AGU Advances

Authors' Response to Peer Review Comments on

Significance of diapycnal mixing within the Atlantic Meridional Overturning Circulation

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Response to Reviewer #1

Review of "Significance of diapycnal mixing within the Atlantic Meridional Overturning Circulation", by Laura Cimoli et al, submitted to Advances.

The authors provide a multi-approach and largely observation-based analysis of the diapycnal mixing and associated water mass transformation and tracer propagation in the Atlantic Ocean. They found an overall buoyancy gain for North Atlantic Deep Water south of 24{degree sign}N and estimate a resulting diapycnal upwelling of up to 4 Sv. They finally attempt to estimate a typical vertical diffusive length scale, and investigate the vertical transfer of tracers and buoyancy and their associated timescales in a zonally-averaged numerical model of the meridional overturning circulation.

This is a concise article, well written and organized, and with clear take-home messages. It is largely based on the combination of already-approved methodologies for deriving dissipation rates or diffusivity estimates from observations. Those are here applied to the particular Atlantic Ocean in order to infer the importance of diapycnal mixing within the deep limbs of the overturning circulation. I do not think that the following comments will require substantial revisions, so I am here asking for "minor revision". However, several points deserve more care I believe.

We are grateful to the reviewer for their positive and constructive feedback. We have taken all of their comments into account in the revised manuscript, as we discuss point-by-point below.

1. I have appreciated the qualitative discussion about uncertainties, but I missed a quantitative feel about them. There is, for instance, a striking discrepancy between the inverse model estimate and the other three (Argo-based, strain-based, or tidally-driven). Part of the discussion (l. 471-498) seems to imply that the former is some kind of truth to which the later ones should be compared, notably because the data or model underlying the local estimates cannot capture all processes near steep and rough boundaries (whereas the bulk estimate implicitly includes them). The uncertainties of the bulk inverse estimate can also be pretty large, however (synoptic transport estimates, biased air-sea fluxes, ...). In fact, I am wondering if an uncertainty could be added to this estimate (one that would be propagated from the uncertainties usually provided as outputs by the inverse model). One might expect an uncertainty on the transformation rates nearly as large as the signal... I would also suggest to not only provide the ~4 Sv inverse estimate in the abstract and key points, but rather the range of maximum rates obtained from the 4 approaches (0.8 Sv - 4 Sv).

We agree that the uncertainties in inverse estimates are large. We have now added (to Fig 4a) the uncertainties on the Lumpkin and Speer 2007 inverse estimates from their original work. We have adjusted the manuscript in the relevant discussions to highlight the fact that within the uncertainties of the bulk estimate, there could be a much smaller or much larger gap between the inverse estimate and the others. Thanks for this excellent suggestion.

2. The time-spans covered by the four estimates are not mentioned and discussed in the text. For instance, I imagine that the inverse estimate is largely WOCE-based (1990's) while the other two are probably more representative of the latest two decades (e.g. Argo, or microstructure). Would the difference in the diapycnal mixing estimates be partly related to decadal AMOC variability (e.g. stronger state in the 1990's than in the 2000's in the North Atlantic)?

All our estimates are based on WOCE climatology: tidal maps are directly built on it, and the Argo-CDT and microstructure estimates of transformation rates are also built on WOCE climatological stratification. However, the epsilon estimates used in the latter group come from averages of float-based, CDT-based, or microstructure-based measurements. For all these, we lack sufficient statistics to construct a temporal evolution of epsilon (e.g. ALL Argo float data from all years are used and binned into lat-lon bins to give a bin-average estimate of epsilon). So, while in this work we do not engage with the temporal evolution of mixing, it is of significance and how it relates to AMOC variability is an intriguing question for future research. We have added text to the manuscript to make this point clear.

