

Reviewer 2

egusphere-2025-2102

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

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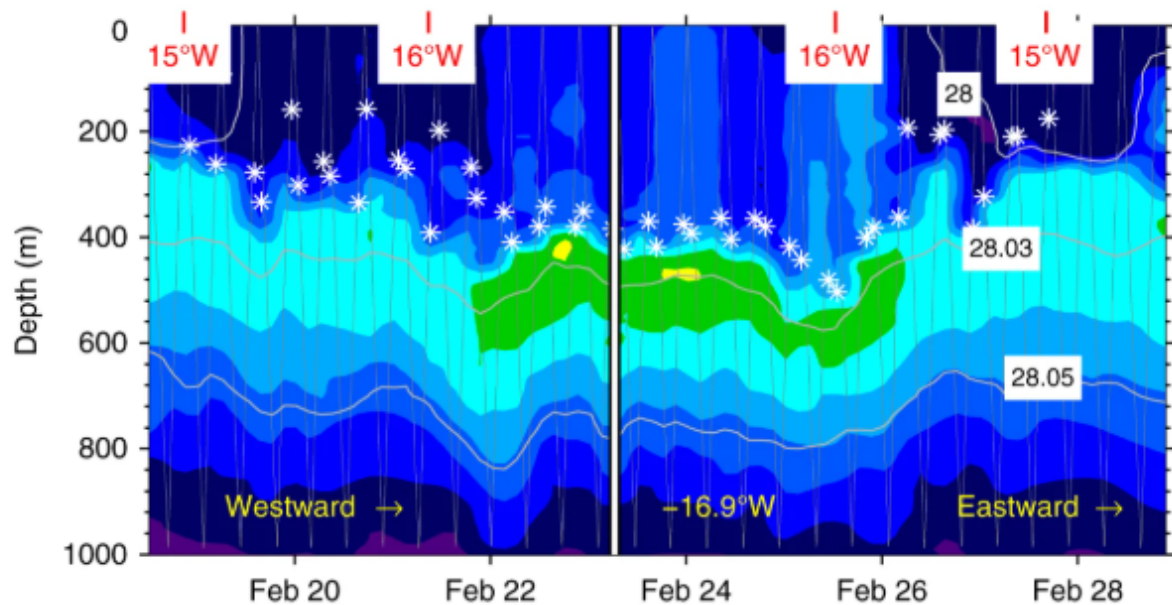
In this manuscript, the rich hydrographic data set collected through long-term monitoring efforts is used to characterize stratification and mixed-layer properties around Iceland. Three distinct regions are identified, characterized as α -, β -, or transition-oceans based on whether temperature or salinity dominates stratification, and implications for water mass transformation are discussed. Straddling the Greenland-Scotland Ridge, Iceland is situated at a key location for the overturning circulation in the Atlantic Ocean, particularly as regards the supply of dense water masses to its lower limb. I think this is an interesting and timely contribution to improve our understanding of the unique importance of Icelandic waters, but I have some concerns about the determination of mixed-layer depths, water mass definitions, and the one-dimensional mixed-layer model simulations that I hope the authors will address.

Major comments:

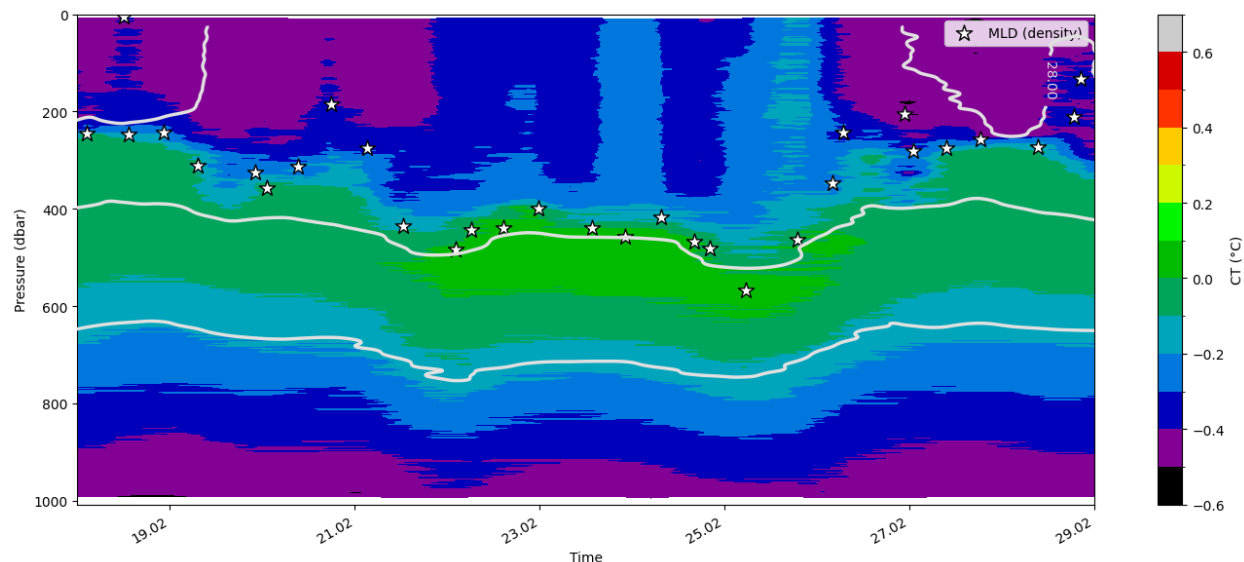
Your method for determining mixed-layer depths, with an adjusted density criterion, is commonly used. Verifying the outcome of the routine by visual inspection is a good way to ensure that the results are realistic. However, I think that you also need to document this, or at least be more specific about what the mixed layer depth you arrive at represents. Is it the surface mixed layer that is currently in direct contact with the atmosphere or the deepest weakly-stratified layer formed the same winter that may better represent the depth that convection has reached? In the subpolar North Atlantic and the Nordic Seas stacked mixed layers are prevalent and automatic routines are generally not very adept at identifying the depth of wintertime convection (Pickart *et al.*, 2002; Va'ge *et al.*, 2015; Brakstad *et al.*, 2019). I encourage the authors to consider additional verification of the mixed-layer depths and expand the discussion about the limitations of the routine and what the outcome actually represents.

Indeed, estimates of MLD are or could be misleading. We understand the referee is particularly concerned about stacked mixed layers. For the summer profiles north of Iceland we do not see substantial evidence of remaining ML from the preceding winter. For the winter and particularly in the south of Iceland, the deep convection is so intense that we do not see any residual ML.

We have elaborated in the revised version of the manuscript about the validity of our method and that we are careful of not picking residual ML's. Taking advantage of your suggestion and also the references, we looked at the glider data from the Iceland Sea that we found in Våge et al., 2018. Since the data is open-access, we computed the MLD with our method (modified density threshold) to reproduce the MLD shown in that manuscript. Surprisingly, a quick check of the values gives a good qualitative agreement. The Iceland Sea is a challenging region as the winter ML can be shallow and quite complex in the internal structure. This structure is often compensated for in density and this is why we believe our method works.



We can confirm that our method with the density calculation and adapted threshold works quite well. Moreover, we corroborate as well that the temperature threshold method does not work in this region. We had come up with that conclusion before but now we have an extra verification bench mark for this.



More details about the PWP model (Price *et al.*, 1986) should be included in sections 2 (Data and methods) and 4 (MLD driving mechanisms from a 1D model). In particular, I miss information about how the model functions, about the initial conditions and forcing time series, and about the integration period and time step used for the simulations. I am sceptical about some of the results from the simulations, in particular the substantial impact of freshwater forcing on stations FX9 and ST5. 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.

The information on the model inputs and details has been better detailed in Section 2 and supplementary material attending to this comment and a similar one from Reviewer 1. Likewise a new figure with the model outputs have been drafted for summer as it had older data, thanks for noticing it. We have also included the plots for the initial conditions and forcing time series in the supplementary materials.

I am not fully convinced that these water mass definitions (Figure 2, Table 2), dating back to Rudels *et al.* (2005) and primarily focused on polar conditions, have high fidelity around Iceland today. For example, the 0°C isotherm no longer provides accurate distinction between Atlantic- and Arctic-origin water masses (Vağge *et al.*, 2022). Perhaps these definitions should be updated or adapted for current conditions around Iceland? It matters when these definitions, without additional evidence, are used to determine the origins of different water masses. For example, are you confident that the dominant water masses near the surface at SI8 are of polar origin (line 172)? At times, the cold, fresh profiles in Figures 2c and 2d are unmistakably polar

(compare to Figures 11c and 12c in Rudels *et al.*, 2005), but while there may be some trace of polar water at SI8 the rest of the time I am not convinced that it is the predominant surface water mass. The same applies to the Atlantic-origin water at the same station (line 173). This could be more recently-formed intermediate water in the Iceland Sea that may be warmer than 0°C (Våge *et al.*, 2022). A closer inspection of each profile, to verify that it displays the intermediate temperature- and salinity maxima characteristic of Atlantic-origin water (Mauritzen, 1996), would likely be necessary for unambiguous identification.

