Author response for egusphere-2025-2326

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August 19, 2025

Letter to the Editor and to the Reviewers

Dear Editor, Reviewer 1, and Reviewer 2,

We are extremely grateful to your comments and suggestions regarding our submission. We have addressed these comments and have reflected the suggestions in a revised version of our manuscript, although not all specific points are individually addressed within this response letter for brevity. The manuscript has undergone substantial revision to provide an improved overview of our methodology, a clearer articulation of our motivation, more descriptive results, and a strengthened perspective on the significance of our contribution to the scientific community upon publication of our manuscript.

Sincerely,

Fernanda D. A. O. Matos

(On behalf of the authors)

Response to the Editor

0.1 EC1's major comments

The article content reads back-to-front to me: having read through it, what I was expecting to see at the front is nested in the middle and the back. The slight result is that (to me at least) it is not entirely clear what the scientific question or motivation is as written relating to the two AMOC diagnostics. In this sense I guess I am biased because I roughly know what the article would be about because I use ρ-AMOC quite often (although I don't claim to be in the "serious people" group mentioned by Henri), but the framing is not entirely helpful for the more general audience in my opinion.

#Response#: We acknowledge that we should have been more clear on our scientific motivation, as well as more careful with structuring our manuscript. We have re-written multiple parts of the manuscript towards successfully transmitting the relevance of our study to the scientific community, and furthermore to address all concerns raised by the reviewers and by you.

The article currently reads like "we did this and we got this", when it could read more "we think this so we did this and we got this", because some of the content to demonstrate the "we think this" part is actually in the middle/end of the article. The fix is then relatively easy: copy/move/anticipate some of the relevant text discussion up to abstract, introduction and/or section 2. This would help re-balance the article, because section 1 could do with a stronger or more concrete problem statement/ hypothesis, and section 2 as the theoretical/ scientific foundation stone for the article is also a bit short.
#Response#: We thank the editor for this comment and would like to add that this study was motivated by our curiosity on the value of diagnosing ρ-AMOC for warmer climates. While existing literature provides substantial insight into ρ-AMOC through observational and modelling studies, a gap exists concerning its applicability in the context of abrupt climate change. Our hypothesis is that a transition from z-AMOC to ρ-AMOC becomes crucial in warmer climate even where nowadays it is deemed irrelevant to use density space, like in the subtropical North Atlantic, where most studies show that ρ- and z-AMOC are quite similar. Our findings definitely corroborate our hypothesis, as the ρ-AMOC at 26°N diverges both in magnitude and variability from z-AMOC in our 4xCO_2 simulation. We also acknowledge that the initial manuscript presentation may have obscured the rationale and significance of this research as well as our motivation. Consequently, the manuscript has been substantially revised to enhance clarity regarding our motivation, the pertinence of results presented, and the rigor of the applied methodology.

I am also of the strong opinion the authors need to stress that z-AMOC and ρ-AMOC are different "diagnostics" and not "AMOCs": there is the model AMOC somehow nested in the diagnosed variables, but there are different representations of it. (cf. a "vector" is the mathematical object, but there are different "representations" of it depending on the choice of basis, and some representations are more useful than others depending on the context.)

Following on from that then

- **The model AMOC is exposed to the same drivers, but this driving is represented differently in the different diagnostics.**
- **The fact they have different distributions and/or magnitudes are not surprising, since they are different diagnostics and measure different things.**

The article text needs a change of tone and some content to reflect that. The results are fine, but it is a little oversold at the moment to me at least, although I think the referees agree. See "minor comments" of where I think text can be changed/ moved/ copied/ anticipated.
#Response#: We appreciate the insights given through these comments. We have revised the manuscript to clarify that we compare different diagnostics of the same circulation, and we now furthermore argue for

the relevance of each diagnostics depending on the scope of one's study.

As mentioned by the other referee, most MOC calculations use meridional velocity v, so why is w used, particularly when it can be noisy and contribute to uncertainties? Please comment accordingly.

