Author response for egusphere-2025-2326-RC2

Fernanda DI Alzira Oliveira Matos, Dmitry Sidorenko, Xiaoxu Shi, Lars Ackermann, Janini Pereira, Gerrit Lohmann, Christian Stepanek

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Letter to RC2

Dear Reviewer,

We deeply appreciate your comments and suggestions to our manuscript that greatly helped us to improve the level of detail required for the understanding of our methodology and results. Please find below our responses to your comments.

Sincerely,

Fernanda D. A. O. Matos

(On behalf of the authors)

Response to RC2's major comments

1. Overselling how prominent z-AMOC diagnostics are. **The main motivation for this study is, as stated by the authors, that "The majority of AMOC estimates is provided in depth space". While they cite Sidorenko et al. (2021) here, that study does not actually provide any quantitative evidence in support of this claim. Instead, that study shows the difference between depth and density-AMOC in a single model. Their introduction cites a few papers that use depth-space analysis, but it is nowhere near the kind of exhaustive review you would need to make this statement. While the Foukal and Chafik (2024) paper is focused squarely on this question, they are also vague and qualitative. From where I sit in the field, z-AMOC is already well known to be a flawed diagnostic and anyone serious is already using ρ-AMOC. Like Foukal and Chafik (2024), this paper concludes by advocating "for including ρ-AMOC model output in studies focusing on warmer climates, and observational diagnostics". This does not recognize that the community is already doing this. OSNAP outputs their streamfunction in potential density coordinates and msftyrho is**

a CMOR variable that is already contributed to CMIP archives (although not as frequently as msftmz). I suggest the authors follow one of the two paths to address this, in addition to providing more context: either come up with a more rigorous estimate of how prominent z-AMOC is vs. rho-AMOV or else soften all of your language about how prominent z-AMOC is.

#Response#: We acknowledge the validity of the reviewer's observation and concede that the original phrasing regarding the community's reliance on z-AMOC was overstated and too general. Our study primarily focuses on comparisons between our findings and AOGCM/CMIP models, where the depth-coordinate streamfunction (e.g., msftyz/msftmz), as you observed, remains the standard and most frequently utilized AMOC diagnostic. While the density-coordinate streamfunction (msftyrho/msftmrho) was defined in the CMIP6 request (Griffies et al., 2016), it is, as you observed, archived less consistently and its presence is model- and experiment-dependent. This prevents the usage of $\rho-$AMOC output as extensively for research and model-intercomparisons as is possible for the z-AMOC output. For example, in Baker et al. (2025), only 5 of the 35 models featured in the article had ρ-AMOC output for the *abrupt-4xCO2* experiment. More generally, we also find evidence for less wide-spread production and usage of ρ-AMOC vs. z-AMOC from CMIP6-archive download statistics. For example, while Omon.msftmz is available for 39 models and has download rank 240[1] (i.e., 239 other variables were downloaded more often than Omon.msftmz), Omon.msftmrho is available only for 8 models and has been downloaded much less often (download rank 1159[2]). We do not believe that this disparity reflects a lack of community interest, and is likely attributable to the additional computational cost associated with deriving ρ-AMOC output. Consequently, a complete transition to ρ-AMOC in model intercomparisons and in research remains an ongoing challenge, especially within large model ensembles and for studies that require long integration periods and/or high resolution. We believe that the mentioned computational cost explains, at least partially, the continued use of z-AMOC in AOGCM studies, even as many researchers prefer ρ-AMOC when feasible, and when high-frequency output can be better resolved, as suggested in Megann (2024). Furthermore, based on our experience, the representation of AMOC in depth space remains widely applied even when models provide output in density space, owing to the community's familiarity with this representation.

While we believe that there is some merit in repeatedly highlighting the continued use of z-AMOC in many fields of climate research, we have refined and softened our language throughout the revised manuscript to emphasize that our statements regarding the application of z-AMOC stem mostly from coupled-model-based assessments from the CMIP and related model-intercomparison efforts. We now also recognize in our manuscript the strong momentum towards wider acceptance of the density-space based diagnostics in recent work, highlighting as examples the OSNAP and RAPID arrays (Frajka-Williams et al., 2019), and several modeling studies, such as Jackson and Petit (2023), Fu et al. (2023), van Westen et al. (2025). Such acknowledgment, along with a reference to Griffies et al. (2016) for a technical perspective on the CMIP6 data request, has been incorporated into the introduction and discussion sections.

