Reviewer 1

This manuscript takes advantage of a long-term hydrographic data set from a series of stations encircling Iceland to assess major characteristics of upper-ocean variability and to evaluate long-term trends in the region. The study shows that salinity governs stratification at stations northwest of Iceland, while temperature governs stratification to the south. To the north of Iceland, alternating impacts from the North Icelandic Irminger Current and the East Icelandic Current lead to a mixed response.

The data set used for the study offers a rich supply of information, and the authors have chosen an interesting question to pursue. Parts of the analysis would benefit from more detail. I feel that the manuscript will likely be suitable for publication after careful revision.

1. The overall analysis of the manuscript addresses several distinct issues that are not always tightly linked together. One focal point is temperature vs salinity controls on mixed-layer depth or stratification, including consideration of the seasonal cycle. A second thread considers long-term trends in mixed-layer temperature and salinity along with the quantities with which they correlate. A third aspect assesses the multi-year linear trends in mixed-layer temperature in summer and winter. Analyses explore the mixed-layer evolution on the seasonal scale using a one-dimensional mixed-layer model and look at historic temperature vs salinity domination on a regional and seasonal scale. These are interesting and related analyses, but they are not fully linked together to provide clear and targeted interpretation of the results. For example, the long-term temperature trends in Figure 6 are interesting but not well connected with the rest of the analysis. In a rewriting, the manuscript should be more tightly focused to identify clear and linked results, well grounded in robust statistics.

We thank the reviewer for the valuable suggestions, which have helped us improve the quality of the manuscript. Our new version of the manuscript tries to link together those three stories, including the results presented in Figure 6.

2. One gap in the manuscript is a lack of statistical detail. This gap is particularly noticeable in Figure 5, in which the authors show time series of mixed-layer temperature and salinity anomalies, mixed-layer depth, and the North Atlantic Oscillation. The authors discuss correlations between these records but do not report correlation coefficients or statistical significance. To show that the patterns that the authors observe in their plots are robust, they should report quantifiable statistical metrics.

We agree with the reviewer. We have now computed Pearson correlations between all variables, and the results are detailed in Table 1.

Table 1: Pearson correlations between different variables. Non significant correlations have been omitted. The shown correlations are significant at 95% confidence (p<0.05), and those significant at 99% (p<0.01) are in bold.

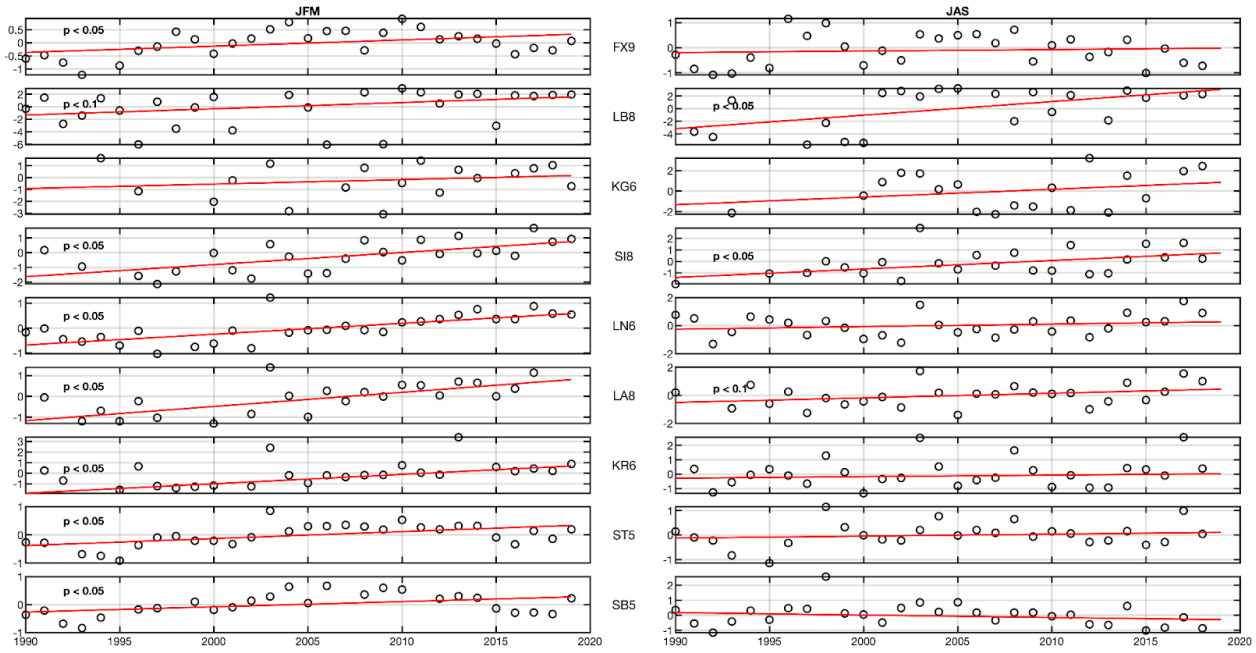
	FX		SI		LB	
	Correlation	p-value	Correlation	p-value	Correlation	p-value
MLT-MLS	**R=0.69**	**p<0.01**	**R=0.74**	**P=<0.01**	**R=0.95**	**P<0.01**
MLT-MLD	-	-	-	-	**R=0.76**	**P<0.01**
MLS-MLD	-	-	-	-	**R=0.68**	**P=<0.01**
NAO-MLD	**R=0.53**	**P<0.01**	-	-	-	-
NAO-MLT	R=-0.41	P<0.03	-	-	-	-
NAO-MLS	-	-	-	-	-	-

For the NAO and ML depth, temperature and salinity we have computed lagged correlations, in order to test if the impact of the NAO on the mixed layer properties exhibited a lag. The best correlation between the NAO and MLD, was R=0.53, p-value<0.01 at lag zero; for MLS R=-0.52, p-value<0.01 at lag -2 years (NAO leading), and for MLT R=-0.49, p-value<0.01 at lag -1 year (NAO leading (Fig. 5g, h). However, we consider that a 2-year lag lacks a realistic physical explanation, thus, we prefer to not to consider this as a reliable correlation and we only analyzed the correlations at lag zero (as in Table 1).