#Response#: For reference, we duplicate here our answer to RC2:

"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:

'Please note that, in our study, the AMOC is computed using vertical velocity, w, rather than the conventional approach based on horizontal velocity (v; see Table A1 for the equations). We make this choice because, while both methodologies are mathematically equivalent (see Sidorenko et al., 2020b, for a detailed

comparison), using the meridional velocity would require integration along the boundaries of the control volumes, which is less convenient for arbitrary unstructured meshes, and it would also neglect important information about diapycnal velocities. In contrast, the vertical velocity approach naturally yields the AMOC in density space, reduces noise introduced by the beforementioned integration along the boundaries of the control volumes, and enables more efficient online diagnostics in FESOM2 (Sidorenko et al., 2020a, 2021).'

Additionally, as our response to RC2 implies, due to the structure of FESOM2, using vertical velocities to diagnose MOC actually introduces less noise and uncertainties in our results.

Following on from that and as mentioned by the other referee, how is convection represented in the model? Because if it is explicit then it would manifest as a w, but if it is as an enhanced vertical mixing then would one convert a diffusive flux into an effective velocity, or something else? It is thus not clear what w_ρ actually includes, and is therefore not entirely clear what ψ_σ is measuring, which is kind of important since that definitions of those are the scientific foundations of the present article. Please clarify accordingly.

#Response#: For reference, we duplicate here our answer to RC1:

"In FESOM2.5, the K-profile parameterization (KPP; Large et al., 1994) scheme was employed for vertical mixing. Consequently, convection is parameterized through enhanced vertical mixing rather than explicit advection of water parcels. In practice, when convective conditions arise, the model artificially increases the vertical diffusivity (on the order of 0.01 $m^2\ s^{-1}$) to homogenize the water column. This approach is common in hydrostatic ocean models, which cannot explicitly resolve convective plumes or advective exchange across isopycnals due to the hydrostatic approximation. Fully resolving convective overturning would require a non-hydrostatic model with horizontal grid spacing comparable to the vertical scale ($\sim$ 1 m), an unattainable resolution for global simulations on current supercomputers.

We added the following text into Section 2.1:

'Vertical mixing in FESOM2 is parameterized using the K-Profile Parameterization (KPP; Large et al., 1994) scheme, through which locally enhanced vertical diffusivity, set to 0.01 $m^2\ s^{-1}$, is implemented to represent convection arising from local static instability.'"

At this resolution some sort of GM scheme is used presumably, then is w* included in these calculations (probably in ψ_z because GM is supposed to be adiabatic)? This needs clarifying (if GM is not used then please just say so)

#Response#: We thank the editor for highlighting that we should inform the readers about our scheme for parameterizing mesoscale activity. In this sense, we have included the following sentence in Section 2.1:

"Given that the mesh employed in this study is not eddy-resolving, mesoscale eddy stirring is included via the Gent–McWilliams (GM) parameterization (Gent and McWilliams, 1990), implemented according to the explicit eddy-induced stream-function algorithm of Ferrari et al. (2010), as detailed in Danilov et al. (2017) and evaluated in Scholz et al. (2019)."

The maths presentation in text and in some of the figures is inconsistent and needs fixing, see below.

#Response#: Based on the further comments outlined in this review, we have re-written the Mathematical Framework section, and included more details on definition of the mathematical equations.

EC1's minor comments

Section 1

line 58: Remove comma

#Response#: We appreciate the correction and have removed the comma.

Section 2

line 79: Weird sentence and probably missing the word "unstructured" (because you can't assume people know about details of FESOM). Reword accordingly, e.g. "The unstructured mesh is such that there are approximately 127,000 mesh nodes at the ocean surface." or similar

#Response#: We acknowledge that the description of FESOM2.5 is brief in our manuscript and have extended the model description session to account for more details on all model components.

line 84: 89 density bins seem a bit small in terms of numbers, and are the bin sizes uniform? Normally I do about 160 to 200 and above uniformly spaced (a bit less if I have it unevenly spaced), but I don't to do averaging in density space online unless I am using MITgcm. Any comments on the dependence/sensitivity to the choice of bin numbers.

