Review of ESD manuscript egusphere-2025-2653 **-** Historical Climate and Future Projection in the North Atlantic and Arctic: Insights from EC-Earth3 High-Resolution Simulations by KARAMI et al.

In this study, the authors discuss the performance of high-resolution climate model simulations with EC-Earth3, contrasting it to its counterpart used in CMIP6 / HighResMIP and focusing on the Arctic and North Atlantic. In the final section of the paper (Section 4.4), the authors discuss processes driving the weakening of the AMOC that is seen in this EC-Earth3 simulated under the SSP2-4.5 scenario.

Overall impression

The authors thoroughly analyze key features of the simulation, and how these compare to earlier versions and reanalyses. As such, it provides a benchmark for EC Earth users and is a valuable resource for those interested in these regions / this model. The manuscript is clearly written and well-structured.

However, I found section 4.4 very puzzling, i.p the calculation and physical interpretation of the deep water formation (DWF), which the authors claim is a novel way of examining the processes contributing to the AMOC strength and changes therein. In my view, that part therefore requires substantial revision. This holds for the accompanying text in the discussion too (from l.591 onwards).

Overall assessment:

minor revisions up to section 4.3; major revisions for section 4.4.

We sincerely thank the reviewer for her constructive and detailed comments. Our point-by-point response is provided below (our text is in blue). Where appropriate, we quote revised text directly; otherwise, we describe the changes that will be implemented in the revised manuscript. The revised DWF calculation using a uniform 1000 m reference depth has been implemented for the response and is illustrated in the response figures.

<u>Major comments</u>

Throughout section 4.4, the authors appear to mix up the depth (AMOC_z) and density (AMOC_rho) space perspective on AMOC (weakening).

In this paper, AMOC metrics are assessed in the depth perspective [AMOC_z; Fig 8, 9a]. In addition, metrics for density changes at high latitudes are discussed [surface density and DMV; Fig 10, 11], which relate to AMOC_rho but are discussed as if the authors expect a very tight correspondence with the AMOC_z metrics [Fig 12b]. Finally, the authors introduce the DWF metric, which relates to AMOC_z. I have questions about how it is defined / calculated and thus how the presented results should be interpreted.

Our analysis and all AMOC diagnostics presented in the manuscript are defined and interpreted in z-space and we do not intend to conflate AMOC_z and AMOC_ρ metrics. However, we acknowledge that parts of the methodological description and the physical interpretation in section 4.4 may not have been sufficiently explicit, which could give the impression of an assumed one-to-one correspondence. We clarify these aspects and the definition and interpretation of the DWF metric in detail in our point-by-point responses below.

A - AMOC index versus DMV

On l.449-480 the authors introduce and analyze the DMV metric. I have several questions about the chosen approach and (linked to that) the interpretation of the outcomes that I request the authors to respond to.

(1) L.455 please briefly motivate the choice of each of these depths for defining DMV.

In particular, the choice to assess DMV > 1000m for the Irminger Sea seems remarkable as that region is known to generally display shallower mixed layers than the Labrador Sea, resulting in DMV ~ 0 for the Irminger Sea more or less by construction [Fig 11b]. The choice of depth level thus appears to affect the conclusions: on l.461 ["consistently weak convection"] versus l. 472 "progressively shallower" [based on Fig A5b, which uses z=500m rather than 1000m to assess DMV]. To the reader, it remains unclear how the authors in the end interpret the (evolution of) DMV in the Irminger Sea

