We thank the reviewer for their time and effort, and the constructive comments, which helped to improve our manuscript.
Below are our detailed point-by-point replies and suggested manuscript improvements (blue) for each comment (black).

Minor Comments:

Page 2:

l.3-4: This is the first time future AMOC stability is mentioned. It may be worthwhile to add a few sentences linking past and future AMOC stability.

We agree that the mention of future AMOC stability at this point is not well-connected to the rest of our manuscript. Since our study is only concerned with AMOC stability at pre-industrial and colder temperatures, we remove the sentences on future AMOC stability.

l.45: It would also be helpful to provide a bit more context on thermal thresholds. The previous two paragraphs mostly talk about the haline part (i.e. surface freshwater input and salinity redistribution). Which models have been used to analyse thermal AMOC thresholds and for which climate states? And could you comment on whether the AMOC in intermediate complexity models tends to be more or less or similarly stable as in fully coupled earth system models (e.g. the AMOC in ocean-only models is known to be more prone to instabilities than in coupled GCMs).

We add more information about the models used in studies investigating thermal forcings on AMOC, and provide more references. Further, we now explicitly mention that bistability of AMOC under thermal forcing has been observed in both coupled and uncoupled GCMs. The updated paragraph in the introduction will read:

"Such possible circulation state shifts were first identified in box models (Stommel 1961) and confirmed in intermediate complexity models and global circulation models (Jackson and Wood, 2018, review in Jackson et al, 2023). Systematic testing of AMOC stability is done more easily in lower complexity models than General Circulation Models (GCMs), but the existence of multiple AMOC equilibria seems to be determined by the model-dependent existence and strength of feedbacks, with more complex models including more feedbacks that might change AMOC stability (Weijer et al., 2019)"

"Besides salinity changes, numerical experiments with GCMs also show that the vertical temperature profile affects AMOC stability (Haskins et al., 2020). Short-term AMOC weakening in response to warming has been simulated by a wide range of GCMs (e.g. Mikolajewicz et al., 1990, Gregory et al., 2005, Weijer et al., 2020). Thermal forcing of the North Atlantic has also been found to cause longer term gradual changes in AMOC strength in intermediate and higher resolution models (Knorr and Lohmann, 2007, Zhang et al., 2017, Galbraith and Lavergne, 2019). In addition, bistability of AMOC under thermal forcing has been found in uncoupled and coupled GCMs (Oka et al., 2012, Klockmann et al., 2018), and thermal forcing, especially of the Southern Ocean, can cause abrupt AMOC state transitions similar to freshwater hosing in the North Atlantic (Oka et al., 2021, Sherriff-Tadano et al., 2023)."

Page 4:

l.16-17: Can you briefly explain why it is a useful approximation to use the LR04 stack as a scaling for the dust radiative forcing?

We now mention the close correlation of reconstructed dust fluxes with ice volume and provide a reference in the revised manuscript. The new text reads:

"The LR04 stack was chosen because it is the only complete record with constant temporal resolution over the simulated period. In our experiments, we applied spatially-uniform radiative forcings, to account for uncertain atmospheric optical depth changes due to changes in aerosols and dust, in addition to the better constrained temperature changes due to orbital changes and greenhouse gases, hence termed dust forcing. The scale of this forcing varies between the simulations. The maximum radiative dust forcing, defined to occur at the LGM, is a free parameter, ranging from 0 to -8 W/m2 relative to PI (Simulations A.0 to A.8). To construct the forcing, we scaled the maximum forcing linearly with the smoothed LR04 stack, given the close correlation of reconstructed dust fluxes and ice volume likely due to the dominant role of wind field, sea level and hydrological cycle on dust fluxes (Winckler et al., 2008)"

Page6:

Fig1. Could you also show the combined radiative forcing of all three forcings? That would make it easier to identify periods of changing radiative forcing.

We add the combination of our dust forcing and the radiative forcing from greenhouse gases to Fig. 1. The orbital forcing does not cause substantial variations of the global radiation balance but rather the spatial and seasonal distribution of insolation. Hence, we prefer to keep showing the insolation changes at 65°N separately as an indication of how high-latitude radiative forcing evolved over the last glacial cycle.

Page7:

l.24-26: How do you assess stability here?

