Response to referees' comments on Fox *et al.*, "Seasonality of meridional overturning in the subpolar North Atlantic: implications for relying on the streamfunction maximum as a metric of AMOC slowdown". https://doi.org/10.5194/egusphere-2025-616

ANONYMOUS REFEREE #1

Review of "Seasonality of meridional overturning in the subpolar North Atlantic: implications for relying on the streamfunction maximum as a metric of AMOC slowdown" by Fox et al.

The manuscript presents an interesting analysis of the relationships between different metrics of the meridional overturning circulation at seasonal timescale, and discusses their drivers using diverse decompositions of the overturning streamfunctions. The main conclusion of the manuscript is that each metric responds to different physical processes, with the maximum of the overturning streamfunction (MOC) being dominated by Ekman transports and surface density. As such, it would be valuable to routinely use the other metrics (i.e., density, heat and freshwater fluxes) in addition to the MOC when studying the overturning variability at this timescale.

The study provides a valuable evaluation of the different metrics that have been used to study the overturning variability. It is well-written, and the results are interesting and timely. However, it currently suffers from shortcomings that cast doubt on some of the results obtained, as indicated in the main comments below. I hope the authors find my comments and suggestions useful when revising the manuscript.

> We thank Anonymous Referee #1 for their useful comments and suggestions. We will reply to these fully with a revised manuscript in due course, but we would like to take this opportunity during the discussion period to respond briefly to the three Major Comments.

Major Comments

1) I find the discussion of the processes involved in each metric a bit hasty. I agree that the decomposition of the overturning streamfunction support the main conclusions of the study, but the authors repeatedly associate these processes to specific mechanisms and locations over the subpolar gyre without providing figures or references to support these statements. For example, lines 230-236, it is not clear how the dipole shown in Figure 6d can be interpreted as an underestimation of seasonal cycles in the North Atlantic Current and Labrador Current outflow? Again, lines 254-255, it is not clear how Figure 7 shows that the temperature-driven MOC anomalies are dominated by variability in the southward western boundaries rather than at

another location along the section (e.g., interior pathways?). The comment applies to the seasonal cycle of the metrics at OSNAP East and OSNAP West individually (e.g., lines 261-262, lines 279-282 or lines 350-353).

The authors should develop these arguments and walk the reader through these interpretations of the results or show convincing figures to support these results (e.g., similar to Figure 14).

> We hope we have managed to clarify all these points. We have done this in a couple of principle ways. Firstly, we spend some time at the start of the results sect describing in more detail the features of the streamfunction hofmuller, seasonality plots which underpin all these results. Secondly, while we recognise referee#2's request not to add more figures, we think that many of the clarifiactions required here are most easily achieved by the addition of a mean density/velocity plot. We have added this as a new panel in figure 5. Together with revised wording in each of the sections highlighted, and also in the specific comments, we hope that we have clarified the link between features of the overturning streamfunction plots and the underlying mechanisms.

2) One of the conclusions of the paper (line 506, also in the title) is misleading and should be rephrased: an AMOC slowdown would be identified on longer timescales than seasonal, so it is not clear to me how the MOC or any other metrics at seasonal timescale could miss or help to detect an overturning slowdown.

> We're not sure we accept that the conclusion (and section of the title) referring to the possible problems of relying on the streamfunction maximum (MOC) as a metric of AMOC slowdown is 'misleading'. We think it is an important conclusion - that seasonality of the MOC metric does not represent seasonality of overturning. Extrapolating this conclusion to the MOC metric performance in measuring longer-term 'AMOC slowdown' may be considered controversial, as may the inclusion of the statement in the article title. In our defence, the characteristics of MOC metric which cause problems when sampling the seasonal cycle — the domination of the metric by movements of a single density interface — are not specific to short timescales. Models of overturning slowdown (see for example Baker et al. 2025) show a combination of reduced volume transformation across the density of maximum overturning (sampled by the MOC metric), and reduced density difference between northward and southward flows (not sampled by MOC but captured by density flux). It is easy to imagine the current, often near exclusive, focus on the MOC metric underestimating long-term societally relevant AMOC decline. The exploration of various AMOC-associated metrics was always a central aspect of this study and as such we think it needs to be reflected in the title. We have attempted to express these points better in the conclusions and rephrased the title.