#Response#: We acknowledge this concern and, in AWI-CM3, the number of bins used can indeed introduce small-scale recirculations in the diagnosed MOC (Sidorenko et al., 2020a,b, 2021). However, we based our decision to use 89 uneven density bins on the assessment provided by Sidorenko et al. (2020b), wherein the authors describe the sensitivity of AMOC representation to the choice of density bins. For brevity, and because we do not believe it is under the scope of this manuscript, we do not to explicitly include this discussion in our manuscript. We have, however, provided the scripts to generate our plots under Matos (2025), where one can see that the chosen density bins are unevenly spaced and are defined as follows:

Density bins: 0.0000, 30.00000, 30.55556, 31.11111, 31.36000, 31.66667, 31.91000, 32.22222, 32.46000, 32.77778, 3.01000, 33.33333, 33.56000, 33.88889, 34.11000, 34.44444, 34.62000, 35.00000, 35.05000, 35.10622, 35.20319, 35.29239, 35.37498, 35.41300, 35.45187, 35.52380, 35.59136, 35.65506, 35.71531, 35.77247, 35.82685, 35.87869, 35.92823, 35.97566, 35.98000, 36.02115, 36.06487, 36.10692, 36.14746, *36.18656, 36.22434, 36.26089, 36.29626, 36.33056, 36.36383, 36.39613, 36.42753, 36.45806, 36.48778, 36.51674, 36.54495, 36.57246, 36.59500, 36.59932, 36.62555, 36.65117, 36.67621, 36.68000, 36.70071, 36.72467, 36.74813, 36.75200, 36.77111, 36.79363, 36.81570, 36.83733, 36.85857, 36.87500, 36.87940, 36.89985, 36.91993, 36.93965, 36.95904, 36.97808, 36.99682, 37.01524, 37.03336, 37.05119, 37.06874, 37.08602, 37.10303, 37.11979, 37.13630, 37.15257, 37.16861, 37.18441, 37.50000, 37.75000, 40.00000*

line 95 + 96: Probably swap colon for a full stop and start a new paragraph as is done already, or follow on straight away (doesn't really matter)
Remove all instances of "Eq. X below:", because this is forward referencing and the arguably that text is redundant anyway (don't really need it)
Eq 1 and 2: Need to be clear that these are cumulative integrals in y and full integrals in x. In that sense the integral limits need to be $\int_{West}^{y}$.
Eq 1 and 2: Why are these flipped from the usual orientation? Would have expected South to North and West to East (which introduces two minus signs that cancel I suppose).
Eq 1 and 2: Because of the unstructured mesh one presumably needs to do something in order to do zonal/meridional integrals, so what is actually done? There is a citation to Sidorenko et al (2020a) but this is not that helpful in that there could also be a brief description of what is actually done, because there is unnecessary ambiguity. (Re-interpolation? If so, nearest nearbour, linear or something else? Evaluation of basis element even though this is finite volume?)
Equations: Need punctuation to go after them as they should be regarded part of the sentences. So full stops after the symbols at Eq. 1, 3, 5, and commas in Eq. 2 and 4.
line 102 + 110: Remove indentation, this is not a new paragraph (don't give it an extra blank line after `end equation`
#Response#: We appreciate the comments on Section 2.2 and have completely re-written this part of the manuscript to apply necessary modifications. We modified equations to match completely with the description from Sidorenko et al. (2020a,b, 2021), and provide consistency in comparison with other studies employing a similar mathematical framework (e.g., Xu et al., 2018; Megann, 2018; Megann et al., 2021).

