Reviewer 1

In this revision of the manuscript submitted earlier in 2025, the authors have made a number of revisions, notably improving the statistics to demonstrate the robustness of their trend analysis. These revisions are helpful and clarify the impact of the results. I continue to think that the manuscript contains a valuable set of analyses that will be of interest to the oceanographic research community. In my original review, I also specified, “In a rewriting, the manuscript should be more tightly focused to identify clear and linked results, ….” In the revision, the authors have not (yet) made significant revisions to help readers connect the results. Thus, I have further suggestions aimed at making this manuscript more impactful for readers.

1. Introduction. The manuscript would benefit from a rewriting of the introduction to clearly specify the objectives of this study. In the present version and in the original submission, the major articulation of the manuscript objectives seems to appear in the final sentence of Section 1: “In this study, by using a long time series of hydrographic observations around Iceland spanning 29 years, we aim at unraveling the different scales of variability of the water-mass properties, MLD, and stratification around Iceland.” The manuscript in fact has a broader scope than this, addressing trends, correlation with the NAO, and vertical and horizontal patterns of variability. The introduction should be reframed so that it explains to readers what questions will be addressed and why these questions make sense as a package contained within a single manuscript. As a general rule of thumb, the questions answered in the conclusions section of the manuscript should be posed in the introduction.

We thank the reviewer for following up with this important point. We have tried now to re-write the introduction including the NAO on it and carefully verifying that the material in the conclusions is included in the introduction.

2. Cardinal directions. Oddly, one of the challenges for me as a reader comes in capitalization of regions. In general, in English, directions should be capitalized if they refer to specific places (e.g. “The North” as a specific named region) but not as a general direction (e.g. “hydrographic stations north of Iceland”). In the manuscript, regions are sometimes capitalized and sometimes not. The abstract specifies three important regions: the South, the North, and the Northeast. This makes sense, so in reading the manuscript, I sought a clear definition of the three regions. But the manuscript discussion turns out to be more complicated. At line 23, the abstract mentions the Northwest. I wasn’t sure if this was a typo or a new specific location. The authors have options for this. They could remove capitalization of regions to allow a little flexibility, or they could retain capitalization and clearly define the geographic bounds of

each region and identify the stations contained within each region. At line 202, capitalization of “Northwestern” stations implies a specific region, but it isn’t defined earlier. The figures discuss different sets of stations in confusing ways that do not naturally help readers organize information to create a clear mental picture of the patterns of variability. I would recommend creating a table defining regions and matching stations to named regions.
Thanks for the suggestion, we have now removed all capitalization in all cardinal directions to allow greater flexibility.
Regarding the table, we believe that adding this information to Table 1 complements the association of stations with their corresponding region. Table 1 now includes an additional column titled “Stratification regime”, which lists, for each station, the regimes that are shown in Figures 3 and 4. We agree with the reviewer, that including this information to a table helps summarizing the results.

3. Part of the clarity gap involves changes in notation. In Figure 5 and Table 1, stations are identified without numbers (e.g. FX), while elsewhere they appear with identifying numbers (e.g. FX9). This requires readers to do a lot of cross-checking to see if the comments refer to the same stations.
Thanks for the observation. In the revised manuscript, we have corrected Figure 5 to ensure consistent labeling and improve clarity. We have checked all the Figures and now the stations are all consistent.

4. The first reference to the NAO is in line 258. The acronym needs to be defined, and more importantly, if the NAO is an important metric for variability, then the hypothesis that the NAO matters should be laid out early in the manuscript. Typically this should appear in the introduction.
In the revised version of the introduction we are including the NAO there to improve the general context.

