Summary

This manuscript compares two different frameworks for diagnosing the Atlantic Meridional Overturning Circulation (AMOC): a latitude-depth streamfunction based on zonal integration and a latitude-density streamfunction based on density coordinates. They show that the structure of these circulations differ a bit in preindustrial circulations but highlight even larger differences in terms of the response of the circulation to climate change. They show that an additional advantage of the density-based MOC framework is that it relates directly to water mass transformations, which provide more mechanistic insights.

Overview

The analysis is interesting and technically correct. It will make a valuable contribution to the peer-reviewed literature. However, I think the contextualization of the results in terms of the broader literature needs to be improved before it can be accepted. Additionally, some of the results and implications of the work seem to be a bit overblown, and are less novel than the others make them out to be.

While I consider these concerns “major”, they do not require any

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 rho-AMOC. Like Foukal and Chafik (2024), this paper concludes by advocating “for including rho-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.

2) **The authors employ a different definition for rho-AMOC than most.** The conventional way of diagnosing rho-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.
3) **I am not convinced that the maximum rho-AMOC is a meaningful metric.** While the authors have indeed shown that the maximum z-AMOC and rho-AMOC are very different, a large fraction of this difference is due to the strong recirculation cell in rho-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?
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 rho-AMOC.
5) **The Figure with the rho-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 rho-AMOC and facilitates direct comparisons with the z-AMOC.

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

Minor comments

L. 33- “at approximately 1000 meters” is misleading, since that is where the streamfunction reaches its maximum, not the northward transport.
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?
L. 168- Okay, but why doesn’t this also apply to 4xCO2? Do isopycnals become more titled with climate change?
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?
L. 262- What do you mean by “surface transformations *trigger* interior mixing”? Is this deep convection?

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.

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.

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).

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).

Feel free to reach out to me for clarifications via email.

Henri Drake
UC Irvine
henrifdrake@gmail.com