It is a valid point, we understand the concern of the reviewer about the definitions of water masses; we looked at the suggested reference (Våge *et al.*, 2022) and we cannot find any definition that could be used to correct ours. We did look at Swift and Aagaard, 1981, who suggested a similar cautionary message about the 0°C limit. We have updated our manuscript by adding the full reference to the definitions used in Casanova *et al.*, 2020, which are based on definitions from Rudels *et al.*, 2005 and Våge *et al.*, 2011. We would be very happy to re-define the water masses if the reviewer could kindly point at us what would be the best definition we have for Icelandic waters. Recent studies support this simple definition using radioactive tracers, those techniques seem to clearly distinguish PSW and on a TS space they do fall in the same definition we are using (Dale *et al.*, 2024). While we understand these definitions should be carefully used or revisited, this is not the main goal of our paper. The main objective of our manuscript is to highlight the richness of water masses around Iceland and we used Casanova-Masjoan *et al.* (2020) to be consistent with previous studies done with the dataset (e.g. Centyfoyen *et al.* 2023).

Particularly, for SI8 we have discussed with our local experts who just confirmed in SI8 CTD data from 2005 – 2019: Water of polar origin is present at the surface in most profiles at SI8, as indicated by salinity <34.9. While in summer, there is a rather shallow (<100 m) but very fresh PSW layer above a subsurface AW core, in winter the ML reaches deeper (150 m) and salinity is higher, likely due to some AW contribution. This is the case both in winter (upper 150 m) and in summer (less deep, but even lower salinities). Typically, ML salinity at SI8 is even lower than 34.8, so the exact criterion does not change this notion.

In deeper layers, a gradual warming is observed as elsewhere in the deep Nordic Seas. Using the 0°C criterion to distinguish between ArOW and AtOW results in a reduced fraction of ArOW which might be biased. However, only the deepest and densest part of the profiles falls into the ArOW category. The water formed in the winter ML at SI8 was below 0°C until ca. 2000, but predominantly >0°C since then. This would indeed change the classification from ArOW to AtOW although the physics remained the same, only at a higher temperature level.

Nevertheless, our study is not based on water mass transformations thus we have added a caveat on this concern stating that the fixed definitions might lead to biased estimates as waters are generally warmer than 20 years ago.

In the revised version we have added the following sentence: “It is noteworthy that using fixed definitions of water masses may lead to biased estimates, as these water masses have been steadily warming over the past two decades”.

General comments:

The paragraph on Atlantification of the Arctic Ocean in the introduction (line 63) appears unmotivated and its purpose is not immediately clear. The transition into an α -ocean north of Iceland is later ascribed to Atlantification, though Atlantification is more generally considered where Atlantic properties displace Polar properties in the Arctic Ocean. If applicable north of Iceland, I think you need to start making a stronger case for the relevance of Atlantification already in the introduction and follow that up in the discussion (e.g., line 373).

This is a great observation, often Atlantification is associated with the Barrents, Kara and Eurasian basins. We conceptualized Atlantification as an increasing influence of Atlantic Water (AW) in the Arctic Ocean, which has consequences such as the ones we observe. For example warmer subsurface layers including the mixed layer. Iceland is at a unique boundary, a sub-Arctic region that shares both Arctic and Atlantic characteristics. I believe that questioning these concepts in the light of the alpha-ocean propagation north of Iceland would be valid, and may represent a process that has not been reported before within the Atlantification umbrella.

Why did you limit the data set to include observations only to 2019 (line 105)? The MFRI monitoring program is ongoing, so more recent data should be available. You document interesting signals toward the end of the record (Figure 5) that could be further examined.

This is a very good point, the datasets from MFRI are/were not open access. We used the data that we had access to. The current datasets are also being post-processed and quality-controlled. Perhaps we could have added one or two more years but we do not think this would have made any relevant change to our analysis. Finally, we would have loved to have access to all the possible databases as we believe in open-access policies and we know this is currently changing within the Institute.