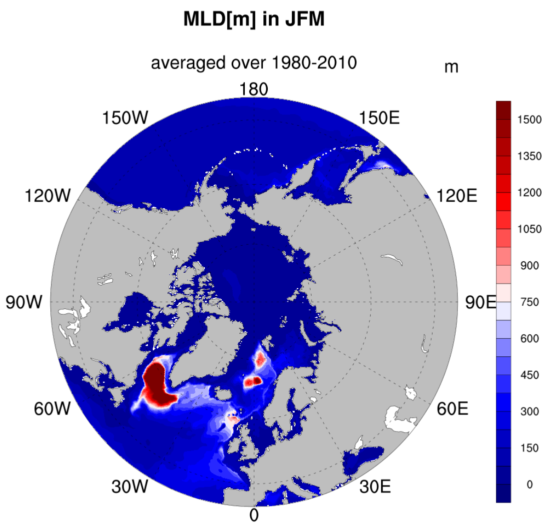
We chose to follow the definition of deep convection adopted by Brodeau & Koenigk (2016), who themselves build on Marshall & Schott (1999). These studies emphasise that there is no universal depth criterion and that the appropriate threshold depends on the renewal depth of the water masses in each basin. For the Labrador Sea, observations support a threshold near 1000 m, while in the Nordic Seas, the relevant depth is determined by the Denmark Strait overflow sill, which motivates the use of 700 m. For the Irminger Sea, however, there is no consensus. While observations show mixed layers that are generally shallower than those in the Labrador Sea, deep events exceeding 1000 m have been reported (Våge et al., 2009; Piron et al., 2017). Furthermore, our EC-Earth3-HR simulation produces mixed layers that are deeper than 1000 m during the historical period, albeit over a limited area (response Figure 1). Using 1000 m does not yield a DMV of zero, but rather isolates only the deepest Irminger events and thus results in weaker values compared with those in the Labrador and Greenland Seas. In the absence of an established, basin-specific threshold, we therefore choose 1000 m as our primary choice.

Clarifications regarding the choice of reference depths used to define DMV have been added to the DMV section of the revised manuscript, as shown below:

> "To compute the DMV, a reference depth must be specified to define deep mixed layers. We follow the definition of deep convection adopted by Brodeau and Koenigk (2016), building on Marshall and Schott (1999), which emphasises that the depth threshold should reflect the renewal depth of water masses in each basin. Accordingly, Brodeau and Koenigk (2016) used an observation-based threshold of 1000 m for the Labrador Sea and a shallower threshold of 700 m in the Nordic Seas, reflecting the depth of the Denmark Strait overflow sill. For the Irminger Sea, however, no consensus threshold exists; while mixed layers are generally shallower than 1000 m, deep convection events exceeding this depth have been reported (Våge et al., 2009; Piron et al., 2017) and are also reproduced in our EC-Earth3-HR simulation. In the absence of a basin-specific criterion, we therefore adopt 1000 m as the reference depth for diagnosing deep convection in the Irminger Sea."

To address the reviewer's concern regarding interpretation, we additionally assessed a shallower threshold (500 m), as already shown in Figure A5 of the submitted manuscript. While the 1000 m threshold isolates only the deepest Irminger events, the 500 m threshold is used to diagnose the evolution of less deep events. We now removed the mentioned sentence as it was not clear and revised the explanation as following:

> “Sensitivity analyses using different critical depth thresholds were also performed with a shallower criterion (500 m; Fig. A5), and the results confirm the robustness of those obtained using the 1000 m criterion discussed above.”



Response Figure 1. EC-Earth3-HR winter mean (JFM) mixed layer depth (MLD, m) averaged over 1980-2010.

(2) On l.464-469 the authors present correlations between the timeseries of the AMOC_z-index and DMV, which are very high for the Labrador and Greenland Seas. While the authors seem to interpret this as a positive and encouraging outcome (l.467), for me it raises questions. It is not explicitly mentioned, but I suspect these correlations have been calculated from the curves in Fig 9a and Fig 11. If so, the high correlation values are merely a result of the fact that both curves display a dominant downward trend. The low correlation for the Irminger Sea is corroborates my suspicion, as that DMV curve has no clear trend. If the curves have not been detrended, the high correlations should not be interpreted as tight physical relations between the two variables.

The correlations were indeed computed from the full, non-detrended time series and were intended to quantify how strongly the long-term AMOC_z weakening is statistically associated with regional DMV changes. In that sense, our focus was on the trend-related co-evolution rather than on variability after detrending. However, we agree that these correlations mainly reflect co-trending and do not demonstrate a tight physical coupling, and they are not essential for our main conclusions. To avoid potential overinterpretation, we have therefore removed this correlation analysis for DMV in the revised manuscript.