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[1] `https://airtable.com/app0cSa4gXyzHThmm/shrkayK0bes58Zu45/tblpo5L8maBIGlM1B/viwNNzrqK5oPL7zk2/recJ5HHT1QhjvpmvJ`, accessed Aug 7, 2025

[2] `https://airtable.com/app0cSa4gXyzHThmm/shrkayK0bes58Zu45/tblpo5L8maBIGlM1B/viwNNzrqK5oPL7zk2/recJ0Oyp8BBdOE7sB`, accessed Aug 7, 2025

2. The authors employ a different definition for ρ-AMOC than most. **The conventional way of diagnosing ρ-AMOC is by integrating meridional velocities (binned in density coordinates, as in msftyrho), not by integrating diapycnal velocities. Additionally, the authors do not explain how they diagnose their diapycnal velocities, which is non-trivial in models with a Lagrangian vertical coordinate. In fact, what they call the diapycnal velocity (following Sidorenko 2020) is different from what other people call the diapycnal velocity, because it is the Eulerian part of the diapycnal velocity that does not account for movement of isopycnal surfaces is time (Marshall 1999, Ferrari 2016). I recommend a clearer terminology and notation, perhaps reconciling yours with recent broader reviews on Water Mass Transformation methods (Groeskamp 2019, Drake 2025) that are not AMOC-specific. This is an important issue because the authors are advocating for more widespread adoption of these diagnostics but are advocating for different diagnostics than those used by most others.**

#Response#: We appreciate the reviewer's insightful comment, and want to emphasize the robust equivalence of diagnosing MOC using vertical velocities in comparison to the more conventional usage of meridional (horizontal) velocities. Therefore, we expand below on three main aspects:

1. **Equivalence of the two approaches.** Using vertical velocities to compute MOC offers a computationally sound and completely equivalent alternative to integrating binned horizontal velocities. Such equivalence is demonstrated in Sidorenko et al. (2020a,b), wherein the authors explained that binning of horizontal divergence into density classes is done using instantaneous isopycnals, enabling diapycnal velocity calculation after removing the mean drift of isopycnals. The latter is negligibly small in our simulations. Because binning is done with respect to instantaneous isopycnals our AMOC diagnostics are equivalent to those derived using the horizontal velocities. We furthermore note that the use of vertical velocities, instead of horizontal velocities, is more a necessity than a deliberate choice due to the structure of the FESOM2 (see below).
2. **Vertical coordinate in FESOM2.** We apologize for any confusion: FESOM2 does not use Lagrangian vertical coordinate. We implement the Arbitrary Lagrangian-Eulerian (ALE) scheme in a finite volume sense (see lines 80-82 of the original manuscript and Scholz et al. (2019) for more information).
3. **Concern using horizontal velocities.** While computing ρ-AMOC using horizontal velocities is feasible and in principal equivalent to MOC computation with vertical velocities, this approach on an unstructured grid, such as the FESOM2 mesh we employ requires careful and non-trivial "broken-line" integration along control-volume boundaries (Sidorenko et al., 2020b) following the discretization of the continuity equation. Furthermore, doing so is less advantageous as it conceals critical information concerning diapycnal velocities. Additionally, diagnosing MOC in density space using vertical velocities has proven more efficient for online diagnostics on the FESOM2 unstructured mesh (Sidorenko et al., 2020a).

We hope this clarifies both the theoretical equivalence and the practical motivations for our chosen diagnostic. We did not, however, include this discussion in our manuscript, for brevity and because it is not within the scope of our study. In the revised manuscript, we included the equation to diagnose MOC using horizontal velocities in the appendix and include the following text to section 2.2:

"In this study, the AMOC is diagnosed using vertical velocities, as opposed to the conventional application of meridional velocities (equations using meridional velocities are detailed in the Appendix). This methodology, while mathematically equivalent, presents notable benefits, especially concerning the unstructured FESOM2 mesh, as it directly incorporates diapycnal velocities, and facilitates more efficient online diagnostics (Sidorenko et al., 2020a)."

