

## **Stratification and mixed layer depth around Iceland, characterization and inter-annual variability**

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The revised version of this manuscript is a substantial improvement on the original submission, in particular the more robust and quantitative statistical analyses. However, my major comments from the first round of reviews have, at best, been partially addressed. My concerns regarding the one-dimensional simulations, in particular, remain. I think the simulations have great potential to identify which forcing mechanisms are important for deepening the mixed layer and to distinguish the responses to cooling by  $\alpha$ - and  $\beta$ -oceans, but I am not convinced that the current simulations achieve that objective. As such, I recommend that the manuscript must be revised before it can be accepted for publication.

### **Major comments:**

In the first review I requested more details about the PWP model (Price *et al.*, 1986) in Sections 2 (Data and methods) and 4 (MLD driving mechanisms from a 1D model). I think it is important for the reader to gain a basic understanding of how the model functions and for more clarity regarding the initial conditions and forcing period. From the text and Figure 7, I glean that the model is initialized using the mean February profile and integrated through July, but then the description of the mixed-layer development in Section 4 is misleading. The model's wintertime MLD must necessarily be consistent with observations since you compare with the MLD of the initial profiles. I suggest instead to use the fall profiles as initial conditions and integrate the model to February, when you have the next set of profiles. That would give you a better opportunity to quantify the importance of the different forcing mechanisms and distinguish the responses of *alpha*- and *beta*-oceans, and then you could use the winter observations for direct comparison with the model's final profiles and mixed-layer depth at the end of the simulation. Applying the winter profiles as initial conditions and then simulating the mixed-layer development through restratification and well into summer, does not capitalize on the PWP model's strengths.

More importantly, my concern about the substantial impact of freshwater forcing on stations FX9 and ST5 has not at all been addressed. From the first review: *Freshwater fluxes are usually of lesser importance for the mixed-layer development and tend to restratify the mixed layer (i.e., have a stabilizing influence) (e.g., Brakstad et al., 2019), yet on these two stations they appear to cause a mixed-layer deepening of more than 300 m, which would require substantial evaporation in a region where precipitation generally dominates. At these two stations, wind-mixing is also surprisingly deep. Please confirm that the model simulates the mixed-layer development correctly and expand the discussion about these surprising results.* This matters because the impact of the freshwater fluxes on the the development of the mixed-layer depth is not intuitive, possibly also not physical unless the evaporation rate is very high. If these results are presented without further discussion and verification, readers may doubt the realism of the entire set of PWP simulations.

Shown separately in Figure 7, are the MLD developments resulting from the driving mechanisms freshwater, heat fluxes, and wind stress additive? There are probably some non-linear interactions, so would be good to also show the MLD development for the full simulations with all driving mechanisms as a separate trace in the figure. Please keep in mind that other than through evaporation and brine release, freshwater forcing will restratify the mixed layer, so I think you need to revisit this figure.

Additional verification of your MLD routine using independent glider data was a good idea. However, the sentence in the manuscript may be misleading. The comparison amounts only to about 30 data points during a period of active convection, while the glider profiles likely number in the hundreds over a variety of conditions. Specifically, it does not address the existence of stacked mixed layers which are prevalent in the Nordic Seas and Subpolar Gyre in winter (I'm not concerned about the summer mixed layers). Rather than the additional verification, I suggest instead to include a few sentences in the methods or discussion sections to address this limitation of automatic MLD detection routines.

I sympathize with the request for a better set of water mass definitions for Icelandic waters, but apologize for not having any to offer. My concern is not only that the original definitions were primarily developed for the Arctic Ocean, but also that over the nearly 30 years that the data set spans, the water masses will have changed substantially. For the most part, this may not be a major concern – Atlantic and Polar water masses are regardless clearly distinct. But the colder, denser water masses are in close proximity in TS-space, and applying constant limits in temperature and salinity to distinguish these changing water masses may be misleading. The reason why this is important is because the classification into a specific water mass has wide-ranging implications about where the water mass originates and the processes that lead to its formation. I think that adding a sentence about possible biased estimates to the revised manuscript was good and partly addresses this concern, but I also suggest to elaborate on how the results may be biased and to exercise a bit more caution when discussing the origin of different water masses based solely on these water mass definitions.