To be honest, I would be rather worried if a model has an almost one-one-one positive correlation between variations in DMV [the volume of dense water present] and the AMOC_z strength. DMV is the net outcome of formation (which acts to increase DMV) and export (decreases DMV). AMOC_z, in contrast, quantifies the overall sinking that occurs north of where its index-value is taken. Deep convection represents strong vertical mixing / densification of waters and is associated with hardly any mean vertical motion – where the water actually sinks is governed by vorticity dynamics (see for example Spall and Pickart 2001; Marotzke and Scott 2002; Katsman et al 2018, Bruggemann and Katsman 2019) so these are physically different things

We thank the reviewer for this very helpful physical clarification and for pointing us to the relevant literature. We fully agree that DMV and AMOC_z quantify fundamentally different physical processes: DMV reflects the net balance between dense water formation and export, whereas AMOC_z represents the integrated overturning and is not directly tied to local vertical sinking associated with deep convection. We also agree that a near one-to-one correspondence between the two would indeed be physically questionable. Our original intention was not to imply such a direct mechanistic coupling, but rather to explore their statistical co-evolution under long-term forcing. In line with our response above, this correlation analysis is therefore no longer included in the revised manuscript to avoid any misleading physical interpretation.

(3) L.468: the outcome that the strongest correlation occurs at lag zero is something that I would not expect either. While it is obvious that densification of waters at subpolar latitudes is a necessary ingredient for AMOC_z/rho it is not sufficient: these dense waters also need to be exported southward to contribute. Since DMV is assessed at high latitudes, even if it was related one-on-one to AMOC_z I would expect some time lag between their signals.

We agree that, from a dynamical perspective, a time lag between high-latitude densification and the AMOC response is generally expected, since dense waters must first be exported southward to contribute to the overturning circulation. The absence of a clear lag in our original analysis could arise from the fact that the correlations were computed using the full, non-detrended time series and are therefore dominated by the common forced trend rather than by propagated variability. When focusing on detrended signals or non-transient simulations, a lead–lag relationship between DMV and AMOC does emerge, consistent with previous findings (Fig. 14 in Brodeau and Koenigk, 2016). In line with our response above, we therefore no longer include this correlation analysis in the revised manuscript to avoid potential misinterpretation.

B - Definition of DWF

On l.486-500 the authors introduce and analyze the DWF metric. I also have several questions on this.

(1) To calculate DWF, the volume budget within a 2-layer system is considered, with the boundary between the upper and lower layers chosen at a fixed depth [l.494]
- Why is this fixed depth now referred to as the critical depth? What is critical about it?
- The chosen depths are “as defined above” – I interpret this as the depths given on l.455 so 1000m for Irminger and Labrador Seas, 700m for Greenland Sea. Is that correct?

In the original manuscript, the term *critical depth* was adopted from from Brodeau and Koenigk (2016); however, it was not intended to imply a dynamical threshold and could therefore be misleading in this context. In the revised version, we no longer use this terminology and instead refer simply to a “reference depth”.

The reviewer is also correct that the reference depths in the original submission were 1000 m for the Labrador and Irminger Seas and 700 m for the Greenland Sea, following the choices used for the DMV metric. As explained in our reply to comment (2) below, we now adopt a single, uniform reference depth of 1000 m for all regions to ensure a consistent depth-space framework.

(2) The volume budget for each layer is considered for the three regions defined in Fig 9b, which have some lateral boundaries in common over which exchanges are assessed. If my deduction above that depths separating upper and lower layers in the three regions differ is correct, - How are transports at depths 700-1000m between the Irminger and Greenland Sea handled? Do they end up in a different layer by construction? If so what does that represent physically? What impact does it have on DWF calculated in this way?

- The sketch in Fig 12a does not reflect this difference in layer thickness between the regions; if that is what is done I think it would make sense to visualize it as well
- If DWF is indeed calculated with differing depths for the different regions and the authors can explain and motivate this, I would like to see a discussion of how the results should be interpreted. Do the differing definitions of the 2-layer system make Fig 12b an 'apples and oranges comparison' for the DWF curves? In particular, the study by Sayol et al (2019) showed that the depth at which North Atlantic sinking peaks varies by region – how do they relate their results to this?

