Persistence of a Subsurface Water Mass in a Deep Mid-Latitude Fjord

By: L Bianucci

The authors use a high-resolution, unstructured model to investigate the persistence of a cold, oxygen-rich sub-surface layer formed during the preceding winter. They show that the presence of the layer – and the stratification changes that it brings about – changes the background circulation from a three-layer to a four-layer system, and suggest that increased mixing at the head of the fjord reduces the estuarine circulation.

The paper is clear and well written – but the scientific argument is relatively weak, and the results could be better quantified and presented. Rather than exploring how the cold anomaly can persist, which is what they set out to do according to the abstract – the paper is a comparison of the circulation within the fjord during a short period in June for experiments with and without the cold layer present. We are shown that the circulation changes – but the authors do not explain why. Mixing is stated to be weaker due to the circulation changes, but this (or the effect on the cold layer) is not shown/quantified. Is the difference in mixing between the two scenarios larger than the difference between the model and the observations? (which is mentioned in the text and, seen in the excessive "smoothing" of the modelled T-profiles in Fig. 3). What is the "normal" residence time for water at one level in the fjord – and how does that change with the "perturbed" stratification?

The model was run for one month – but there is no mention of how the boundary conditions change throughout the period (or the year) and how this would affect the circulation.

In addition, I find that the choice of figures illustrating the points could be improved (see detailed suggestions below for a few suggestions).

I can recommend publication only after major revision.

We appreciate Reviewer1's (R1) insightful comments, which we believe will help improve our manuscript. We plan to modify both the title and focus of the manuscript, while improving some descriptions/explanations. In particular, we will now emphasize that the overall sluggish circulation of this long, deep inlet is the main reason why the winter-formed subsurface water mass persists (the slow circulation, typical of long and deep fjords, is seen in both simulations, although it is even slower when the subsurface water mass is present). A new particle tracking analysis shows that >97% of the particles released at the start of the simulations from the location of the temperature minimum layer (hereafter referred to as "Tmin") remain inside the inlet after 34 days (see more details in response to L228). We will add this analysis to our manuscript, adding a co-author to the paper (Wendy Callendar contributed the particle tracking experiments).

We address R1's general comments regarding circulation changes, mixing, model diffusivity, and residence times in the responses to the specific comments below. We will make sure the text is clear regarding the circulation, which is driven mainly by density. Furthermore, we plan to reduce the focus on the layering of the circulation in the simulation with the subsurface Tmin (baseline simulation) and will emphasize the slower velocities (i.e., advection) as the reason for the persistence of the subsurface feature. The new title will be "**Fjord circulation permits persistence of subsurface water mass in a long, deep mid-latitude inlet**" and we will adapt the abstract and main text to reflect the subtle change of narrative.

Lastly, regarding R1's comment on the boundary conditions and how they change during the month of simulation: given the transit time analysis (well over a month for waters that inflow into Bute Inlet; see more details in response to L160), it is unlikely that the conditions at the open boundaries (Johnstone Strait and the Strait of Georgia) will affect the fjord during the simulation period. Those open boundary conditions are taken from a larger scale regional model. River forcing (mostly dominated by the Homathko River and followed by the Southgate River) had its most rapid change during May 2019, such that during our simulation period, discharge is already high (reflecting the snow and glacier melt that drives discharge in these watersheds). Wind forcing is also stable (as shown by the wind rose in Fig R1.1), mostly blowing from the south. All of this information will be better explained/added into the methods section of the manuscript.

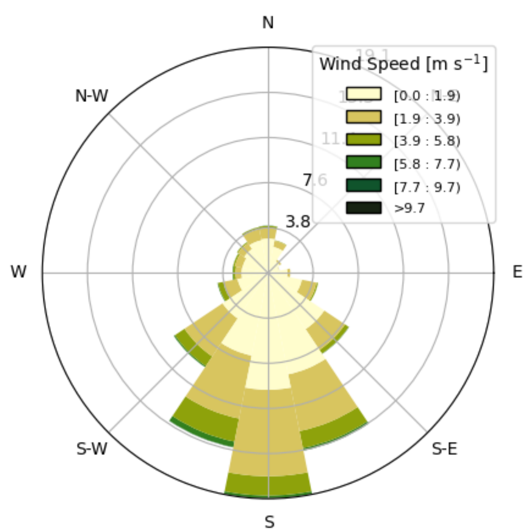


Fig R1.1: Wind rose of the atmospheric forcing over Bute Inlet. Most of the winds blow from the south. Colours indicate wind speed; the radius represents the percentage of winds in a given direction (the latter discretized every 22.5 degrees).

Specific comments:

L 47: Explain here how this changed the stratification/layering of the fjords (e.g. using text from line 220) - and refer to Fig. A1 (which ought to be included in the main paper). Consider including also a profile from a non-Arctic outflow year in Fig A1.