#### **General comments:**

I do not disagree that the expansion of temperature-stratified *alpha*-ocean conditions into the waters north of Iceland may fall under the Atlantification umbrella. But I still think that the paragraph appearing on line 64 in the introduction appears unmotivated and without a clear purpose. A decrease in sea-ice extent and a weakening of the cold halocline are not relevant for the central Iceland Sea. To demonstrate why this may be important also for the Iceland Sea, perhaps show that the concept has been applied to the central Greenland Sea and northeastern shelf (Gjelstrup *et al.*, 2022; Strehl *et al.*, 2024) and allude to similar changes taking place in the Iceland Sea.

Good that you tried to increase the number of stations! Even if the larger data set was more challenging to report on, it would be great to add a few sentences to the manuscript to mention that other stations were also considered and whether they corroborate the results from the deepest stations.

Figure 5 and elsewhere:

Nice re-design of the figure, that was a substantial improvement. But I would still like to see the mixed-layer density. This is what matters in terms of contributions to the overturning circulation, not mixed-layer depth.

#### **Detailed comments:**

Line 16 (*unanswered comment*):

Dense-water formation is generally considered a necessary condition for a global overturning circulation rather than a driving mechanism (e.g., Kuhlbrodt *et al.*, 2007).

Line 40:

I do not think Petit *et al.* (2020) is an appropriate reference for wintertime mixed-layer depths in the Nordic Seas. That paper focuses on the Irminger Sea and Iceland Basin.

Line 47:

The cooling and densification of AW takes place primarily in the eastern part of the Nordic Seas. Mauritzen (1996) and Huang *et al.* (2023) would be more appropriate references here than Våge *et al.* (2015, 2018).

Line 56:

You write that wind stress and sea-ice retreat drive transformation east of Greenland. Sea-ice retreat is a necessary condition for water mass transformation to occur. Wind stress helps precondition for water mass transformation by shifting the Polar Front toward the west, and may also be important during water mass transformation. But the high heat loss that occurs during cold-air outbreaks is surely a main factor driving water mass transformation.

Line 61:

I still do not think it is clear what you mean when you write “from the Arctic”. Unless specified, this is ambiguous and can be interpreted many different ways.

Line 62:

Please introduce the acronym EGC when the East Greenland Current is first mentioned in the text (line 50).

Line 70: (*unanswered comment*):

Please be more specific, under what conditions does wind forcing enhance turbulent mixing that deepens the mixed layer? The following sentence does not add clarification regarding the impact of wind forcing, even though it alludes to an example of this process.

Line 91:

The Nordic Seas, not the Arctic Ocean, is located north and northwest of Iceland.

Line 93:

Please be consistent regarding the capitalization of cardinal directions (e.g., lines 91 and 93).

Line 159:

There should be a comma after diurnal heating/cooling.

Line 253:

Do you mean “second freshest”?

Line 303:

The Future *et al.* (2022) paper is missing from the reference list.

Line 311:

In this sentence you state that MLDs are shallow because the stratification is high. I disagree. I think mixed layers in summer are shallow primarily because the buoyancy forcing abates in spring and becomes largely positive in summer and that this is a main reason why stratification is high. Please rephrase.

Line 316:

The shoaling of the mixed layer would be caused by **reduced** heat fluxes.

Line 498:

My interpretation of this sentence is that deeper convection caused by Atlantification may result in the Iceland Sea becoming a source of dense water to the NIJ. Please rephrase if that is not your intended meaning. I agree that Atlantification, in principle, can lead to deeper convection. But in the central Iceland Sea, reduced atmospheric forcing has led to shallower and less dense, not deeper mixed layers.

## References

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