This information has now been added to the manuscript

3. Similarly in Figure 6, the authors fit linear trends to plotted mixed-layer temperature records, but they do not specify the slope of the linear trends. In addition, the caption to Figure 6 does not indicate what the p values represent. These pieces of information should be added.

We agree that Figure 6 should include the linear trends and more information. The p values are a result of the linear regression and we split the values in groups of “statistically significant”, p<0.05, and “marginally significant”. We translated Figure 6 from the bars into a plot with the datapoints and a linear trend included, which is shown below and we are happy to use this version if the reviewer thinks it is a suitable version. Notice that linear regressions with no p-values are not statistically significant.



4. Given the discussion in Figure 5 and given the character of the records in Figure 5, I was surprised by the decision to fit trends in Figure 6. The discussion in Figure 5 emphasizes the specific relations between plotted quantities rather than long-term trends, so I was expecting Figure 6 to report correlations. It would be interesting to see the correlations between NAO, mixed-layer depth, and MLT mapped out for the full set of stations.

We thank the reviewer for this suggestion. In this study, one of the objectives was to address the long term variability of the seawater properties. In this sense, our choice was to address both, linear trends, as well as interannual variability. The two figures are then complementary., For figure 5, we chose representative stations, to analyse the interannual variability and its possible link with climate modes, but the correlation with the NAO is rather weak in most of them, it is only significant in the westernmost stations. Figure 6 would only expand on a question that we find to be already answered with Figure 5

Figure 6 is then focused on linear trends. Despite the presence of interannual variability on some of the stations, and even if the length of the record is not long enough to clearly detect anthropogenic trends, we do observe significant linear trends (particularly in winter) appearing over the interannual variability. This suggests that these should be explored in more detail in future studies, when longer time series will be available. This the message of Figure 6.

5. Since the analysis of Figure 6 focuses on trends, and the overall goals of the manuscript are directed toward alpha and beta oceanic regimes, the authors could/should expand the manuscript discussion to indicate how the trends (and regression coefficients, perhaps) inform their understanding of alpha vs beta ocean regions.

We thank the reviewer with this follow up over Figure 6. By adding the trends to each station, we can now argue that a transition into an alpha-ocean within the ML is underway. This signal exhibits higher statistical significance over the winter. The new version of Figure 6 clearly shows the statistically significant trends, supporting the interpretation of an ongoing transition toward an alpha-ocean regime around Iceland, suggested by Figure 8.

6. Line 133. “great score”. In this usage, “great” sounds like a word that expresses an opinion. This point needs to be quantified, and more neutral wording should be used to express the skill of the density threshold method.

We agree with the reviewer, we have now rephrased the sentence using neutral wording, it now reads *“...it shows to be effective even for cases…”*

7. Lines 206-207. “The southern stations ST5 and SB5, have a minimal contribution from salinity, which may be associated with the numerous river discharges and the proximity to the continental shelf.” This is an interesting point. Does the fresh water budget support this hypothesis? It would be useful to quantify the volume of freshwater discharge and its expected impact on salinity. Precipitation or oceanic circulation would be other factors that could influence salinity.

We have revisited this sentence and we believed that we did not look carefully at the data and that statement does not fully support this hypothesis. ST5 and SB5 are fully immersed in Atlantic waters, their TS diagrams look very similar to Figure 2a,b. The freshwater discharge around Iceland has characteristic peaks in January, May, and September with the maximum values in the South West (Reference Figure 2,4 Whitney, 2025).

