Response to Reviewer#1

We thank you for your constructive and thoughtful review of our manuscript. Your suggestions have been invaluable in improving the quality and clarity of our work. In particular, your feedback prompted several substantial revisions: (1) replacing the 2018 dataset with newly available spatial surveys from summers 2022-2024, (2) conducting a thorough spatial analysis based on these new surveys, and (3) incorporating the newly available Monitoring Station observations from 2024. These revisions were central to addressing your major comments and have considerably strengthened the manuscript.

Please find our detailed responses in blue below. In the few cases where it was not possible to follow your suggestions directly, we provide a detailed explanation in our responses.

Best regards,

Linda Latuta on behalf of all co-authors.

Reviewer#1 Summary

This paper presents observational hydrographic data from Disko Bay, Greenland, which sits near a large, fast tidewater glacier draining the Greenland Ice Sheet (Sermeq Kujalleq). They combine existing data sources from a nearby monitoring station (GEM site), two profiling floats, and re-analysis atmospheric & satellite data over two seasonal cycles (the years 2022-2024) along with weekly repeat stations taken over Fall 2023. In the end, they describe a seasonal cycle that is consistent over both years, showing both the modulation of the mixed layer and when the warmest and densest Atlantic-origin waters arrive at the mouth of Ilulissat Icefjord (which leads to Sermeq Kujalleq, SK hereafter). Overall, they present a detailed account of the hydrography in the bay's water mass layers, which should be beneficial to future studies looking to oceanic change as a driver for mass loss from Greenland. Although many pieces of the study are already known, I find the most novel results to be their discussion of the deep-water renewal and lifting of the Atlantic-origin waters above sill depth at the mouth of the icefjord. However, the paper seems overly complicated for the conclusions it reaches, and the figures could be clarified and improved given all the disparate sources of data. For example, explicitly stating what is novel here and what is validation of previous conceptual models of the Bay's exchange with the coast and the icefjord would be helpful. One way to do that would be to update or revise the schematic from Gladish et al. part 2 (his Fig. 15). But there are other ways too. I was also not convinced of the Fall along-isopycnal warming signal they attribute to outflowing glacially modified waters (GMW). Finally, a new paper (Picton et al. 2025) came out in JGR-Earth Surface recently that uses some of the same data (floats and GEM site), although their focus is slightly different.

More on these comments below, as well as line by line comments. I certainly believe these data and this write-up to be publication worthy, but their impact will be significantly improved through revisions.

Major comments:

1) During the discussion period, another paper came out that studies the same region with some of the same data sources (Picton et al. 2025). It would be good to include this reference in the

revision. For example, that paper shows temperature vs time at 240 m depth and seems to show a similar Fall warming and seasonality as described in this current study. I think discussion of this new publication will only strengthen this one. In terms of other references, I believe the authors could certainly expand their sources. I point to a few examples in the comments below.

This was a helpful suggestion, and we also believe the new Picton et al. (2025) provides valuable context for our study and strengthens both the Introduction and Discussion. In the revised manuscript, we have incorporated this reference and expanded our literature base to provide a more comprehensive context for our findings.

2) In terms of the along-isopycnal warming trend they document and discuss, I am not convinced it's entirely from the outflow from the icefjord. I think what they are saying is that the summer outflow signal is delayed and lagged as it transits Disko Bay, so the warming they see through the Fall is from that entrained WGIW. However, the melt season at the glacier (e.g., look at a subglacial discharge time series for SK) is relatively short and the warming seems to continue beyond the length of a typical melt season. Although they calculate advective time scales for other processes in the paper, they don't estimate the time scales here. Are there other mechanisms that could cause this warming trend through the Fall?

We have now further extended the observations, made some new figures, and modified our views and discussion accordingly. The observed along-isopycnal warming in autumn has two possible sources, and it is unlikely to be solely driven by outflow from Ilulissat Icefjord. The relative importance of the two sources is hard to pin down, but we now suggest that the advection from outside the bay is dominating. The new profiles shown in new Fig. 4 (from north of Aasiaat) show this clearly.