3. I am not convinced that the maximum ρ-AMOC is a meaningful metric. **While the authors have indeed shown that the maximum z-AMOC and ρ-AMOC are very different, a large fraction of this difference is due to the strong recirculation cell in ρ-AMOC. This needs to be explained much more clearly. How should we think about what this means, conceptually or mechanistically? Is the formation part of this recirculation cell mixing via deep convection or via interior entrainment in overflows, for example? Why is this cell largely closed by diapycnal upwelling between 20ºN and 50ºN? Is this a region with strong interior mixing? If the point is to have a metric for the global-scale AMOC, wouldn't the transport that actually makes it out of the North Atlantic be a better metric of the circulation than something that largely reflects a local overturning cell?**

#Response#: We acknowledge the reviewer's concern regarding the distinction between the AMOC_{max} index and the net export across the North Atlantic basin. Nevertheless, we argue that in our manuscript, the index is a valuable metric for emphasizing the importance of the AMOC in density space, as opposed to depth space. Perhaps, the definitions of surface-forced diapycnal and interior-mixing-induced water mass transformations, as well as interior mixing, may have caused confusion.

Figure 4 of the manuscript presents the mean surface and interior transformations, which, when summed together with the model drift (that is deemed negligible and therefore not considered in the manuscript), yield manuscript Figure 1, panels a and b. Specifically, for pre-industrial (PI) conditions, between $20 - 50°$N, diapycnal upwelling is confined to density classes ranging from $33\,\mathrm{kg\,m^{-3}}$ to $36\,\mathrm{kg\,m^{-3}}$, where water mass transformations are predominantly surface-forced. Consequently, this recirculation cell is primarily driven by surface buoyancy fluxes confined to the surface mixed layer depth, rather than interior entrainment of overflows, which are incorporated within the interior-mixing-induced water transformation component of the AMOC in density space. For clarification:

- Surface transformations, denoted as ψ_S in Figure 4, represent the component of the AMOC in density space that is forced by buoyancy fluxes confined to the surface mixed layer depth.
- Interior transformations, denoted as ψ_I in Figure 4, represent the component of the AMOC in density space that is forced by mixing with water masses that are advected from other regions or modified through thermobaricity and cabbeling.

Both surface and interior transformations can contribute to the formation of denser waters (positive recirculation cell represented in red) and to the formation of lighter waters (negative recirculation cell represented in blue). Given that the aforementioned recirculation cell is confined to a lighter density class than the maximum ρ-AMOC as defined by the index, it does not affect the metric, and consequently, this subtropical cell (associated with the Subtropical Mode Water formation) is not extensively addressed in the manuscript.

In our PI simulation, the maximum ρ-AMOC, represented by the recirculation cell confined between $40 - 60°$N and $36.20\,\mathrm{kg\,m^{-3}}$ to $36.90\,\mathrm{kg\,m^{-3}}$, is formed partially by surface buoyancy fluxes, but is primarily caused by interior transformations (including interior entrainment of overflows), as clear through comparing figures 4a and b. Under 4xCO_2 conditions, interior transformations act towards lighter waters, thereby weakening the positive recirculation cell in Figure 1. Overall, Figure 1a represents the sum of Figures 4a and 4b, while Figure 1b represents the sum of Figures 4c and 4d. As the changes in the ρ-$AMOC_{max}$ index capture this significant shift in the driving mechanisms of AMOC strength (surface versus interior transformations), we maintain that it is a meaningful metric for the global-scale AMOC.

4. Vertical velocity is not a "mechanism", it is just the variable that feeds into the z-AMOC diagnostic. **I think referring to it as a mechanism actually weakens your argument. You should more forcefully emphasize that there is no mechanistic framework to quantitatively explain what causes the vertical velocities that feed the z-AMOC, whereas diapycnal transformations do provide a mechanism to understand the drivers of the ρ-AMOC.**

#Response#: We appreciate the reviewer's feedback. To address this issue we modified the sentences where we refer to vertical velocity as a mechanism.

