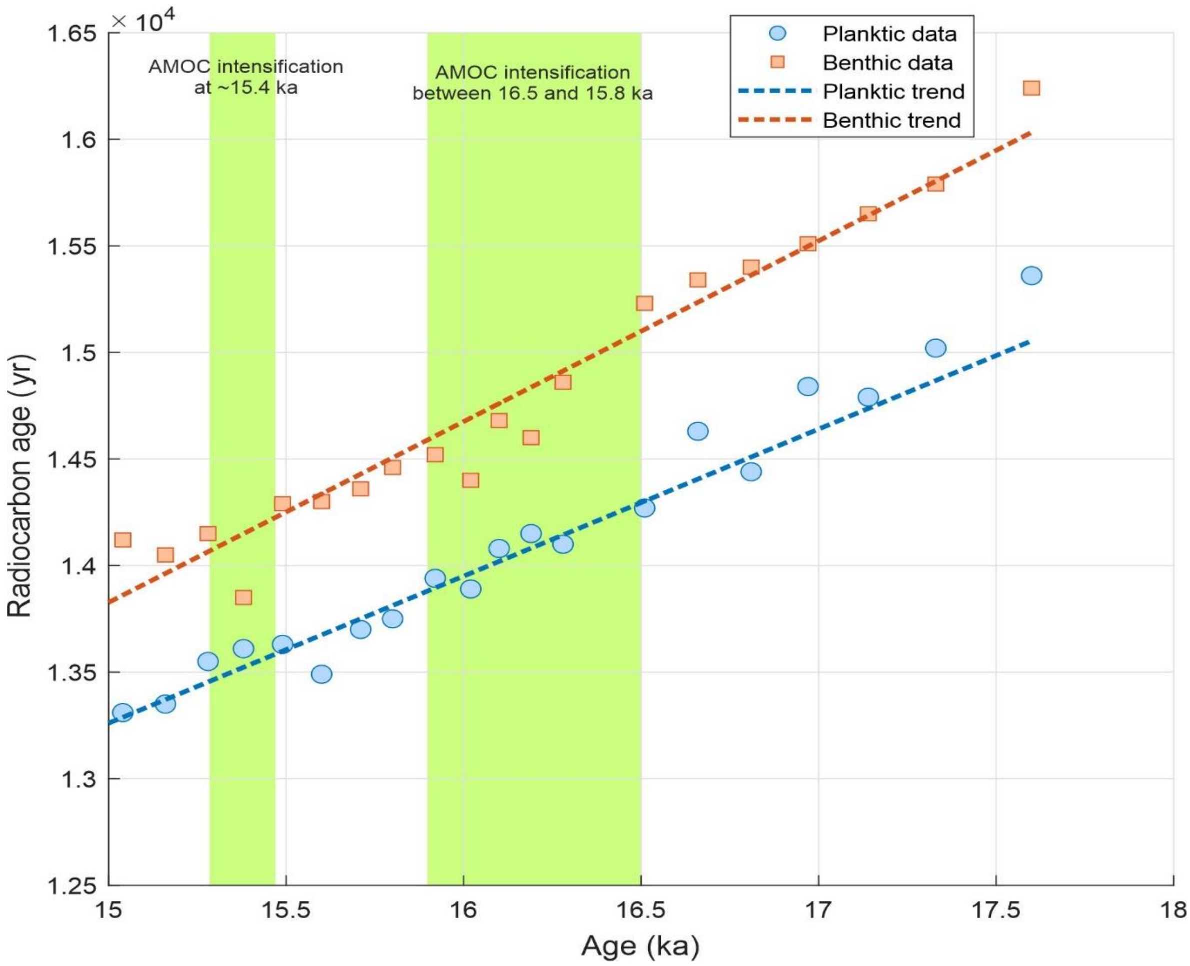


Figure RL1: Variations in benthic and planktic radiocarbon ages during Heinrich Stadial (HS) 1. The dotted line indicates a linear fit (trendline) of the data points. Please note that the lower B-P ages during ~16.3 and ~15.4 ka are caused by significant shift in the Benthic ages.

L338 and onward: The summary of processes influencing deglacial pCO2 change is improved, but the term "flushing processes" is unclear. What does flushing mean, and does increased advection lead to more CO2 release?

Response: We thank Reviewer #1 for this important comment. In our definition, "flushing" refers to the transfer of CO_2-rich waters to the Southern Ocean. Increased advection of CO_2-rich waters to the Southern Ocean, combined with upwelling, lead to more CO_2 release. In addition to flushing and upwelling, other processes like the presence of excess CO_2 remaining in surface waters after capture by the solubility and biological pumps also play a role in the oceanic release of CO_2. In the Discussion section (lines 352–371), we clearly defined the term "flushing" and emphasized that these additional processes also significantly contribute to changes in atmospheric CO_2.

L368 and onward: The paragraph on B-P age and short-term deglacial pCO2 release includes confusing or potentially erroneous statements. For instance, how does intensified AMOC reduce upwelling? If AMOC or NADW production increases, upwelling should strengthen to close the overturning system. Additionally, the claim that lower B-P age (stronger AMOC) leads to Southern Ocean SST drop due to constrained meridional heat transport (citing ref13) is problematic. Ref13 (via Shakun2012Nat) shows a negative North-minus-South SST due to the bipolar seesaw. However, new records (e.g., Dai2022NatComm) show SST increases at ~15.4 or 16.3 ka, suggesting the solubility pump plays a major role, conflicting with this study's conclusion. I suggest reviewing new Southern Ocean SST records and refining the discussion on drivers of short-term pCO2 change.

Response: We agree with Reviewer #1 that an intensification of the AMOC requires an intensification of the upward movement of northern component water elsewhere. Today, much of the North Atlantic Deep Water formation is balanced to a large extent by upward diapycnal diffusion in the Pacific and Indian Oceans (see Talley, 2013; https://doi.org/10.5670/oceanog.2013.07). The reduced upwelling in the Southern Ocean (i.e., during Holocene and BA) is likely not directly connected to the upper overturning cell in the Atlantic Ocean, but rather a consequence of reduced ventilation and vertical convection with Antarctic Bottom Water Formation. For example, Willeit et al. (2024, https://doi.org/10.1038/s41561-025-01814-0) find a decrease of Southern Ocean convection with an intensification of AMOC (and vice versa). Thus, our suggestion of changes in the strength of AMOC, flushing and upwelling are not in contradiction. Discussed in the manuscript in lines 371-373.

The drop in Southern Ocean and Antarctic surface temperatures during times of intensified AMOC is well documented in ice cores and sediment records (EPICA community members. 2004, https://doi.org/10.1038/nature02599; Barker, et al., 2009, https://doi.org/10.1038/nature07770). For example, ice cores show that the BA (strong AMOC) is associated with a cooling event, the so called "Antarctic cold reversal". We have added a statement in line 371 to explain this point- "Importantly, an intensification of the AMOC is not necessarily liked to enhanced upwelling in the Southern Ocean, since today much of the North Atlantic Deep Water formation is balanced to a large extent by upward diapycnal diffusion in the Pacific and Indian Oceans"

We agree that the study of Dai and Yu (2022, https://doi.org/10.1038/s41467-025-57677-x) provides an excellent contribution to understanding millennial-scale changes in carbon cycle dynamics. However, we believe that the record presented by Dai and Yu (2022, https://doi.org/10.1038/s41467-025-57677-x) does not contradict our centennial-scale results, because: (i) the temperature record (core MD97-2106) originates from the Pacific sector of the

Southern Ocean and therefore does not record temperature variations in the Atlantic sector of the Southern Ocean; and (ii) the temperature record does not indicate two distinct events around 16.3 ka and at ~15.4 ka, but rather shows a nearly continuous temperature increase throughout HS1. Nevertheless, we acknowledge the importance of this paper in explaining carbon cycle changes and have accordingly incorporated its findings into our discussion (lines 387–388).

Section 3.4: The revised model description is much improved, with Figs S7 and S8 showing good model-data consistency. The 5-degree resolution model captures deglacial C14 changes, despite neglecting biological C14 processes and surface-disequilibrium-driven C14 changes. How much might these processes, especially surface disequilibrium, impact results on short timescales?