It was not our intention to imply that the GMW was the predominant source, just that it was a physically plausible mechanism. We noted a warm signal near the fjord that later appeared at the Monitoring Station, which we suggested could contribute to the autumn warming through advection. However, the 2018 spatial surveys were not concurrent with the 2022-2024 seasonal time series, as the reviewer also pointed out. This and the absence of profiles upstream, inside the fjord, or on the shelf, made it difficult to determine the origin of the along-isopycnal warming with certainty. We therefore suggested GMW as a possible source.

In the revised manuscript, we replace the 2018 dataset with newly available spatial surveys from summers 2022-2024. This includes profiles upstream of Ilulissat Icefjord at the southwestern end of Disko Bay. Accordingly, we revised the methods, results, and discussion. These surveys show that the warm along-isopycnal anomaly was consistently more prominent at the southwestern end of the bay than near Ilulissat. Furthermore, we show that by October-November, hydrographic properties at the Monitoring Station closely matched those measured at the SW entry to Disko Bay in August, approximately two to three months prior (3 months in 2022 and 2024, 2 months in 2023). Moreover, the magnitude of the autumn warming at Monitoring Station along a given isopycnal (e.g. 1.65°C in 2022, 1.1°C in 2023, and 0.48°C in 2024 between August and November at $\sigma_0 = 26.8$ kg m^{-3}) is similar to the along-isopycnal temperature difference between Aasiaat and the Monitoring Station in the spatial survey data in August of those same years (1.89°C in 2022, 1.1°C in 2023, and 0.56°C in 2024).

This was consistent for 2022, 2023 and 2024 (new Fig. 7) and suggests that the autumn along-isopycnal warming at the Monitoring Station is more likely linked to this signal advected cyclonically around the bay. We updated the discussion to reflect this change and to address this major comment explicitly. In the updated discussion, we interpret the autumn along-isopycnal warming primarily as the seasonal signal of the WGC, while GMW exported from Ilulissat Icefjord may provide a secondary, but smaller, contribution.

3) The presentation quality is average. I think the figures could be improved for clarity and I mention specific instances in comments below. Also, in all figure captions, it would be helpful to specify what data sources were used, as it was hard to follow along and keep things straight, given there was the 'monitoring' site, the floats, and the 2018 spatial survey all used for different things. Simplifying the results section would help here, as a lot of discussion seems to occur in the results, in addition to the formal discussion section.

In the revised manuscript, we simplified and reorganised the Results section to improve readability and reduce overlap with the Discussion. We also revised all figure captions and legends to state the data sources used clearly.

4) The conclusion that GMW entrain Atlantic-origin water in Greenland tidewater glacier fjords and produce outflows with temperature anomalies relative to their depth is not new. This has been shown over and over again in many different systems (e.g., Straneo et al 2012 looked at several sites around the ice sheet). Many more references since have examined this dynamic.

We agree with the reviewer that the role of Atlantic-origin entrainment in producing warm anomalies in the outflows is well established for Greenland fjords. Our observations were also consistent with this process, so we had, and still have, included it in our discussion. In the revised manuscript, we improved the discussion by providing a clearer context and citing a more comprehensive set of relevant studies, including Straneo et al. (2012) and subsequent work.

5) In the results and abstract they mention that renewal can occur in Fall given upwelling winds. However, it appears like there were consistent upwelling winds in the second year as well, yet no similar renewal occurred. Was there a reason for that? It seems hard to claim that it is part of the seasonal cycle if you have it observed for 1 out of 2 years of data collected, i.e., some other process(es) must be relevant.

In the revised results, we now distinguish more clearly between autumn 2022 and autumn 2023. In autumn 2022, strong and persistent upwelling-favourable winds coincided with a rapid increase in basin density and temperature. In contrast, during autumn 2023 the WGIW boundary shoaled by ~50 m, but no increase in basin density or temperature was observed, indicating that renewal did not occur. We have also made a more quantitative comparison of wind forcing and upwelling velocities between the two autumns, showing that brief episodes of negative wind stress were present in October-November 2023, but weaker and less persistent than in autumn 2022. Vertical velocities were also lower in comparison. While we observe a shoaling of the WGIW boundary in autumn 2023, densities of water over the sill did not exceed those of the basin, and thus basin renewal was not observed.