5. The Figure with the ρ-AMOC remapped into depth space should feature in the main text (e.g. as another column in Figure 1, although I would probably then swap the columns and rows). **Additionally, you should add a little more explanation of what this means in the caption. Presumably you compute the zonal-mean depth of each isopycnal at every latitude. This has become a very standard way of displaying the ρ-AMOC and facilitates direct comparisons with the z-AMOC.**

#Response#: We thank the reviewer for raising this point. We have updated Figure 1 to include the ρ-AMOC remapped into depth space and updated the figure caption:

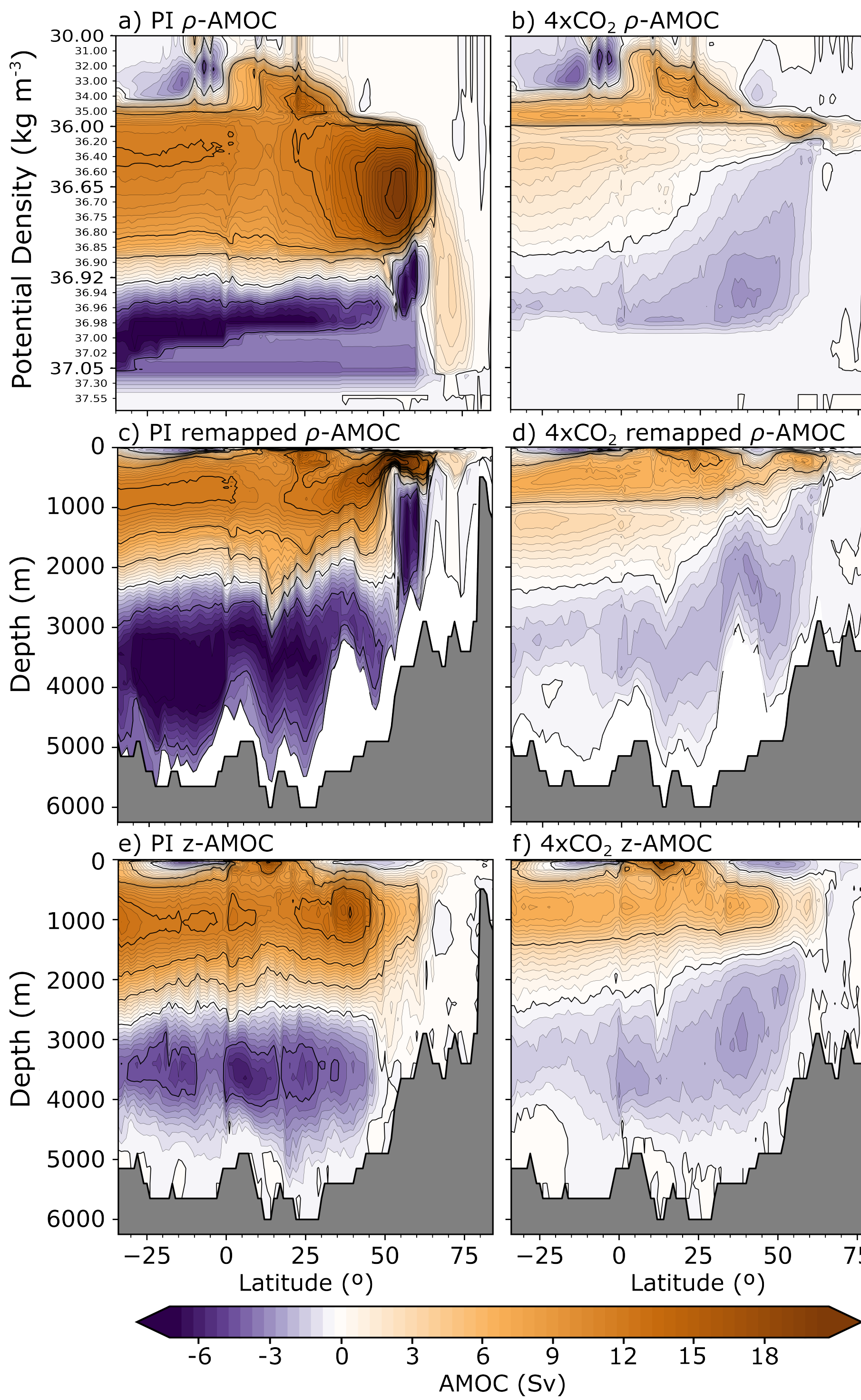

Figure 1: Mean AMOC strength in units of Sverdrup ($1\,\mathrm{Sv} \equiv 1 \times 10^6\,\mathrm{m}^3\,\mathrm{s}^{-1}$) averaged over the last 50 years of the PI and $4xCO_2$ simulations. Panels show (a, b) ρ-AMOC, (c,d) ρ-AMOC remapped into depth coordinates, and (e, f) z-AMOC for (a, c, e) PI and (b, d, e) $4xCO_2$. In (c,d), ρ-AMOC is remapped into depth coordinates by loading the mean layer thickness of each density class, cumulatively summing these thicknesses to obtain the bottom depth of each class, then shifting the cumulative sum down one index (with the surface layer reset to zero) so that each transport bin appears at the depth of its upper boundary.