Thank you for the suggestion, we will better describe the formation of the cold, oxygenated subsurface water mass in the introduction (adding information like that described in line 220 of the original manuscript). However, we do not believe that figure A1 belongs to the introduction section because of two main reasons: 1) this figure has information related to the two simulations, neither of which has yet been introduced at this point of the manuscript; 2) the feature can be easily observed in the cited literature (Jackson et al 2023 and also MacNeill 197 and Pickard 1961), so we argue that a reference to the literature should suffice here. Given our decision to not to refer to Fig A1 here, we are not adding the non-Arctic outflow year to the figure (more on "normal years" in response to comments L222). However, please note that we will add a panel to Fig A1 showing N^2 for the observations and both simulations (shown in Fig R1.6).

L 97: The description of the river-forcing is very detailed – consider moving it to the appendix.

This description will be simplified in the main text (e.g., removing references to specific river gauge IDs)

L127: What do you mean by "mostly limited to"

We wanted to convey that we did not have access to many other datasets other than CTD profiles (e.g., we could only evaluate our model against CTD profiles and sea surface elevation from one tidal gauge). We will simplify this sentence to read "Observed vertical profiles were available from both bottle and CTD measurements"

145 and on: How useful are these metrics - as used here - when the larger part of the water column does not change during the short simulation, i.e. it's all about the initial conditions? In addition, there are now observations of velocity, on which the paper's main results are based.

We could certainly provide the model evaluation for the top 100 m of the water column, which would not be as affected by the bottom conditions (that do not change a lot during the month of simulation). We include below the evaluation (plots and metrics) for the upper 100 m and compare those against the plots and calculations for the whole water column; the overall performance does not change significantly if we only focus on the upper waters. The bias and RMSE are larger in the 100 m metrics, partly due to the numerical diffusivity issues discussed in the next response (L158). We argue that the qualitative (plots) and quantitative (metrics) evaluation of the model performance is an important component of a modelling paper. Therefore, we believe we should keep these metrics. We plan to show the plots for the whole domain evaluation, but add text to refer to the top

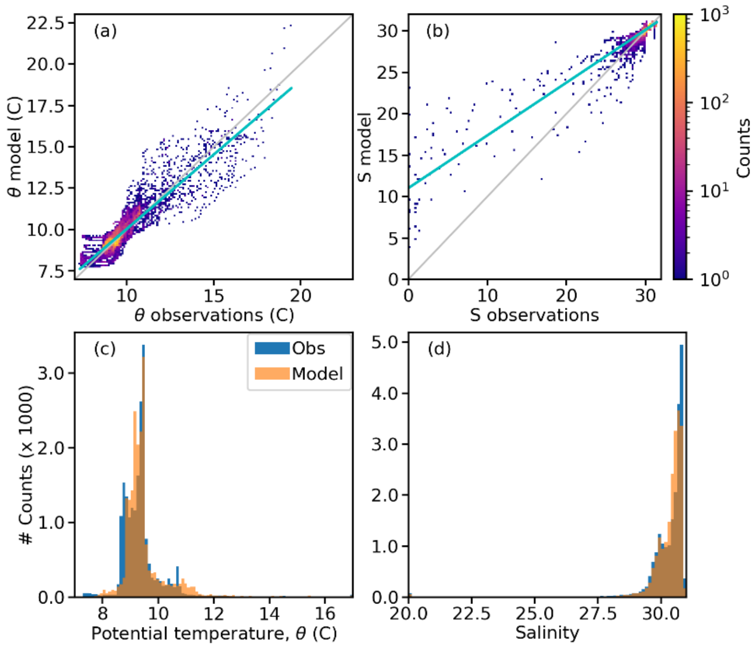
100 m evaluation (unless the editor/reviewers feel strongly that we should take the opposite approach, and show the 100 m evaluation and only discuss the full domain metrics).

Lastly, we know that instruments to measure currents in Bute Inlet are going into the water in 2024 (by The Hakai Institute), but are not yet deployed; therefore, we are not sure about the observations of velocity that R1 is referring to. The only current observations we found were from downward-looking ADCPs that focused on near-bottom currents in 2018 (for the study of turbidity currents, see Lewis et al, 2023), so not meaningful to evaluate the model results on the upper part of the water column that are the focus of this paper.

<u>Reference</u>:

Lewis P.B., M.A. Clare, E.L. Pope, I.D. Haigh, M.J.B. Cartigny, P.J. Talling, D.G. Lintern, S. Hage, M. Heijnen. Predicting turbidity current activity offshore from meltwater-fed river deltas, Earth and Planetary Science Letters 604. Doi: 10.1016/j.epsl.2022.117977, 2023.

Model evaluation for the whole water column
(Fig 2 in manuscript)



Model evaluation for the upper 100 m
(analogous to Fig 2, but only for top 100 m)