Response: We thank Reviewer #1 for raising this point. We are not able to decompose the signal of $\Delta^{14}C_{DIC}$ into equilibrium and disequilibrium components of DIC because our model does not include a full representation of the marine carbon cycle. However, we reiterate that the effect of marine biogeochemical processes on $\Delta^{14}C_{DIC}$ (and hence on B-P ages) is less than 5 % compared to the effects of radioactive decay and transport Butzin et al. (2024, https://doi.org/10.5194/gmd-17-1709-2024), which is already mentioned in the manuscript (lines 516-517). The uncertainty range in Butzin et al. (2024, https://doi.org/10.5194/gmd-17-1709-2024) was estimated from steady-state conditions. In the meantime, the authors (led by a co-author of this study, Martin Butzin) have extended their analysis and studied the abrupt and massive $\Delta^{14}C$ perturbation due to the atmospheric ^{14}C bomb pulse. The differences between the $\Delta^{14}C$ approximation and the full carbon cycle approaches culminate in 1964 / 1965 CE at the sea surface and rapidly decline afterwards. As shown below, the relative uncertainty still remains less 5 % in the global mean as well as in the source regions of NADW. This could be considered as a measure of short-term deviations from surface equilibrium, that is, thus, smaller than 5%.

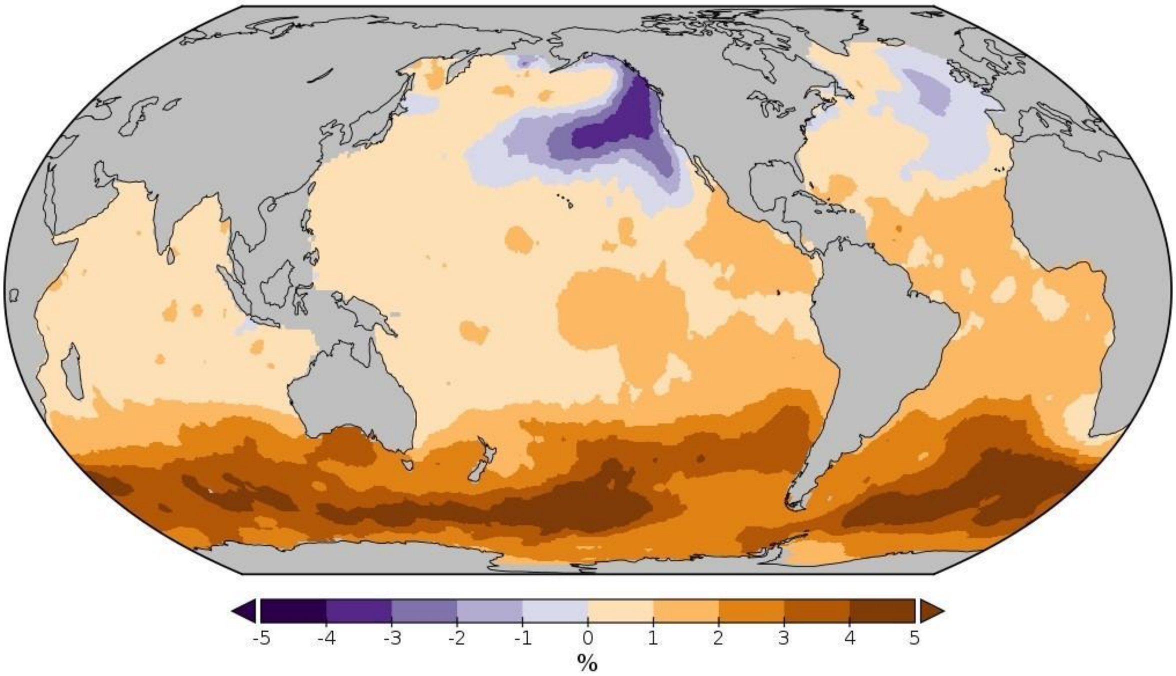


Figure RL2: Relative differences between two ways of simulating radiocarbon ($\Delta^{14}C$) in the ocean: a simplified $\Delta^{14}C$ approximation (used in this study) versus a full marine carbon-cycle model. The map shows the maximum difference (in % of $\Delta^{14}C$ of dissolved inorganic carbon, DIC) that occurs in the surface ocean during the period 1850–2015 CE, with the largest mismatch appearing around 1965 CE, when atmospheric nuclear bomb testing caused a rapid rise in $\Delta^{14}C$ (“bomb pulse”). Warm colours indicate where the simplified approach predicts higher $\Delta^{14}C$ than the full model, and cool colours show where it predicts lower values. The comparison is based on bomb-pulse simulations with the FESOM2.1–REcoM3 model (transient extension of the steady-state carbon-isotope simulations in Butzin et al., 2024, https://doi.org/10.5194/gmd-17-1709-2024)..

Model-Results Mismatch: The model setup seems similar to the PaleoMist_full experiment in ref65, yet AMOC strength differs (e.g., nearly 0 Sv in this study at HS1 minimum vs. ~10 Sv in ref65). This may stem from: 1) the longer simulation (>70 kyr) in this study, or 2) using only insolation, GHG, and ice sheets as forcing, neglecting FWF. If FWF was excluded, what caused the AMOC weakening at HS1?

Response: We thank Reviewer #1 for this comment. Indeed, our simulation period is much longer than that in Willeit et al., (2022, https://doi.org/10.5194/gmd-15-5905-2022) who start at 25 ka from a spun-up LGM equilibrium state. Moreover, we use a different and more recent model version (CLIMBER-X version 3 instead of version 2 in Willeit et al., (2022, https://doi.org/10.5194/gmd-15-5905-2022), which is now specified in the revised manuscript (line 502). Freshwater forcing is always included in terms of the meltwater discharge from the continental ice sheets, which is also now clarified in the model description of the revised manuscript (line 508). Finally, the AMOC weakening is caused by the freshwater discharge to the North Atlantic during HS1.

Fig 1b: The low variability of B-P age and AMOC strength during HS1 is surprising, given clear long-term B-P age fluctuations and milder AMOC variability elsewhere. The authors attribute this to model architecture, but I suspect the model may be less sensitive when AMOC is weaker. This lack of variability hinders mechanistic understanding of the B-P data during HS1.

Response: We thank Reviewer #1 for raising this point (the Reviewer is presumably referring to Figure 2b, as Fig. 1b does not display any model result.) The model may underestimate AMOC and B-P age variability (not only) during HS1 because the PaleoMIST ice sheet reconstruction has a time step of 2500 years and the model considers interpolated values in between. We have mentioned this in line 183. However, the model outputs were strictly used to understand the relationship between AMOC and B-P ventilation on millennial timescales while the proxy results were used to understand centennial-scale changes

Fig 3: I suggest adding in-plot legends/annotations to clarify the meaning of 'BPr'/'BPm'/'AMOCm', as these terms are not commonly used.

Response: We thank Reviewer #1 for the suggestion. We modified Fig. 3 accordingly.

Reviewer #3 (Remarks to the Author): (the text in black and italic shows the comments made in the first round of revision by Reviewer #2, while text in black and no italic shows the comments from Reviewer #3 made to this second round of revision)

I thank the authors for revising the manuscript in light of Reviewer #2's insightful comments. As the third reviewer, I acknowledge that the authors present a valuable new high-resolution dataset, provide important constraints, and offer a study of broad interest. While many suggestions have been addressed, some critical concerns remain insufficiently answered and are dismissed as being outside the study's scope. In this reviewer's opinion, addressing these concerns is essential for publication in Nature Communications, and the authors appear to have the necessary modeling data to do so. Therefore, while I appreciate the authors' extensive efforts, I recommend another major revision before the manuscript can be considered for publication. I remain optimistic that the authors can successfully address the outstanding issues.

Response: We sincerely appreciate the thoughtful evaluation and constructive feedback from Reviewer #3. These valuable insights significantly improved the quality of the manuscript. All comments have been carefully addressed, and appropriate revisions have been incorporated into the text. Detailed responses are provided below, outlining the specific changes made where applicable.