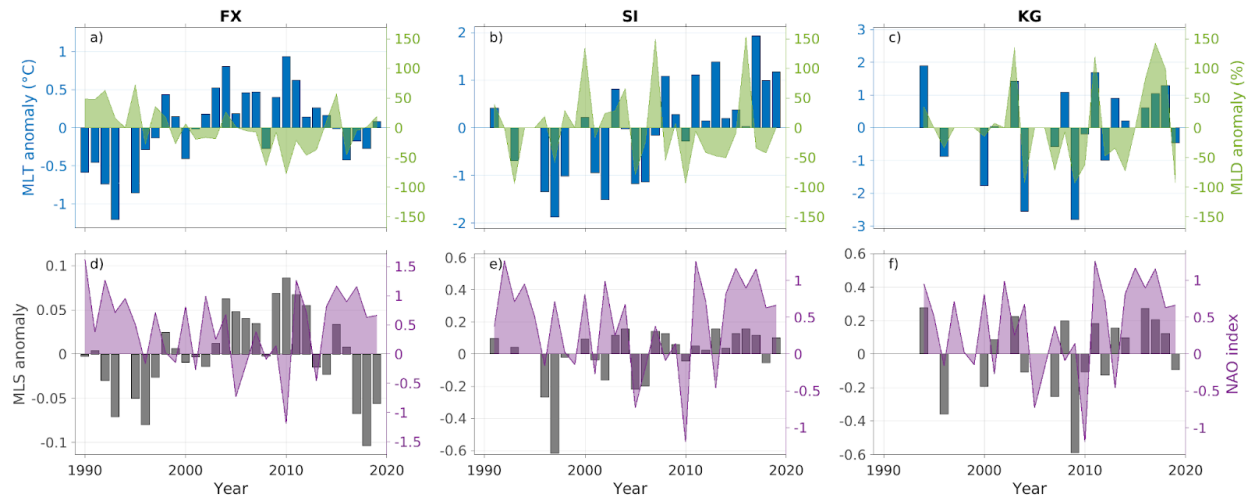
Did you consider including other stations than the deepest on each transect in the analysis? At least some of the transects have several stations offshore of the shelf break. Perhaps for one transect in each hydrographic region, it would be very interesting to know if the other deep stations corroborate the results – that would make your conclusions more robust.

This is a very good point, we did try to increase the number of stations for our study but this was resulting in a larger set of results that we found harder to pack together in a single manuscript. The variability resulting from using more stations increased, thus, we decided to only carry on this analysis for only the deepest stations.

While LB8 may be representative for a β -ocean regime, located in the middle of Denmark Strait it is also characterized by substantial high-frequency variability (von Appen *et al.*, 2014; Lin *et*

al., 2020). Perhaps KG6 would be a better choice for investigating interannual to decadal variability in mixed-layer properties in this region?

Following the reviewer's suggestion we have tried and replaced LB station by KG in Figure 5. (see below). However, KG is less well sampled, and some of the years are missing. Moreover, even if it is true that LB station might be subjected to higher high-frequency variability, this variability would be filtered out by the annual means. Since Figure 6 already shows all the stations, we think that keeping LB is a better choice for figure 5.



I think the text would be more clear if the past tense was consistently used for past events (e.g., line 248). At least, avoid switching between past and present tenses within the same paragraph.

Thanks for the suggestion, we have tried to correct this and hopefully we did not miss this suggestion throughout the text.

The NAO primarily represents atmospheric variability on time scales shorter than the ocean may fully adjust. Variability in circulation and mixed-layer depth/properties may be expected (e.g., Yashayaev, 2007), but to a lesser extent wholesale changes throughout the water column, hence line 257 could be phrased more conservatively.

We agree with the referee about this lag and we have now added a paragraph with the correlation values and the lags. The paragraph now reads: *“The observed variability of the ML and its temperature in FX9 exhibits certain correlation (R) with the NAO; for MLD, the best correlation was $R=0.53$, $p\text{-value}<0.01$ at lag zero; for MLS $R=-0.52$, $p\text{-value}<0.01$ at lag -2 years (NAO leading), and for MLT $R=-0.49$, $p\text{-value}<0.01$ 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. More qualitatively, positive NAO at the beginning and the end of the time series, corresponds with deeper colder and fresher MLs, while negative NAO between 2000 and 2015 roughly corresponds with shallower, warmer, and saltier MLs”.*

If the mean mixed-layer depth at SI6 is less than 100 m (Figure 4), how can anomalies in mixed-layer depth exceed 100 m in both positive and negative directions (Figure 5b, e)?