3. On the topic of the diffusive length scale, I wondered how sensitive the result was to the estimate of the transit time? I understand that the latter calculation is rather a "back-of-the-envelope" scaling (e.g. use of single-latitude decadal-mean velocity profile), but maybe more should be said on the underlying calculation because the authors build part of their contextualization/interpretation on this vertical length scale. I was particularly surprised by the very long residence times used (up to 350 years), given the approximate mean speed of the DWBC measured by current meters (5-10 cm.s-1) or mean basin-wide velocities derived from tracer measurements (2-2.5 cm.s-1), and which would give transit times of a few tens of years at most. Agreement with Arctic and Antarctic ice cores are mentioned without an explanation of their pertinence for a 48{degree sign}N-32{degree sign}S transit time. Also, would it be possible/useful to refine the length scale estimate by separating interior and boundary regions, with weak and strong mixing rates and meridional velocities, respectively? The authors suggest that this is possible for the tidally-driven estimate (l. 233-236). Finally, are those vertical length scales in line with recent tracer release experiments (e.g. BLT) or older ones (e.g. Brazil Basin)?

We have streamlined the definition of the diffusive length scale and modified fig 4b accordingly. We also have explained how it relates to a southward flow that considers all of the basin-wide dynamics and not just the western boundaries. We also explain how the boundary vs interior separation is much more complex and perhaps a topic of future work. The following text, copied from the revised manuscript, should hopefully be more clear:

Following Fick's law of diffusion, the vertical diffusive length scale is

$$\mathcal{L} = 2\sqrt{\kappa \Delta t} = 2\sqrt{\frac{\langle \mathcal{M} \rangle}{\langle N^2 \rangle} \Delta t}, \tag{4}$$

where κ is the basin-average diapycnal diffusivity (used for estimate 4), and $\langle \mathcal{M} \rangle$ is the basin-average buoyancy flux (used for estimates 1-3 described in Section 2). The average N^2 is calculated from WOCE hydrographic climatology. The residence time Δt is intended to be the average time it takes for a tracer to transit via the AMOC. Such inter-hemispheric transit involves not only north-south transport via the strong western boundary currents but also lateral mixing of tracers between boundary currents, gyres, and equatorial currents, as well as vertical (diapycnal) mixing (Fine et al., 2002; Lozier, 1997; Holzer & Primeau, 2006; Bower et al., 2009; Rhein et al., 2015; MacGilchrist et al., 2017; Lozier et al., 2022).

We estimate the residence time Δt as the ratio of the distance between 48°N and 32°S to the mean southward velocity. The distance is 8000 km. A mean velocity of 0.8±0.2 cm/s is estimated from the time-average (yr 2004-2010) meridional transport measurements from the RAPID-MOCHA array at 26°N (Moat et al., 2022), for the depth range ~1000-4000 m characteristic of the NADW. Assuming that this velocity can be applied at every depth and latitude, which is clearly an oversimplification, the estimated residence time is Δt is ~300 years.

Figure 4b shows the resulting diffusive vertical length scales as a function of density. For mixing estimates (1-3), $\mathcal{L}$ is between 500 and 1400 m, while for the bulk inverse estimate (4), it is much larger, between 1300 and 4000 m. By construct, $\mathcal{L}$ is an order-of-magnitude estimate. The large values, especially in the denser waters, imply that mixing is sufficient for tracers to mix across the entire depth range of the southward flowing NADW from the subpolar Atlantic to the Southern Ocean. This further implies the potential mixing of tracers with the upper northward branch of the AMOC or the deeper abyssal circulation. Thus, mixing within the southward flowing limb of the AMOC can significantly alter tracers' global pathways and residence time. Of course, an accurate measure of the integrated effect of mixing on tracer dispersion can only be achieved by consideration of the full range of dynamical processes comprising the AMOC, the spatio-temporal variability of mixing, and the spatial distribution of a given tracer. While we leave such comprehensive analysis to future work, we explore the integrated effect of mixing on tracer dispersion using a simple zonally-averaged AMOC framework in the next section.