Section 3

Sec 3 first paragraph: "Averaging in time" is implied but no mention of time window, although this is in Fig 1 caption. This is not entirely helpful, so should mirror that detail around here in the text.
#Response#: We agree that the time-averaging window should be stated. As a solution, we have added the following statement after the second sentence of the first paragraph in Section 2.1:

"Analyses of mean large-scale processes were performed utilizing the output of the final 50 years of each simulation."

Fig 1 axis labels and elsewhere: Rather than "kgm-3" it should be "kg m-3" and similar. To do this in LaTeX I guess you would do something like "$kg\ m^{-3}$" (as is done in the text).

Degrees symbol is slanted here and is inconsistent with how it is used in text; try ° if that isn't already what is used (if it is then I don't know what the problem is).

#Response#: In the first version of our manuscript, we have used the latex package `siunitx` that handles SI units. Therefore, we used `degree` unit embedded in the package, for example, for latitude/longitude, and the sequence `kilo gram per cubic meter`, for "$kg\ m^{-3}$". As a solution, we replace the `degree` unit with ° for the coordinates, and only apply the copernicus package for handling other units.

line 142: The two AMOC measures differ in their "spatial distribution" but the comparison shown in Fig 1 is not evidence to support that, because the vertical co-ordinates are completely different. Either

- **say they differ in the meridional distribution**
- **remap the z-AMOC into density co-ordinates**
- **remap the ρ-AMOC into depth co-ordinates if you have a mean isopycnal depth variable computed**

#Response#: We thank the reviewer for calling our attention to this expression. We have replaced "spatial" with "latitudinal" to avoid any confusion. In terms of the third item, upon suggestion from Reviewer 2 (see answer to RC2), the ρ-AMOC, remapped onto depth coordinates, that was provided in the appendix in the original manuscript, and is now added to the main text in the revised version.

line 143 + 144: "Distinct driving mechanisms" make no sense to me, because your model is "driven" by the same thing, while the AMOC diagnostics are just that, diagnostics computed from the model variables. Probably lessen or remove related text. Relates to the above point that it is not clear that the two diagnostics are just that, different measures.

#Response#: We thank the editor for drawing our attention to the fact that this sentence is confusing. We acknowledge that the model is driven by the same set of boundary conditions. The intended meaning was to convey that the divergence between the vertical velocity and diapycnal velocity fields stems from an underrepresentation of these driving forces when considering vertical velocity, as opposed to diapycnal velocity. We have addressed this point in the revised manuscript to clarify this contradiction.

line 155: The "annual and 15 year means" are out of place / unbalanced if there is no "50 year" mean mentioned when talking about the AMOC at the beginning of the section.

#Response#: We apologise for the confusion. As introduced in line 151, the AMOC indices represent the temporal evolution of the AMOC across the 200 simulated years. These indices are graphically depicted in Figure 2, wherein annual means are rendered as thin lines and 15-year rolling means are superimposed as thick lines. To ensure clarity, the text from lines 149-159 has been revised and modified to:

"To evaluate the consistency of these phenomena across our 200-year integration period, we define two AMOC indices in both density and depth spaces, derived from the streamfunction of each model year: (1) $AMOC_{max}$, denoting the annual maximum overturning between $30-65°N$, representing subpolar AMOC; and (2) $AMOC_{26}$, denoting the annual maximum overturning at $26°N$, representing the subtropical AMOC. The upper limb was isolated in both depth and density spaces by implementing the vertical depth and density boundaries specified in Table 2 for the PI and 4xCO_2 climates, generating continuous 200-point time series for each index. These boundaries were defined to isolate the AMOC index from signals emerging from overturning in the upper ocean or contributions from underlying water masses like the AABW, especially in 4xCO_2, where the abrupt nature of the experiment might induce noise to the AMOC signal we wanted to capture.