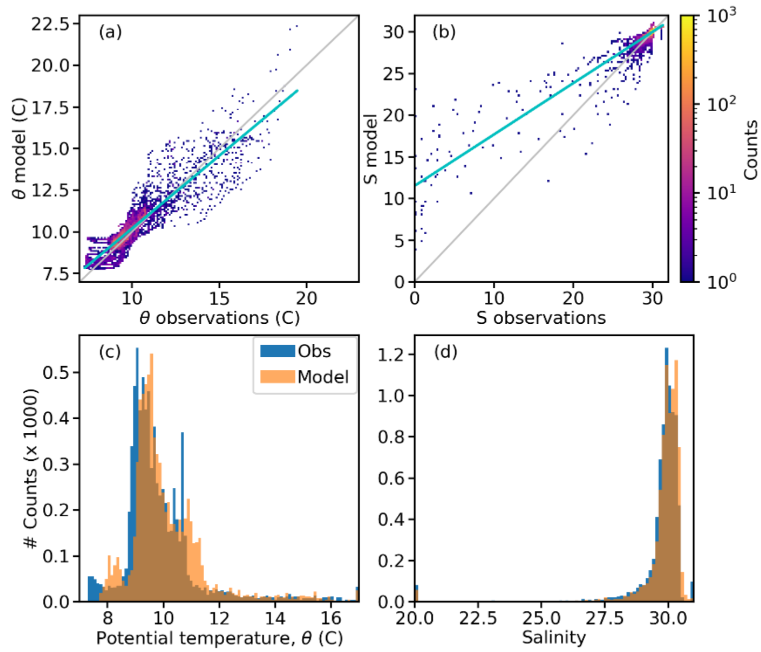


Table R1.1: comparison of metrics for the full domain (Table 1 in manuscript) vs. top 100 m.

Metric	Potential temperature, θ		Salinity	
	Full domain	Upper 100 m	Full domain	Upper 100 m
Bias	0.08 °C	0.22 °C	0.05 g kg^{-1}	0.18 g kg^{-1}
RMSE	0.44 °C	0.66 °C	0.73 g kg^{-1}	1.14 g kg^{-1}
Skill	0.81	0.80	0.80	0.79
Willmott skill	0.95	0.95	0.93	0.92
R^2	0.82	0.82	0.87	0.86
N	20147	7818	20231	7902

158 and on: Nor sure I agree - if you zoom in on the cold, subsurface layer and use a scale that's adapted, there are quite some differences in the depth, “sharpness” and the vertical extent of the cold layer already a fortnight after model initialization. I'd suggest plotting the profiles of temperatures from the three occasions on top of each other (modeled in one panel and observed in one) to show the time development in the model vs. obs. If one assumes advection to be negligible, one can (I think?) use the differences in the profiles to infer an estimate of observed/modeled diffusivity.

We did not intend to ‘over sell’ the ability of the model to represent the vertical structure of the observations. We will improve the wording to make sure the model performance is not overstated. We did mention in Line 160 of the original manuscript that the model did not show the temperature minimum as sharply as the observations, due to numerical diffusion. It is a very common problem for diffusive models (such as FVCOM) to represent maxima/minima features.

We show below the figure suggested by R1 (Fig R1.2), with observed and modelled profiles of T and S at the station BU6 (in the middle of the inlet, see inset) in June 12 and June 26 (timing of the observations available for model evaluation). The observed profiles do not show much mixing, since the Tmin basically maintains the same value (difference of Tmin is 0.01°C). While the difference in Tmin between both dates is larger in the baseline simulation (0.15°C), it is not many orders of magnitude larger; the same is true (with even smaller differences) for salinity (ΔS at the location of the Tmin for the observations is 0.04 and for the baseline, 0.03 g/kg). Furthermore, below 300 m the observations show small changes in T and S (deltas < or ~0.01 for both) in the 14-day period. The model shows a bit larger differences with both ΔT and ΔS around or less than -0.06 near the bottom. In a month (30d), these numbers could indicate a change in S & T of around -0.13 units.

While the model is diffusive, we argue that the main density driven circulation is still well represented in a month-long simulation. Fig R1.3 shows that the pycnocline is well represented (see more density discussion in the response to comments for Fig 3, particularly for the representation of density at the depth of Tmin). Numerical diffusion could become an issue for longer simulations if it took over the main density structure.

We note the challenges of modelling a fjord that is ~2km wide and ~700m deep using a modelling framework that’s typically diffusive. However, we argue that despite this drawback FVCOM still has advantages that make it an appropriate choice for the modelling of fjord systems (e.g., the flexibility provided by the unstructured triangular grid to represent a region replete of channels and islands). We will add a discussion of the advantages and disadvantages of FVCOM in section 5 of the revised manuscript.