This is because mixed layer depth anomalies are expressed in percentage. We chose this metric because it takes into account the variability regarding the MLD value, as, let's say, a 20-m anomaly is not as important in a mixed layer of 50 m as in a mixed layer of 300 m depth.

For the statistical analyses (Figures 5 and 6), please provide the values for the correlations and trends reported in the text as well as their uncertainties. That would help make the analysis more quantitative and robust.

Thank you for your suggestion. 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	-	-	-	-	-	-

We have also computed lagged cross correlations between the mixed layer properties and the NAO index.

Please provide some information about the gridding or averaging used for the data set that you calculate the spice frequency shown in Figure 8 from.

Thanks for pointing out the missing information. Figure 8 uses the Brakstad et al (2023) hydrographic dataset, which is an open source product largely used for studies in the Arctic and SubArctic. We have made reference to this dataset at the end of the section Data and Methods. The gridding is explained in the reference and the method used for this product derivation. The spice frequency is described in Strehl et al. (2024).

Detailed comments:

Line 16:

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 24:

Contributions should be in plural. [Fixed](#)

Line 40:

As a proper name, ridge must be capitalized in the Greenland-Iceland-Scotland Ridge. [Fixed](#)

Line 44:

The Faroe Islands would be more appropriate than just Faroe. [Fixed](#)

Line 52:

Regarding the transformation of AW into AtOW, it is unclear what is meant by “along the Norwegian Current western intrusions”. Presumably that water mass transformation occurs along the Norwegian Current, which is a main mode of water mass transformation that is otherwise not mentioned here (e.g., Mauritzen, 1996; Huang *et al.*, 2023). [Thanks for the observation, we have now removed the “western intrusions” to keep the text simpler.](#)

Line 56:

Very likely heat loss is important also for water mass transformation east of Greenland, but wind forcing and sea-ice retreat are necessary for preconditioning this region before convection can occur. [We have tried to re-phrase this sentence but we are still unsure about the specific change the reviewer is suggesting. We would appreciate a more detailed recommendation to address this point.](#)

Lines 60:

Do you mean from the Arctic Ocean? The Arctic, by itself, is ill-defined and has multiple interpretations. The reference to Arctic as north and northwest of Iceland (line 91) does not help clarifying. [Indeed it is an ill-defined, we believe in this case it is the Arctic as the main source of the freshwater is on land and would not necessarily mean Arctic Ocean. Nevertheless the one in line 91 we completely agree that should be Arctic Ocean, thanks for pointing that out.](#)

Line 64:

The word “extent” is used twice in the same sentence. [Thanks, fixed.](#)

Line 71:

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 79:

The definite article in front of 40% should be removed. [Thanks, fixed.](#)

Line 82:

It would be good to be more specific about what kind of mixing you refer to, perhaps diapycnal mixing or convection? [Thanks, fixed.](#)

Line 170:

There should be a comma after southwest of Iceland. [Thanks, fixed.](#)

Line 185:

A reference to Jo´nsson and Valdimarsson (2012) would be appropriate here. [Thanks, fixed.](#)

Line 214:

What do you mean by “hydrographic onset”? [We have rephrase this to hydrographic characteristics](#)

Line 226:

The sentence starting with “Therefore, ...” is unclear and should be rephrased. [Thanks, fixed.](#)

Line 228:

It should be: "... the potential for deep convection ..." [Thanks, fixed.](#)

Line 241:

The paragraph starting on this line is repetitive and could be written more concisely. [Thanks, fixed.](#)

Line 256:

The Feucher et al. (2022) paper is missing from the reference list. [Thanks, fixed.](#)

Line 278:

Please specify what the anomalies are computed from. Are they anomalies relative to the record-long mean at each station? [Yes, they are relative to the record long. We have now added this to the manuscript.](#)

Line 286:

The term aliasing has a specific meaning in time series analysis, where high-frequency variability appears as a lower-frequency signal due to inadequate sampling. It is not clear that the term was used appropriately in this sentence.