Figure 2 displays the time series of both indices, with annual means represented by thin lines and 15-year running means superimposed as thick lines to attenuate interannual fluctuations and accentuate multi-decadal variability. A first-degree polynomial trend was fitted and subtracted from the multi-decadal time series to facilitate correlation analysis using Pearson's correlation test. Furthermore, the magnitude of AMOC variability was quantified by its standard deviation (σ). Please note that 50-year averages previously referenced relate exclusively to the climatological fields (mean state) depicted in Figure 1. "

paragraph beginning line 160: As above, the two AMOC diagnostics are just measuring different things so the differences are not that surprising. They measure different physical effects, so in this case you probably care more about ρ-AMOC so just say that.
#Response#: We appreciate the comment and these concerns are now addressed in the revised manuscript.

line 181 to 187: Would recommend some of this text to be copied/moved up to introduction or section 2 to frame the article more concretely.
#Response#: We appreciate the comment and these concerns are now addressed in the revised manuscript.

line 199: Don't need the "AMV" acronym because it's never used again anyway.
#Response#: We appreciate the correction and have removed this term from the text.

Section 4

line 219 + 220: Analogously worded sentence should be up in introduction and/or section 2.
#Response#: We appreciate the comment and these concerns are now addressed in the revised manuscript.

Fig 6 caption (but do this also in text): Over what depth/density classes and averaged how? (full depth?)
#Response#: Our choices are as follows:

- PI: the surface transformations and diapycnal velocities are plotted for the density of 36.68 $kg\ m^{-3}$, whereas the vertical velocity is plotted for the depth of 910 m.

- 4xCO_2: the surface transformations and diapycnal velocities are plotted for the density of 35.87 $kg\ m^{-3}$, whereas the vertical velocity is plotted for the depth of 790 m.

We also provide this information in lines 264-265, where we refer to the table that contains these levels. As for the average, we have added in Section 2.1 that all mean-state plots were obtained through the annual mean average of the last 50 simulation years. We hope that this addition, together with information already provided, suffices for the understanding of the depth/density classes to which the figures are referenced.

line 263: Either "w_ρ" is meant, or need to state why "w_p" is different to "w_ρ"
#Response#: We thank the editor for bringing this mistake to our attention. w_ρ is meant.

line 280: The model AMOC weakens by the same mechanism presumably but these are projected differently onto the different AMOC diagnostics. Reword accordingly.
#Response#: Please see answer to comment on lines 143-144. We appreciate the comment and these concerns are now addressed in the revised manuscript.

Section 5

line 300 to 302: Analogously re-worded sentence should be up in introduction and/or section 2 to anticipate this sentence coming up here.
#Response#: We appreciate the comment and these concerns are now addressed in the revised manuscript.

line 338 to the end: Analogously re-worded paragraph should be up in introduction and/or section 2.
#Response#: We appreciate the comment and these concerns are now addressed in the revised manuscript.

Response to Reviewer 1

0.2 RC1's major comments

1. The one suggestion of mine that the authors have not responded to is the one concerning the representation of convection in FESOM2.5. Section 2.1 needs to include a statement of how FESOM addresses density inversions: does it massively enhance vertical mixing, as, is done for instance, in NEMO? Or is there an explicit advective exchange of water parcels? I have been unable to find an explicit answer to this question in previous FESOM papers (e.g. Timmermann 2009, Sidorenko et al. 2014). In the context of the present paper, which

distinguishes in detail between downwelling in depth space and density transformations in density space, I think such an explanation is essential.

#Response#: We thank the reviewer for raising this point, giving us the opportunity to clarify the manuscript text accordingly. In FESOM2.5, the K-profile parameterization (KPP; Large et al., 1994) scheme was employed for vertical mixing. Consequently, convection is parameterized through enhanced vertical mixing rather than explicit advection of water parcels. In practice, when convective conditions arise, the model artificially increases the vertical diffusivity (on the order of 0.01 m^2 s^{-1}) to homogenize the water column. This approach is common in hydrostatic ocean models, which cannot explicitly resolve convective plumes or advective exchange across isopycnals due to the hydrostatic approximation. Fully resolving convective overturning would require a non-hydrostatic model with horizontal grid spacing comparable to the vertical scale ($\sim$ 1 m), an unattainable resolution for global simulations on current supercomputers.