To avoid any misunderstanding, we clarify that the two-layer framework is used to diagnose regional volume budgets across a fixed reference depth, based on transports evaluated at the gateway or boundary sections. The two-layer split is therefore not meant to imply an explicit vertical structure within the interior of the boxes, but provides a consistent depth-space framework for closing the volume budget using boundary exchanges. Transports at shared boundaries (e.g. between the Irminger and Greenland Seas) are always evaluated using the same reference depth at a given section and are therefore identical.

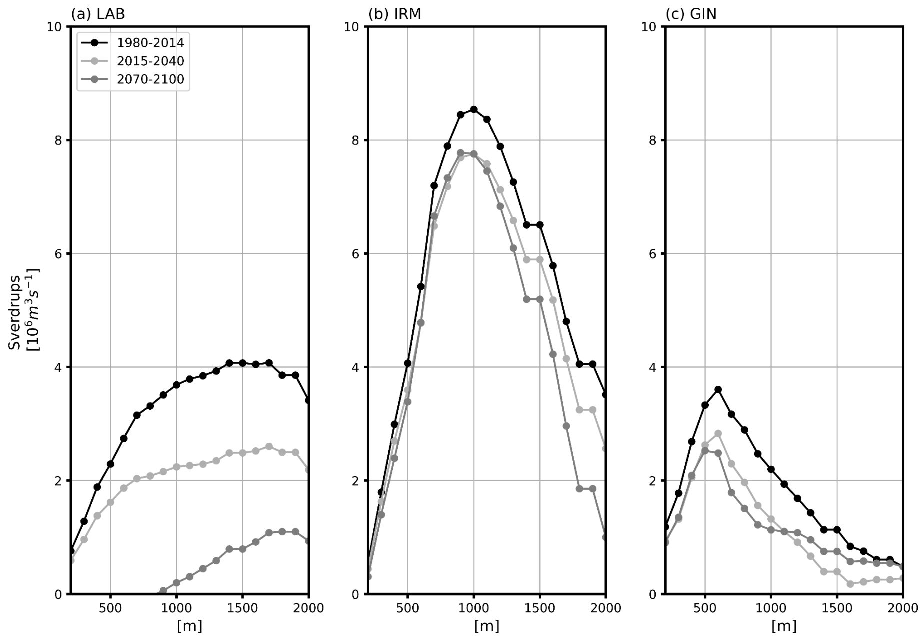
In this study, we construct a volume-budget diagnostic formulated in depth space. For each region, we ask the following question: given the horizontal inflow above a chosen reference depth and the horizontal outflow below that depth at the region's boundaries, what net downward transfer across that depth is required by volume conservation within the region? This residual defines our depth-space deep water formation (DWF) index. This diagnostic does not imply that sinking is necessarily maximised at the reference depth; rather, it provides a budget-consistent measure of the net downward volume transfer across that depth surface.

We agree with the reviewer that the interpretation of DWF index becomes ambiguous if different reference depths are used for different regions, as a two-layer volume budget only closes cleanly if all boundary exchanges for a given region are expressed relative to the same interface depth. To remove this ambiguity and to ensure direct comparability across regions, **we therefore adopt a single, uniform reference depth of 1000 m for all gateway and section transports in the revised manuscript**. With this choice, all transports are consistently partitioned into 0–1000 m and 1000 m–bottom contributions. This brings the method into closer alignment with common depth-space formulations of the overturning circulation, for which transports near 1000 m are often used to characterise the depth of the AMOC_z maximum. It should be noted that if a boundary section is shallower than 1000 m, transport below 1000 m is zero by geometry, and contribution is only to the net top flow.