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

Line 289:

The reference to Polyakov *et al.* (2017) is not appropriate here, they did not consider the mixed layers around Iceland or the NIIC in their study. [Thanks, removed.](#)

Line 295:

Mixed layers in summer are shallow primarily because the buoyancy forcing abates in spring and becomes largely positive in summer, episodic wind and wave mixing (also reduced in summer) ensure that there is a mixed layer. This is a main reason why stratification is higher in summer than in winter. Please rephrase. [Thanks for the complementary information. In this sentence we are not discussing the mechanism that sustains the ML here; rather, we are stating the observation that summer stratification is high and that the MLD is shallow. We believe that adding a discussion of the underlying mechanism at this point would be confusing.](#)

Line 311:

Do you mean SI8 rather than LB8, if you refer to the region north of Iceland? We started the discussion with LB8 and we considered the stations KG6, SI8 northeast, which are b,c,d. We have now explicitly written those names in the text.

Line 312:

Wind-stress, by itself, will not cause convection - for that you need buoyancy forcing. This process would more appropriately be referred to as wind-mixing.

We agree with the reviewer and we have changed this sentence. We were trying to point out that station LB is equally balanced with wind mixing and convection. We tried to make the difference that it has some convection potential from the AW but it is reduced compared to the southern stations that have only AW. We have now added "wind-mixing".

Line 350:

There is at least a preposition missing near the end of this sentence. Thanks, fixed.

Line 356:

The surface water masses of the EGC would have a strong Polar (not Arctic) influence. Thanks, fixed.

Line 375:

An increasingly temperature-dominated stratification may in principle lead to deeper convection, but reduced atmospheric forcing and shallower, less dense mixed layers (Moore *et al.*, 2015; Våge *et al.*, 2022) strongly suggest that the Iceland Sea is not likely to become an important source to the NIJ. Indeed, we agree with the reviewer, we are not suggesting that this region would be an important source but more like it could contribute or even modify (by diapycnal mixing) the dense flow carried by the NIJ.

Line 390:

The warming trend of 2°C, was that over the entire record length? When reporting a trend, please also provide a time reference. We are sorry for misleading, we are trying to report the mixed layer temperature range from winter to summer, not a warming trend. The warming trends are now reported in Figure 6.

Figure 5:

It would have been illuminating to see panels of mixed-layer density anomalies as well. [Thanks,](#)
[Figure 5 now has been re-designed.](#)

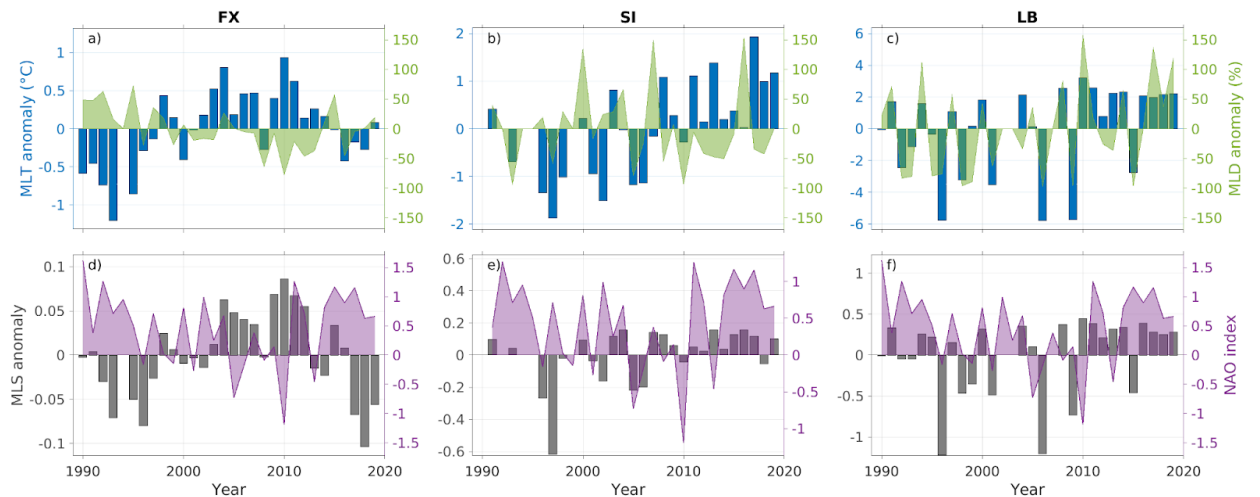


Figure 6:

I think it would be advantageous to use the same color scale for all panels, even if that means that some colors occasionally will saturate, to facilitate direct comparison between stations.