We added the following text into Section 2.1:

"Vertical mixing in FESOM2 is parameterized using the K-Profile Parameterization (KPP; Large et al., 1994) scheme, through which locally enhanced vertical diffusivity, set to 0.01 m^2 s^{-1}, is implemented to represent convection arising from local static instability."

0.3 RC1's minor comments

2. Technical corrections

L304. "Here, we examined..." is awkward. I suggest replacing with "We have examined...".

L309. "Yet..." would read better if it were replaced by "Nevertheless...".

L342. "and rampant" is unnecessary – I would suggest deleting it.
#Response#: We appreciate the corrections and have replaced the terms accordingly.

Response to Reviewer 2

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

[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

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.

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:

'Please note that, in our study, the AMOC is computed using vertical velocity, w, rather than the conventional approach based on horizontal velocity (v; see Table A1 for the equations). We make this choice because, while both methodologies are mathematically equivalent (see Sidorenko et al., 2020b, for a detailed comparison), using the meridional velocity would require integration along the boundaries of the control volumes, which is less convenient for arbitrary unstructured meshes, and it would also neglect important information about diapycnal velocities. In contrast, the vertical velocity approach naturally yields the AMOC in density space, reduces noise introduced by the beforementioned integration along the boundaries of the control volumes, and enables more efficient online diagnostics in FESOM2 (Sidorenko et al., 2020a, 2021).'

***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 $33-36\ kg\ m^{-3}$, 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 $36.20-36.90$ $kg\ m^{-3}$, 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:

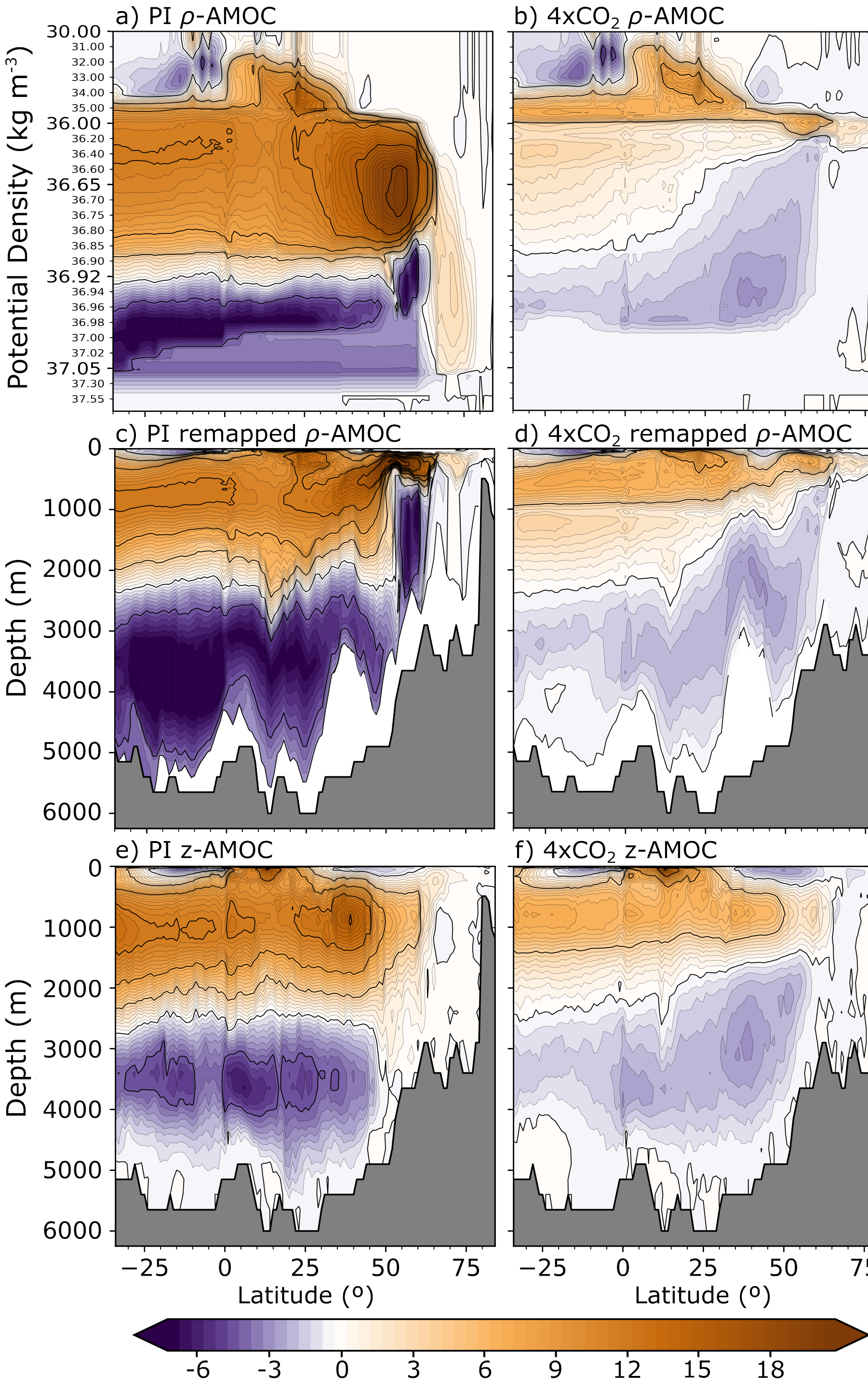

Figure 1: Mean AMOC strength in units of Sverdrup (1 Sv $\equiv 10^6\ m^3\ s^{-1}$) averaged over the last 50 years of the PI and 4xCO_2 simulations. Panels show (a, b) ρ-AMOC, (c,d) ρ-AMOC remapped into depth coordinates, and (e, f) z-AMOC for (a, c, e) PI and (b, d, e) 4xCO_2. In (c,d), ρ-AMOC is remapped 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 $30 - 65°N$*."*

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 $36.20 - 36.90 kg\ m^{-3}$) 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.

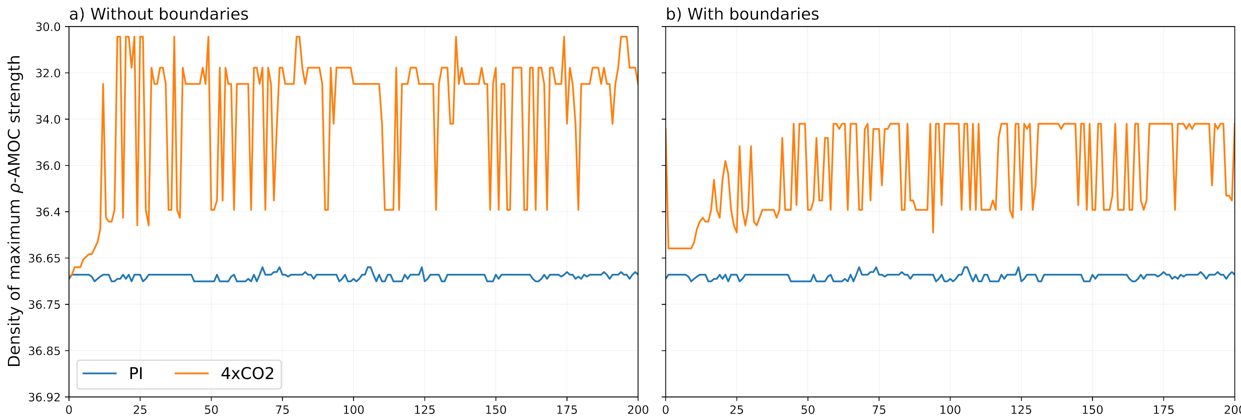

Figure 2: Density and depth of maximum AMOC at $30 - 65°$N with (right) and without (left) upper and lower boundaries.