5. Minor comments:
Thanks a lot for pointing out the typos and misuse of English. We believe the manuscript now reads much better. We have followed your suggestions and commented individually on a few points within this section.
a. Title: Comma really should be a semi-colon: Stratification and mixed-layer depth around Iceland: Characterization and interannual variability
b. Line 80: “high latitude” -> “high-latitude”
c. Lines 81 and 85: Correct accent in Sallée.
d. Line 83: “underdgoing” -> “undergoing”

e. Line 86: “on Climate Change, IPCC”. It’s not clear if this references a specific IPCC publication, in which case it’s missing a year, or if it is a general reference to climate change studies, in which case more detail would help.
Thanks for the observation, we have now removed that reference and used the original of Fox-Kemper et al., 2021, which is associated with the Ocean, Cryosphere and Sea Level Change. Working Group I to the AR6.

f. Line 93: “and associated” -> “and the associated”; “delays” -> “delay”; “South of Iceland” -> “south of Iceland”
g. Line 99: “of the oceanic” -> “oceanic”
h. Line 114: “these stations” -> “stations”
i. Lines 130 and 132: “Montegut” -> “Montégut”
j. Line 133: “shows to” -> “proves to” OR “appears to”; OR “shows itself to”
k. Line 138: no indent after equation
l. Lines 128 and 138: I don’t understand the notation used for potential density. After studying this for a while, I’m guessing that the intent was to use sigma_theta, but the notation didn’t work properly in the main text, and subscripting didn’t work in equation 1.
m. Lines 138, 151: N^2 should be in math font to match its representation in equations (1) and (2).
Thanks for the observation, we did have a typo here and we have now amended that.
n. Line 156: “furthermore” -> “further” (The term “furthermore” is too argumentative.)
o. Lines 163, 166: K^2 should be in math font
p. Line 175: “dispersions” -> “variance”?
q. Lines 184-185: “due to seasonal warming of the upper layers”: Phrase seems misplaced. I would move it to the start of the sentence: “Overall, due to seasonal warming of the upper layers, the summer profiles …..”
Thanks for the suggestion, this has been fixed.
r. Line 209: “exhibits” -> “exhibit”
s. Line 252: “West” -> “west”
t. Line 255: “moderate” -> “moderately”
u. Line 258: “certain correlation (R)”. The word “certain” is unclear. Can it be removed? Is the intent to say that the correlation is statistically significant?
Thanks for the suggestion, this can totally be removed as it did not add anything to the sentence.
v. Line 261: “explanation,” -> “explanation;”
w. Line 268: “North” -> north”
x. Line 297: “South” -> “south”
y. Line 298: “28-years” -> “28-year”
z. Lines 344-345: It’s best to avoid the “southern (northern) …. temperature (salinity)” construction because readers have a difficult time parsing information. Instead, break

the sentence into two parts. For example, “The spiciness distributions shown in Figure 8 reveal that temperature dominates on the southern side of Iceland, marked by an alpha-ocean regime, while salinity dominates on the northern side, marked by a beta-ocean.”
Thanks again for this suggestion, we have now corrected those sentences.
aa. Line 370: “South” -> “south”

Reviewer 2

Stratification and mixed layer depth around Iceland, characterization and inter-annual variability
by A. Ruiz-Angulo, E. Portela, C. de Marez, A. Macrander, S. R. O´lafsdo´ttir, T. Meunier, S. Jo´nsson, and M. D. Pe´rez-Herna´ndez

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 α- and β-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.

We thank the reviewer for emphasizing this crucial point. We have carefully revisited the 1-D PWP simulations and addressed several issues that limited their ability to meet the original objective. In the revised manuscript, we have implemented modifications to the simulations and clarified the analysis so that the simulations clearly identify the forcing mechanisms responsible for the ML deepening in both α- and β-oceans. The specific changes and additions are described in detail in the responses below.