Reviewer #2 Comment:
Ventilation age and AMOC discussion is not clear and precise at various places in the manuscript. A linear relationship is discussed/inferred but when one looks at supplement Fig. 5 there is a very rapid change in ventilation age over just a couple of Sv AMOC change when

-AMOC is weak (e.g. between 3 and 5-6 Sv there is a change in ventilation between >800 and <400 years—which covers most of the reconstructed variability). The current way it is discussed does not really take this into account. The paper should acknowledge that most of the signal at the core site (based on the model, but also comparison to hydrography) may be driven by relatively modest changes in AMOC when AMOC is relatively weak. Put another way, ventilation at the site is very sensitive to AMOC changes when AMOC is low, but less sensitive to monitoring changes when AMOC is high. This might also explain how/why you get such rapid apparent shifts in ventilation age when other records have not recorded clear/major variability in AMOC at these times.

In response to this the revised manuscript has categorized the data points (Supplementary Fig. 5) into two groups: (i) data points with AMOC < 10 Sv; and (ii) data points with AMOC > 10 Sv. They show a clear distinction emerges between the two groups, with the weak AMOC phase displaying a stronger correlation between AMOC and radiocarbon ventilation age (R2 = 0.67) versus the correlation between the strong AMOC and radiocarbon ventilation ages (R2 = 0). However, this raises another question that during strong AMOC the authors find no linear relationship. They should explain this in detail if they want to support their finding that only weak AMOC phase display a stronger correlation between AMOC and radiocarbon ventilation age. And if so, why?

Response: We sincerely thank Reviewer #3 for this thoughtful comment. The lack of a strong relationship between ventilation ages and AMOC strength during periods of strong AMOC is likely an artifact of our study location. Strong AMOC conditions coincide with low B–P values in our core, indicating a good ventilation of the mid-depth western equatorial Atlantic. Low B-P values are more easily affected by two factors: (i) variability in planktic radiocarbon values due to changes in surface reservoir age, which introduces additional variability in B–P values independent of AMOC strength; and (ii) benthic radiocarbon values that are similar to planktic values due to the proximity of our site to the formation sites of northern component water in the North Atlantic, limiting ^{14}C decay. Consequently, the associated uncertainties when calculating B-P values are relatively high when AMOC is strong. The overall process linking B-P ventilation ages at our core site and AMOC strength, however, is the same under a weak or a strong AMOC.

We suggest that such effects would likely diminish at locations farther away from the sites of northern component water formation, where B-P values are relatively higher. Moreover, since the centennial-scale intensifications of AMOC strength described in our manuscript occurred under a weak AMOC background (i.e., HS1), we are confident that our proxy is applied under optimal conditions.

Following the suggestion from Reviewer #3, we discussed this point in lines 199-203 of the main text and added these points to caption of Supplementary Fig. 5.

Reviewer #2 Comment:

There are some other issues that could be improved when discussing the model results and AMOC-ventilation relations:

(a) What is actually done with the model is not clear as the materials and methods (section 3.4) is pointed to but there are very few details here, just which model and boundary conditions and the statement "the implementation of marine D14C essentially follows ref 69" when one looks that up, a number of things were done in that paper making it very unclear what was used from that implementation here. Interestingly when one looks at that paper they suggest that the best LGM state had a 40% reduction in NADW export while in this paper the LGM state in figure 3 has a very similar AMOC to the Holocene?

The revised manuscript should provide more detail on the model boundary conditions for clarity. The model is forced with insolation, greenhouse gases, continental ice sheets, and atmospheric $\Delta^{14}C$. However, it remains unclear how AMOC changes are simulated. For example, most model experiments (including TraCE-21ka) simulate AMOC weakening through explicit freshwater forcing. Does the employed model include meltwater flux from continental ice-sheet melting? The authors argue that AMOC weakening and increased ventilation age coincide with IRD records, so they must explain how AMOC changes are generated in their experiment.

Response: We thank Reviewer #3 for raising this point. Yes, the model considers the meltwater fluxes from reconstructed continental ice sheets by using the PaleoMIST (Willeit et al., 2022, https://doi.org/10.5194/gmd-15-5905-2022) reconstruction. This is now clarified in the model description (line 475). The weak AMOC in the model around HS1 was triggered by these freshwater fluxes.

In addition, the simulated AMOC during the Bølling–Allerød (BA) and late Holocene appears equally strong, despite the BA being substantially warmer and previous transient simulations (e.g., TraCE-21ka) showing AMOC overshoot during the BA. This reviewer finds AMOC shoaling during the LGM and stronger AMOC in the late Holocene compared to the BA, yet maximum AMOC strength appears similar across the LGM, BA, and late Holocene. These discrepancies should be clearly discussed and illustrated (e.g., in supplementary figures).

Response: We thank Reviewer #3 for this this valuable comment. We do not think that the apparent AMOC similarity during the late Holocene and the BA is at odds with our key findings. Instead, we think it is relevant to consider the AMOC transition from the LGM through HS1 until the BA. In fact, our simulation also yields an AMOC overshoot of ~ 30 Sv for the beginning of the BA (see Figure 2 where it is somewhat hidden by the opal curve). In any case, the model outputs were strictly used to understand the relationship between AMOC and B-P ventilation on millennial timescales while the proxy results were used to understand centennial-scale changes.

The definition of simulated AMOC strength must also be specified (e.g., maximum stream function below 500 m between 20°N–60°N). Similarly, since the model is prescribed with both $\Delta^{14}C$ and greenhouse gases (please clarify whether this includes CO_2, CH_4, etc.), the authors should explain how simulated ventilation age changes can be linked to atmospheric CO_2. Thus, the authors should explain this in more detail and whether a fully coupled climate–carbon cycle model is required to substantiate the proxy–model comparison?

Response: The AMOC strength is calculated as the maximum of the meridional stream function in the Atlantic northward of 30°S below 700 m. This is now clarified in the manuscript in the caption of Figure 3 in lines 171-172. Greenhouse gas forcing includes CO_2, CH_4 and N_2O, which is also now clarified in line 474. The ventilation of the ocean with $^{14}CO_2$ depends on the partial pressure of atmospheric CO_2 (as well as on its solubility and on wind speed). Rising CO_2 levels lead to faster air-sea exchange and lower planktonic ^{14}C ages (by about 200 years between 18 and 14 ka, as indicated by Figure 2 of Butzin et al. (2017, https://doi.org/10.1002/2017GL074688). In turn, B-P ages decrease which is opposite to our observations in the present study. For our study of ^{14}C-based ventilation ages it is not necessary to include a full-climate carbon model; please see our detailed reply to Reviewer #1 regarding other marine biological processes further above (lines 203-230 of this response letter).

Finally, clarification is needed on how land–sea boundaries evolve in the simulation. Changes in the North Atlantic land–sea boundary could influence convection sites in the Nordic Seas and, in turn, affect AMOC strength.

Response: The deglacial sea level rise was taken into account in our simulation. The AMOC variability in our model is primarily controlled by freshwater input. This is now included in the revised version of the manuscript in lines 479-480.

Reviewer #2 Comment:
a) You argue that the similarity between the model ventilation age change and ventilation ages to that which you observe, underpins their relationship. Its fairly convincing (NB caveats noted above) when one looks at the whisker plots and covariations in different states. However, the focus of the paper is not about these states, it is about the interpretation of the very brief deviations in ventilation age that interrupt these different modes. These are very short lived, but when I look at the model changes in ventilation age, it is not clear to me if such sudden deviations and recoveries are possible for this model (the model ventilation age curves seem to respond too slowly, but its hard to tell). Thus, how relevant/applicable is the model framework for representing these very short lived deviations? It's a question out of interest and it might be worth mentioning or caveating (if for example the model would not predict the magnitude and rate of the observed ventilation changes in responses to AMOC reduction/intensification). In this context its also important to note that very large ventilation changes are found with only minor changes in AMOC in the model (supplement Fig. 5 and comment above).