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 $4xCO_2$.

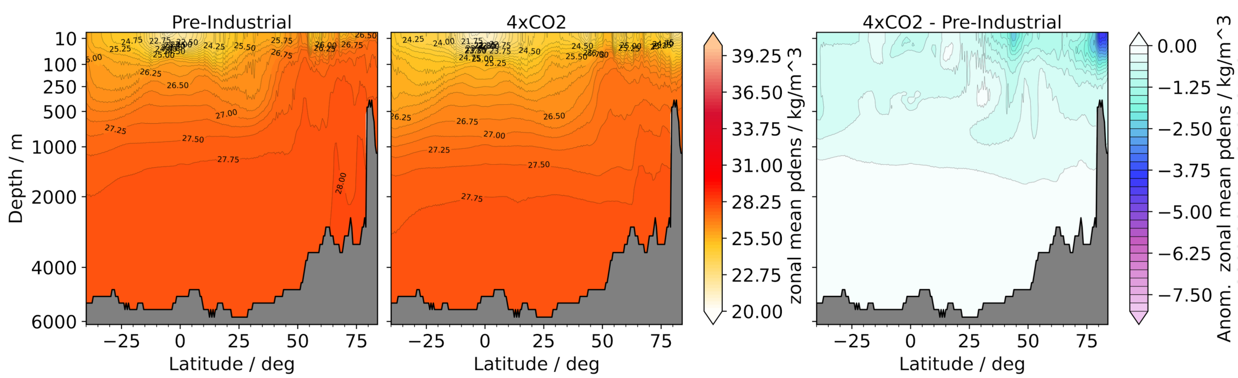

Figure 3: Potential density in $kg\ m^{-3}$ in the last 50 years of simulation for PI (left panel), $4xCO_2$ (middle panel), and the anomaly between PI and $4xCO_2$ experiments (right panel).

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 ($\sigma_2 = 35 - 36.4kg\ m^{-3}$). 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.

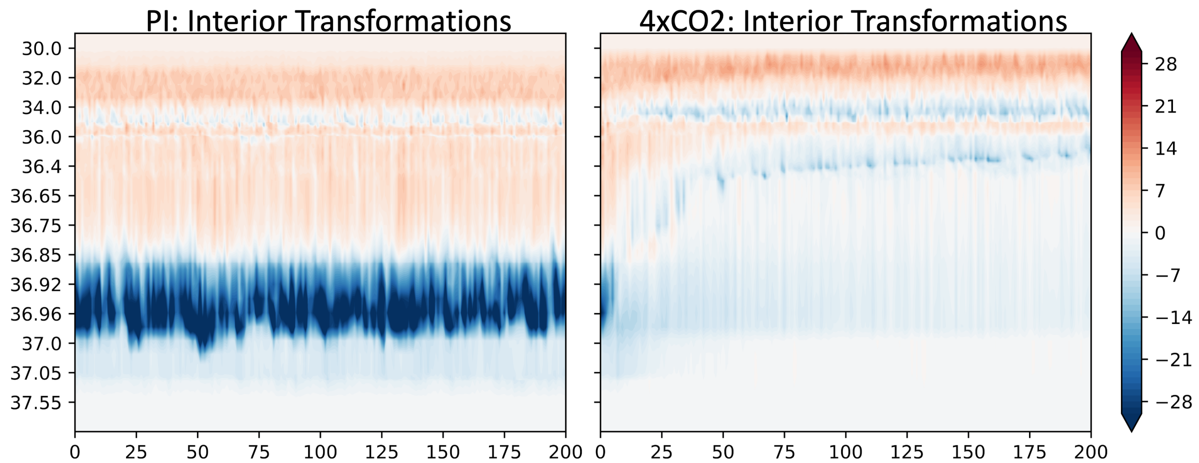

Figure 4: Time evolution of interior transformations for PI (left) and 4xCO_2 (right).

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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