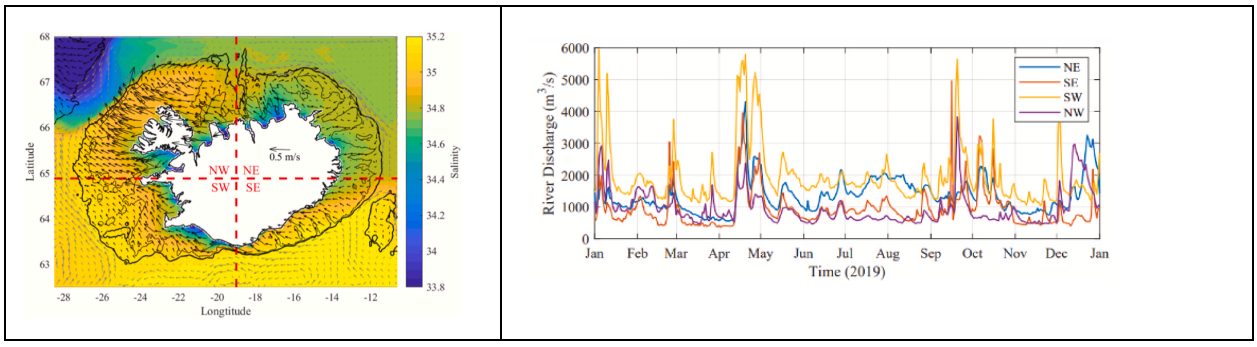
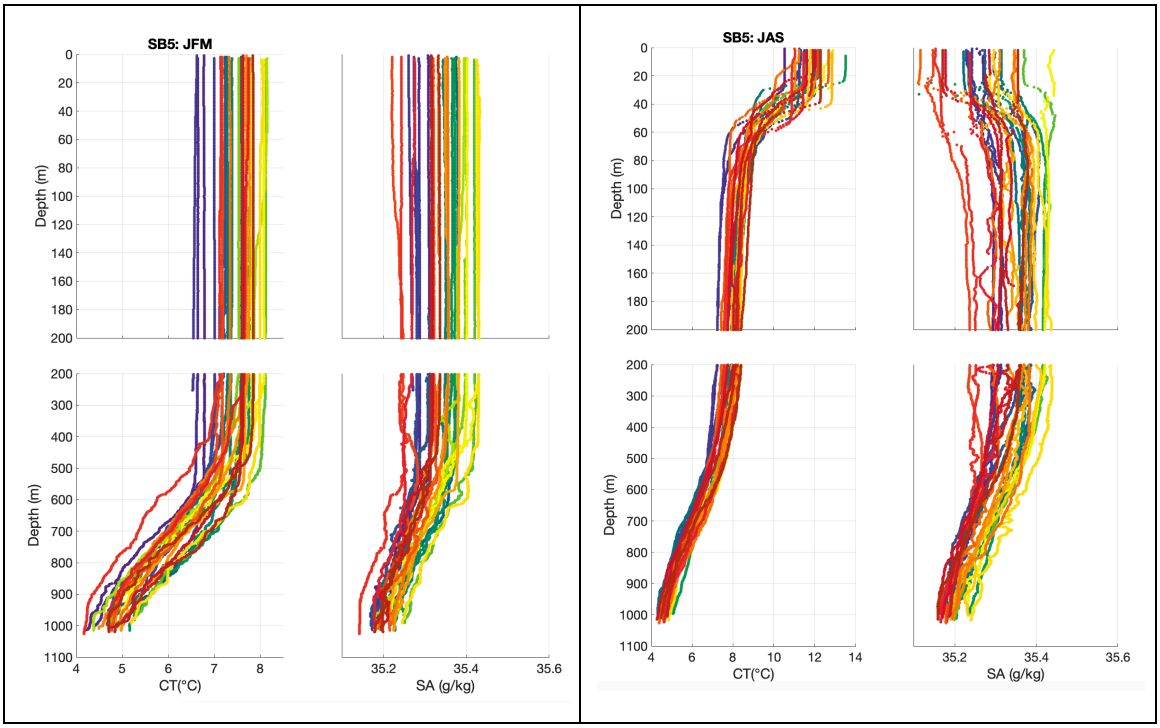


Figure AR2. (Left) Annual mean near-surface salinity and surface currents around Iceland; (Right) River discharge time series for each of the 4 quadrants (Whitney, 2025).

The contribution of this fresh water seems to generate a small summer halocline, which is observed only in Figure 3 SB5 station. The winter convection is capable of eroding this small contribution to stratification. The winter and summer profiles for temperature and salinity for ST5 and SB5 are shown below and it is possible to observe the top small freshening (upper ~20m), which then disappears in the winter (Figure AR3). Moreover, SB5 is really close to the largest river discharge in Iceland, the South West. We have now rephrased the sentence adding the reference to the river discharges and explicitly mentioning the contribution to stratification.



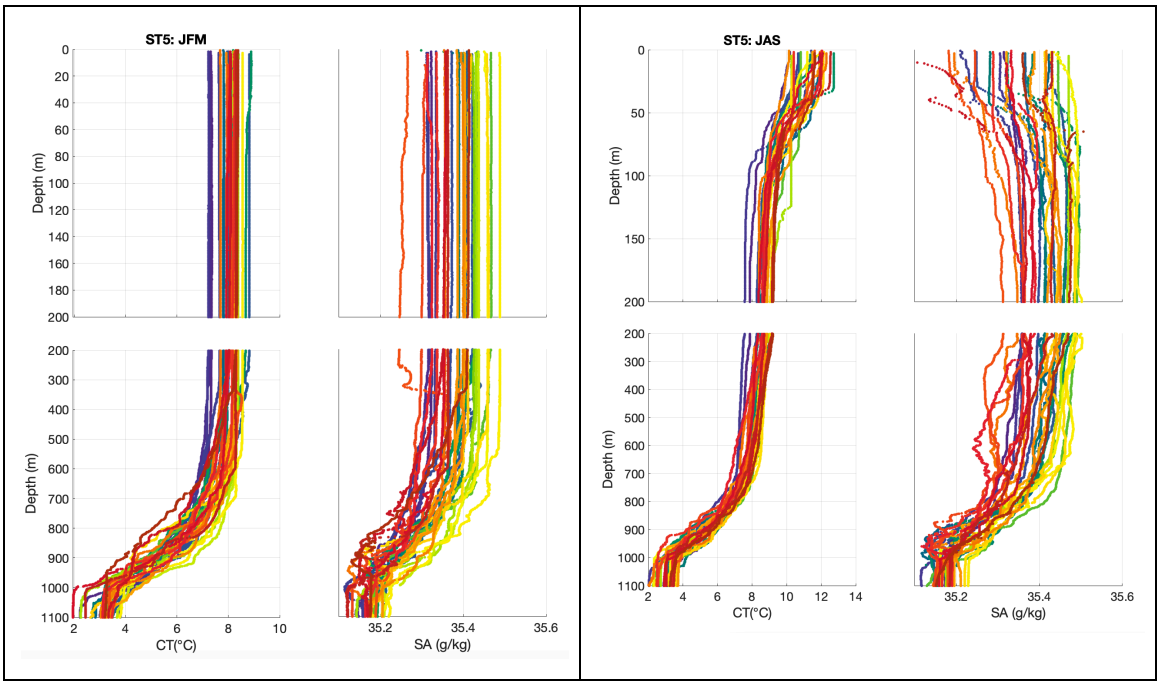


Figure AR3. (top) Winter/summer vertical profiles for temperature and salinity for SB5 and (bottom) ST5.