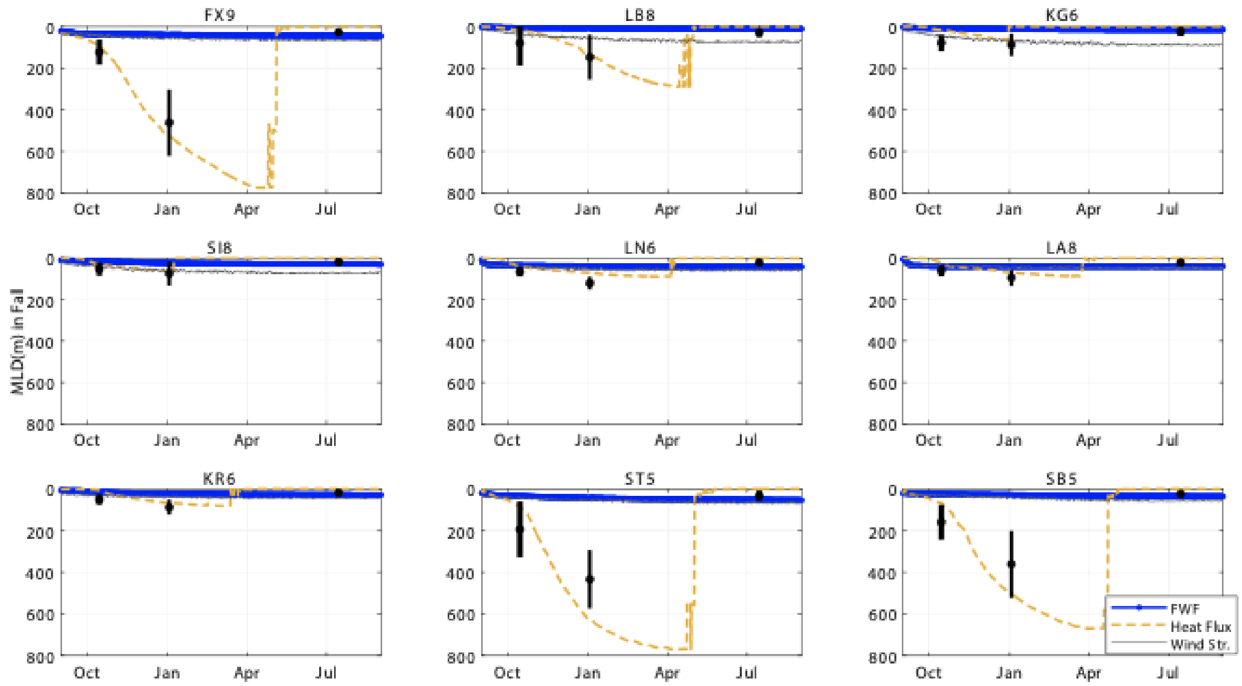
Major comments:

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 *al pha*- and β-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.

Thanks for your suggestion, we have now elaborated on the basic principles of the PWP model. The revised version of the manuscript included in Data and Methods a short explanation on how the PWP model works and reads as follow:

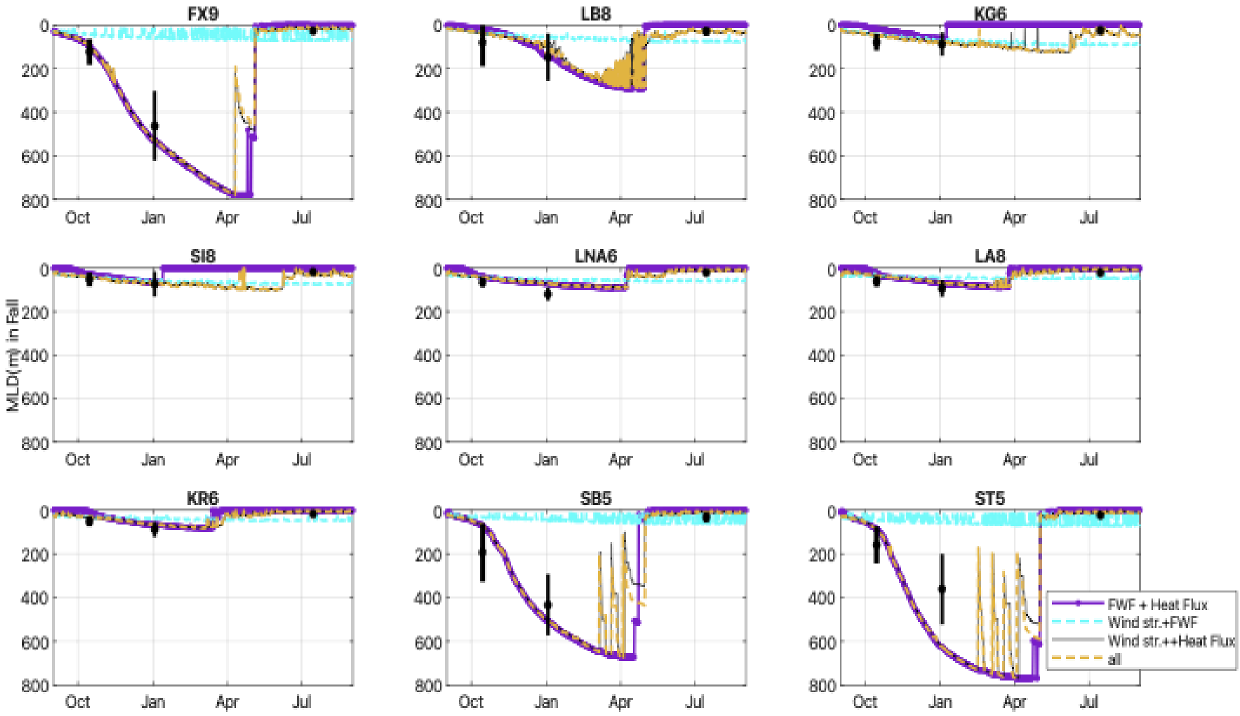
To investigate further the driving mechanism of the MLD we used the Price-Weller-Pinkel (PWP) one-dimensional model (Price et al., 1986). The PWP model is a one-dimensional vertical model used to simulate the evolution of the ocean mixed layer in response to atmospheric forcing, including wind stress, heat fluxes, and freshwater fluxes. The model evolves vertical profiles of temperature, salinity, density, and horizontal velocity based on surface forcing and a set of physical stability criteria. The first criterion is convective overturning. If surface cooling increases the density such that the water column becomes gravitationally unstable (i.e., denser water overlies lighter water), the model applies vertical mixing until static stability is restored. The second criterion is based on the bulk Richardson number, which represents wind-driven mixing. The mixed layer deepens until the bulk Richardson number ($Rib=(g\Delta\rho h)/(\rho 0(\Delta U)2)$ reaches or exceeds the critical values 0.6. The final criterion is the Gradient Richardson Number $Rig=N2/(\partial U/\partial z)2$ which accounts for shear instability. When $Rig<0.25$ local vertical mixing is applied. The PWP model is initialized with ERA-5 12-hour dataset of wind stress, heat, and freshwater fluxes (Hersbach et al., 2020) and the summer/winter averaged vertical profiles of temperature and salinity from the observations presented here (Fig. S1-S3). The 1D model allows to address the relative contributions from diurnal heating/cooling, freshwater fluxes, and wind mixing.

Following your suggestion a new figure with MLD simulations starting in fall has been generated. The old Figure 7 has been replaced with the one that is below. We have also modified the Figures S1-S4 in the supplementary material.



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 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.

Following this suggestion a new figure including the different possible combination of forcings is made and added to supplementary:



By combining the last three comments of the reviewer the new ‘Section 4: MLD driving mechanisms from a 1 D model’ has been rewritten and now it reads as follow:

> *The summer stratification around Iceland in summer is roughly an order of magnitude higher than in winter, largely due to positive buoyancy forcing, resulting in shallower and more variable MLDs. Therefore, we study the atmospheric effect on these α- and β-ocean regions by implementing the Price et al. (1986) one-dimensional model. The model is forced starting in the fall before the deep MLD develops. It is worth mentioning that the 1D model estimates the MLD from temperature-based profiles while the estimates from observations are density threshold based. The model results of the MLDs shown in Fig. 8 are within the range of the observed average ± standard deviations (thick black dots and lines in Fig. 8). For most stations a spring shoaling of the MLD is driven by reduced heat fluxes, while the MLD remains relatively deep due to the wind-stress. The choice of PWP model was made to support the idea that β- and transition oceans do not develop deep mixed layers, which is shown in Fig. 8.*
>
> *In the model, the stations embedded within the α-ocean with AW (Fig. 8: FX9, SB5 and ST5) present the largest MLDs exceeding 300 m depth, which is consistent with the observations. In this α-ocean region, the development of a deep ML is driven mainly by heat. However, within these stations, the*

wind-stress steadily contributes to the development of the ML. During the summer, shoaling of the mixed layer is likely influenced by the changes of both heat and freshwater fluxes, with their effects on the MLD partially offset by wind stress (Fig. 8: FX9, SB5 and ST5).