We agree with the reviewer that observing renewal in only one autumn limits the strength of the seasonal interpretation. Accordingly, we have revised the manuscript to de-emphasise autumn renewal as a regular feature of the seasonal cycle. Instead, we now frame it as a process that can occur under favourable conditions, with variability between years.

Line by Line Comments

Line 25: AW can enter shallower than 200-250 m too (as you show later in this paper!), but the sill sets the maximum depth of water that can flow into the icefjord.

Thank you for pointing this out. We clarified the text to avoid implying that AW is confined to these depths.

Line 40: I am a bit worried throughout by the use of a summertime 2018 spatial survey with the time series data from 2022-2024. Summer 2018 was at the end of the cool period noted by other authors. Is there some way to give context for the interannual variations in these data? Maybe the new Picton et al. paper could help.

We appreciate the reviewer's concern. We have also come to fully agree that combining a spatial survey from summer 2018 with the time series from 2022-2024 introduced some inconsistencies. Particularly given that 2018 occurred near the end of a relatively cool period in Disko Bay, as described by Khazendar et al. (2019), Joughin et al. (2020), and more recently by Picton et al. (2025). To address this concern and provide a temporally consistent framework for our analysis, we have revised the manuscript to replace the 2018 spatial survey dataset with newly available spatial surveys from the summers 2022, 2023, and 2024, conducted by the GEM programme.

The change has multiple benefits that will be reflected in the manuscript:

- Temporal consistency: all spatial and time series data now originate from the same period (2022-2024), ensuring that our analysis avoids mixing data from two distinct hydrographic regimes.
- Stronger spatial analysis: the temporal overlap between the new spatial surveys and the time series we focus on in the manuscript enhances our ability to address spatial variability, which was raised in other comments. We refer to the new spatial analysis in response to these comments.
- The updated surveys include new profiles from regions further upstream of Ilulissat Icefjord, which help clarify the origins and pathways of the warm signal discussed throughout the manuscript.

Accordingly, we have revised the Methods section "Spatial hydrographic surveys in 2022-2024" and added a new Results section "Spatial variability".

Line 43: Using these initial guiding questions to structure your discussion might be a way to tie the paper together more. Otherwise, they do not need to be included here. Overall, the tone of the paper is informal and often falls into the trap of (i) telling us what you are about to write about, (ii) telling us, and then (iii) summarizing what you just told us. That lengthens the paper quite a bit. I would try to cut out the preambles on all your results, etc. and just get to the point. One example is at line 79 where you could delete "Below we describe each dataset".

Thank you for your helpful suggestions. In the revised manuscript, we have removed unnecessary openings and adopted a more concise tone. We have restructured the results and discussion to avoid repetition.

Line 59: Is the WGCC relevant to Disko Bay given this uncertainly around whether it even exists at this latitude? That is, does it matter if it's the WGC or WGCC?

We agree with the reviewer that the distinction between the West Greenland Current (WGC) and a possible West Greenland Coastal Current (WGCC) is not well established at the latitude of Disko Bay. In the revised manuscript, we have updated Section 2 to reflect this uncertainty and now refer only to the WGC.

Figure 1: I think you should try to add on the 2018 survey locations to this figure, so all the observations are on one map. They could be small dots. Also, it is impossible to see the crosses used for the float's first profile. Finally, can you label Vaigat Strait here too since it is mentioned a couple times in the text?

This was a helpful suggestion. In the revised manuscript, we have (i) added the 2022–2024 survey station locations to Figure 1, (ii) replaced the float first-profile crosses with larger diamond markers to ensure visibility, and (iii) labelled Vaigat Strait on the map.

Line 103: float data 'were' (data are plural)

Corrected.

Line 110: Not clear what CTD observations the float data were compared to?

This has been clarified, thank you. Salinity obtained from both floats was compared against CTD observations collected at the Monitoring Station (now extended until November 2024). This was done during periods of temporal overlap, as well as against spatial hydrographic surveys (new 2022-2024 data). These comparisons confirmed that the T-S relationship at higher densities exhibits spatial heterogeneity across Disko Bay. Accordingly, float salinities in these density ranges were compared against the full set of available CTD observations in T-S space. Based on this, we found that salinity sensor drift did not exceed 0.02 PSU over the period of data used from either float.