8. Figure 3. The station labels are much smaller than the other figure labels and are too small to read clearly. The figure should be redrafted with larger labels.

We agree with the reviewer, the labels are too small. We have now fix this in both Figure 3 and 4.

9. Line 214. “hydrographic onset”. The meaning of this is unclear. Does it refer to the top of the hydrographic profile or the seasonal onset of a change in the hydrographic profile?

Thanks for pointing this out, we mean that the state of the ocean is a lot different in the winter compared to the summer. We have now rewritten the sentence and it now reads: “*The hydrographic conditions are very different for winter ….* “

10. Figure 4. The figure shows open circles for deep mixed layers. The justification for this is not clear, since deep mixed layers can be as dynamically relevant as shallow mixed layers. Further explanation is needed.

We apologize for the confusion, perhaps both our figure and the caption were misleading. Figure 4 shows the MLD and the stratification decomposition of the average profiles for 9 stations. We do not have open circles for the deep mixed layers, the MLD are represented

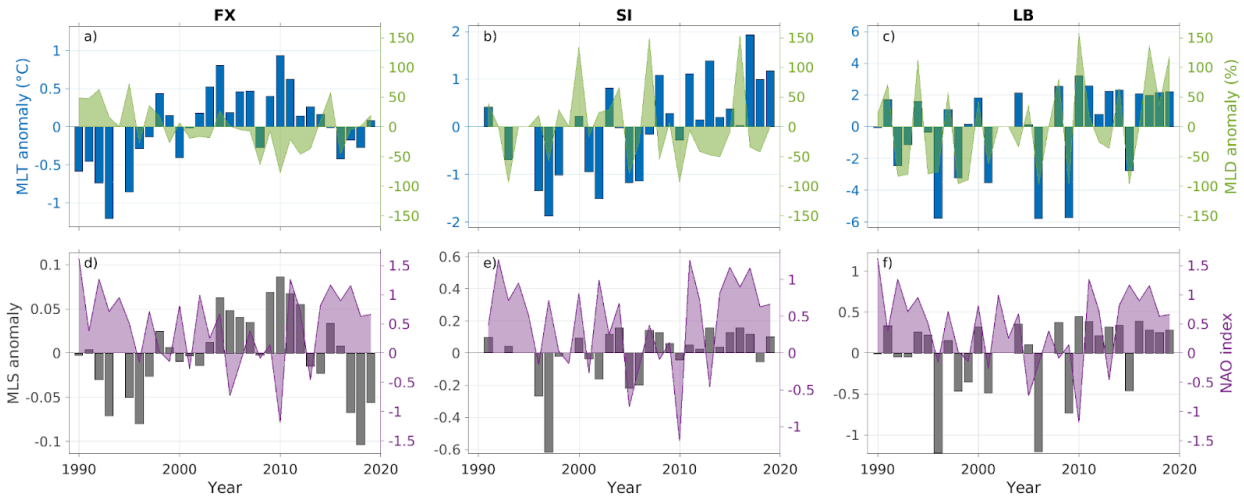
by the solid black circles at the far left of each box. The open circles in Figure 4 represent the region where the data shows no significant dominance on the stratification decomposition, i.e., neither alpha nor beta oceans. In the revised version of the manuscript, we have corrected the caption of Figure 4 to clarify this point, explicitly explaining that the colorless or open circles correspond to areas with extremely weak stratification, where neither temperature nor salinity dominates. This clarification should help to avoid further confusion.

11. Line 270. Correlations with the NAO should be quantified. As noted above, the manuscript should report correlation coefficients and evaluate statistical significance.

The correlations have been quantified and they are now reported in the manuscript.

12. Figure 5. The repetition of panels g, h, and i seems unnecessary. Could the NAO time series be superimposed on the panels above (along with the addition of concrete correlation statistics)?

We completely agree with the reviewer. We have modified the figure following your recommendations:



13. Line 286. “aliasing”. The term “aliasing has a specific meaning in time series analysis, and the usage here seems inconsistent with that usage. This could be described as “superimposed on”.