The station LB8, despite being in Denmark Strait and presenting a large contribution of PSWw and PSW in the upper layers (driving a β-ocean stratification), shows that the development of MLDs can be influenced by both, heat flux and/or wind stress (Fig. 8: LB8). However, the contribution of wind-stress and freshwater cannot lead to MLDs deeper than 100m (Fig. S4:LB8). Beneath the PSW and PSWw at LB8 we find AtOW. Hence as the wind-stress develops, the MLD evolution erodes the PSWw strata reaching the AtOW layer, allowing reduced heat fluxes to contribute to the MLD development. This erosion is not visible on the stations embedded within the EIC.

Wind stress becomes the leading forcing mechanism northeast of LB8 at stations KG6, and SI8, coinciding with the shift from α- to β- ocean stratification (Fig. 8). This region has a lower convective potential than regions with pure AW and therefore does not produce large MLDs (Fig. 4 and 8). The MLD there results from roughly equal contributions of convection and wind-driven mixing. At these stations, the best performance of the PWP model isobtained when both heat flux and wind-stress are included (Fig. S4). Notably, the summer MLD remains shallow and roughly with the same order of magnitude across all stations around Iceland (Fig. 8 and S4).

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.

Thanks for your suggestion, we have kept the sentence of validation with glider data as we believe it could be useful but we have also added a paragraph addressing the limitations of these methods. *"However, automatic detection methods have limitations, as they may miss, for example, stacked mixed layers and other non-canonical representation MLs".*

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.

We agree with the reviewer that water mass definitions may not be invariant over time, particularly given the nearly 30-year span of our dataset. In this study, we use definitions published in multiple papers classifying water masses around Iceland, not limited to the Arctic Ocean, relying on peer-reviewed sources. We acknowledge that applying constant limits in temperature, salinity, and even density may introduce some bias. Particularly, we agreen on the point for colder, denser water masses that are close in TS-space. However, our primary goal is to characterize the mixed layer around Iceland and report its temporal changes. Therefore, while the absolute classification of specific water masses should be interpreted with caution, we believe our results remain robust for analyzing mixed layer properties over time.

General comments:

I do not disagree that the expansion of temperature-stratified *al pha*-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.

Thanks for your observation, we do agree that the Introduction needed some polishing and we have almost rewritten it including the additional suggestion. Our paragraph now reads:

"The Arctic Ocean is warming much faster than the global average, a process known as "Arctic Amplification," which is also associated with the "Atlantification" of the Arctic (Polyakov et al., 2017; Dai et al., 2019). While the causes are still debated, Arctic Amplification has evident consequences, such as a decrease in seasonal sea-ice extent and a weakening of the cold halocline (Polyakov et al., 2020; Dai et al., 2019). Although these changes are less pronounced in the central Iceland Sea, similar processes have been observed in the central Greenland Sea and the northeastern shelf (Gjelstrup et al., 2022; Strehl et al., 2024), suggesting that Atlantification may also influence the Iceland Sea. Changes in temperature and salinity in the upper ocean modify upper-ocean stratification, which partially controls the mixed layer depth (MLD)."

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.

Thank you for your encouraging suggestion. We feel that incorporating material from other stations, even just a few sentences, could potentially lead to misunderstandings about our current work. This version already contains a substantial amount of material to digest, and our main objective is to provide a regional study of the ML around Iceland, highlighting the differences between the north and south and the ongoing changes. We greatly appreciate your suggestion, but we would like to keep this section simple.

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.