Line 129: Unless the Semper et al 2024 paper is published, it is not helpful to cite here. I would outline the method more completely or maybe show an example or two in a supplement. It's also not stated what threshold you use for the 'sharp increase' in normalized sum-of-squared errors or if the final MLDs are sensitive to this threshold. Finally, it's a two-step method and I clearly see the 'first' step. Is the second step the checking with the 1 standard deviation envelope?

The Semper et al. (2025) paper has now been published, and we cite it directly. Nonetheless, we have expanded the description of our method for determining MLD in the revised manuscript to ensure clarity and reproducibility.

First, for each profile we compute the normalised sum-of-squared errors (SSE) between individual density values and the depth-averaged density, calculated from the surface down to all possible depth intervals. The mixed layer depth (MLD) is identified at the depth where a sharp increase in normalised SSE occurs, indicating a transition from a well-mixed surface layer to more stratified layers below. We applied a threshold of 1.5×10^{-4} kg^2 m^{-6} to detect this transition, which we found to be robust across the profiles.

Second, we verify the computed MLD by assessing whether temperature, salinity, and density from the surface to the computed MLD fall within one standard deviation of their respective mean values (Pickart et al., 2002; Semper et al., 2024).

Additionally, we have clarified that in some profiles, no MLD was detected, either because the mixed layer was shallower than the first measured depth or because the surface layer was stratified.

Line 135: The PW layering in Fig 2b is very muted, especially compared to other profiles and systems I've seen published. I assume this is because there are many profiles with surface layers that are not PW? I know that you lump all cool, fresher waters together in this PW layer, but it might be stated more up front before talking about this figure. In addition, the different sources/types of PW presumably have their own seasonality (and in fact some of your discussion of how the mixed layer changes over the year is linked to this).

This was also a helpful comment. We agree that the layering of PW appeared muted in the original version of Fig. 2b and that further clarification improved clarity of the PW definition and the seasonal variability associated with its different sources.

To address these points, we have made the following revisions:

Figure 2 updates:

- In the updated Fig.2 we changed the (b) panel to show average seasonal profiles of temperature and salinity based on all observations, instead of lumping them together into one average.
- These seasonal mean profiles more clearly show PW structure, particularly in summer, when the temperature minimum below a warm surface layer becomes more distinct (less muted).
- This new representation improves the visualisation of PW stratification and its seasonal evolution, as well as makes it possible to show the PW/WGIW layering in more detail than in the previous version of the figure.

Clarification in Methods Section:

- As suggested, we have added an explicit clarification of PW definition. We now clearly state that we adopt a broad definition of PW, encompassing a range of cool, fresh water types.
- This clarification acknowledges that each component may vary seasonally, and that the resulting PW structure reflects the cumulative influence of these sources.

Revised Results Section:

In the revised results, we now explicitly integrate the concept, as highlighted by the reviewer, that different components of PW exhibit distinct seasonal behaviours. This includes the development of a stratified surface following sea-ice melt and contributions from other freshwater sources later in the season (see updated Figure 2).

Line 154-155: Is a mixed layer always present? Inside the icefjord and I imagine in Disko Bay at least at times there is a stratified surface layer due to ice melt or other freshwater inputs.

Good question, the answer is no. The presence of a mixed layer is not always observable. To clarify this, we've updated the former Figure 4 and the methods of how we determine MLD. We also added empty markers to this figure to indicate instances where the profile lacked a detectable mixed layer or where the layer was shallower than the first near-surface observation. Consequently, in these cases, the mixed-layer depth, temperature, and salinity are not plotted or calculated. We corrected the manuscript text accordingly.

Line 156-162: I think this paragraph could be shortened, specifically the part about icebergs. Just say they are part of the freshwater flux.

We agree, this is shortened in the revised manuscript.

Line 172: For the wind stresses, you show multiple equations for the drag coefficients, but never the equation for the actual wind stress (which presumably has the drag coefficient in it). Given the relatively small amount of discussion on the Ekman pumping calculation, this section seems long.

Agreed. The wind stress formulation is included, and the section is shortened in the revised manuscript.