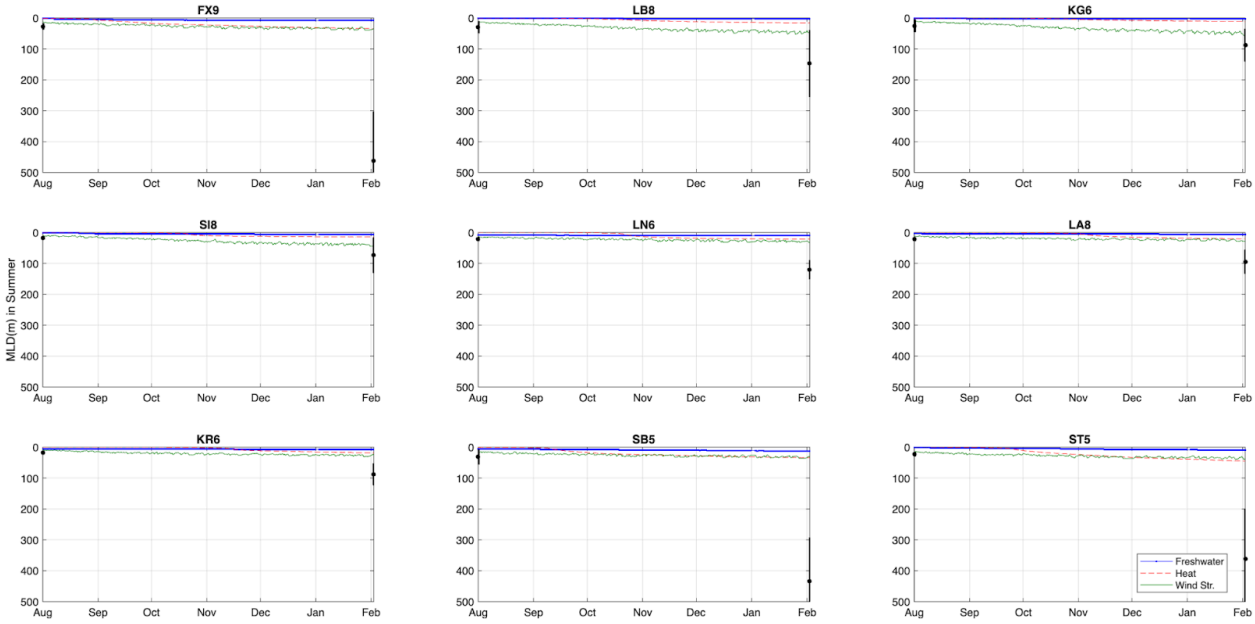
We agree with the reviewer, aliasing is not a suitable word here and it has been modified in the revised manuscript.

14. Lines 295 and following. Choice of one-dimensional model. The Price-Weller-Pinkel model has been used extensively over the last four decades for upper ocean analyses. It is not the only possible model, and other recent studies have made use of GOTM or a stripped-down form of KPP. Thus, it's important to justify the choice of the PWP model.

We used the PWP model because, as you said, it not only has been extensively used in the Arctic and Subarctic regions but also because it is a simple 1D model that does not take into consideration advection or advected mix-layers. Hence, it is ideal to understand how local processes such as heat/freshwater fluxes or wind modify the vertical profile. Similar results were obtained using a ROMS model in the area (paper in preparation), therefore we did not explore other methods.

15. Lines 295 and following. The focus of the one-dimensional mixed layer analysis on winter only also needs clarification and should be more carefully described to explain that the analysis is really looking a the winter-to-summer transition. The PWP model has previously been used over a broad range of latitudes and for all seasons. Thus, a priori, there's not an obvious reason to exclude summer.

We excluded summer mainly because the MLDs developed are quite shallow, and even if it is forced with the atmospheric summer-to-fall- inputs shown on the supplementary material, the average profile does not end up producing the winter MLD in the PWP model. See figure below:



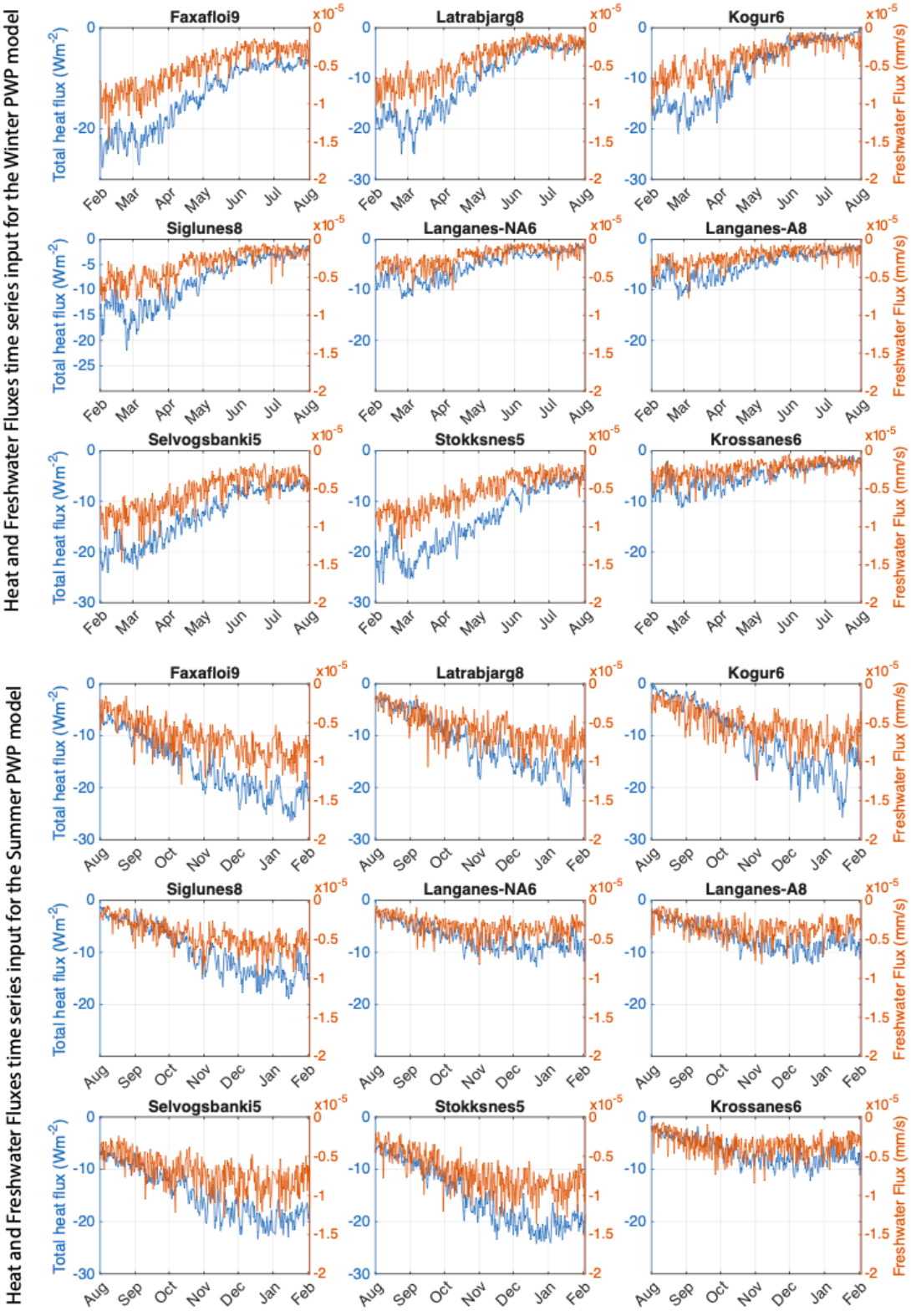
We have added a sentence in the revised version of the manuscript mentioning that the summer stratification is too shallow to appreciate the decomposition of processes.

