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.

<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.

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

(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.
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

(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.

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?

(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?

(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

(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]

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

(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)

(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

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

- Sayol, J. M., Dijkstra, H., & Katsman, C. (2019). Seasonal and regional variations of sinking in the subpolar North Atlantic from a high-resolution ocean model. *Ocean Science*, *15*(4), 1033-1053

Minor comments / questions

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]
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
3. L. 68: 'and the processes behind the simulated weakening' – what is this statement based on?
4. L.287 A brief summary of section 3 would be in place here
5. L.379: 'indicating that…' – what is this statement based on?
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
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

Figures

- Figure 6: legends are very small
- Figures 7 / 9 / 10:
  - 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
  - Fig 9b: density change - unclear at which depth this is
  - Why are different months/periods used for Fig 7 [=JFM], Fig 9b [=unspecified "wintertime"] and Fig 10 [DJF] ?
- 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)
- Figure 11: indicate in caption that chosen depth levels for defining DMV differ [info on l.455]

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?