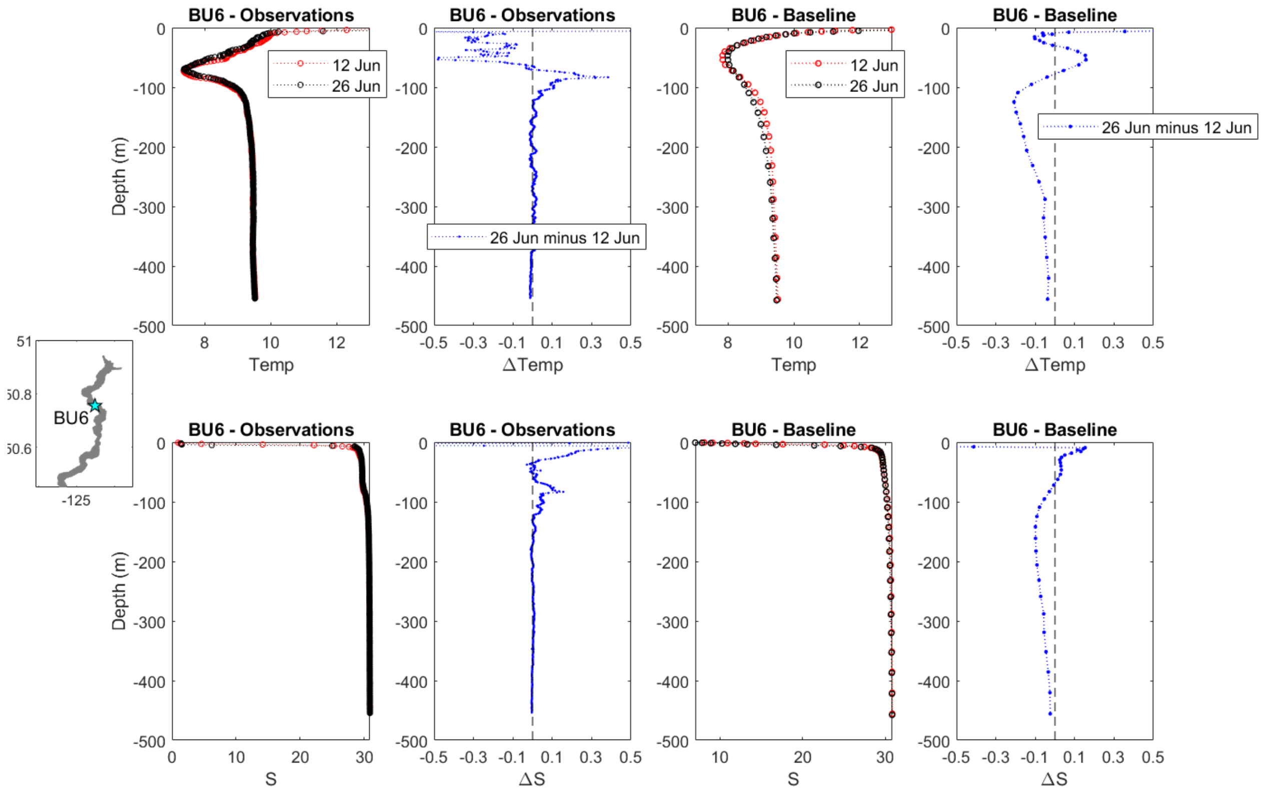


Fig R1.2: Observed and modelled profiles of (top) temperature and (bottom) salinity, in June 12 and 26 in station BU6 (mid-inlet, shown in inset). Also showing the difference (Δ) between profiles in the two dates, for both observations and model (baseline simulation that has Tmin in initial conditions).

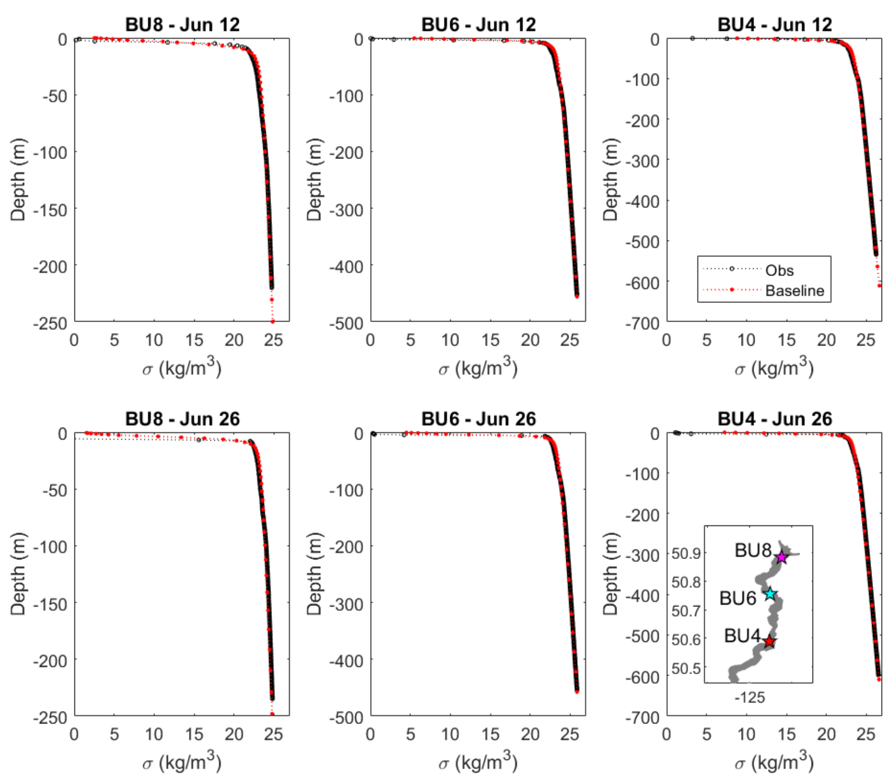


Fig R1.3: Observed (black) and modelled (red) density profiles at three stations (BU8, BU6 and B4 – see inset for location) for (top) June 12 and (bottom) June 26.

160. Well this is not very surprising, you initialized the model two weeks earlier with the cold layer present. How long is your run compared to the normal residence time of water in the fjord?