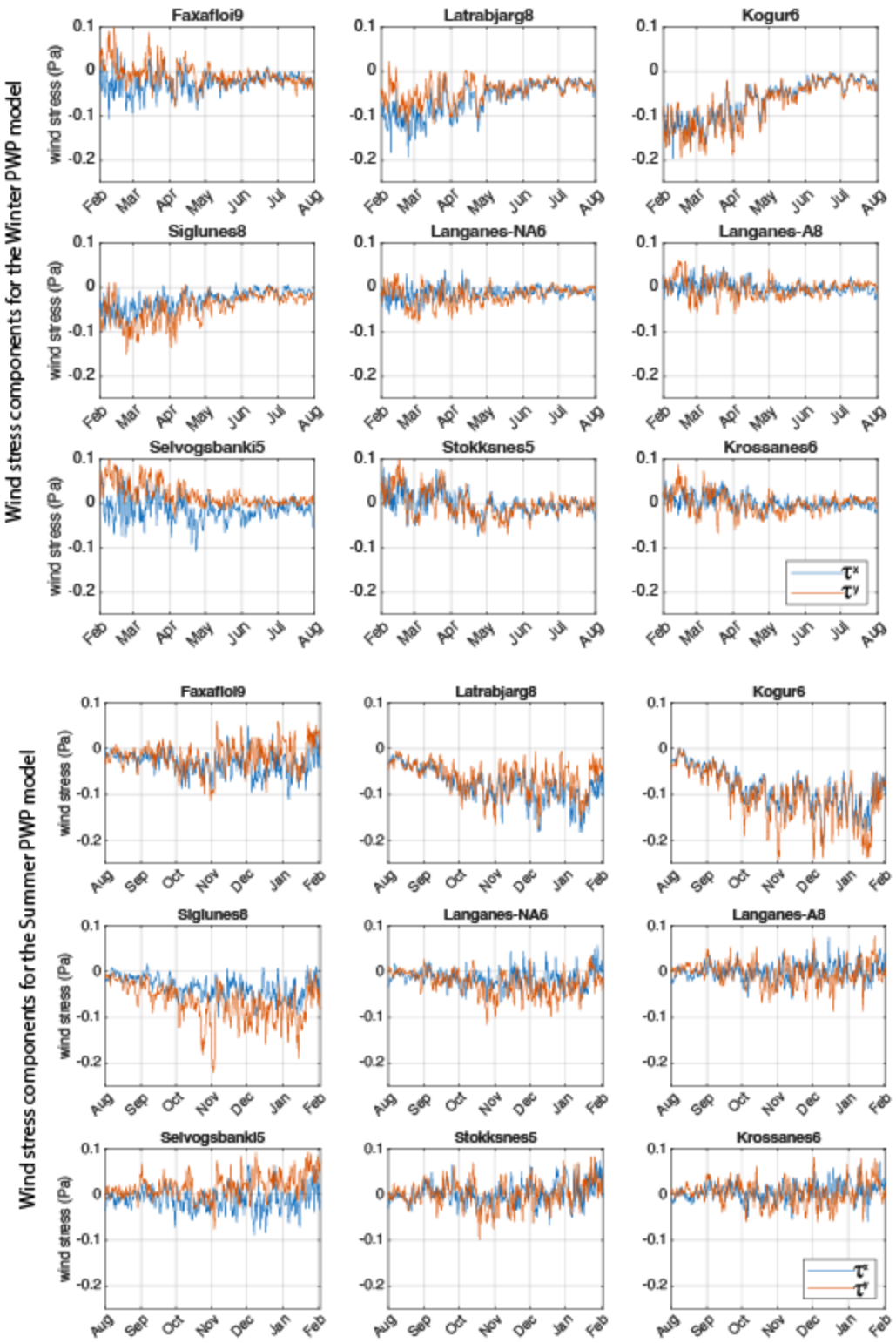
16. Figure 7 calculations. How is the mixed-layer model initialized? Does it start with stratification typical of February? It's surprising that the mixed layer in the model appears to deepen at the outset. I would have expected it to be initialized with a profile that matches the climatological observations. This should be explained.

The 1D ML model is initialized with: freshwater flux, heat flux (each component and the sum) and wind speed obtained from ERA 5 (Copernicus). These parameters have been interpolated to each of the standard MFRI stations. In addition we used the CTD averaged summer and winter profiles at each station. All of these inputs are now shown in Supplementary material and Section 2 has been modified accordingly as follows:

Section2: “To investigate furthermore the driving mechanism of the MLD we used a one-dimensional model (Price et al., 1986) initialized with ERA-5 12-hourly 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 (see supplementary material). The 1D model would reveal the contribution from diurnal heating/cooling freshwater fluxes and wind mixing”.