6. Some of the discussion of the water mass transformations is more confusing that it is clarifying (see specific comments below).

#Response#: We appreciate the comment and have revised the text to clarify the specific comments drawn on section 4 and improve the overall description of our results.

Response to RC2's minor comments

Section 1

L. 33- "at approximately 1000 meters" is misleading, since that is where the streamfunction reaches its maximum, not the northward transport.
#Response#: We thank the reviewer for pointing out the ambiguous sentence. To avoid confusion, we have adjusted our wording accordingly by modifying the sentence to:
"The maximum AMOC overturning strength occurs within its mid-depth cell, centered around 1000 meters below the ocean surface and between $30°$ *to* $65°N$*."*

Section 3

L. 153-155- What do you mean by "The indices are then further adjusted in density and depth spaces as well in PI and 4xCO2 to capture only the AMOC strength of the upper cell?" Shouldn't you have a generalizable metric that doesn't require manual adjustment in a different climate?
#Response#: With this sentence, we mean that we isolate the upper cell (confined, in PI, between $40 - 60°$N and $36.20\,\mathrm{kg\,m^{-3}}$ to $36.90\,\mathrm{kg\,m^{-3}}$) according to their minimum and maximum density (depth) during simulation runtime to avoid including any spurious or shallow/tropical/GIN seas recirculation within our derived indices. Our cell-by-cell approach, contrary to a more generalized metric, clearly separates the upper and lower cells under their different timescales of response, including the potential shoaling and poleward shift in these cells and a strengthening of the subtropical cell under warmer climates.

Upon careful analysis of the time evolution of the upper cell against vertical coordinates we defined the AMOC indices in a way that strictly captures the maximum overturning of the upper cell, that relates solely to the Labrador Seawater formation in the Labrador and Irminger Seas. Figure 2 clearly illustrates that not defining the upper and lower limits would in simulation in 4xCO_2 capture streamfunction values at lighter density classes than those of the upper cell.

We observe that AMOC indices defined in different studies vary, like using the maximum streamfunction at depth excluding the first 500 meters, or just considering a specific latitude for the AMOC indices like the maximum streamfunction between $40 - 60°$N (Sidorenko et al., 2020a), or at $26°$N and other fixed latitudes (Frajka-Williams et al., 2019), for example. All of these metrics were defined according to the scope of the specific study, and we have similarly tailored our analysis towards the scope of our study.

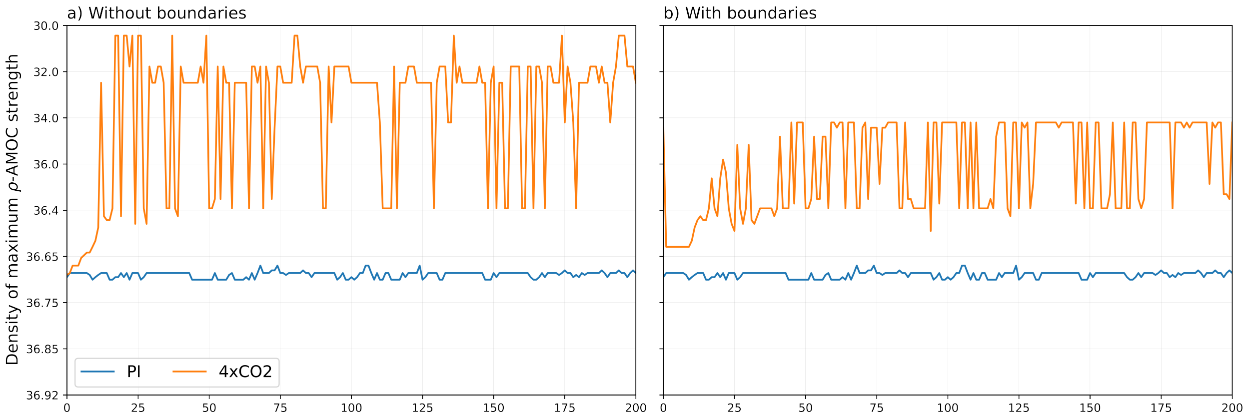

Figure 2: Density and depth of maximum AMOC at $30-65°$N with (right) and without (left) upper and lower boundaries.