3) Can the authors comment on the significance of the relationships shown between the metrics? It can shed light on the differences noted between the 6-year and 20-year simulated timeseries.

> We aren't quite sure what the referee is asking for here. We think this is partly a question about the theory — needing better explanation of exactly what the density flux is,

physically (c.f. referee's specific comment lines 67-71). We have substantially rewritten the description and derivation of the various metrics in the methods section. Hopefully as part of this we have improved this explanation on how certain modes of variability in the streamfunction will impact MOC and density flux coherently, whereas other modes will impact them differently. It is an interesting question whether the metric characteristics, and relationships between the metrics, could shed light on the differences noted between the 6-year and 20-year simulated timeseries. We considered this carefully, but could draw no conclusions beyond the shorter timeseries having more high-frequency variability, and the Ekman-driven component being stronger – and closer in magnitude to the observations - in the shorter model timeseries. This latter observation suggests that the seasonal cycle in the westerly winds may well be stronger between 2014-2020 than in the 2010-2020 average. We have highlighted these observations in section 3.2.1.

Specific Comments

Lines 25-29: In addition to theoretical models, ocean models and state estimates, the use of coupled models also helped to better understand feedback mechanisms between ocean and atmosphere. I encourage the author to discuss Swingedouw et al. (2007) for an overview of the AMOC feedback mechanisms.

Although the timescales we are looking at are generally shorter than those studied in coupled climate models, we agree that this was a gap in our introduction and we have added a few appropriate references. We also revisit this briefly in the discussion.

Line 53: What is meant by "In a wider, ocean conveyor belt, sense"? We moved from this view of the overturning circulation from Lozier et al. (2010), can you clarify?

This sentence start was clumsy and unnecessary and didn't really mean anything, we have removed it.

Lines 67-71: The introduction of density flux needs a little more explanation to discuss its differences with the MOC metric. Related to this comment, can the authors justify more clearly in the introduction why looking at these specific metrics to represent the AMOC in density space instead of other ones that are also commonly used to measure the North Atlantic Ocean (e.g., NAO, subpolar gyre index, surface forced water mass transformation)?

We have added further explanation for the introduction of the density flux metric. On the wider question of other North Atlantic metrics, we set out here to specifically examine the seasonal characteristics of overturning (in density space) as measured by basin-wide mooring arrays. We think that is clear from the introduction. Of the other metric the referee mentions, only surface forced transformation is really a measure of overturning. The relevance of surface forcing to the observed seasonal cycles is discussed extensively in the manuscript already. We have added a little more introductory detail on the Mercier formalism.

Lines 80-87: I encourage the authors to better introduce the data used in the study. The horizontal and vertical resolutions of the data are a minimum. They should also indicate the forcings used at the boundary for the model, whether they are using monthly outputs, and comment on the representation of the simulated North Atlantic subpolar gyre as compared to observations.

> Agreed, done.

Lines 85-86: I also encourage the authors to add a map showing the locations of the OSNAP East and OSNAP West sections in the observations and/or the extracted sections in the model.

> We have added a location map.

Equations (9) and (13): As I understand it, these 2 metrics are not used in the study and can be removed.

> While results of these metrics are not included, we think it is important to retain the equations to show the parallels with those in density space. The metrics are included in figures 1 and 2, and now mentioned in the text.

Line 136: The “maximum” should be replaced by “extrema” considering that MOC_S is estimated as a minimum of the overturning streamfunction in salinity space.

> Done

Line 140: The equations in this section are clearly explained but I would recommend clarifying with a sentence at the beginning of section 2.3 what are main goals by looking at these decompositions of the overturning streamfunction.

> Done, the motivation for doing this is now also stated in the Introduction.

Line 142: There is a missing “following” before “Mercier et al. (2024)”.