Figure 2: Note that iceberg or glacier melt would pull water masses along a line with the same slope as the SMW line, but not necessarily on that exact line shown. That is, the slope is important, but not the exact location and that is true for SGD too, as it matters where the outflowing plume reaches neutral density. All those pink dots in the GMW section seem like they are being pulled down the melt line, but this isn't clear from the figure or the text.

This was also a helpful comment, and we fully agree. It is the slope of the mixing lines between water masses and SGD(runoff)/SMW that carries physical significance in the context of meltwater mixing, and the exact position of these lines depends on local conditions. This point has now been clarified in both the revised figure and the updated manuscript text, as outlined below:

Figure 2 revision:

The original figure (where colour represented depth and all observations were shown in a single panel) has been replaced with a more informative, multi-panel figure:

- Panels a-c (instead of former panel a): Show all T-S observations, colour-coded by season and separated by year (2022, 2023, 2024), highlighting seasonal and interannual variability and more clearly illustrating temporal development.
- Panels d-e (instead of former panel b): Present vertical temperature and salinity profiles, with mean seasonal profiles and clear demarcation between PW and WGIW. This update also responds to the "Line 135:.." suggestion.
- The SMW and SGD mixing lines have been retained, but now are clearly intended as reference slopes, rather than having a specific mixing line with WGIW as before.

The revision in Results :

We have revised the text that could imply that observations follow a specific mixing line between WGIW-SGD or WGIW-SMW. Instead, the new text now describes that some observations align with the slope of SGD (runoff) or SMW mixing lines.

Line 202: ‘Both’ is wrong, as you list three things here...and then you have a 1 sentence paragraph.

Corrected. Thank you.

Fig 5,6,8: I wonder if these figures could be combined somehow for more visual impact and being able to compare the timing. I think this is where my confusion over the source of the deeper warming comes in, as these figures are all getting at the same thing but are separated.

Good suggestion, and we agree. We have combined the former Figures 5 and 8 into a single figure in the revised manuscript, improving clarity and enabling a direct comparison of timing. To complement this, we expanded and updated the former Figure 6, which remains important as a stand-alone figure presenting distinct results. Specifically, we now show T-S diagrams for the Monitoring station for the summer-autumn periods of 2022, 2023, and 2024. We use these T-S diagrams, overlaid by spatial survey data from August each year, to show that the October-November properties closely resemble those observed upstream two months earlier (addressing the Major comment #2).

Fig 8 and analysis: Float 2 seems to stay relatively stationary but Float 1 moves quite a distance. In Figure 8 and in the results, how much of the ‘Float 1, 2022-23’ variability is due to time and not a spatial gradient? It’s interesting that the second-year variability is lessened given that that float didn’t move as much.

In the revised manuscript, we now base our Polar Water analyses solely on the stationary Monitoring Station. We agree that float measurements can alias spatial and temporal variability, the former being particularly pronounced in PW as the revised “Spatial Variability” showed with newly added 2022-2024 hydrographic surveys. Nevertheless, to address this question: Float 2 covered autumns 2023 and 2024, and the reduced variability it recorded (particularly in 2024) is consistent with the new 2024 Monitoring Station data, which also show that the temporal variability of autumn warming was larger in 2022 than in either 2023 or 2024. Thus, while we no longer use floats directly in PW analysis, the larger variability observed by Float 1 still reflected a real temporal signal, not only a spatial artefact.

Line 263: ‘representation’ of what?

This has been revised. We wanted to state that comparing properties along isopycnals rather than fixed depths provides a more accurate description of the water mass differences across the bay.

Figure 6: The colored dots on the T-S diagram are fairly hard to distinguish. Either try different colors or make them bigger markers.

The new version of Fig. 6 has increased marker size to improve visibility.

Figure 7: Labelling regions on Fig 7a would be helpful.

In the revised manuscript, Figure 7 (2018 spatial analysis) has been removed and replaced with updated spatial analyses from 2022–2024. In all the figures, we have now ensured clear labelling of data sources and regions/locations.

Line 280: But the subglacial discharge that you surmise is causing this is certainly a transient feature, as it ramps up each melt season and dramatically ramps down by end of August or Sept. If the along-isopycnal warming commenced in August, does that imply a 2-month lag time in the icefjord for waters to come out given Float 2's position? Does that jive with estimates of the icefjord circulation/advective scale?