L. 168- Okay, but why doesn't this also apply to 4xCO2? Do isopycnals become more titled with climate change?

#Response#: One might expect that enhanced tilt of isopycnals under $4xCO_2$ would cause the different magnitude and variability in ρ- and z-AMOC at 26°N, but Figure 3 shows virtually identical σ_2 contours in the subtropics for PI and $4xCO_2$.

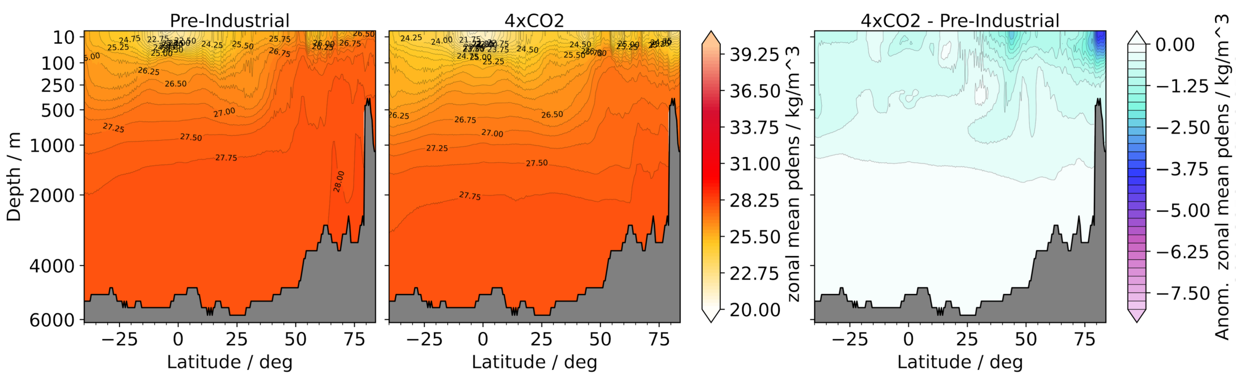

Figure 3: Potential density in $\mathrm{kg\,m^{-3}}$ in the last 50 years of simulation for PI (left panel), $4xCO_2$ (middle panel), and the anomaly between PI and $4xCO_2$ experiments (right panel).

Instead, the divergence between ρ- and z-AMOC in $4xCO_2$ arises from a change in interior transformations (Fig 4): whereas in PI both surface and interior transformations act towards denser water mass formations, in $4xCO_2$ there are competing signals of interior transformation towards lighter waters below transformations towards denser waters within the AMOC upper cell ($\sigma_2 = 35\,\mathrm{kg\,m^{-3}}$ to $36.4\,\mathrm{kg\,m^{-3}}$). Because the ρ-AMOC is the sum of surface and interior transformations, this net lightening weakens the return flow at 26°N, driving ρ-AMOC variability exhibited in Figure 2 and preventing it from exhibiting the same recovery behavior as z-AMOC. These dynamics highlight the importance of diagnosing AMOC in depth space, even when one focuses only on the AMOC at 26°N.

