Author response for egusphere-2025-2326-EC1

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August 8, 2025

Letter to the Editor (wrt. EC1)

Dear Editor,

We are extremely grateful to the two reviewers who raised valid points regarding our submission. We have addressed these comments and have reflected the suggestions in a revised version of our manuscript. Furthermore, we very much appreciate additional comments and suggestions that were raised in EC1. Thanks to the comments by the reviewers and by you we feel that the relevance of our findings will be presented in a more concise and clearer manner. We would like to point out, however, that not all specific points are individually addressed within this response. 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. We consequently do not address comments that do not apply anymore as a result of the substantial changes.

Sincerely,

Fernanda D. A. O. Matos

(On behalf of the authors)

Response to 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:

'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)."

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 $0.01\,\mathrm{m^2\,s^{-1}}$) 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:

'In FESOM, the K-Profile Parameterization (KPP; Large et al., 1994) scheme is employed for vertical mixing. Consequently, convection arising from local static instability is parameterized through locally enhanced vertical diffusivity, set to $0.01\,\mathrm{m^2\,s^{-1}}$*.'*"

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: *"As the low-resolution mesh employed in this study is not eddy-resolving, mesoscale eddy stirring is included via the Gent–McWilliams (GM) parameterization (Gent and McWilliams, 1990) and implemented according to the explicit eddy-induced stream-function algorithm of Ferrari et al. (2010), as described in Danilov et al. (2017) and evaluated in Scholz et al. (2019) for FESOM2."*

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.

Response to EC1's minor comments

Section 1

line 58: Remove comma

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

Section 2

Section 2.1

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

Section 2.2

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 using 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 $^\circ$ if that isn't already what is used (if it is then I don't know what the problem is).

#Response#: In 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. However, for other SI units, we see that there is a space (i.e. kg m^{-3}) correctly set when using the siunitx package and decided to keep using the `siunitx` package.

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:

"Figures 1b and d highlight two major consequences of quadrupling atmospheric CO_2 concentrations to the AMOC in both frameworks: the weakening and shoaling of the upper cell (e.g NADW), and the northward-upward expansion of the abyssal cell (e.g. AABW). To assess 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 cell was isolated in both depth and density spaces by implementing the vertical and density boundaries specified in Table 2 for the PI and $4xCO_2$ climates, generating continuous 200-point time series for each index.

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 multidecadal 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 the previously mentioned 50-year averages relate exclusively to climatological fields (mean state) and not to Figure 2."

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.

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