Figures added in supplementary material:


Heat and Freshwater Fluxes time series input for the Winter PWP model
Faxafloi9
Latrabjarg8
Kogur6
Siglunes8
Langanes-NA6
Langanes-A8
Selvogsbanki5
Stokksnes5
Krossanes6
Total heat flux (Wm-2)
Freshwater Flux (mm/s)
x10-5
Feb Mar Apr May Jun Jul Aug
Heat and Freshwater Fluxes time series input for the Summer PWP model
Faxafloi9
Latrabjarg8
Kogur6
Siglunes8
Langanes-NA6
Langanes-A8
Selvogsbanki5
Stokksnes5
Krossanes6
Total heat flux (Wm-2)
Freshwater Flux (mm/s)
x10-5
Aug Sep Oct Nov Dec Jan Feb


Wind stress components for the Winter PWP model
Faxafloi9
Latrabjarg8
Kogur6
Siglunes8
Langanes-NA6
Langanes-A8
Selvogsbanki5
Stokksnes5
Krossanes6
wind stress (Pa)
0.1
0
-0.1
-0.2
Feb
Mar
Apr
May
Jun
Jul
Aug
τx
τy
Wind stress components for the Summer PWP model
Faxafloi9
Latrabjarg8
Kogur6
Siglunes8
Langanes-NA6
Langanes-A8
Selvogsbanki5
Stokksnes5
Krossanes6
wind stress (Pa)
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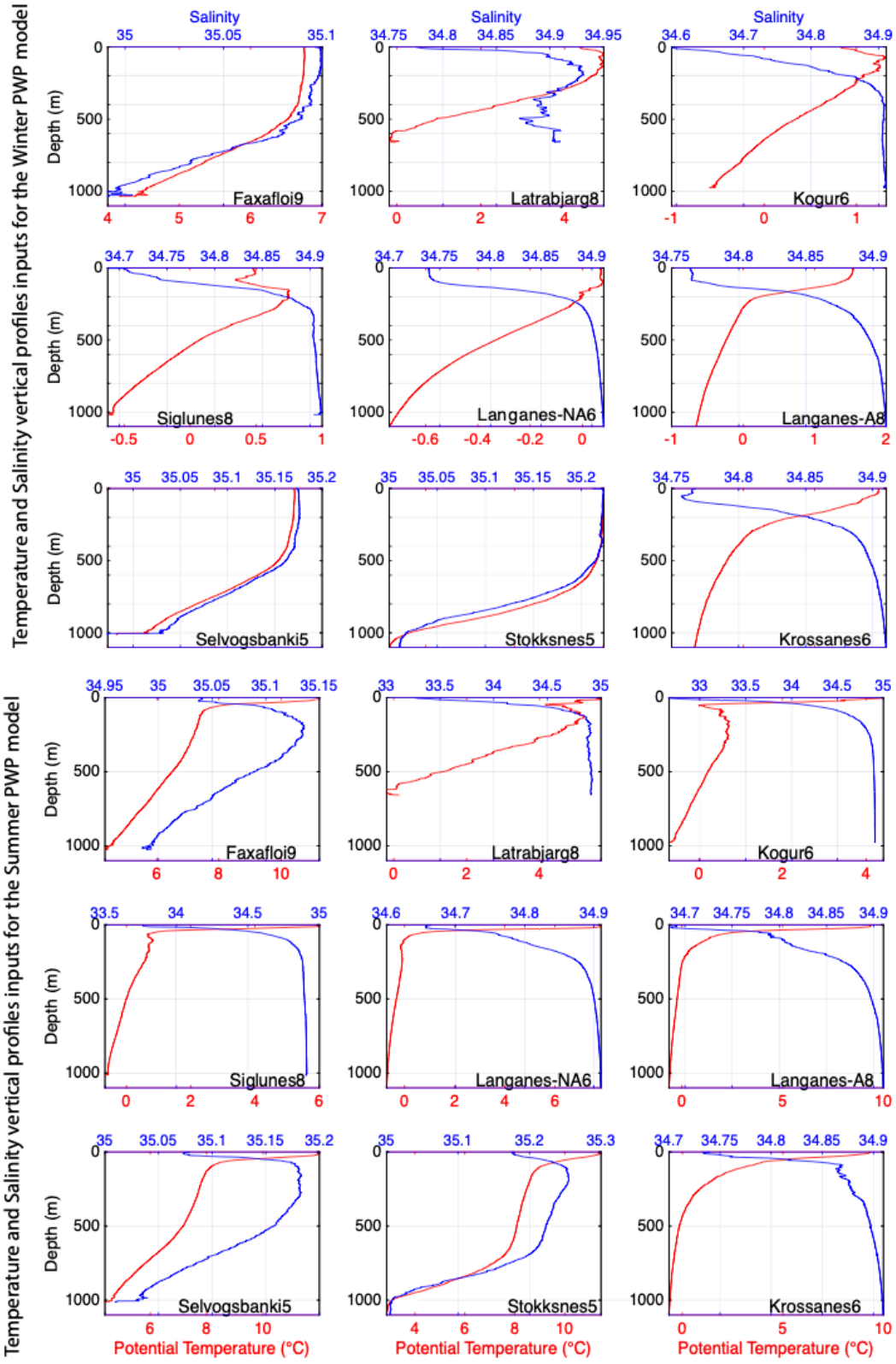
0
-0.1
-0.2
Aug
Sep
Oct
Nov
Dec
Jan
Feb
τx
τy



17. Figure 7 color scale. Please check colors. Green/red contrasts can be challenging for readers with limited color vision.