Some other comments:

Figure 1: I am guessing that neutral density is used here but it is not specified.
Yes, it is now specified both in the text and caption.

l. 142: some supplementary materials are mentioned here, but I cannot find any with the submission (I am guessing those were included in an earlier submission - hopefully I did not miss key information here).
This was a mistake. There is no supplementary material. Such references are now removed.

l. 155-156: why is this improvement limited to below 2000m depth?
We have removed the sentence. Adding microstructure data improves the estimate at all depths, particularly in regions with more data.

l. 177: Figure 2(f-h)?
Yes, the detail of the figure label was added.

l. 226: following microstructure measurements (?)
The sentence has been clarified and changed to “in agreement with microstructure measurements”.

l. 240: the authors should explain here how they define the boundary region (offshore of some given isobaths?)
We had defined the boundary region simply as where the divergence of the buoyancy flux is positive next to the seafloor. We have edited the terminology used in this paragraph (and in the legend of fig 3c) to avoid using “interior VS along-boundary” terminology but rather focus on upwelling VS downwelling patterns. This also helps account for the interior upwelling.

l. 248-251. I think this sentence needs to be rephrased. Red and blue (empty) bars seem to be always opposite for light density classes, so I am not sure to understand why the authors say that "waters upwell both in the interior and in the proximity of the boundary”.
We agree. The sentence was redundant and apparently confusing, so we decided to remove it.

l. 280-283: The "similarity" mentioned here seems to neglect the factor 3 between the inverse estimate and the other ones.
We have added a comment to say that the similarity is qualitative (same patterns). In addition, we have now added error bars to the inverse estimate product (see the answer to main point #1 above), which further emphasize that the agreement is qualitative but not quantitative.

Figure 4: I am probably missing something here. I would have thought that any differences between the four water mass transformation estimates (equation 3, figure 4a), as well as between the four length scale estimates (equation 4, figure 4b), would be due to their different diffusivity or dissipation coefficients. In other words, I would have thought that the buoyancy b, the area of density surfaces A, and the transit time dt, would be identical for each estimate (I cannot find any information on how and from which dataset b and A were computed). Yet, the bulk estimate and the tidally-driven estimate show similar length scales but very different transformation rates for the layer 27.6-27.9 (for instance). Why?
The diffusive length scale is based on the square root of diffusivity, where the diffusivity is directly proportional to turbulent flux. The transformation rate, however, is based on the vertical divergence of the flux. Thus, crudely speaking, the diffusive length scale is an integral measure (in depth or density) of the transformation rate. The gap between the diffusive scale based on inverse estimate and the other curves simply implies that the transformation rates from the inverse estimates are mostly larger than those due to other estimates. We hope that the modified version of figure 4 and streamlining of the discussion of the diffusive length scale (as explained above) help make this more clear in the revised manuscript.

Another point: What is the difference between the green bars on Figure 4a and the filled blue bars (total) in Figure 3b? I thought they were the same (water mass transformation rates for the tidally-driven estimate) but they are not, see for instance the density layer 27.9-28.05.
Thank you for spotting this mismatch. In the previous version, the integration area was limited to 48N-32S in one of the two plots and not the other. We have now made them consistent and now they match as they should.

l. 315: which decade? please give year range.
The year range has now been added.

l. 319: How was the 48{degree sign}N-to-32{degree sign}S distance computed?

It is computed as a direct meridional distance between the two points, which is a conservative choice as tracers' pathways are winding. We have more explicitly mentioned this in the manuscript now (as we mentioned earlier in our answer to Q3).

l. 329-330: one can wonder whether those near-bottom "very large values" are realistic or not.
Again, please see our response to Q3 above. As we say in the revised version, "By construct, $\mathcal{L}$ is an order-of-magnitude estimate. The large values, especially in the denser waters, imply that mixing is sufficient for tracers to mix across the entire depth range of the southward flowing NADW from the subpolar Atlantic to the Southern Ocean. This further implies the potential mixing of tracers with the upper northward branch of the AMOC or the deeper abyssal circulation."

l. 385-386: this increase toward the northward flow is not obvious to me. Could the authors precise a bit more what they see?
We agree that the statement was not clear from Fig. 6. We have edited the sentence to "The net effect of enhanced tracer mixing is an increase in the diapycnal transfer of the tracer towards the abyssal cell, as well as enhanced recirculation of the tracer within the ANDW layer".

l.417: "modestly but significantly" sounds a bit contradictory to me (unless "significant" has here a statistical meaning but I doubt so since uncertainties are omitted in this analysis).
We have removed "but significantly" from the sentence.

l. 431-433: sparse sampling of which quantity? tracers?
Sampling of turbulent mixing. It is now clarified in the manuscript.