The authors responded that their model output is intended to demonstrate that the observed changes in ventilation age at the core site largely reflect variations in AMOC strength, particularly during periods of weaker AMOC, such as during HS1, which is the focus of their study. The model is not intended to resolve short-term (centennial-scale) intensifications within broader millennial-scale AMOC slowdowns like HS1. Firstly, they should mention this simulation caveat in their manuscript. Secondly, the authors should be careful since they repeatedly associated simulated AMOC and ventilation ages to explain their first-of-its kind centennial AMOC intensification ~16.3ka and ~15.4ka.

Response: We thank Reviewer #3 for this comment. Our study includes both modelled and reconstructed B–P ventilation ages. While the model results do not capture high-resolution variability (and is not expected to do so), the proxy-based reconstructions reveal changes at centennial timescales.

Following the suggestion from Reviewer #3, we have now clearly mentioned (in lines 182-184) that the model is not intended to resolve short-term (centennial-scale) intensifications, that is done with the proxy reconstruction.

We clarify that the events at 16.5-15.8 and ~15.4 ka are seen in a record that has been built with radiocarbon measurements from a sediment core. The high-resolution record is not from the model, but rather from the proxy data.

Reviewer #2 Comment:

D)if ventilation is a reliable indicator of AMOC, why is there an offset in supplement figure 6 between increased ventilation and reduced AMOC (increased Pa/Th)--this appears much too large to be proxy response time related.

This is a very important point. This reviewer agrees with the authors that the relatively low resolution of the Pa/Th data (Voigt et al., 2017) fails to capture the centennial change in AMOC during HS1. In my opinion, the authors may say something like that their proxy study strength is higher resolution record which is not seen in previous proxy records.

Response: We thank Reviewer #3 for this valuable comment. In the revised version of the introduction, we now highlight the scarcity of highly resolved Pa/Th records (lines 84–85). Furthermore, while outlining the objectives (in lines 112-120) of our study, we emphasize that our work provides a high-resolution record that was lacking in previous studies.

Reviewer #2 Comment:

To me the stated relation between weak ventilation and intense IRD intervals is not so clear as presented in the text. From the figure it appears that this is sometimes true and other times not true (well ventilated period in 1b is associated also with high or rising IRD).

This is a very important point. The authors have explained their low AMOC/high ventilation ages matches with the IRD events therefore providing the possible cause of causes of centennial-scale AMOC intensifications. While, sometimes this relationship is

not seen therefore this reviewer strictly feel that this point must be answered in the manuscript. Since the authors include simulated AMOC strength, it is within their domain to try to explain the possible causes of these simulated changes in AMOC and ventilation ages.

Response: We thank Reviewer #3 for raising these insightful points. We consider that Reviewer #3 is referring to Figure 4d, where the magnetic susceptibility record shows a slight offset (~300 years) relative to our records. This discrepancy is most likely due to limitations in the age–depth model of core SU8118 (Thouveny et al., 2000: https://doi.org/10.1016/S0012-821X(00)00155-2). When age-depth model uncertainties are considered, the overall alignment appears reasonable.

Regarding the use of numerical modelling to investigate this issue, our current model is not able to resolve centennial-scale variability. Hence, we are unable to test the suggested hypothesis at this stage. Nevertheless, we sincerely appreciate the valuable suggestion from Reviewer #3, which could be explored in future studies.

Reviewer #2 Comment:
Lines 309-316; you discuss flushing of the tropical thermocline might be driven by the same type of processes as rapid ventilation in the subpolar/drake passage region? The wording is quite vague here making it hard to sort out what exactly is happening in the two areas (and the potential relationship), but I'd be surprised if they were identical processes in these very different hydrographic settings/regimes. Can you please be more specific about your inferences of what processes are going on and how these are or are not related? Given the difference in timing between the events in the two locations, why do you infer they have a common source/cause/dynamic? Why are some important for CO2 and some not? This latter point is critical to answer given the end focus on the implications for co2.
This reviewer is of the opinion that addressing these additional events lies within the scope of this present study and therefore must be addressed. Further, the authors could also use their model simulations to show does the simulated centennial scale AMOC strengthening actually enhance northward oceanic heat transport resulting in ITCZ shift and reduced precipitation changes over northeast Brazil. Meanwhile, they could also see the simulated Southern Ocean upwelling to explore its role during centennial scale AMOC change and potential carbon dioxide degassing.

Response: We are grateful to Reviewer #3 for this comment. Unfortunately, the model is not capable of reproducing centennial-scale changes. This limitation was added to lines 184-190. Therefore, the centennial-scale results are based on the high-resolution proxy record. On the other hand, if we consider millennial timescales, the link between AMOC and Brazilian precipitation has been well understood through proxy and model records (i.e., Campos et al., 2019: https://doi.org/10.1016/j.quascirev.2019.105990; Mulitza et al., 2017: https://doi.org/10.1002/2017PA003084; Portilho-Ramos et al., 2017: https://doi.org/10.1038/S41598-017-01629-Z).

Reviewer #2 Comment:

Section 2.4 (discussion of relation between ventilation and atmospheric co2).

This reviewer acknowledges and appreciates the authors for improving the understanding of this section as per the comments. However, the authors should make it clearer and more concise for better understanding.

Response: We thank Reviewer #3 for this comment. We have modified section 2.4, in particularly lines 391-413, for more clarity. The paragraph now presents a clear explanation of short-term atmospheric CO_2 changes and its link to oceanic processes.

Reviewer #2 Comment:

Stepping back, I'm struck that you see such intermittent/variable ventilation changes during the Heinrich event. Doesn't this influence how we think about ocean circulation and dynamics and our ability to model them? You end the paper mentioning the challenge of modelling the brief co2 pulses, but doesn't it also pose challenges for modelling circulation dynamics as well? How can circulation return to full strength briefly and then be curtailed again—flickering on during a period of suppression. Is this a forced (relaxed forcing) behavior or is it a dynamic system jumping between attractors? Do any models see such rapid state changes (see earlier comment here as well about model representation of short ventilation events)?

This reviewer still believes that an evaluation on the causes of such events is within the scope of this study as it solidifies their attempt to publish at the prestigious Nature Communications journal.

Response: We thank Reviewer #3 for this remark. However, we hold that these points would require a stand-alone manuscript. In addition, many of these points have been extensively investigated by Willeit et al. (2024, https://doi.org/10.5194/egusphere-2024-819.) using the same model.

Response to reviews on the manuscript titled "Centennial-scale intensifications of the Atlantic Meridional Overturning Circulation during Heinrich Stadial 1" (NCOMMS-25-07736)

We sincerely thank the Reviewers for their valuable and constructive feedback. We believe that the manuscript and supplement have improved even more as a result of their insightful comments. In the following point-by-point response, the Reviewers' comments are presented in black, while our replies are shown in blue. The cited line numbers in this response refer to the revised version of the manuscript with changes tracked. We provide marked and clean versions of both the manuscript and the supplement for clarity. Citations from the revised manuscript are indicated by quotation marks (" ") in this response letter, followed by the corresponding line references.

We truly hope that our revisions and clarifications satisfactorily address all the concerns and meet the expectations of both the Editor and the Reviewers.

Reviewer #1 (Remarks to the Author):

During this round of revision, the authors have addressed most of my concerns. The section on the AMOC-CO2 relationship is now sufficiently clear and coherent, although, as the authors acknowledge, a more accurate or realistic investigation would require the use of biogeochemical models. I have only a few minor and specific comments and suggestions. Please see the attached.

Response: We sincerely appreciate that Reviewer #1 considered that we appropriately addressed most of their concerns, pending a few minor and specific points that we address in this round of revision. We are grateful for the thoughtful evaluation and constructive feedback. We believe that addressing these minor and specific points have strengthened the manuscript even more. Each point has been carefully considered, and corresponding revisions have been made in the manuscript as appropriate. Detailed responses are provided below, highlighting the specific changes implemented where applicable.

During this round of revision, the authors have addressed most of my concerns. The section on theAMOC-CO2 relationship is now sufficiently clear and coherent, although, as the authors acknowledge, a more accurate or realistic investigation would require the use of biogeochemical models. I have only a few minor and specific comments and suggestions.