To avoid problems with colors we have decided to add different line styles to the new plot.

18. Line 389. “not correlated with the NAO”. The lack of correlation should be quantified in the main body of the text, particularly if it is referenced in the conclusions. Better language would specify that the correlation with the NAO is not statistically different from zero.

Following both reviewer’s suggestions, correlations have been quantified and this information has been added to the manuscript, which now reads: “*Except for the southern stations, influenced by the subpolar gyre, the interannual variability was not correlated with the NAO. For example, FX9 shows a significant negative of MLT with the NAO (R=-0.41 and p-value < 0.03)*”.

19. Line 390. “at times reaching 2 degrees C”. This number should be reported as a rate, in units of change in temperature per unit time. Please specify the time interval over which this estimate is computed.

Based on the previous comments we have modified Figure 6, which now shows the trends. The maximum observed trend with statistical significance is 0.08 C/year at SI8, which over roughly 30 years is about 2.4C. This station is considered a transition station and it is on the pathway of the progress of the alpha-ocean into the north. We have now modified the text using trends instead of absolute values. Once more, thanks for this observation.

20. Minor grammatical points

    Line 17: “sections” -> “section”
    Lines 19 and 20: It would be good to make a decision about consistent capitalization of regions (“South” or “south”, etc.)
    Line 24. “alternate the temperature and salinity contribution to stratification”. Wording is unclear. Maybe the authors could write, “while in the North, the North Icelandic Irminger Current and East Icelandic Current alternate seasonally, shifting the region between temperature-dominated and salinity-dominated stratification.”
    Line 28. Remove comma after “locally”.
    Line 32. “their link” Wording is confusing. The word their implies a plural reference point, but the grammatical structure of the sentence does not clearly identify what this reference should be. Maybe “This study provides an unprecedented and detailed description of the seasonal to multi-decadal variability of mixed-layer depth and stratification around Iceland, showing links between this regional variability and the changing North Atlantic….”
    Lines 42-46. Capitalization and punctuation are inconsistent for numbered points. All three items could be capitalized as separate sentences, or all

three could be started with lower case letters, with semi-colons to separate the items. But mixed punctuation and capitalization is confusing.
Line 45. Remove “with”
Line 55. “drives” -> “drive”
Line 57. “heat fluxes are the main drivers” or “heat flux is the main driver”?
Line 57. “on the center” -> “in the center”
Line 58. “Nordic Seas have been previously described as a ‘melting pot’”. Inconsistent plurals. The Nordic Seas region is a melting pot? Or Nordic Seas are melting pots?
Line 59. “Nordic Seas are also a large repository”. Same thing. “The Nordic Seas region is a large repository”?
Line 65. “of the Arctic Amplification” -> “of Arctic Amplification”
Line 65. “the decrease” -> “a decrease”
Line 74. “forcings” -> “forcing”
Line 74. “to control” -> “for controlling”
Line 74. Add comma after “mixing”
Line 76. “of the strong” -> “of strong”
Line 99. “hinders” -> “hinder”
Line 101. Add comma after “MLD”
Line 114. “IB” -> “IH”
Lines 128-137. It’s standard practice to subscript theta in sigma_theta.
Line 137. The line following equation (10) continues the sentence containing equation 1 and should not be capitalized or start a new paragraph. Equation 1 should be punctuated with a comma rather than a period
Line 137. “decomposed on” -> “decomposed to show”
Line 138. “contribution of the salinity” -> “contribution of salinity”
Line 146. “Where” is a continuation of the sentence containing equations (3) and (4). No capitalization and no indenting.
Line 148. Add comma after “salinity” since this is a compound sentence.
Line 151. “have” -> “has”. (The sentence structure implies that only one component needs to have an impact, so the verb should assume a singular subject.)
Line 164. No indent. Please check all equations for this issue.
Line 187. “strike out” has a couple of distinct usages, but this reads as if it is using the baseball metaphor, which means to fail completely. Maybe use “are strikingly saltier”.
Line 205. Missing words. Maybe "despite the fact that stratification ...."
Line 251. “correlates” -> “correlate”
Line 258. “in the order” -> “on the order” OR “are the same order of magnitude as”

Line 261. “neither …. nor” is not used correctly here. Change to “do not seem correlated with the MLT/MLS or with the ….:
Line 264. “it is”. The text is not clear about the meaning of “it”. Clarify whether “it” is station LB8 or the winter MLD.
Line 266. Start a new sentence: “front, fresher” -> “front. Fresher”. Add a verb: “MLs associated” -> “MLs are associated”
Line 270. “of NAO” -> “of the NAO”
Line 271. “MLS” -> “MLS,”
Lines 325-326. “distribution (Fig. 8) … southern (northern) … alpha- (beta-) …” Avoid using opposites in parentheses since opposites are also used for clarifications (e.g. Fig. 8 is likely not a match to “northern”). In general, this opposite-in-parentheses structure is difficult for readers to parse. If the point is worth making, then it can be spread into two sentences.
Line 325 and discussion of Fig. 8. The method underlying the results in Fig. 8 is shown in the Introduction. Here the text could reference Equation (5) to point readers to the relevant aspect of the computational approach.
Line 338. “200” -> “2000”?
200 is correct, we are trying to capture only the upper ocean.
Line 364. “northwestward, hence”. This is a comma splice. Start a new sentence instead.
Line 366. Don’t put opposites in parentheses. Write a clear two-part sentence instead.
Line 370. “variability, they”. Comma splice. Start a new sentence with “they”
Line 374. Add a comma after “ML’s”