> Done

Line 152: Please indicate in the text that the RHS are “velocity” integrals.

> OK. We have also highlighted which terms integrate the mean velocity fields and which the anomalies.

Line 162: Can the authors indicate the time period over which the mean of salinity and temperature are estimated? In particular, is it the same climatology used when discussing the 20-year or 6-year model results?

> The means discussed here are long-term, rather than monthly, means. We have clarified this in the text. All long-term means are calculated over the length of the dataset being examined, so the observations it is a 6-year mean, in the model results it is a 20-year mean (or 6-year model mean for results from the shorter timeseries in the Supplementary Information). We have made this explicit in the text.

Lines 245-246: If I am correct, the salinity-driven anomalies are expressed in MOC because its peak in density is close to sigma_moc. I suggest the author to comment on this aspect for clarity.

The referee is correct. The seasonal salinity anomalies are seen over a density range that spans sigma_moc, so show up in the MOC. The single-peaked form of the anomalies in density space means that these salinity-driven anomalies take the same form in density flux as MOC, with stronger MOC coinciding with more southward density flux. We have updated to text to state this.

Line 292: There is a missing "difference" after "most noticeable".

Thanks. Done.

Line 334: Consider indicating that the competition is won by the southward flow "in spring".

We have rephrased this to try to make it clearer. It is the timing of the full seasonal cycle -- both the spring peak and autumn minimum -- which is dominated by seasonal cycle of near-surface density of the southward flow.

Line 336: I am confused by this statement. It is not clear to me why the seasonal cycle of sigma_moc depth is related to MOC at seasonal timescale, or where it was shown in the study. Please justify whether these 2 timeseries co-vary in the model at this timescale. Considering the importance of Ekman forcing on the MOC variability, would you say that the sigma_moc variability is also mainly driven by this surface forcing?

This sentence and the paragraph following are entirely concerned with the density-driven part of the seasonal cycle, so with the mean velocity field, and hence mean Ekman transports. The relationship between sigma_moc depth and moc seasonal cycle comes from Equation 20. MOC is given by evaluation of (20) at sigma = sigma_moc; the time-variability in the density-driven term comes entirely from time-variability in isopycnal depth, z'(sigma_moc). And in particular how that isopycnal depth variability, which is a function of location (x), interacts with the local mean velocity field. We now state this at the start of the previous paragraph. Sigma_moc depth is a function of x and t, which complicates the comparison with MOC(t), and whether the two co-vary. Largely the aim in these two paragraphs and figure 15 is to work out the locations which dominate the density-driven MOC anomalies. It would be expected that in these locations sigma_moc would co-vary with MOC.

We have tried to clarify these relationships and results in the text here and in the previous paragraph.

Line 345: I think it is Fig. 15 instead of Fig. 13.

Thanks. Fixed.

Line 372: There is an extra "is" in "the property variability term also plays a role".

Deleted

Lines 375-378: Related to my first main comment, I am not sure to understand the argument here. Can the authors justify why "the largest seasonal salinity variability is confined to the southward flow at the western boundaries"?

The highlighted statement is really just an observation, based on figure 14 and similar figures for the full OSNAP section. We have reworded this to aid understanding and added a reference to figure 14 which showed much more horizontal structure in the salinity variability than in the temperature or density variability, with strongest variability in the western boundary.

Line 398: I am not sure that “simple” is the right term to use here. It is not more difficult to estimate the MOC, both MOC and density flux being based on overturning streamfunction.

We would like to stick with ‘simple’ but have altered the text to make our meaning clearer. For example, the annual mean density flux the arithmetic mean of the monthly density fluxes. For MOC, taking the arithmetic mean of monthly MOCs will not give the annual mean MOC because each monthly maximum is at a different sigma_MOC (indeed the annual mean MOC will always be lower than the mean of the monthly MOCs).

Lines 401-404: I am not sure to understand why the NAC would be included in the density flux but not in the MOC metric? Please clarify.