Response: We thank Reviewer #1 for their valuable feedback. We are glad that the AMOC-CO2 section is now sufficiently clear. We have carefully addressed their few minor and specific comments and suggestions, further improving the manuscript. Our point-by-point responses to each comment are presented below.

L102-103: The meaning of this sentence is unclear to me. As the biological and solubility pumps act to draw down atmospheric CO2, a strengthening of these pumps should reduce atmospheric CO2 levels. In particular, ref20 indicates that the effects of the biological and solubility pumps differ during the later phase of HS1, with the solubility pump playing the dominant role.

Response: We thank Reviewer #1 for pointing out this confusing statement. In this part, our aim was to highlight that during the centennial-scale AMOC strengthening events, the strengthened biological and solubility pumps do not contribute to atmospheric CO_2 rise (as pointed out by Reviewer #1). Here, we wanted to further emphasize the role of the AMOC in driving the centennial-scale CO_2 jumps. We have modified the sentence for a clearer formulation, as follows:

"Notably, during these periods of intensified AMOC, strengthened biological and solubility pumps cannot be responsible for CO_2 release to the atmosphere[19,20], as they act to draw down atmospheric CO_2, further highlighting the dominant role of AMOC-driven flushing of stored carbon."

L114: 1367 m

Response: Thank you for noticing the mistake. We have corrected it.

L216: From Figure 3, BPm aligns closely with AMOCm, likely due to the relationship between ventilation and water mass formation in the North Atlantic. Upon closer inspection of BPr and BPm, however, they appear more consistent during HS1 than during other periods, particularly the BA or Holocene. This pattern aligns with points raised by Reviewers #2 and #3, namely that B-P ages perform better under a weakened AMOC mode. I suggest addressing this observation here.

Response: We are grateful to Reviewer #1 for raising this point. We agree that at our location, B-P ventilation ages respond better to AMOC strength during weak AMOC conditions such as HS1. Therefore, we performed two changes in the manuscript.

First, in line 199, after we discuss the strong AMOC - B-P ventilation age relationship below 10 Sv (this is the modification we implemented after comments from Reviewers #2 and #3 in the previous round of revision), we added that:

"In Fig. 3, the close agreement between BPm and BPr during HS1 emphasizes that B-P ventilation ages at our site are more reliably captured during this interval."

Second, in line 226, we rephrased the concluding sentence of this section to explicitly address this point. The revised sentence now reads:

"The results from our model simulation (Fig. 3; Supplementary Fig. 5) indicate that AMOC strength was the primary factor driving changes in B-P ventilation ages, and that the B-P ventilation age at our site responds most strongly during periods of reduced AMOC."

Furthermore, the authors state that the AMOC primarily controls B-P ages, even though they earlier note that several other factors may influence B-age (L202 and onward). While I agree that the AMOC exerts the strongest control, the claim that "AMOC was the primary factor ..." should be qualified to account for potential contributions from other factors. Specifically, if I understand correctly, factors (ii) and (iii) could, in principle, be diagnosed from the model results.

Response: We thank Reviewer #1 for this suggestion. A robust diagnosis of these factors (mixing between water masses) from the model results would require the determination of the various source water regions and a Lagrangian pathway (trajectory) analysis from the source regions to the core site. This is not implemented in the model.

Yet considering the relevance of these factors, a full note about them is available in the supplementary file. The note is supported by available proxy records. Please see Supplementary Note 2 for the detailed discussion.

L357: and -> and/or

Response: We thank Reviewer #1 for suggesting this correction. We corrected it accordingly.

L347 and onward: The narrative concerning the AMOC-CO2 relationship has been substantially improved. The proposed storage of excess carbon at intermediate depths in the Atlantic is supported by several B/Ca proxy records (indicators of carbonate ion concentration). An additional relevant point is the Pacific-Atlantic carbon seesaw, which may have facilitated carbon transfer between basins during HS1 (e.g., Menviel et al., 2012, 2014).

Response: We sincerely thank Reviewer #1 for suggesting additional studies that support and strengthen the AMOC-CO2 relationship. We have included the suggested studies in the discussion, in the following sentence (lines 423-425):

"Indeed, carbon was generally stored in the Atlantic mid-depths during HS1 as documented by lower resolution pH sensitive proxies (Lacerra et al., 2017; https://doi.org/10.1002/2016PA003081) and may have been further enhanced by the Pacific-Atlantic carbon sea saw processes that facilitated inter-basin carbon transfer (Menviel et al., 2014; https://doi.org/10.1002/2013PA002542; Menviel and Joos, 2012; https://doi.org/10.1029/2011PA002224)."

L492 and onward: The description of the simulation methodology has been considerably improved. The model verification presented in Figures S7 and S8 is satisfactory. I suggest adding a plot of the meltwater discharge into the North Atlantic to provide readers with a clear indication of the freshwater forcing magnitude applied (e.g., similar to Figure 1 in He, 2013, Nature, or Figure 1 in Menviel et al., 2018).

Response: Thank you for this suggestion, we have added supplementary Figure 9 showing the meltwater discharge from the ice sheets to the North Atlantic.

Reviewer #3 (Remarks to the Author):

Lines 185–189 of the revised manuscript state that the PaleoMIST ice-sheet reconstruction has a temporal resolution of 2500 years and that the model interpolates conditions between these time steps. The authors argue that, as a result, the model is not expected to capture variability within HS1, and that their simulations are used only to examine millennial-scale relationships between AMOC and B–P ventilation, whereas proxies are used to infer centennial-scale variability. However, this argument is inconsistent with the modelling approach and its application in the manuscript. Moreover, although the model setup closely resembles the PaleoMIST_full experiment, the AMOC strength differs substantially (e.g., ~0 Sv at the HS1 minimum in this study compared to ~10 Sv in PaleoMIST_full), which substantially limits the interpretability and applicability of the simulations presented here.

Response: We thank Reviewer #3 for raising this important point, which highlights limitations of the model results. We would like to clarify that, due to our choice of input parameters, the model employed in this study is not designed to resolve centennial-scale variability. Nevertheless, the model is capable of capturing abrupt transitions, as demonstrated by the clear responses at the onset and termination of HS1 (Fig. 2b).

Importantly, these limitations do not affect our interpretations, as the conclusions of our study are exclusively based on proxy reconstructions. In light of the constructive comments from Reviewer #3, we recognize that the wording in some specific passages of the previous version of our manuscript may have been misleading and could have implied a stronger reliance on model simulations than intended by us. This was not our intention. Accordingly, we substantially revised the text throughout the manuscript to clarify the role of the model (namely, to check the relationship between AMOC and B-P ventilation age at our study site) and to ensure that our interpretations of tropical precipitation changes and atmospheric CO_2 variability are explicitly derived from the proxy evidence. These changes are summarized below.

Lines 33-36(abstract):

"Here we use radiocarbon ventilation ages from a western equatorial Atlantic sediment core with a remarkably high sedimentation rate to reconstruct AMOC variability during the last deglaciation, with a focus on Heinrich Stadial 1 (HS1). Results from model simulations indicate that ventilation ages serve as a sensitive proxy for AMOC variability under weak overturning conditions (i.e., during HS1) at our study location". The statement now clearly conveys that the main source of information in our study is proxy results.

Lines 116-123 (last paragraph of the introduction in which we define the objective of our study):

"In addition, results from an Earth system model of intermediate complexity are used as an independent benchmark to assess whether B-P ventilation ages from our core site serve as a robust indicator of AMOC variability". This statement clearly indicates that the simulations were only used to check how well B-P ventilation ages and AMOC strength agree at our study location.

We agree that the similarities and differences in model setup between the PaleoMIST_full experiment performed in Masoum et al. (2024, https://doi.org/10.1029/2023GL107310) and our experiments warrant clarification. We employ a more recent version of the model (namely CLIMBER-X version 3, rather than version 2 used by Masoum et al., 2024). In our version, the momentum equations use a different parametrization of frictionational processes in the bottom water layer which are linked to the drag exerted by the bottom topography. We note that this updated parametrization is also used in other CLIMBER-X related publications more recent than Masoum et al. (2024), such as in the analysis of millennial-scale AMOC variations carried out by Willeit et al. (2024, https://doi.org/10.5194/egusphere-2024-819). The model version has been clarified in section 3.4.