This is an excellent question. As noted in our response to earlier comment ("Fig 8 and analysis: …"), we now base all PW results on the Monitoring Station data. The onset of along-isopycnal warming in August is consistently observed in 2022, 2023, and 2024. As discussed in our earlier response to Major Comment #2, we suggest that this reflects the advection of a warm signal entering the bay from the southwest and propagating cyclonically. We estimated the lag between the SW'rn entry to the bay (station near Aasiaat) and the Monitoring Station is approximately 2-3 months.

Although we cannot resolve the precise advective pathway, synoptic cruise data show consistent T-S differences that support cyclonic circulation past the vicinity of Ilulissat Icefjord. A similar analysis to the one outlined in Major Comment #2, but using a station north of Ilulissat, indicates a comparable relationship in one August survey, but with a shorter (≈1 month) lag. In all years, the Aasiaat station was the warmest in August, with the anomaly weakening progressively along the cyclonic path.

We can make a rough estimate of the mean subsurface velocities:

- The distance between Aasiaat and Monitoring Station of ~170-200km and a 2-month lag implies mean velocities of ~3.3-3.9 cm/s; the same for a 3-month lag implies ~2.2-2.6 cm/s.
- The distance between north of Ilulissat Icefjord and the Monitoring Station of ~90km and a 1-month lag implies mean velocities of ~3.5 cm/s.

To our knowledge, advective pathways and velocities within Disko Bay are not yet well constrained beyond the presence of a cyclonic circulation. Nevertheless, our estimates of ~2.2-3.9 cm/s fall within a plausible range and are comparable to the <10 cm/s velocities at 160m depth recorded in Disko Bay by Sloth and Buch (1984) and ~2-7 cm/s subsurface WGC velocities (Curry et al., 2014). These are also within ranges measured within the top ~200m in Ilulissat Icefjord Gladish (2015, part 1), and mean velocities of ~4 cm/s in Sermilik Fjord (Jackson and Straneo, 2016; Jackson et al., 2018).

Figure 9: It took me a while to infer these data were from the floats (I think?). Indicating that in the caption would be helpful. Also, you mention shading for d-e, but the shaded areas are different. There is also no mention in the text of the large data gap in the second year, which precludes the specification of the start of the renewal.

We updated the former Figure 9 (will be new Figure 8) to include labels and clarified the caption for panels (e-f), where data comes from the Floats (as Monitoring Station observations don’t extend to 400m depths). To avoid confusion with a mismatch of shading, we removed the shading from panel (d) and replaced it with horizontal lines indicating the mean WGIW in the periods between abrupt changes.

In addition, we revised the text to acknowledge two data gaps in the second annual cycle shown in the figure (October 2023-January 2024; January-March 2024). This is because no float observations were deep enough during these periods, now explained clearly. Nevertheless, as density and temperature did not increase between October 2023 and January 2024 we conclude that basin renewal likely did not occur that autumn. For the second gap, between January and March 2024, we now note that density and temperature increased, so the spring renewal has likely begun in that period.

Figure 9 discussion: Again, as for the float data above, I would like some justification for using the float data as time series again given the movement of Float 1. How much variability would you expect over this distance (maybe you can use the 2018 spatial survey to estimate this)?

Thank you for raising this important question. To address it, we conducted a spatial analysis using the 2022-2024 hydrographic surveys and added a new section, “Spatial variability”. We analysed spatial variability within PW and WGIW across five near-synoptic cruises with repeated sampling locations (August 2022, May and August 2023, May and August 2024) and included data from Float 1 and 2 that overlapped with these cruises.

For PW properties, we found considerable spatial variability along isopycnals and at constant depths. The revised manuscript will state that “understanding PW seasonality is better achieved using fixed-point observations from the Monitoring Station, as the pronounced spatial variability within PW will interfere with the seasonal patterns inferred from Float data”.