We have followed your suggestion, which has improved the analysis. When computing the density within the ML, we have found that the density at FX9 has decreased, at least showing a negative anomaly in the new Figure 5g. In addition, we calculated the ML density for all stations and observed that all those in the south of Iceland or within the alpha-ocean show a significant decrease in density. We have now included a new Figure 6 that shows these trends. The changes are stronger during winter, the rest of the stations around Iceland do not show a significant change. This suggests that, at those locations, density changes may be compensated by salinity variations, whereas in the alpha-ocean, this compensation to keep the density constant does not appear to occur.

We thank the reviewer again for emphasizing this important point, as it has led to this nice extra analysis and proves that the reviewing process is improving the manuscript.

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).

We apologize with the reviewer for the unanswered comment. We cannot really find in L16 (Abstract) any mention to dense-water formation. However, we can see that using "transformation" in the abstract may not be appropriate and we have now modified the first sentence to this *"The ocean around Iceland is a key region where major water masses and currents interact, influencing the global ocean circulation".*

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.

We partially agree with the reviewer as Iceland shares half of the water masses within those two basins and the other half on the Nordic Seas, I do think it is not representative for those but it does a good job on those too. We were afraid of using multiple definitions of MLD as it would not make the results very consistent.

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).

Thanks for the suggestion, we have modified this accordingly and the reference to Huang et al., 2023 has been added to the list.

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.

Thanks for the clarification, we have now changed the sentences to: *"Wind-stress, sea-ice retreat and high heat loss due to cold-air outbreaks drives the transformation east of Greenland (Våge et al., 2018)"*

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.

We have now removed the mention to the Arctic and kept this sentence only as: "*The Nordic Seas are also a large repository of freshwater, primarily originated from glacier melt and river discharge"*

Line 62:
Please introduce the acronym EGC when the East Greenland Current is first mentioned in the text (line 50).

Thanks, fixed now.

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.

We agree with the reviewer that this was confusing. We have now removed that sentence but retained the last sentence regarding the shorter time scales for MLD modulation due to heavy storms. This experiment was part of NISKINE (Skyllingstad et al., 2023), and was conducted using a wave glider synchronized with a microstructure-equipped-glider. The experiment showed that storms can inject enough energy to deepen the mixing layer by several meters compared to storm-free conditions. However, for our work, this is not relevant, as we are not colocating storms with the data collection.

Line 91:
The Nordic Seas, not the Arctic Ocean, is located north and northwest of Iceland.

Thanks for the clarification, this mistake has been fixed.

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

We agree with the reviewer. Following your suggestion, as well as similar comment from the other reviewer, we have now removed capitalization and now use lowercase for all cardinal references.

Line 159:
There should be a comma after diurnal heating/cooling.

Fixed

Line 253:
Do you mean “second freshest”?

Yes, thanks!

Line 303:
The Future et al. (2022) paper is missing from the reference list.

Thanks for the observation. This was a mistake; it should read Feucher et al., 2022, which is in the references 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.