We do not have information on the residence time in the fjord, but have calculated the transit time with our model results (i.e. length of Bute Inlet divided by the mean velocity in Bute). At the depth range of the Tmin in the baseline simulation (40-50 m), the transit time is over 100 days. Furthermore, in the new particle tracking analysis (see the response to L228) we show that over 97% of particles released at the location of the Tmin layer stay within the fjord after the end of the simulations. Therefore, we should definitely expect the model to have a Tmin in June 12 and 26 (19 and 34 days after initialization, respectively).

L 170 It is not easy to see the structural difference between Fig. 4 a and d that you describe in the text. To me there're four layers in both of the figures: red, blue, red, blue – but I understand that what you refer to as four layers are red, blue, blue red, where the two blue layers are separated by white?

We will clarify and improve the description if Figure 4a,d in the text. We had previously ignored the very bottom blue layer, which we will describe when revising of the manuscript. We added a horizontal dashed line at 50 m, to highlight the (co-)location of the Tmin and the region with almost zero velocities (Fig R1.4). Please note that we will not be focusing as much on the layering in the new version of the manuscript (although we will mention it and discuss it).

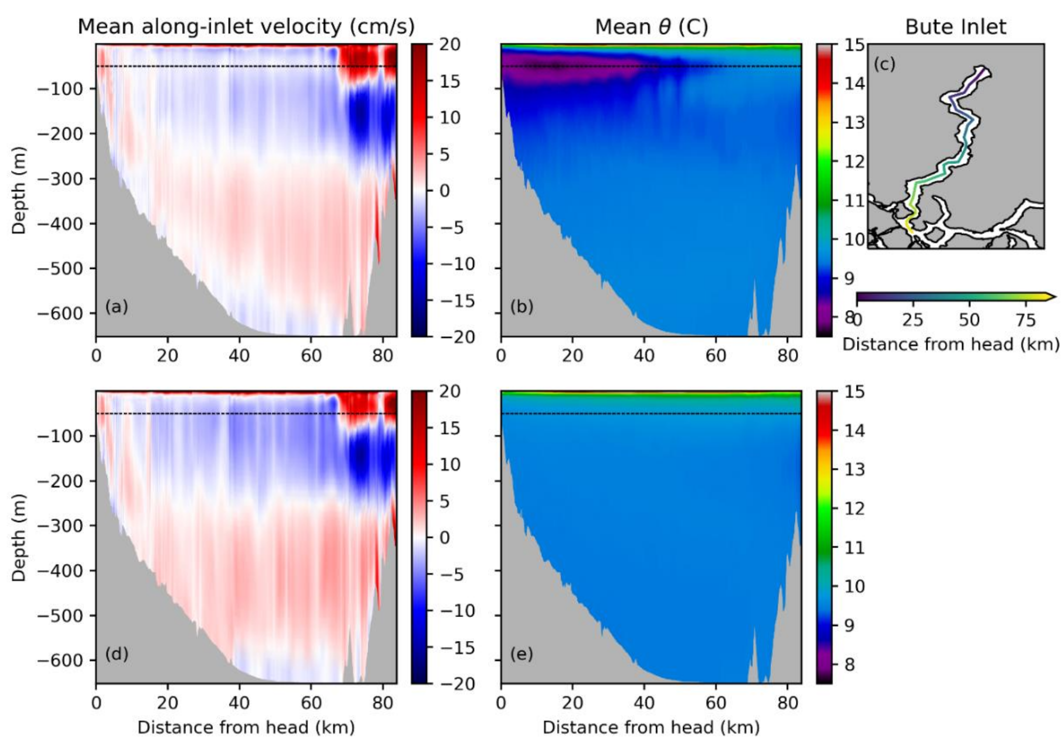


Fig R1.4: this figure will replace Fig 4 in the original manuscript, adding to the caption that the horizontal line at 50 m in all panels highlights the co-location of the Tmin and V~0 layers in the baseline experiment.

173. How can there be a net along fjord circulation below sill depth?

We will improve the wording, since outflow "below the sill" sounds counter intuitive. However, it is a feature of the model circulation that the mean direction of the flow below ~280m (approximately the depth of the sill) is towards the mouth of the inlet; this is also seen in the model's across-inlet transects. Observational studies have shown this same feature in other inlets (e.g., Baker and Pond 1995, Castillo et al. 2012, Wan et al 2017) and it was consistent with the expectations for such a deep fjord, following the δ analysis (Valle-Levinson et al., 2014) discussed in section 5.

References:

Manuel I. C, O. Pizarro, U. Cifuentes, N. Ramirez, L. Djurfeldt. Subtidal dynamics in a deep fjord of southern Chile, Continental Shelf Research 49, 73-89, doi:10.1016/j.csr.2012.09.007, 2012

Baker, P. and Pond, S.: The Low-Frequency Residual Circulation in Knight Inlet, British Columbia, J. Phys. Oceanogr., 25, 747–763, 1995

Valle-Levinson, A., Caceres, M. A., and Pizarro, O.: Variations of tidally driven three-layer residual circulation in fjords, Ocean Dynamics, 64, 459–469, doi:10.1007/s10236-014-0694-9, 2014.