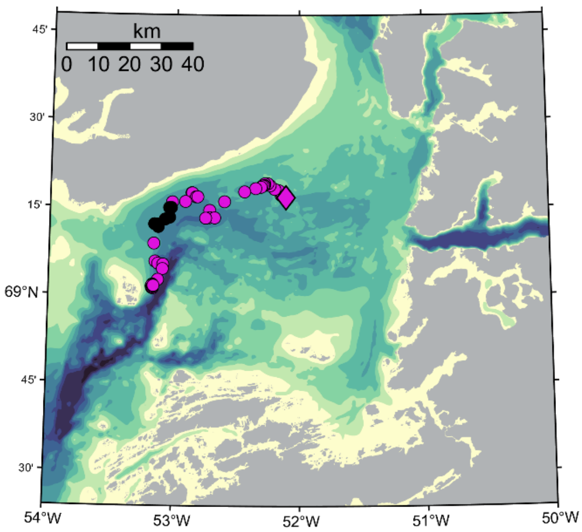
In contrast, WGIW exhibits minimal spatial variability along isopycnals. Synoptic observations within the WGIW fall along a narrow line in the T-S space for all five cruises. Some spatial variability is present at constant depths, but it is relatively small and decreases with depth. We will also provide a summary table of this analysis, which is also shown below. Across the five cruises PW/WGIW boundary varied with a standard deviation of 20 to 30m in depth among all sampled locations. Density variability averaged approximately 0.023 kg m^{-3} at 300m and 0.014 kg m^{-3} at 400m depths, based on within-cruise variability in density at each depth.

Given that Floats sampled the deeper parts of the bay, our analysis of WGIW seasonality relies largely on float data. These observations can be used to assess WGIW variability along isopycnals confidently, but also along constant depth levels, provided that the signals exceed the background spatial variability noted above.

We now address your question regarding Float 1 specifically. Its wide spatial coverage between August 2022 and May 2023 is considered acceptable for studying WGIW properties, because the temporal signals this Float captured with respect to WGIW renewal at 400m depth (0.05 kg m^{-3} increase in November-December 2022 and 0.03 kg m^{-3} increase in spring 2023) exceed the background spatial variability observed in the summer cruises of these years.

Year	Mean WGIW boundary depth (m)	SD of WGIW boundary depth (m)	SD of density at 300 m ($kg\ m^{-3}$)	SD of density at 400 m ($kg\ m^{-3}$)
2022 Aug	250	30	0.0187	0.0132
2023 May	156	27	0.0156	0.0148
2023 Aug	244	24	0.0201	0.0048
2024 May	238	23	0.0262	0.0175
2024 Aug	320	23	0.0343	0.0217

Moreover, Float 1 moved very little during the spring renewal. As we noted in the methods, between the 6th of February and the 4th of April, 2023, the float was profiling underneath the sea ice, with no known positions. But between the acquisitions with known positions, the float's position changed only by 3.1km. During the autumn renewal in November-December 2022, Float 1 drifted by about 13km. Below is a figure showing positions covered during this time with black markers. Yet, over this time, the WGIW boundary shoaled by >100m, by far exceeding the standard deviation in boundary depth noted in spatial analysis. This demonstrates that the observed renewal reflects a robust signal rather than an artefact of float drift.



Line 319: I don't follow the statement that wind forcing was not evident during this period. It certainly looks like upwelling favourable winds were present at that time?

Thank you for pointing this out. The original statement about wind forcing not being evident during spring 2024 was unclear. Our revised results (strengthened with a more quantitative analysis of winds in response to the second reviewer's suggestion) confirm that upwelling-favourable winds were indeed present during this period, although they were weaker than those observed in spring 2023.

Line 323-334: A lot of this paragraph would fit better into the discussion. There is some redundancy between the results and the discussion, which I think leads to some confusion and adding to the length of the paper.

We moved this into discussion and updated the manuscript to reduce such redundancy throughout.

Line 333: I don't see 1.5C, maybe 1C increase?

Corrected.

Line 359: You cite 'typical' ranges here from previous work, but not anything about the range? That is, is it reasonable for this feature to be advective given variability in these conditions?