AMOC stability is a key concept for our study. However, we have not objectively defined it here, as the varying boundary conditions make it difficult to define an objective criterion that identifies the AMOC stability correctly in both full interglacial and glacial states. Instead, we chose to refer to 'stable modes', which are modes that are occupied by the AMOC most often, as diagnosed from Fig. 2. We agree that this is misleading and remove mentioning of stability from this paragraph, writing instead about the frequency of occurrence.

Page8:

Fig.3 could you rotate the maps in the upper panels by 45°, so that the perspective on the North Atlantic becomes more easily comparable to the lower panels?

We change the maps to focus on the North Atlantic region specifically.

l.22 and 26: Do you show stratification? The lower panles of Fig.3 only show surface density changes. Would it make sense to show stratification? Or do you infer increased stratification simply because of the lighter surface waters?

We change the figure to include panels that show the density difference across the upper 1000 m of the water column as a metric for stratification.

Page 9:

l.5-10: I found this paragraph difficult to read and follow. If none of the differences is statistically significant, would it not be sufficient to report that MBT has no statistically significant effect on the AMOC response?

We shorten this paragraph by removing details about the differences, only mentioning their non-significance, as suggested by the reviewer.

Page 11:

l.16-19: Does this refer to Fig.5 d? And in general: more specific references to Fig.5 could be made though out this page, to make it easier to follow. It is not always clear whether the text on this page refers to Fig.5, some other Figure or to results not shown.

We reference figures more explicitly for the description of the processes.

l.20-21: Can you name the two processes and timescales explicitly? I guess they are N.Atl. freshwater changes (fast) and AABW propagation (slow), but it would be good to have them spelled out.

Yes, these were the processes we referred to. However, changes in the North Atlantic are more relevant for the observed AMOC changes, AABW propagation seems to have more of a stabilising rather than destabilising effect. We remove this sentence and instead discuss changes in the North Atlantic and Southern Ocean separately.

Page 12:

l.20-24: I do not really see the further reduction in NADW export. To me, the distributions of all three water masses look almost identical at 23 and 24.5 kyr. Also, I do not really see NADW replacing AAIW, the upper NADW boundary does not seem to change and if anything, the southward extent of NADW also decreases.

We agree, our descriptions here were not accurate. We clarify this as follows:

"The first abrupt shift in AMOC strength at 24.5 kyr in B.slow has only small effects on the water mass distribution. It mainly leads to a reduced concentration of NADW at intermediate depths of the North Atlantic >45°N and a small increase of AABW concentration in the abyssal North Atlantic (Fig. 5d)."

Page 13:

l.25: What about the strong variance at 6kyr?

The strong variance at 6 kyr is associated with density changes in the North Atlantic. However, the AMOC appears to not undergo a state transition during this time. We add a description of this to the discussion of simulation B.slow to section 3.2 as follows:

"Initially, the whole Atlantic surface ocean cools and freshens, leaving the meridional temperature and salinity gradients almost unchanged (Fig 5e). However, NADW becomes less salty and colder as a consequence of the changes in the surface ocean (not shown) and the vertical density profiles in the subpolar North Atlantic steepen due to the temperature and salinity changes (Fig. SI.7-8). After about 6 kyr, the changes in the North Atlantic density profile result in shifts in the spatial pattern of NADW formation. NADW formation moves south as vertical density profiles in the subpolar east North Atlantic stabilise under a freshening of the surface and density profiles further south steepen due to surface cooling combined with subsurface warming (Fig. SI.7-9). These changes do not cause a step-change in AMOC strength,but freshwater and heat advection into the North Atlantic is reduced, sea ice expansion increases in the eastern North Atlantic and AMOC variance (calculated over a moving 50-year window) is increased (Fig. 5). Transport of heat and salinity into the North Atlantic decreases (Fig. 5f, g) and North Atlantic SST and SSS decrease (Fig. 5e). Reduced influx of subtropical surface waters also causes sudden cooling and freshening in the Irminger Sea (Fig. SI.8)."

Page 14:

l.32-40: Is this part meant in contrast to other models? The last sentence is also almost impossible to follow. Please consider a clearer formulation.