We agree with Reviewer #3 that the model results have limited applicability for reproducing absolute centennial-scale AMOC magnitudes. This is an artefact of the choice of input parameters rather than the model itself. In the revised manuscript, we have clarified the model setup, its scope, and the limitations of the input parameters to improve the transparency and interpretability of the presented simulations (lines 186 - 192). We hope that the added clarifications improve the transparency and our change of texts more clearly describe the aim of the manuscript.

The authors repeatedly state that their AMOC results should not be interpreted on centennial timescales. Yet this position is difficult to reconcile with the fact that the manuscript draws heavily on model output to support its central conclusions. It is also concerning that many of the mechanistic questions raised by Reviewer #2 and Reviewer #3 in earlier rounds—questions fundamental to evaluating the validity of the modeled responses—have been dismissed by the authors as requiring a "stand-alone manuscript." This rationale is not scientifically convincing: if key mechanisms underlying the model–proxy comparison cannot be articulated, then the manuscript is not conceptually complete.

Response: We thank Reviewer #3 for raising this important point. We would like to reiterate that the main conclusions of our manuscript are not derived from the model results, but are instead based on the high-resolution proxy record. The model results are used solely to assess the relationship between AMOC strength and B-P radiocarbon ventilation ages at our core location.

At the same time, we acknowledge that our previous wording may have led to some confusion regarding the role of the model in the manuscript. To address this, we substantially revised the abstract (lines 34-36), introduction (lines 117-120), and discussion (lines 188-193) so that the objectives and scope of the study are now more clearly stated.

With regard to the mechanistic questions raised by Reviewer #3, we agree that our model configuration is not able to fully address these issues. This limitation arises from the temporal resolution of the prescribed ice-sheet forcing of originally 2500 years, which prevents the model from capturing centennial-scale variability. Nevertheless, our proxy data provide new insights that refine current understanding of AMOC variability during this interval. To support our results in a broader mechanistic context, we therefore take help from the mechanism proposed by Willeit et al. (2024, https://doi.org/10.5194/egusphere-2024-819), which describes conditions under which AMOC may exhibit flickering behaviour. These mechanisms have been incorporated into our discussion (lines 332-337).

The authors assert that the processes in question have been "extensively investigated" in Willeit et al. (2024). If so, the authors should at minimum cite this work and clearly explain the relevant mechanisms in the present manuscript. Furthermore, their explanation regarding the absence of a linear relationship during strong AMOC conditions must transparently acknowledge that this is largely an artifact of the study location.

Response: We thank Reviewer #3 for this critical comment. Willeit et al. (2024, https://doi.org/10.5194/egusphere-2024-819) provided important results that can help explain the proxy results observed in our study. We included the following sentences to the discussion (lines 332-337)

"A recent model study indicates that when the AMOC operates in a weak background state close to convective instability, relatively small perturbations in surface buoyancy forcing can trigger abrupt transitions (at least in the CLIMBER-X model) toward a stronger overturning mode. This mechanism provides a physically plausible explanation for the rapid strengthening of the AMOC inferred from our ventilation age record during mid-HS1."

We agree with Reviewer #3 that the lack of a linear relationship between B-P ventilation age and AMOC strength under strong AMOC conditions is a characteristic of our core location. We also agree that it needs to be strictly highlighted in the manuscript. We, therefore, modified line 203 to:

"The absence of a strong relationship during periods of strong AMOC at our study location likely results from (i)…"

In addition, we added another statement in lines 211-213 specifying this point:

"Please note that the lack of a linear relationship between B-P ventilation age and AMOC strength under strong AMOC conditions is a site-specific artefact, and a stronger relationship is expected at deeper and more distal locations."

Given the continued absence of mechanistic interpretation after two rounds of revision, the manuscript's core conclusions remain descriptive rather than mechanistically supported. The lack of substantive improvement in addressing these scientific concerns leads this reviewer to conclude that the manuscript is not suitable for publication in Nature Communications in its present form. I therefore recommend rejection at this stage, with the possibility of resubmission should a rigorous mechanistic framework be provided.

Response: We sincerely thank Reviewer #3 for his critical evaluation of the manuscript. Their comments helped us to substantially improve the clarity, rigor, and overall quality of the study. In sum, our manuscript provides a conceptual mechanistic understating of AMOC changes within HS1 and its implications for tropical precipitation and atmospheric CO_2 changes. The novel results of our study refine current understanding of AMOC variability and its influence on atmospheric CO_2 changes. This represents a meaningful advance in our understanding of Earth system processes and align well with the aims of the journal. We hope that the changes made in the manuscript in response to the reviewer's comments now clearly convey the aims and significance of our study.

Response to reviews on the manuscript titled "Centennial-scale intensifications of the Atlantic Meridional Overturning Circulation during Heinrich Stadial 1" (NCOMMS-25-07736)

We sincerely thank the Reviewers for their valuable and constructive feedback. We believe that the manuscript and supplement have improved even more as a result of their insightful comments. In the following point-by-point response, the Reviewers' comments are presented in black, while our replies are shown in blue. The cited line numbers in this response refer to the revised version of the manuscript with changes tracked. We provide marked and clean versions of both the manuscript and the supplement for clarity. Citations from the revised manuscript are indicated by quotation marks (" ") in this response letter, followed by the corresponding line references.

We truly hope that our revisions and clarifications satisfactorily address all the concerns and meet the expectations of both the Editor and the Reviewers.

Reviewer #1 (Remarks to the Author):

The authors have addressed my remaining minor comments from the previous round. The addition of Supplementary Figure 9 showing the meltwater discharge is also appreciated, as it allows the reader to evaluate the freshwater forcing applied.
Regarding my comment on L216, the authors now acknowledge that B-P ventilation ages at this site respond most strongly during periods of reduced AMOC (L199, 226), and that the absence of a linear relationship under strong AMOC conditions is site-specific (L210-211). These are appropriate clarifications. However, the request to diagnose factors (ii) and (iii) from the model was addressed only through proxy-based arguments in Supplementary Note 2, rather than through model diagnostics. While I accept this as a practical limitation, it does leave the question of what controls B-P ages at this site less fully constrained than it could be.

Response: We sincerely appreciate that Reviewer #1 considered that we appropriately addressed most of their concerns. We are grateful for the thoughtful evaluation and constructive feedback. We also thank the Reviewer for acknowledging both the practical limitations of the model and our effort to address this issue as far as possible using the available proxy evidence.

I have also carefully read the authors' responses to Reviewer #3. The main changes in this round are interpretational: the authors have revised the abstract, introduction (L118 and on), and discussion (L188-193) to reframe the model as a benchmark for the AMOC-ventilation age relationship, and have explicitly stated that the conclusions are based on proxy data. They have also cited the convective instability mechanism from Willeit2024 to provide a physically plausible framework for centennial-scale AMOC change. These textual changes improve the clarity of the manuscript.

That said, I note that the revisions are primarily changes in framing and language rather than new analyses. The substantive issues raised by Reviewer #3 have not been resolved through additional modeling work, particularly regarding the large discrepancy in AMOC magnitude between this study (~0 Sv at HS1 minimum) and the comparable PaleoMIST_full experiment (~10 Sv), and the lack of centennial-scale variability in the model during HS1. The authors attribute the AMOC magnitude difference to a newer model version with updated parameterization, which is a plausible explanation but remains untested. The mechanistic understanding of the centennial-scale AMOC intensifications still relies on citing external work rather than being demonstrated with the authors' own simulations.
In my view, the proxy record remains the principal strength and contribution of this manuscript. The high-resolution B-P ventilation age data from the western equatorial Atlantic represent a genuinely valuable dataset, and the reconstruction of centennial-scale AMOC variability during HS1, including the identification of two intensification episodes, is well-supported by multiple independent proxy records. The model component, while useful as a first-order consistency check, does not add substantial independent evidence beyond confirming that AMOC and B-P ages are correlated at this location.