Thanks for highlighting this. We have changed the wording. It isn’t that the MOC doesn’t respond to the NAC changes, but more that (seasonally at least) it is dominated by surface density variability in regions where sigma_MOC is near-surface, obscuring other signals. The density flux looks like a more balanced measure.

Lines 423-425: The sentence doesn’t read well, please rephrase.

We have removed this sentence and address this in the discussion. In the response to referees comment on Lines 486-487 below.

Lines 459-462: Related to my main comment above, it would be interesting to see a map of the sigma_moc location over the Irminger Sea and its variability to support this statement.

We think this is already included as to Figure 15 (old figure 14) where the sigma-MOC depth variation across OSNAP_E was shown, along with the seasonal range of depths. We have added the figure reference here too.

Lines 486-487: Can the author comment on how the barotropic compensation transport is applied “crudely” in the observations? It would be interesting to discuss what can be done to improve it.

Thanks for highlighting this, we have improved this discussion, properly referenced, removing the ‘crudely’ comment.

References

Swingedouw, D., Braconnot, P., Delecluse, P. et al. Quantifying the AMOC feedbacks during a 2×CO_2 stabilization experiment with land-ice melting. Clim Dyn 29, 521–534 (2007). https://doi.org/10.1007/s00382-007-0250-0

M. Susan Lozier. Deconstructing the Conveyor Belt. Science, 328 (2010). DOI: 10.1126/science.1189250

ANONYMOUS REFEREE #2

This is a very well written paper that will be of great interest to those working on the Atlantic overturning circulation. Research on Atlantic circulation has a strong focus on the MOC metric, that has enabled major advances in our understanding and capability to measure the overturning circulation and meridional transports, but this paper is a timely and important reminder of the importance of other metrics.

There is a lot of information in the paper and many of the 17 figures have multiple panels. Whilst I do not recommend shortening or removing any of the figures, I would discourage lengthening or adding figures during revision.

> We thank Referee#2 for their positive comments. Here is a brief response, a full response to reviews and a revised manuscript will follow in due course.
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> When revising the manuscript, we will try to keep any additional figures to a minimum, though we note that in at least one case Referee#1 has requested additional figures.

I have only a few minor comments:

1. Following equation 5, I think it is correct to ignore the first term, but I am not sure the sentence following the equation explains why (numerically the first term is large, the net transport is small, but sigma_max is large compared to sigma_max minus sigma_min). Observational estimates of the overturning circulation usually impose that the net volume transport is zero, this is partly pragmatic (it would be very difficult to measure) but also because the net transport has little impact on the divergence of heat and freshwater (at least on long timescales). Maybe it would be better just to remove the net transport from the model the same way it is done with observations?

    > We thank the referee for this comment. It highlighted a mistake in the description of the density flux metric and we were also mistaken in our initial response to this comment. We have completely rewritten and corrected this description. The referee is right that the first term on the right of Eq. 5 could be large (indeed the dominant term). We show that with non-zero throughflow, the density flux calculated as the area under the streamfunction curve is equal to the flux of density **relative to reference density sigma_max.** We followed the referee's suggestion and tried removing the net transport from the model, this made very little quantitative, and no qualitative, difference to the results. It can be seen how this might be from original Fig. 1 – the compensation velocity would just show as a thin sliver of extra transport enclosing small additional area.

2. Does equation 1 give the correct sign for the MOC? dz/dsigma is negative, and for densities between sigma_min and sigma_moc the meridional velocity, v, is positive and so integrating up form sigma_min (i.e. dsigma positive) would make the MOC negative. Perhaps the limits on the second integral need to be swapped (similarly for equation 11)

   We have rewritten this whole description, I think we now have the signs correct.

3. In equation (15), and elsewhere, z_sigma(0) looks odd because sigma is never 0. Would it not be better to replace "z_sigma(0)" with "0"?

   We have replaced z_sigma(0) with eta, for clarity and consistency with earlier equations.

REFERENCES

Baker, J.A., Bell, M.J., Jackson, L.C. et al. Continued Atlantic overturning circulation even under climate extremes. Nature 638, 987–994 (2025). https://doi.org/10.1038/s41586-024-08544-0