We rewrite this paragraph for a clearer discussion. The new text in section 3.3 is:

"In a model with a dynamic energy moisture balance component, atmospheric cooling reduces evaporation and the water-holding capacity. With this feedback enabled, cooling can then affect seawater density directly via changing temperatures, and indirectly via changing the meteoric freshwater balance and surface salinities. These changes would induce additional kinematic changes (i.e., in the wind fields) in fully dynamic atmosphere models, but are kept constant in our simulations, i.e. the moisture content of air changes with climate but not the direction or strength of winds which disperse it. In our model, a decrease in the water-holding capacity of air therefore directly leads to a reduction of the large scale atmospheric moisture transport from low to high latitudes. Accordingly, wind stress fields are also kept constant here. Changes in wind stress have been documented to exert important controls on AMOC stability (e.g. Arzel et al., 2008, Yang et al., 2016) and thermal thresholds (Oka et al., 2012). These effects have been investigated in detail with the Bern3D model by Pöppelmeier et al. (2021) focusing on LGM boundary conditions.

Page 16/17:

Meta stable AMOC modes: How are the excitable/metastable states defined? By increased AMOC variance as in Fig.5? How do the metastable states relate to the four AMOC states I-IV from the beginning? Also, please consider adding the corresponding kyrs behind MIS3/4/5e etc, so that it is easier to identify the right parts of Fig.9 for those readers who do not have those numbers at the top of their heads.

We define excitable states as times when AMOC adopts intermediate modes II and III, which show more frequent AMOC strength shifts than the interglacial and glacial modes I and IV, respectively. We also add the requested age information. The new text in section 3.4 is:

"Finally, we can test whether our simulations capture the periods with increased frequency of AMOC transitions that are indicated by proxies over the last eight glacial cycles. Using our 788 kyr long simulations in simulation set A, we determined when the radiative forcing pushed the AMOC into 'excitable' circulation modes, i.e. modes II and III, which show more frequent AMOC strength shifts than the interglacial and glacial modes I and IV (Fig. 1 and SI.2), and how this varied with the applied forcing strength (Fig. 9)."

Page 18:

l.24-27: This would be very interesting indeed. I look forward to the follow-up :)

We too!

Technical/Editorial Comments:

Page 1

l.34: delete "boundary" after "Atlantic"

Deleted.

l.36-42: Very long and hard to read sentence. Consider reformulating for better readability. Also: which climate is being referred to at the end of the sentence? Probably North Pacific climate but it is not immediately clear.

We simplify and clarify this sentence as follows:

"It also affects global climate by shifting the Intertropical Convergence Zone (ITCZ) and monsoon systems (Wang et al., 2001, Bozbiyik et al, 2011), and interacting with the regional climate and deep water formation in the North Pacific (Okazaki et al., 2010, Menviel et al., 2012, Praetorius and Mix, 2014)."

l.43-47: same as comment above. Also: Does the last half sentence ("and by modulating atmospheric greenhouse gas concentrations") still correctly belong to the beginning of the sentence ("It influences deep ocean nutrient and oxygen concentrations")?

We rewrite this sentence and clarify the role for greenhouse gas concentrations. The new sentences read:

"The AMOC furthermore shapes biological surface productivity by regulating nutrient supply to the surface ocean in the Atlantic and Pacific (Tetard et al., 2017, Joos et al., 2017). On its southward path in the Atlantic, it influences deep ocean nutrient, carbon, and oxygen concentrations (Broecker, 1991). By affecting primary production and deep ocean carbon storage, AMOC changes also modulate atmospheric greenhouse gas concentrations (e.g. Menviel et al., 2008)."

Page 2:

l.2: "which had regional [...]" instead of "and had regional [...]"

We amend this according to the reviewer's suggestion.

Page 4:

l.13: What is meant with "rest of the past 800kyr"? Rest with respect to what? The spin up state?

This is a leftover from an amended sentence of a previous manuscript version. We remove "rest of the".

Page 10:

Fig.5: Please increase the font size for better readability.

We increase the font size as suggested.

Page 12:

Fig.6: Please increase the font size for better readability.
We increase the font size as suggested.

Page 14:

l.23: The name of the ocean model is COCO (the ocean component of MIROC)

We amend the sentence accordingly.

Page 15:

Fig.7: Please increase the font size for better readability.

We increase the font size as suggested.

Page 16:

l.28: wrong Figure reference? Should be Figure 1?

That is correct, we change the figure reference accordingly to Fig. 1 in the revised manuscript.

Page 18:

l.14: delete “but”

Deleted.

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