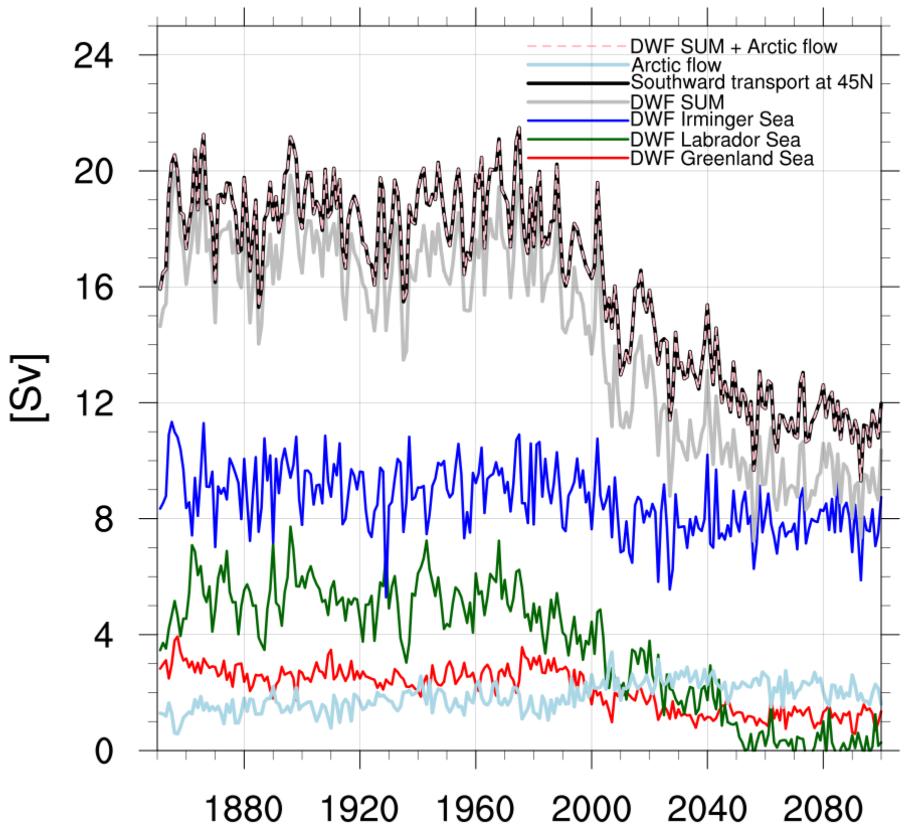
Additionally we performed sensitivity tests using several alternative uniform reference depths (**Response Figure 2**). These tests show that the depth at which the DWF index attains its maximum varies by region—around 600 m in the GIN Sea, near 1000 m in the Irminger Sea, and closer to 1500 m in the Labrador Sea, and remain relatively stable over time, with only a slight upward shift in the Labrador Sea. This is consistent with Sayol et al. (2019), who demonstrated that the depth of maximum sinking varies regionally in the North Atlantic. However, as our primary aim is to decompose the AMOC_z volume budget rather than to identify the precise depth of water-mass transformation in each basin, we retain 1000 m as a pragmatic, common reference depth for the main analysis. Under this choice, the deviation from the local maximum DWF is small in the Labrador Sea but larger in the GIN Sea.

Importantly, the overall conclusions drawn from Figure 12 of the submitted manuscript are unaffected by this choice and remain robust under the revised formulation using a uniform reference depth of 1000 m (Response Figure 3). While the magnitude of the DWF index increases slightly in the Irminger Sea and decreases in the GIN Sea when adopting a uniform 1000 m reference depth, the projected future changes are much less sensitive to the choice of reference depth. Response Figure 2 further confirms that the projected reduction in DWF toward the future is robust across different choices of reference depth. The consistent temporal evolution across reference depths indicates that the signals discussed in this section reflect changes in the overturning-related volume budget rather than artefacts of the layer definition.

These clarifications regarding the DWF methodology, the choice of a uniform 1000 m reference depth, and the resulting updated values will be incorporated into the revised manuscript. Figure 12b is already updated, and Figure 12a will be updated accordingly. We will also document the associated depth sensitivity, as shown in Response Fig. 2, in the supplementary material.



Response Figure 2. Sensitivity of the deep-water formation (DWF) index to the choice of reference depth for the Labrador (LAB), Irminger (IRM), and GIN seas, shown for the historical period (mean over 1980–2014) and two future periods (mean over 2015–2040 and 2070–2100). While the magnitude of the DWF index depends on the reference depth, the relative regional contrasts and projected future changes are robust.