Response: We thank Reviewer #1 for this positive assessment. We are glad that the revised manuscript (following changes suggested by the Reviewers) now more clearly conveys that the main conclusions are based primarily on the proxy data, with the model used as a first-order benchmark for the AMOC-ventilation age relationship. As mentioned, we acknowledged that Willeit et al. (2024; https://doi.org/10.5194/egusphere-2024-819) provides an appropriate mechanistic framework for interpreting the centennial-scale AMOC variability, and was included in the discussion. As already noted in the Methods, the difference from earlier model results reflects the use of a newer CLIMBER-X version with updated bottom-water friction parameterization linked to topographic drag. Finally, we sincerely appreciate the Reviewer's recognition of the importance of our high-resolution B-P ventilation age record, which represents a valuable new dataset and helps refine current understanding of AMOC variability and its climatic implications during HS1.

The concerns raised by Reviewer #3 regarding the model are scientifically valid. The manuscript would be strengthened by a more thorough treatment of these issues. However, given that the conclusions are derived from the proxy data and the model plays a supporting role, I do not consider the modeling limitations to be disqualifying. I leave it to the editor to weigh whether the current level of model-proxy integration meets NCOMM's standard.

Response: We thank Reviewer #1 for this constructive and positive assessment. We appreciate the Reviewer's recognition that, although the modelling issues raised are scientifically important, the main conclusions of the manuscript are grounded in the proxy data and that the model serves a supporting role. We are encouraged that the Reviewer considers the proxy data to be a very important strength of the manuscript.

Reviewer #3 (Remarks to the Author):

Review of Revised Manuscript

During this round of revision, the authors have addressed the majority of my previous concerns, and I appreciate the substantial effort invested in improving the manuscript. The overall quality, clarity, and presentation have significantly improved.

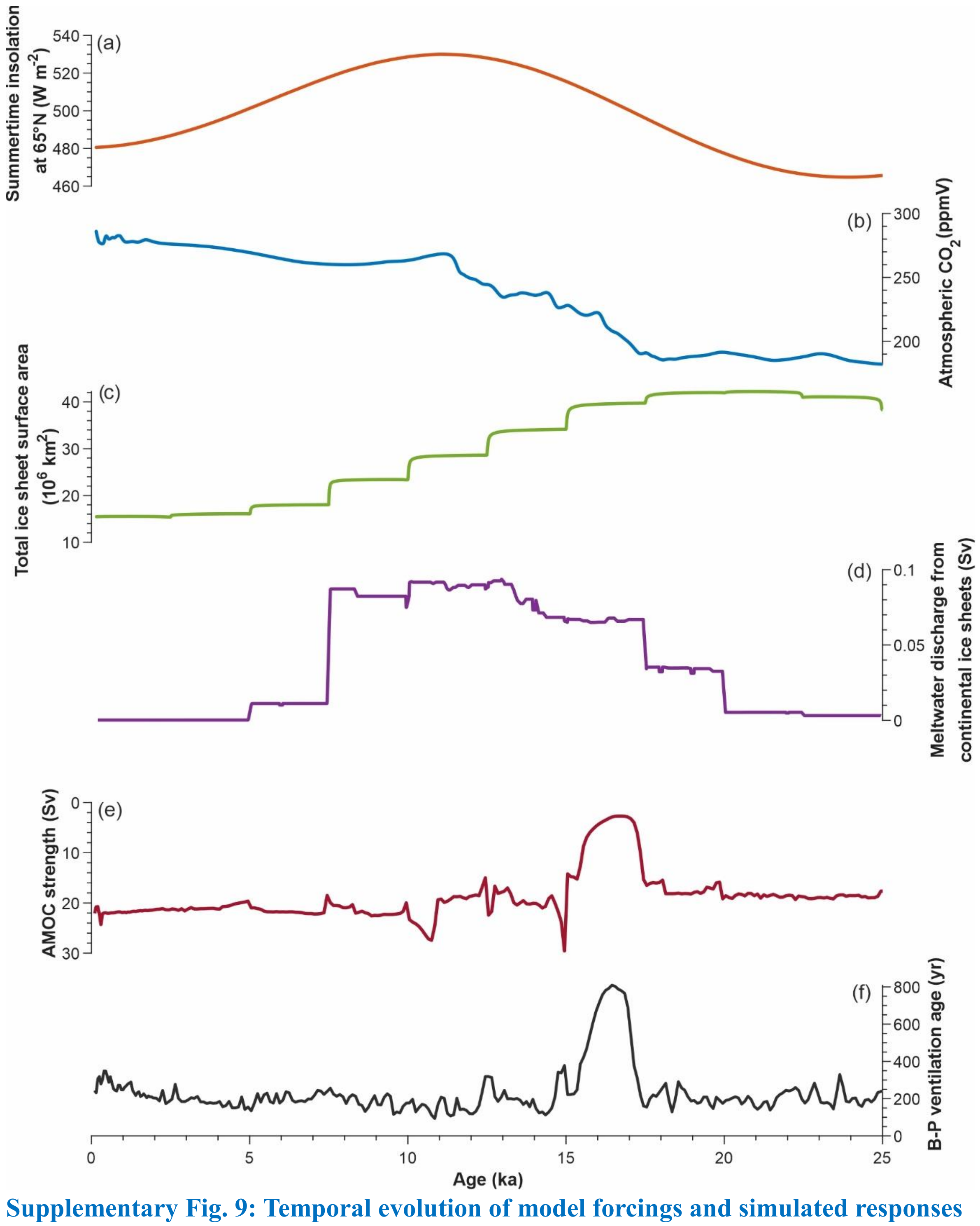
We sincerely thank Reviewer #3 for the positive reassessment of our manuscript. We greatly appreciate the Reviewer's recognition that the majority of the previous concerns have been addressed and that the manuscript has improved substantially in overall quality, clarity, and presentation. We are grateful for this thoughtful evaluation and constructive feedback.

However, I have one remaining point that requires clarification. The authors have added Supplementary Fig. 9 in this revised version showing the temporal evolution of freshwater runoff flux and simulated AMOC strength. While this addition is useful and necessary, it should be clearly specified whether the freshwater flux shown represents only surface runoff or whether it also includes meltwater discharge from continental ice sheets, as prescribed by the PaleoMIST reconstruction referenced in the manuscript. For clarity and completeness, I suggest that the authors explicitly describe this in the text and figure caption in Supplementary Fig. 9.

Response: We thank Reviewer #3 for this important clarification. We have revised the caption of Supplementary Fig. 9 as well as the y-axis title of the respective panel to clearly state the forcing shown in the figure. The label has been changed to: "Meltwater discharge from continental ice sheets to the North Atlantic (Sv)".

More generally, for transparency, it would be helpful to include the temporal evolution of all imposed model forcings—namely, insolation, greenhouse gas concentrations (CO_2), continental ice sheet boundary conditions, and associated meltwater discharge—in Supplementary Fig. 9. This would greatly enhance reproducibility and clarity for readers.

Response: We thank Reviewer #3 for this helpful suggestion. In response, we have modified Supplementary Fig. 9 (see the updated figure below) to include the temporal evolution of all imposed model forcings, together with the key modelled results. The revised figure now shows: (a) summertime insolation at 65°N, (b) atmospheric CO_2, (c) total ice-sheet surface area, (d) meltwater discharge from continental ice sheets to the North Atlantic, (e) simulated AMOC strength, and (f) benthic-planktic ventilation age. We agree that including the full set of forcings substantially improves the transparency, clarity, and reproducibility of the model setup and its interpretation.



Supplementary Fig. 9: Temporal evolution of model forcings and simulated responses over the last 25 ka. Shown are (a) Summertime insolation at 65ºN (Laskar et al. 2004; https://doi.org/10.1051/0004-6361:20041335), (b) Atmospheric CO_2 (Köhler et al., 2017; https://doi.org/10.5194/essd-9-363-2017), (c) Total ice-sheet surface area (Gowan et al., 2021; https://doi.org/10.1038/s41467-021-21469-w), (d) Meltwater discharge from continental ice sheets to the North Atlantic, (e) Atlantic Meridional Overturning Circulation (AMOC) strength, and (f) Benthic-planktic (B-P) ventilation age from the western equatorial Atlantic at 1450 m water depth.