[We have now re-designed this figure also from the comments of the other reviewer.](#)

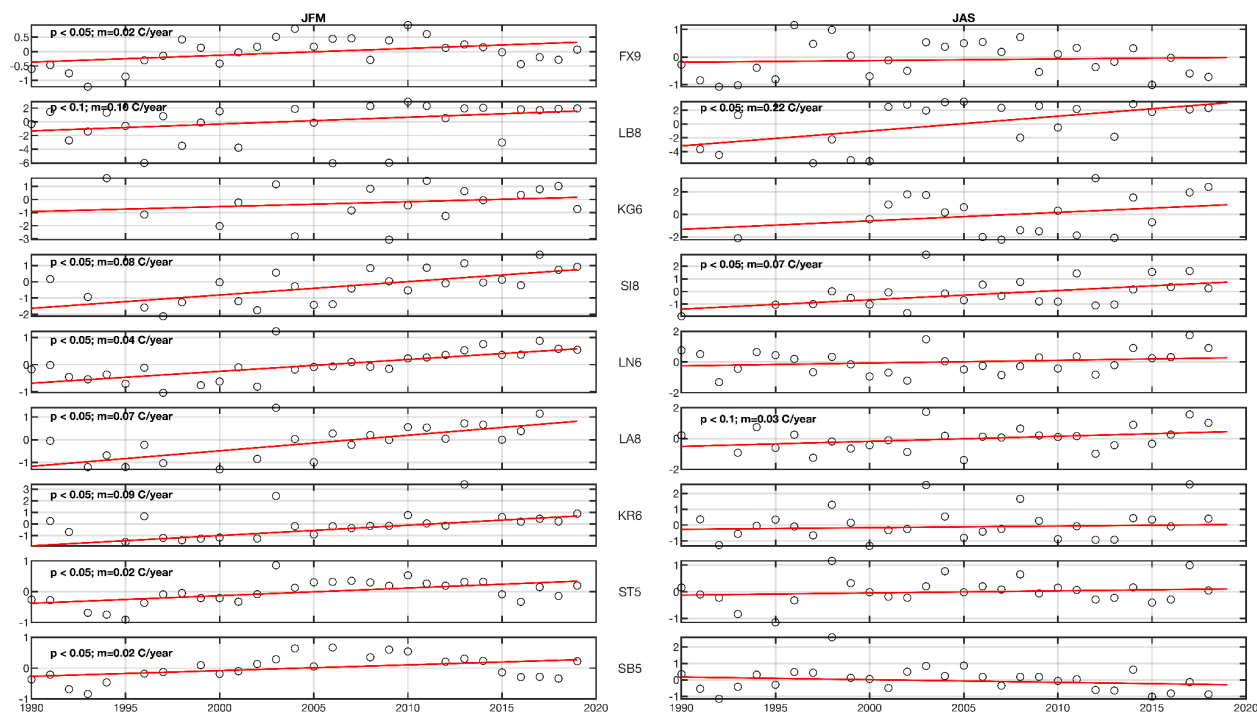


Figure 6, caption:
It should be: "... all stations except LB8 and KG6..."

Thanks, but now Figure 6 has been re-designed so we are no longer using variable colorbars.

Figure 7:

Please also show the total mixed-layer depth resulting from the combination of all driving mechanisms. Do they add linearly? While the observations stem from February and not end of winter when the mixed-layers are deepest, it appears that some of the simulated MLDs will be substantially deeper. In the text, individual panels are referred to by letters, but these letters are not visible on the figure. We think that the purpose of using this 1D model is to illustrate the main mechanisms rather than to reproduce or analyze the full evolution of the MLD. We have indicated the observed MLD with a black dot in the figure representing the winter and the summer. Therefore, we would prefer to keep the figure as it is.

Table 1:

KG6 and HB6 are not located in the Greenland Sea. Referring to this region as Blosseville Basin or western Iceland Sea would be more appropriate. Thanks, fixed.

Table 2:

Some of the less than/greater than signs are in the wrong direction and contradict Figure 2 (e.g., SW temperature, warm PSW density, and AtOW density). Thanks a lot for this correction, now it is fixed

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