Response Figure 3. Updated version of Fig. 12b in the original manuscript; in this figure, all transports and DWFs are computed using a uniform reference depth of 1000 m. Time series of southward deep transport at 45°N (black) and deep water formation (DWF) rates in the Irminger (blue), Labrador (green), and Greenland (red) seas, Arctic flow below 1000m (light blue), together with sum of all DWFs (DWF SUM in grey) and DWF SUM plus deep Arctic flow (pink dashed), from 1850 to 2100 in EC-Earth3-HR. The close agreement between the southward transport at 45°N with DWF SUM plus Arctic flow demonstrates volume conservation of the system. All values are in Sverdrups (Sv).

(3) DWF is defined as the residual of the horizontal transports into/out of a certain region. Assuming mass conservation, DWF is the net vertical transport [i.e I would define it as the regional contribution to AMOC_z at these latitudes]. Notably, DWF is assessed at the depth separating the two layers in that region.

- The authors seem agree that that is what they calculate [l.497] but add an interpretation [l.498] that I do not understand. Please elaborate, in particular for the Greenland Sea. In my view, water that sinks below 700m there does not automatically contribute to sustaining AMOC_z as it needs to cross the Greenland-Scotland Ridge somehow for this
- The above definition of DWF as a residual assuming mass conservation could be made more explicit

We thank the reviewer for helping us to refine our terminology. As clarified in our response to comment (2), the DWF diagnostic used in this study represents the net vertical transport across a fixed reference depth implied by the horizontal volume transports into and out of each region. In other words, it is the residual of the regional horizontal volume budget and corresponds to that region's contribution to the overturning circulation *in depth space* (AMOC_z) at that latitude.

We have revised the sentence as follows:

> "This pattern indicates a net vertical transport across the reference depth, which we interpret as a diagnostic of regional contributions to the overturning circulation."

The reviewer is also correct that our original phrasing ("feeds into and sustains the AMOC") was too strong. The diagnostic quantifies **downward volume convergence across the reference depth**, not diapycnal water-mass transformation or the physical locations where dense waters sink. As the reviewer notes, in the Greenland Sea only a fraction of the water is exported across the Greenland–Scotland Ridge; the remainder may recirculate or be influenced by Arctic inflows. The DWF index should therefore be interpreted as a **regional decomposition of the AMOC_z volume budget**, rather than as implying that all locally diagnosed downward transfer contributes directly to the southward deep limb.

To avoid misunderstanding, the corresponding sentence in the manuscript has been revised to:

> "This pattern indicates a net vertical transport across the reference depth, which we interpret as a diagnostic of regional contributions to the overturning circulation."

(4) On l.515 it is stated that 'the sum of the DWF rates plus the Arctic deep flow into the Greenland Sea equals the net deep southward flow at 45N'.
- Why is this "Arctic deep flow into the Greenland Sea" [2.2 Sv, orange arrow in Fig 12b] not automatically incorporated in the regional volume budget? [according to l.494 the lower layer reaches to the sea floor]

When the three subpolar regions (Labrador, Irminger and Greenland Seas) are considered collectively, they constitute a larger control volume whose southern boundary is defined by the section at 45°N, and its northern boundaries are the Arctic gateways (Fram Strait, the Barents Sea opening, and the northern boundary of our Labrador box). By volume conservation, the deep southward transport across 45°N (black line in response Figure 3) must balance:

1. sum of our DWF indices (DWF SUM; grey line in Response Figure 3) and
2. net deep inflow from the Arctic below 1000m (light blue line in Response Figure 3)

In descriptive form, this relationship can be written as:
deep transport at 45°N = DWF in Labrador + DWF in Irminger + DWF in Greenland + deep net flow (below 1000 m) from the Arctic. To demonstrate this, we refer to the Response Figure 3.

In this framework, the Arctic deep flow closes the volume budget of the three-box system, and it therefore illustrated as a distinct lower-layer source in the combined budget. Fram Strait is the main gateway that provides appreciable exchange below 1000 m, while Denmark Strait and the Barents Sea Opening are shallower and contribute exclusively to the upper-layer budget.

We will clarify this point in the revised manuscript by explicitly including the relevant transports, as shown in Response Figure 3, and by adding the following text: "This balance follows from volume conservation applied to the combined subpolar control volume, whose southern boundary is at 45°N and whose northern boundaries are the Arctic gateways."

