Second 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.

Since in my first review I mainly had remarks on section 4.4, I focused on that part of the paper [and the accompanying text in the abstract / introduction / discussion] in this second review

Line numbers refer to the revised manuscript

Overall impression and assessment
I am happy to see the authors have taken my criticism of this section to heart, and have revised it thoroughly. In particular, the section on their volume-conserving budget for deep water formation is now correct.

I only have minor remaining comments / questions on section 4.4, mostly requests for rephrasing, which should be relatively easy to address.

A final point I want to make is that I disagree with the claim the authors (still) make that they have developed a novel method. From my 1st review it became clear that their approach has been used before. While the paper now actually cites some of these earlier papers, they should in my view more properly acknowledge this by (i) also adding citations to the older literature on this topic [i.e. adding the papers on which Katsman et al 2018 and Sayol et al 2019 build] and (ii) refrain from claiming novelty in this respect.

Section 4.4

1) l.454 Although I appreciate that the authors altered the title of section 4.4 in line with my comment, I find the new version ("Factors associated with the weakening of the AMOC") rather uninformative. Can the authors come up with a phrase that better represents this section's contents?

On the reference depth / DMV analysis:
2) L.492: the statement that the reference depth in the Greenland Sea [700 m] is motivated by the depth of the Denmark Strait sill does not seem in line with the statement directly above it [l.490] that the reference depth should reflect the renewal depth of water masses, as the former is a geographical feature and the latter is a physical feature. Please clarify this in the text
3) L.493-496 I do not find the presented argumentation for choosing a reference depth of 1000m for the Irminger Sea logical. Following l.492, it would make sense to me to base the (regional) reference depths on the (present-day) stratification – for renewal of dense water volume, convection needs to be able "punch through" the stratified layer that is present in fall. The observations generally show shallower MLDs in the Irminger Sea than in the Labrador Sea, which suggests using 1000m underestimates DMV in the Irm Sea, and thus that a shallower reference depth is more appropriate. The authors continue with the statement that ECEarth reproduces this observation [l.494], i.e that the model's MLs are shallowr in the Irm than the Lab Sea, implying that also in the context of the model simulation h=1000m yields an underestimation of DMV and is thus a suboptimal choice. If EC Earth consistently simulates deeper mixed layers than the observations, using 1000m might be justified, but that is not what is written (and it does not look like that is the case - Response Fig 1). Please clarify the text.

4) L.498 - the author state the change in DMV in the Lab Sea is “most pronounced”, as it declines to zero by 2050. However, in that same sentence it is noted that DMV in the Greenland Sea reaches zero earlier in time, which raises the question what “most pronounced” means? The fastest decline? The largest change in m3 of DMV (which seems ambiguous as m3 depends strongly on the chosen reference depth)
5) L.500-501: this needs to be formulated more precisely: when using h=1000m, one can only make the statement that convection rarely exceeds that depth early in the timeseries and that this becomes even less frequent as time progresses m, mmn,m,m,m,nm,m,n m,m,nn, c; by comparing with panel c one can say the convection is weaker than in the Lab Sea rather than generally “weak”
6) L.504 “confirm robustness” – since A5 shows a different variable I do not think this statement is justified; in my view Fig A5b / A5c show that the convection region is more cone-shaped in the Irm Sea and more cylindrical in the Lab Sea; interesting information (and in line with Response Fig 1) but not a confirmation that the conclusions drawn about the evolution of DMV (Fig 10) are not sensitive to the choice of the reference depth – such a statement should be based on repeating the analysis with only a different h
7) L.505-510: this comparison implicitly assumes that the chosen reference depths are also appropriate for EC-Earth-SR. Referring to remark 3), in my view it thus needs to be accompanied by some info on that aspect – discussing the evolution of DMV compared to HR only makes sense if this index correctly captures the convection to begin with [l.486]

On the DWF analysis:

8) L.l.572 this formulation is not correct, the analysis does not assess **water mass transformation** = diapycnal mixing but **dense water formation** as the authors define earlier [personal view: because the terms are so alike and thus easily confused, I much prefer sinking / vertical transport for the variable that is analyzed here]
9) L.584 I think it is worth mentioning that this vertical transport may be partly attributable to waters descending over the Denmark Strait sill [so not necessarily driven by local processes]

<u>Abstract / Introduction / Discussion</u>

The parts of the text referring to Section 4.4 have hardly been altered while the contents of the section it summarizes are very different in the new manuscript version. This needs to be better aligned, in particular rephrasing the following:

- l. 31 the authors they claim to introduce a novel method, I do not think this is appropriate – the DMV analysis builds heavily on the Brodeau and Koenigk (2016) paper, and the DWF analysis is the same as used in Katsman et al (2018) and Sayol et al (2019) - papers that they now cite. The new part is that they apply the latter approach to a climate scenario simulation;
- L.33 is easily misunderstood as “processes in basin X are responsible for… ” – this is not a good representation of the analyses in the paper. What is shown / analyzed is how the vertical transports at h=1000m change in various regions in response to climate warming.
- l.103 repeats the claim of a new method
- L.645 “However, while the DMV index effectively captures… “ – please re-evaluate based on remarks below if this statemen can be made like this
- L.646 “… it does not directly quantify…. “ – this sentence still suggests convection and dense water formation / sinking are the same thing
- L.649 “indicating that DMV and MLD..” – this is not at all new finding as the authors seem to suggest here; it needs to be rephrased as more certain [”confirming once more … ”] + references added
- L.651-653: again the claim the method is developed by the authors and novel – which I do not think is correct, see above

Typos / text suggestions

- l. 478-479 caption refers to fig 9b twice
- l.529 Denmark Strait ~~overflow~~ sill
- L.503 can be more concisely rephrased as “sensitivity analyses using a shallower criterion”
- L.505 grammatical error - DMV [a volume] cannot be stronger
- L.530 please give proper credit to earlier papers emphasizing the difference between convection and siking [see my first review]
- L.562 a depth-specific