Reviewer #2

Summary

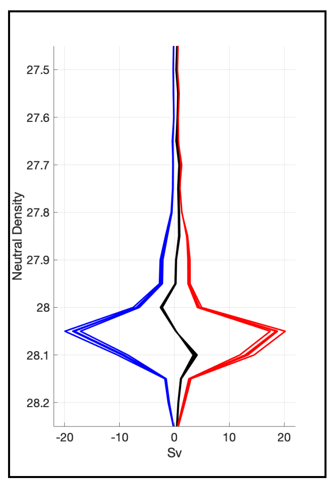
Diapycnal mixing is critical to maintain the current state of the Atlantic Meridional Overturning Circulation (AMOC). Its quantitative contribution to the AMOC and its influence on deep ocean tracer transport, however, are less clear. Using a suite of observation-based estimates of dissipation rate or diffusivity coefficient, Cimoli et al. quantify the diapycnal transformation of the deep waters induced by mixing. They further analyze the impact of diapycnal mixing on tracer pathways in a zonally-averaged model. Results from the study underline the importance of diapycnal mixing in the AMOC and tracer (e.g. oxygen, heat, carbon) distribution, both of these processes are important for the climate.
The manuscript is well-written and the study's focus is of great interest to many people in oceanography. The relevant estimates from a variety of observation-based datasets are quite intriguing. I would recommend publication after addressing comments listed below.

We are thankful to the reviewer for their positive and constructive feedback. We have taken into account all of their comments in the revised manuscript, as we discuss point-by-point in what follows.

Major comments

[1]. One of the key results of the manuscript is the quantification of diapycnal transformation using different dissipation rate datasets. Because the reported net transformation is quite small (0.8 – 4 Sv) compared to the total diapycnal upwelling and downwelling (~20 Sv), one would wonder if the estimated net transformation (i.e. the difference between upwelling and downwelling) is statistically significant. Thus, while I understand the challenges, I still think it is necessary to make an effort on estimating the transformation uncertainties in order to make the conclusion, i.e. diapycnal mixing is significant within AMOC, more convincing.

We agree that having a sense of uncertainty is needed. To this end, we have now added to Fig 4a uncertainty estimates for the inverse calculation of Lumpkin and Speer and have modified the corresponding discussions accordingly. The error bars suggest that for most density classes a statistically-significant net upwelling still exists. Furthermore, we performed sensitivity tests for the transformation rate calculation based on the tidal estimates. De Lavergne et al (2019) proposed five



different sensitivity experiments (see their section 3.3) that returned different dissipation rate estimates. The net water mass transformation rates for these experiments remain sufficiently close (see figure to the right) which leads us to believe the net upwelling (in black), even though much smaller than downwelling (blue) and upwelling (red) rates, is statistically significant.

We also like to point out, importantly, that we do not calculate the net transformation rate across a density layer as the sum of the two (red and blue). We calculate it as a volume flux, as the areal integral in Eq 3 can be rewritten as a volume integral of the divergence of the buoyancy flux, integrated from the seafloor to a given density level. Such a bulk integration over a large volume of the ocean is much more robust and less prone to numerical errors than the areal integral over the density level would be. When we do such calculation using, say the Argo estimate, even within the uncertainties associated with them (a factor of 2-3 from microstructure estimates), we still get a significant net upwelling.

While the broader point about uncertainties is valid, it is challenging to add error bars to all estimates used here, particularly for estimates 1-3. Even if we could, their uncertainties would correspond to very different types of errors given the diversity in the type of measurement and the methodology used to infer mixing rates across the estimates. We hope that adding the error bars from the inverse estimate sheds light on the large uncertainties within which all other estimates lie. We also have added text to the manuscript on the range of uncertainties for the epsilon based on Argo data.