(5) On l. 531-541 the authors present again correlations using timeseries with trends. The reservations expressed in remark A2 hold here as well

As with the DMV correlations discussed in Comment A2, the correlations between DWF and the AMOC index in the original manuscript were computed from the full (non-detrended) time series and therefore primarily reflect the common long-term forced trend. Our intention was not to infer a tight dynamical coupling, but to assess whether the depth-space volume-budget diagnostic (summed regional DWF indices) evolves consistently with the AMOC_z index under anthropogenic forcing. We note that this point is already stated in the original manuscript, where we report the substantially weaker correlations over 1850–1980 to illustrate that the strong values over 1850–2100 are dominated by the forced trend.

To avoid any misunderstanding, we revised the sentence at line 531 as following:

> "In addition to these mean changes, the temporal covariability between DWF and the AMOC index is examined."

(6) L.540 "critical role…": I do not think this statement ["strong relationship", "modulates"] makes sense. If calculated for a consistent depth level and for a set of regions covering all sinking regions, the AMOC_z index and the summed DWFs should match, simply from mass conservation. Any mismatch should be attributable to transports between Atlantic and Pacific via Bering Strait (Katsman et al 2018). However, if DWF is calculated at different depths in different regions, this no longer holds as the sinking does not peak at the same depth everywhere (Sayol et al 2019)

We hope that the clarifications provided in our responses above address the reviewer's concerns and make clear that, within our framework, volume conservation is maintained even when different reference depths are used for the individual basins. Nevertheless, as noted in our responses to comments (1) and (2), we now adopt a uniform reference depth of 1000 m across all regions.

The reviewer is correct that this statement was an overinterpretation based solely on a statistical correlation. We, therefore, removed this sentence.

(7) Since DWF is assessed using a 2-layer system defined in depth space, it can provide information on the regional AMOC_z contribution only [=net regional sinking].
  - The papers referred to on l. 519-520 consider AMOC_rho, which at high latitudes, where the actual water mass transformation takes place, differs from AMOC_z (see references given earlier). I therefore think the DWF results should not be linked to these particular studies – something else is assessed here.
  - I think the authors are overselling their results referring to it as giving insight in processes / drivers of AMOC weakening [l.32, l.423, l.528, l.532] – in my view the study provides a regional decomposition of the (evolution of the) sinking

We agree that our diagnostic, being formulated entirely in depth space, quantifies **regional contributions to AMOC_z** and does not capture water-mass transformation pathways or isopycnal overturning as discussed in AMOC_ρ studies. The reviewer is therefore correct that we should be careful when comparing to other studies based on density-space diagnostics.

With regards to the second point, we agree that our analysis is primarily diagnostic and provides a regional, depth-space decomposition of AMOC_z rather than isolating individual physical mechanisms in a causal sense. To avoid overstatement, we will revise the text at l.32, l.423, l.528, and l.532 to replace references to "drivers" or "processes" with more precise language emphasising regional decomposition and volume-budget diagnostics. We note, however, that the framework does provide insight into how individual subpolar basins and the Arctic contribution project onto the large-scale overturning, thereby clarifying the structural origins of the simulated AMOC weakening.

References:

- Spall, M. A., & Pickart, R. S. (2001). Where does dense water sink? A subpolar gyre example. *Journal of Physical Oceanography*, *31*(3), 810-826.
- Scott, J. R., & Marotzke, J. (2002). The location of diapycnal mixing and the meridional overturning circulation. *Journal of Physical Oceanography*, *32*(12), 3578-3595. - Katsman, C. A., Drijfhout, S. S., Dijkstra, H. A., & Spall, M. A. (2018). Sinking of dense North Atlantic waters in a global ocean model: Location and controls. *Journal of Geophysical Research: Oceans*, *123*(5), 3563-3576.
- Brüggemann, N., & Katsman, C. A. (2019). Dynamics of downwelling in an eddying marginal sea: Contrasting the Eulerian and the isopycnal perspective. *Journal of Physical Oceanography*, *49*(11), 3017-3035
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<u>Minor comments / questions</u>

1. Section 1/2 – several places it is stated that model results are "better" but it is not made explicit in comparison to what; some examples are l.83; l.86, l.139 [the answer is probably what is written on l.184]

We have improved these in the revised version.