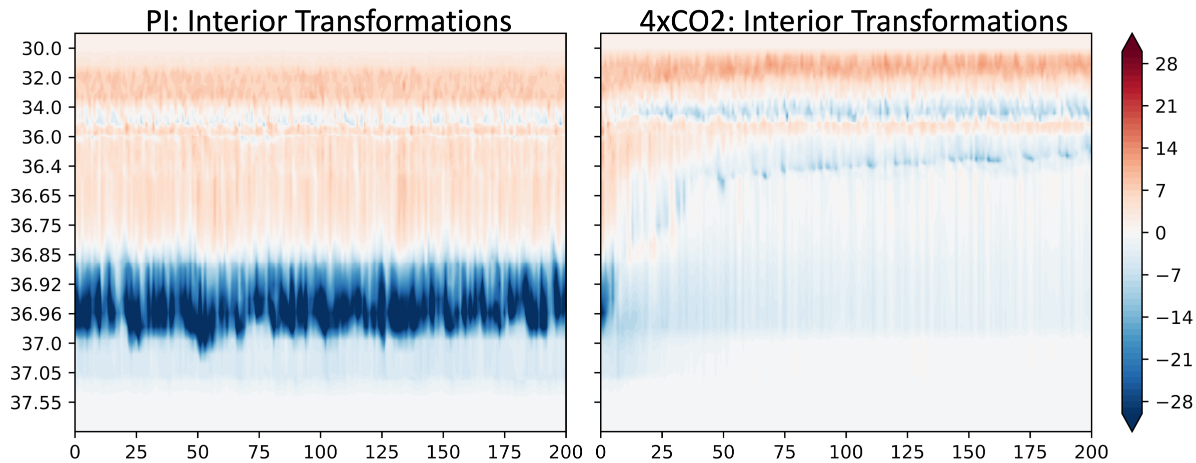

Figure 4: Time evolution of interior transformations for PI (left) and 4xCO_2 (right).

Section 4

L. 258- What do you mean by "interior mixing alone becomes insufficient to sustain deep convection"? Why should we think about interior mixing sustaining deep convection in the first place? Isn't part of the deep mixing is the model *caused* by convection, i.e. unstable density profile triggers some kind of deep convection mixing scheme?

#Response#: We apologise for the confusion caused by our terminology. The correct term is interior transformations, not interior mixing. The usage of the terms interior mixing, interior transformations, and related processes has been reassessed and section 4 has been re-written towards improving the clarity of our results and interpretations.

L. 262- What do you mean by "surface transformations trigger interior mixing"? Is this deep convection?

#Response#: We again apologize for the confusion regarding terminology. As explained in the comment above and in Figure 6 of the manuscript, the surface transformations refer to water mass transformations occurring within the mixed layer that occur due to surface buoyancy fluxes that destabilize the water column and that promote mixing/deep convection. We have re-written section 4 to clarify the terminology, interior mixing vs. interior transformations.

Figure 4 - I think you need to expand on this either in the text or the caption to explain to readers how to read these plots, i.e. they are integrated from the North southwards. A meridional derivative in these quantities corresponds to diapycnal transformation whereas it

being constant means there is no transformation.

#Response#: We apologise for the lack of clear description of Figure 4. As mentioned in our answer to item 6 of the major comments' section, we have extensively re-written Section 4 to clarify terminology and to guide the reader regarding the interpretation of our results.

L. 255-257 and Figure 4d- Are you saying that NADW is lighter than AABW? What is going on in the deep density layers? Is this Mediterranean overflow water that is mixing up at high latitudes? I don't really understand how to think about this.

#Response#: We apologise for the confusion in regarding Figure 4d. This figure illustrates the component of the overturning circulation driven by interior transformations, as detailed in lines 248 and 249 of the original manuscript. Consequently, the panels in Figure 4 do not track specific water masses, but rather depict the mechanisms driving the basin-scale overturning through water mass transformations occurring either within the surface mixed layer or in the ocean interior. As indicated in lines 255-257, the Antarctic Bottom Water (AABW) in 4xCO_2, depicted in Figure 1, occupies lighter density classes with respect to PI due to interior transformations toward lower densities. It should be noted that AABW, as shown in Figure 1, is denser than North Atlantic Deep Water (NADW). As addressed in our response to item 6 of the major comments section, Section 4 has been extensively revised to enhance clarity and facilitate reader comprehension of these figures.

Section 5

L. 307- Be careful here, most of the energy that actually powers the AMOC circulation is mechanically input by Southern Ocean winds or interior turbulent mixing (Wunsch and Ferrari 2004).

#Response#: We appreciate the comment and have revised the sentence to avoid implying direct causality between AMOC strength and diapycnal transformations:

"Additionally, surface and interior water mass transformations play a crucial role in setting AMOC strength and variability. Therefore, accounting for these transformations is essential to advance our understanding of overturning regimes across various scales."

It should be mentioned somewhere that what you call "interior mixing transformation" includes both parameterized physical mixing and spurious numerical mixing (and other residual errors).

#Response#: We thank the reviewer for raising this point. As mentioned before, the correct term that we should use in our manuscript is interior transformations, rather than interior mixing. The usage of the terms interior mixing, interior transformations, and related processes has been reassessed, and Section 4 has been re-written towards improving the clarity of our results. We once more thank the reviewer for drawing our attention to the misapplication of the terms in our manuscript.

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