Wan, D., Hannah, C. G., Foreman, M. G. G., and Dosso, S.: Subtidal circulation in a deep-silled fjord: Douglas Channel, British Columbia: SUBTIDAL CIRCULATION IN DOUGLAS CHANNEL, J. Geophys. Res. Oceans, 122, 4163–4182, doi:10.1002/2016JC012022, 2017.

185 I presume that this is because the effect of the salinity changes on the density is greater than the effect of temp. changes (Please quantify) – they would also have different signs, right? If you lower the temperature you make the water at that level denser, while if you make it fresher you make it lighter.

Indeed, density in this region is dominated by salinity (a beta ocean, as in Carmack, 2007). This is clearly seen in Fig 3 and Fig A1, since the shape of the density profiles match very well the shape of the salinity profiles (both in model and observations). A Tmin would not be possible if temperature dominated stratification. It is interesting to note that the upper ~50 m of the water column (from the surface to the core of the Tmin) is double-stable, since temperature decreases with depth while salinity increases. Below the Tmin, where T increases with depth, stratification is solely driven by salinity. Further quantification was done by calculating the density ratio Rρ ($\alpha\frac{\partial T}{\partial z}/\beta\frac{\partial S}{\partial z}$), but we do not feel it adds too much to the analysis. We will emphasize the role of salinity in stratification in the revised version of the manuscript.

Reference:

Carmack, E.C., The alpha/beta ocean distinction: A perspective on freshwater fluxes, convection, nutrients and productivity in high-latitude seas, Deep Sea Research Part II: Topical Studies in Oceanography 54, 2578-2598, doi:10.1016/j.dsr2.2007.08.018, 2007.

193 (give depth intervals)

We will specify that the stratification decreased below the outward-flowing layer, between ~5 and 50 m deep.

217 Why/how did the temperature minimum create a separation of the return flow?

This statement was poorly written and will be rephrased. The point we will make in the new version of the manuscript is that velocity is slower in the baseline simulation because surface density increased – and it increased more closer to the head than towards the mouth; therefore, the horizontal pressure gradient decreased, slowing down the circulation (see more details in response to L226). The specific density structure leads to a subsurface layer with velocities close to zero at the depth of Tmin. The vertical pressure gradients let us infer that velocities will be smaller at the depth of Tmin, but in such analysis, the zero value would depend on the pressure reference level chosen. Please note that we plan to reduce the focus on the layering of the baseline circulation and will emphasize the overall slow velocities as the reason for the persistence of the Tmin feature.

291 “velocities were weaker” – please quantify (and or make figures where the reader can directly compare the velocity profiles)

We have improved Fig 5 (following some of the advice from R1) such that the velocities x-axis are zoomed in to better represent the velocities below 10 m (i.e., now the limits for the axis are +/- 8 cm/s instead of going up to 35 cm/s). The surface outflow layer velocities are indicated by a red value near the top of each panel. We have also added a dashed horizontal line to highlight 50m depth (approximate depth of Tmin). Please see below the new Fig 5 and its caption (new text in *italics*).

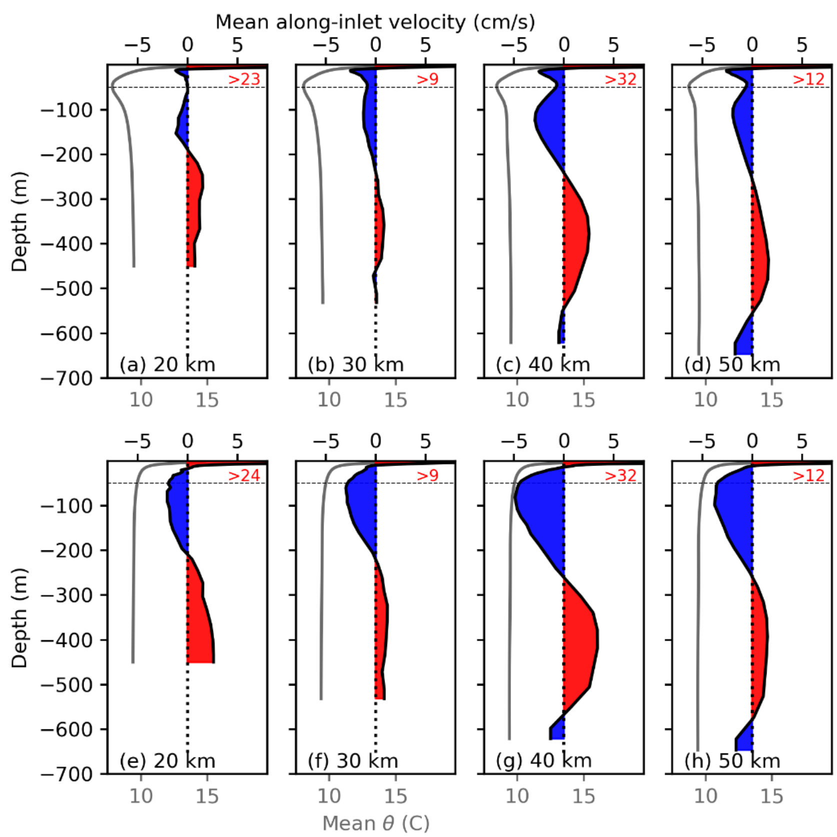


Fig R1.5: New Figure 5. Vertical profiles of mean along-inlet velocity (coloured red/blue) and potential temperature (grey) for the (a-d) baseline and (e-h) sensitivity simulations, at four locations in the inlet (from left to right: 20, 30, 40, and 50 km away from the head; see Fig. 4c). Velocities are positive (red) towards the mouth of the inlet and negative (blue) towards the head. *Velocity values for the outflowing surface layer are given as red numbers on the top-right of each panels (in cm s^{-1}). Horizontal dashed lines highlight 50 m, the approximate location of the temperature minimum in the baseline simulation.*