[2]. In section 3.1, the authors make a nice argument on the spatial (horizontal) variation of diapycnal mixing and propose that this mixing has a critical impact on tracer distribution. However, in section 3.4, the diapycnal tracer transfer is analyzed using a 2-D (i.e. zonally- averaged) model. I wonder if the 2-D model, even run with bottom-enhanced diffusivity, is representative of the actual tracer distribution. In other words, does the spatial variation of diapycnal velocity shown in Figure 3 affect your results in Figure 6?

We agree with the reviewer that the integrated effect of mixing on the distribution of tracer also depends on the three-dimensional flow. While we defer study of the impact of full 3D variability in mixing for the future, we have added a few sentences in section 3.3 to clarify this point:

"Such inter-hemispheric transit involves not only north-south transport via the strong western boundary currents but also lateral mixing of tracers between boundary currents, gyres, and equatorial currents, as well as vertical (diapycnal) mixing"

And later on:
"Of course, an accurate measure of the integrated effect of mixing on tracer dispersion can only be achieved by consideration of the full range of dynamical processes comprising the AMOC, the spatio-temporal variability of mixing, and the spatial distribution of a given tracer. While we leave such comprehensive analysis to future work, we explore the integrated effect of mixing on tracer dispersion using a simple zonally-averaged AMOC framework in the next section."

Minor comments
[1]. Line 26: Please add "... the Atlantic Meridional Overturning Circulation (AMOC)".
Now added.

[2]. Line 28: It would be helpful to specify the isopycnal associated with the 4 Sv transformation. In addition, is 4 Sv a large portion of the total NADW upwelling? I suggest to report the contribution in percentage of this 4 Sv to the total.
We have specified the density range associated with the peak of water mass transformation (line 28). As recommended by the reviewer, we have added the errorbars for the inverse estimate (Fig 4a), and now report that the range of the net upwelling within AMOC is 0.5-8Sv. These values imply that mixing could be less or more significant for closure of the circulation (i.e. as compared to the isopycnal wind-driven upwelling). Considering that the total transport is not too well constrained, we decided to not report a percentage. However, we note that the primary message of the paper is that regardless of how much mixing helps with the net upwelling within AMOC (i.e. whether leading order or not), it can be of leading order importance for tracer budgets. We have added text to section 3.3 and the discussion to clarify this point.

[3]. Lines 31-33: It is quite difficult to understand this long sentence without reading the manuscript. Please re-word.
Thank you for pointing this out, we have modified the sentence to "it indicates that mixing can change where tracers will be upwelled in the Southern Ocean, ultimately affecting their global pathways and ventilation timescales".

[4]. Line 203: There is an extra "is".
Removed.

[5]. Line 206: I am not sure if I understand the phrase "within the AMOC". What about "associated with the AMOC"?

The section title has been changed to “across AMOC density levels”.

[6]. Figure 3c: The comparison between boundary and interior diapycnal transformation is very interesting. If I understand correctly, the scattered positive diapycnal transformation along MAR in Figure 3b adds up to ~20 Sv, whereas the negative transformation over the much greater interior basin adds up to ~14 Sv. This means the along-topography upward diapycnal velocity must be much greater than the downward velocity in the interior. This difference is not discernible in 3b.
Also, the net transformation is a magnitude smaller than the total diapycnal upwelling and downwelling. It makes me wonder how significant this net transformation is, i.e. whether it is significantly different from 0. Please see my major comment.
The boundary upwelling is indeed sharper and much stronger in fig 3b, and we agree with the reviewer that it is not easy to discern this pattern from the figure. We have tried editing the figure with different color schemes, log scales etc., and we still believe the original figure is most clear. We hope that taking fig 3a-b, together with panel 3c, should help illustrate where the upwelling and downwelling occur (panels a,b) and how much (panel c). As for the point regarding uncertainty, please see our response to the first major comment above.