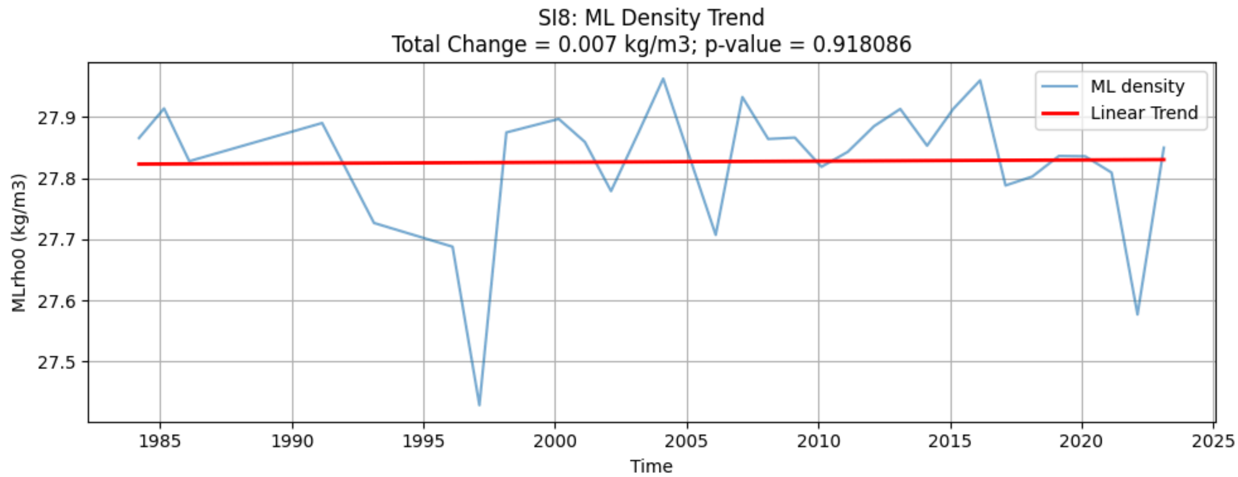
We agree with the reviewer that this phrase is indeed incorrect. The revised version of the manuscript has included a version of your comments and now reads: "*The summer stratification around Iceland in summer is roughly an order of magnitude higher than in winter, largely due to positive buoyancy forcing, resulting in shallower and more variable MLDs*."

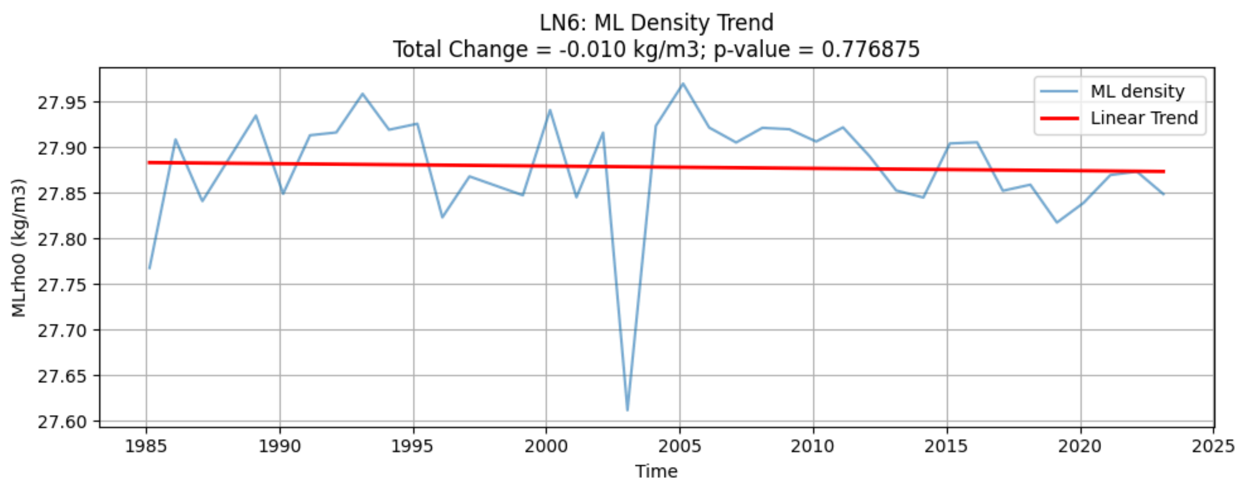
Line 316:
The shoaling of the mixed layer would be caused by reduced heat fluxes.

Thanks, this has been added to the new version of the paragraph and now explicitly reads "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.

We have rephrased the last sentence and replaced "could" with "may." Unfortunately, the stations around Iceland do not extend to the central Iceland Sea. However, following the reviewer's suggestion, Figure 7 shows that all stations in the Iceland Sea with potential to modify the NIJ do not show a significant change in density (see Figures below). The Figures below present the latest curated data, which do not alter the main conclusions of the manuscript. We have not discussed with Hafró the use of the full dataset, but we believe that it would not change the main conclusions of this paper. Finally, in the abstract we have also removed the sentence "allowing for the formation of deeper mixed layers", which is not very conclusive from our study and it is misleading.





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