At ~150 m in Davis Strait, salinity rarely fell below 33.9 g kg^{-1} (never below 33.8 g kg^{-1}), and, unless it was an anomalous year like 2011, density remained above σ_0 = 27 kg m^{-3} over the 6 years shown in Figure 4 of Gladish et al., (2015 part 2). We were examining the minima to understand the origin of the fresh signal extending over 150m in Disko Bay. Thus, we contrast these numbers to density and salinity we observe at 150m depth in Disko Bay, where minima in autumn is lower ($\sigma_0 \approx$ 26.9 kg m^{-3} and $S_A \approx$ 33.84 g kg^{-1} Monitoring St. November 2022; $\sigma_0 \approx$ 26.7 kg m^{-3} and $S_A \approx$ 33.59 g kg^{-1} Monitoring St. October 2023; $\sigma_0 \approx$ 26.7 kg m^{-3} and $S_A \approx$ 33.58 g kg^{-1} Monitoring St October 2024). Given that these values fall below the Davis Strait minima, we find that local freshwater sources within Disko Bay must also contribute, rather than the feature being fully explained by advection from the WGC.

Line 363: You cite both SGD and SMW are sources of freshwater, which is true. But they have one fundamental difference. For iceberg melt, the depth of entrainment is much much shallower than for SGD, so it's really the SGD that is controlling the depth of the GMW layer (well, and the sill). There is some new literature on refluxing at the icefjord sill (Hager et al. 2024) that might be useful as well to explore. This whole section is relatively long for describing a process that other studies have shown already. It's not really the novel or interesting part of this present study- that's more on the WGIW properties in my opinion.

Thank you for this constructive comment and suggestion on how to improve this section. In the revised manuscript, we have substantially shortened and refocused the discussion of freshwater sources within Ilulissat Icefjord. We now outline the key factors that determine the production and depth of GMW, and address iceberg melt separately, highlighting its role in modifying water mass properties within the fjord. In doing so, we refer to Hager et al. (2024) and other relevant studies, but keep the section concise so that we can clearly summarise the sources of freshwater within the fjord before moving on to discuss the properties of its export to Disko Bay, referring to the study of Beaird et al. 2017.

Line 393: do you mean 'width' here instead of height? As you also have draft? I'm not sure any of those papers actually have measurements of iceberg draft, but assume some geometry based on above water volume and ice/ocean densities.

We agree that our wording was unclear, and this has been revised. We now explicitly describe the assumed iceberg geometry: starting from the "small iceberg" with an area of ~1800 m^2 (Scheick et al., 2019), then representing such iceberg as 130m in length and width, from which we estimate it is 65m thick (freeboard+draft, following Enderlin et al., 2016 and their Figure 3a), and has a draft of 55m based on a freeboard-to-draft ratio of 1:7 (Cenedese and Straneo, 2023). We have also replaced "height" with "thickness" to avoid confusion.

Section 5.3: I am not sure this section says very much. A lot of it is speculation about how the results might be important to marine ecosystems. The last paragraph in particular is vague. Adding some meat/content would be good. For example, can you show the cyclonic circulation sense in the 2018 spatial survey (not just in T/S but in dynamic sections)?

We appreciate this comment and agree that Section 5.2 required improvement. In the revised manuscript, we have not included the 2018 data and therefore do not attempt to infer circulation from those sections. Instead, we have revised this section to reduce speculation and to more directly highlight the relevance of our findings. Specifically, we connect the observed seasonality to existing ecosystem studies in Disko Bay, particularly those focused on the Monitoring Station. By grounding the section in ongoing research, we aim to show that a better-resolved oceanographic seasonality provides a useful framework for interpreting ecosystem variability, while avoiding unsupported speculation.

Line 446: ‘de-seasoning’ is not a word.

Corrected.

Line 453: This sentence and the one before it are confusing. They say nutrients are entrained deep in the icefjord, flow out into Disko Bay and then into the icefjord? Maybe I’m missing something here.

The text has been revised for clarity.

General comment: Is it possible to update the schematic or create your own schematic (Fig 15 of Gladish et al (part 2))? This might help the reader understand what is new here or what is validating previous ideas (which is important too). I think the data here are very cool!

Thank you again for your positive and constructive suggestions. We have now created an update to the schematic Figure 15 by Gladish et (2015). Our results provided observational evidence for the mechanism they propose in Spring/Summer (lower panel of their figure), which was well-represented in their schematic. We updated the upper panel to include the possible wind-driven renewal mechanism in Autumn, which was not considered. This will be included as a new figure in the revised manuscript.