Subject to these minor clarifications, I agree with the authors that the manuscript has been substantially improved, and I recommend it for publication in Nature Communications following another round of minor revision. I have also included some minor comments based on the revisions made in the current manuscript version.

Response: We sincerely thank Reviewer #3 for this positive assessment.

Minor Comments:

Line 34 (Abstract): Please specify the timescale of Heinrich Stadial 1 (HS1), i.e., HS1 (17.8–14.8 ka), consistent with the description provided in line 64.

Response: We thank Reviewer #3 for this helpful suggestion. In response, we have revised the Abstract to explicitly define the age range of Heinrich Stadial 1 as 17.8-14.8 ka.

Lines 181–182: Please confirm whether AMOC strength was calculated as the maximum streamfunction value northward of ~30°N (or ~30°S, if applicable).

Response: We checked the model diagnostic and confirm that AMOC strength was calculated as the maximum overturning stream function northward of 30°S. Therefore, no changes were made to the text.

Line 212: The author may consider changing the typography from "artefact" to "artifact."

Response: We have revised the spelling from "artefact" to "artifact"

We sincerely thank Reviewers #1 and #3 for their careful and constructive evaluation of the manuscript. Their comments helped us to substantially improve the clarity, rigor, and overall quality of the study. We are encouraged that the Reviewers found the revised framing appropriate and that the main contribution of the work is now more clearly conveyed. We believe that these novel results refine current understanding of centennial-scale AMOC variability and its broader role in Earth system processes. We are also pleased that the Reviewers are positive about the importance of this work and consider it to be well aligned with the aims of the journal.

General Comments

During this round of revision, the authors have included more description of the model, incorporated extensive discussion of the AMOC-CO2 relationship, and made some re-arrangements of implications. Overall, the quality of the manuscript has improved. However, several caveats or concerns remain that should be addressed:

Model Information: There still lacks some information on the model, such as the fresh water forcing, which is supposedly a major driver of the HS1 weakened AMOC.
Model-Proxy Mismatch: The variability of modeled AMOC seems lower during HS1, while, as noted by the other reviewer, the proxy data appear more sensitive when AMOC is less active.
AMOC-CO2 Relationship: The relationship between AMOC and deglacial pCO2 change is complex and debatable. Some processes in Section 2.4 conflict with existing reconstructions. No ocean carbon cycle-based proxies were included, and no biogeochemical processes were considered in the model. The discussion on the carbon cycle remains vague rather than robust.
Focus of Implications: The authors relate reconstructed B-P ventilation age to both regional climate change and deglacial CO2 increase. While both are important, discussing both may obscure the paper's focus.

Specific Comments

L39 and onward: The statement about CO2 storage change lacks evidence from the data and is speculative. I suggest softening the tone.
L86: Revise the statement to clarify that the Pa/Th data 'show no significant variability within HS1' is partly due to material resolution, to justify the aim of this study.
L216: Regarding glacial Atlantic water masses, I question the use of 'GNADW' as the major body. Previous studies (e.g., Lippold2012NatGeo, Yu2019NatGeo) and models (e.g., Hain2010GBC, Du2022GRL) align on the narrative of shoaling of interglacial NADW to GNAIW. Ref31, based on Nd isotope, requires caution, as several processes can influence these signals.
Fig 4: Fig 4a shows two ventilation peaks (B-P age minima) during Late HS1, but these do not align well with Fig 4e's Pa/Th record. Pa/Th variation is small during HS1, while B-P ages vary greatly. While the authors attribute this to Pa/Th sample resolution, short-lived regional processes may also play a role. Can the authors check whether the B-P age decrease is driven more by B or P? If B, the discussion may be valid; if P, the interpretation is more complex.
L338 and onward: The summary of processes influencing deglacial pCO2 change is improved, but the term "flushing processes" is unclear. What does flushing mean, and does increased advection lead to more CO2 release?
L368 and onward: The paragraph on B-P age and short-term deglacial pCO2 release includes confusing or potentially erroneous statements. For instance, how does intensified AMOC reduce upwelling? If AMOC or NADW production increases, upwelling should strengthen to close the overturning system. Additionally, the claim that lower B-P age (stronger AMOC) leads to Southern Ocean SST drop due to constrained meridional heat transport (citing ref13) is problematic. Ref13 (via Shakun2012Nat) shows a negative North-minus-South SST due to the bipolar seesaw. However, new records (e.g., Dai2022NatComm) show SST increases at ~15.4 or 16.3 ka, suggesting the solubility pump plays a major role, conflicting with this study's conclusion. I suggest reviewing

new Southern Ocean SST records and refining the discussion on drivers of short-term pCO2 change.
Section 3.4: The revised model description is much improved, with Figs S7 and S8 showing good model-data consistency. The 5-degree resolution model captures deglacial C14 changes, despite neglecting biological C14 processes and surface-disequilibrium-driven C14 changes. How much might these processes, especially surface disequilibrium, impact results on short timescales?
Model-Results Mismatch: The model setup seems similar to the PaleoMist_full experiment in ref65, yet AMOC strength differs (e.g., nearly 0 Sv in this study at HS1 minimum vs. ~10 Sv in ref65). This may stem from: 1) the longer simulation (>70 kyr) in this study, or 2) using only insolation, GHG, and ice sheets as forcing, neglecting FWF. If FWF was excluded, what caused the AMOC weakening at HS1?
Fig 1b: The low variability of B-P age and AMOC strength during HS1 is surprising, given clear long-term B-P age fluctuations and milder AMOC variability elsewhere. The authors attribute this to model architecture, but I suspect the model may be less sensitive when AMOC is weaker. This lack of variability hinders mechanistic understanding of the B-P data during HS1.
Fig 3: I suggest adding in-plot legends/annotations to clarify the meaning of 'BPr'/'BPm'/'AMOCm', as these terms are not commonly used.

During this round of revision, the authors have addressed most of my concerns. The section on the AMOC-CO2 relationship is now sufficiently clear and coherent, although, as the authors acknowledge, a more accurate or realistic investigation would require the use of biogeochemical models. I have only a few minor and specific comments and suggestions.

L102-103: The meaning of this sentence is unclear to me. As the biological and solubility pumps act to draw down atmospheric CO2, a strengthening of these pumps should reduce atmospheric CO2 levels. In particular, ref20 indicates that the effects of the biological and solubility pumps differ during the later phase of HS1, with the solubility pump playing the dominant role.

L114: 1367 m

L216: From Figure 3, BPm aligns closely with AMOCm, likely due to the relationship between ventilation and water mass formation in the North Atlantic. Upon closer inspection of BPr and BPm, however, they appear more consistent during HS1 than during other periods, particularly the BA or Holocene. This pattern aligns with points raised by Reviewers #2 and #3, namely that B-P ages perform better under a weakened AMOC mode. I suggest addressing this observation here.

Furthermore, the authors state that the AMOC primarily controls B-P ages, even though they earlier note that several other factors may influence B-age (L202 and onward). While I agree that the AMOC exerts the strongest control, the claim that "AMOC was the primary factor ..." should be qualified to account for potential contributions from other factors. Specifically, if I understand correctly, factors (ii) and (iii) could, in principle, be diagnosed from the model results.

L357: and -> and/or

L347 and onward: The narrative concerning the AMOC-CO2 relationship has been substantially improved. The proposed storage of excess carbon at intermediate depths in the Atlantic is supported by several B/Ca proxy records (indicators of carbonate ion concentration). An additional relevant point is the Pacific-Atlantic carbon seesaw, which may have facilitated carbon transfer between basins during HS1 (e.g., Menviel et al., 2012, 2014).

L492 and onward: The description of the simulation methodology has been considerably improved. The model verification presented in Figures S7 and S8 is satisfactory. I suggest adding a plot of the meltwater discharge into the North Atlantic to provide readers with a clear indication of the freshwater forcing magnitude applied (e.g., similar to Figure 1 in He, 2013, Nature, or Figure 1 in Menviel et al., 2018).