2. Throughout the text: I think it would help the reader if the authors provide more detailed guidance on what to look focus on in the figures, pointing to explicit figure panels and or lines; some examples are l.187-196 – which panel, l.303/Fig 6a - which line

We will consider this in the revised version.

3. L. 68: 'and the processes behind the simulated weakening' – what is this statement based on?

If this was meant for line 64, we will try to be specific or remove the statement.

4. L.287 A brief summary of section 3 would be in place here

Brief summary is included in the Discussion.

5. L.379: 'indicating that…' – what is this statement based on?

This is explained in the sentence before.

6. L.471: sentence is incomplete; also I think the fact that Fig A5 shows area not volume as Fig 11 could be emphasized more

We were unable to identify the incomplete sentence referred to in line 471 and would appreciate clarification if possible. Regarding Fig. A5, the figure intentionally shows area rather than volume, as this representation brings the different regions onto a more comparable range on the y-axis and facilitates visual comparison between the boxes.

7. L.478 Greenland Sea convection shuts down – this seems at odds with Fig 7d that shows that the surface density increases in the region where I would expect convection. Please clarify / show MLDs

In Response Figure 1, we show the MLD. While there are indeed regions in the Greenland Sea where the MLD increases, this is on the order of 200–300 m. These depths are shallower than those required for deep or dense-mode convection and therefore do not contribute to DMV used in our analysis.

Figures

- Figure 6: legends are very small

Will be adjusted

- Figures 7 / 9 / 10:
    - o In light of what is discussed in section 4.4, I think it would be useful to add maps of MLD itself for the 2 periods in the Appendix

      Yes, we can include the figure like Response Figure 1 in the revised version
    - o Fig 9b: density change - unclear at which depth this is

      At the surface
    - o Why are different months/periods used for Fig 7 [=JFM], Fig 9b [=unspecified "wintertime"] and Fig 10 [DJF] ?

      Same JFM was used for all and Figure 10 caption will be corrected.

- Figure 10: the statement on the evolution made on l.434 may be easier to substantiate by also [potentially in the Appendix] showing rho(z,t)-rho(z,year=1850)

While this would be a useful additional diagnostic, including all such analyses would substantially expand the number of figures without changing the conclusions. We therefore do not add this figure, but try to better clarify the text around line 434.

- Figure 11: indicate in caption that chosen depth levels for defining DMV differ [info on l.455]

Will be added in the revised text.

Text suggestions / typos

- L.48 suggests
- L.54 surface temperature
- L.67 which not sure what is refers to; I think now it is AMOC? Is that what is meant? - L.81 verb missing in 2nd half sentence
- L.82 this sounds odd – T, S profiles get better but convection properties are not OK? - L.86 shift in response to what?
- L.96 some of these?
- L.127 reasonably reproduce
- L.243 CAA not introduced
- L.255 sentence not correct [and vague]
- L.339 very vague formulation – I think this can be made more explicit
- L.362 Atl Water into the region
- L.370 SSD anomaly
- l.381 [is linked to / contributes to] and l.398 [is consistent with] it is not explicit on how

strong the authors expect this link / connection between AMOC strength and MLD reduction to be - L.393 and elsewhere: I think streamfunction is one word / no space
- L.411 seems at odds with l.394
- l.424 statement ignores the fact that formation of dense water is not sufficient for AMOC contribution – the dense water also needs to flow southward
- L.426 Fig 9b shows surface density; Fig 7c shows there are large changes in that in the Arctic too; text speaks about convection / deep water formation [which is not in Fig 9b] - L.438 retreat seems an odd word to use here
- L.445 specification that this is winter / DJF belongs right at beginning of the caption; adjust figure label/title too
- L.449 to for?
- L.452 sentence incomplete – add 'and' before thus?
- L.467 'more closely linked' – more closely than what?
- L.473 sentence not correctly formulated, please rephrase – DMV based on both shallow and..? - L.488 relative contribution to what?

All typographical errors and minor textual corrections noted in this comment will be incorporated in the revised manuscript.

References

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