222 “salinity and density were impacted” – please describe how (and quantify) – e.g. referring to a new version of Fig A1 where a profile from a “normal year” is included. As of now, the reader has no means to judge whether the salinity profile used in the sensitivity profile (which determines the density and hence the stratification) is realistic.

We thank the reviewer for this question, that made us re-think some of our wording related to the description of the sensitivity simulation. Reviewing the literature (MacNeill 1974 and Pickard 1961) and all available profiles of temperature in Bute Inlet in publicly accessible datasets (both at https://waterproperties.ca and https://cioos.ca), we confirmed that a Tmin is present in Bute Inlet mostly every spring/summer, even if the feature is very small some years (i.e., Tmin only a few tenth of a degree colder than deeper temperatures) and quite large some others (differences well over 1°C, even up to several degrees – see Fig 3 in MacNeill, 1974). Arctic outflow winds are common in BC’s winters; Jackson et al (2023) estimated the number of outflow events per decade at Bute Inlet to be between 93 (for the coldest decade, the 1950s) and 20 (for the warmest decade, the 2010s). These numbers imply an average of 2+ outflow events per year. Therefore, even a “normal year” might still have evidence of Arctic outflow events and some kind of Tmin feature. Our sensitivity experiment represents an extreme scenario of a winter without deep mixing, which may not be a “normal year”. We argue that it is worth comparing our baseline simulation against the sensitivity experiment (a normal year will be somewhere in the middle), but we will improve descriptions to clarify all of this information in the new version of the manuscript. We can certainly quantify the changes in salinity and density between our two simulations by calculating deltas, although we think those changes are qualitatively displayed in Fig 6a and Fig A1 (we will add references to those figures; Fig A1 will now have a second row, removing the surface values, such that below-surface changes are more easily appreciated – see Fig R1.6 below).

References:

MacNeill, M. T.: The mid-depth temperature minimum in B.C. inlets, Master of Science, The University of British Columbia, Vancouver, BC, 91+viii pp., 1974.

Pickard, G. L.: Oceanographic Features of Inlets in the British Columbia Mainland Coast, J. Fish. Res. Bd. Can., 18, 907–999, doi: 10.1139/f61-062, 1961.

225 Rather than referring to Fig. 6c, refer to a new version of Fig A1 which also includes a panel comparing the initial N2-profile

We will add a reference to a modified Fig A1, which includes a fourth column that shows the initial N^2 profiles for the observations, baseline, and sensitivity simulations. It also has now a second row that focuses on the below-surface values:

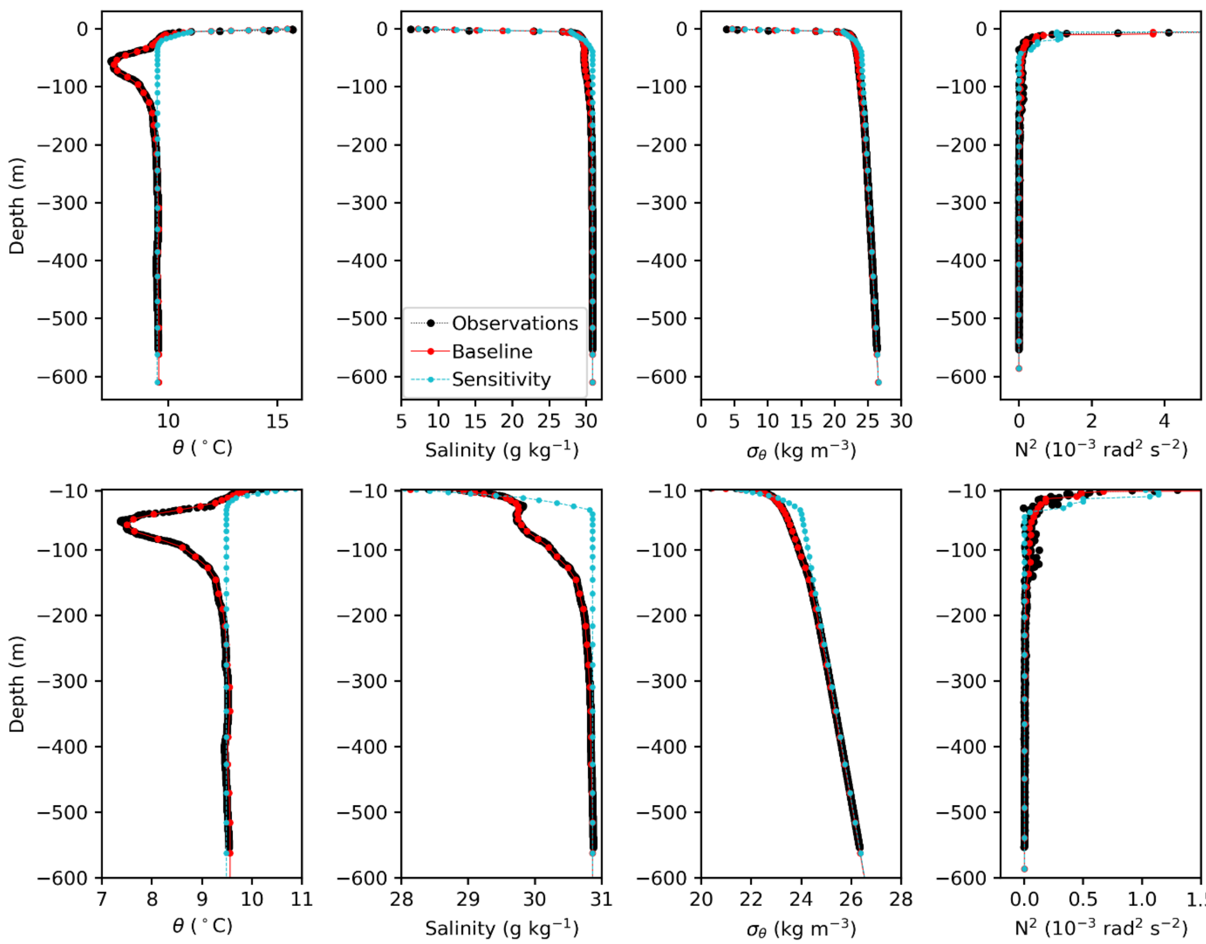


Fig R1.6: New Figure A1. Profiles of potential temperature, salinity, density (as σ_θ), and Brunt-Väisälä frequency (N^2) at station BU4 (middle of the inlet at 50.6°N and 124.9°W) for the observations on 23 May 2019 (black) and the initial conditions for the baseline (red) and sensitivity (blue) simulations. Top row shows the whole water column and bottom row focuses on values below 10 m.

226 Give depth range

We will add the depth range of the denser upper waters (~5 to 50 m)

226: Please include figures/numbers that show the reduced density difference/estuarine circulation.

We will add references to Fig 5, which will now have a few changes to make it easier to see the difference in the circulation of both simulations (see new version of Fig 5 shown here as fig R1.5). The difference in density will be highlighted by referring to Fig 6b and adding a new figure (or likely, new panel in Fig 6) that shows the profile of $\Delta\rho$ in 2 locations along the inlet. An example of such a figure/panel is shown below (Fig R1.7):

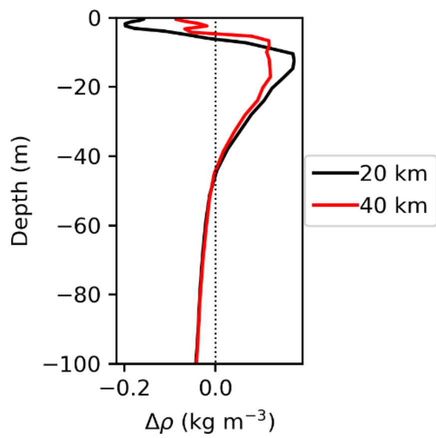


Fig R1.7: New figure or new panel in Fig. 6. Profiles of difference in mean density between baseline and sensitivity experiments at three locations in the fjord: 20 and 40 km away from the head (Δρ = 29-day average baseline minus 29-day average sensitivity; positive values indicate that the baseline is denser than the sensitivity simulation).

Below the surface outflow, between ~5-50m, the baseline simulation is denser. Also, the density increase in the baseline simulation is higher nearer the head of the fjord (20km, black line) than closer to the mouth (40km, red).

L228: “decreased mixing”. What is this statement based on? Fig 6b shows that the density decreased all the way to the bottom for the baseline exp? I presume / guess / hope that the difference below e.g. 250 m between the simulations is zero at the start of the simulations.

This sentence will be reformulated and improved. First of all, we are focusing on the top 100 m of the water column, where the Tmin is found. The weaker velocities at those depths (particularly around 50 m) in the baseline simulation lead to decreased advection. Regarding “decrease mixing” we were referring to the smaller horizontal mixing eddy parameters that we found in the baseline simulation (up to 25% or ~1 m^2/s smaller in the upper 100 m, figure not shown). However, we realized that those changes are quite small and likely not a key player for allowing the permanence of the Tmin water mass at the subsurface of the inlet. Furthermore, we did a new particle tracking analysis that emphasizes the dominant role of advection. We used weightless virtual particles that flow with the 3D current field – these particles are at all times neutrally buoyant (i.e., no sinking towards the bottom nor rising towards the surface). We deployed these particles at the start of the simulation in the location of the Tmin (i.e., in every model node and level with T<8°C inside Bute Inlet in the initial conditions). By the end of the simulation, over 97% of the particles are still within Bute Inlet in both simulations (see Fig R1.8).

Therefore, we will remove the reference to mixing in this sentence, emphasizing the role of advection in the persistence of the Tmin feature. We will also add the new particle analysis to the manuscript.

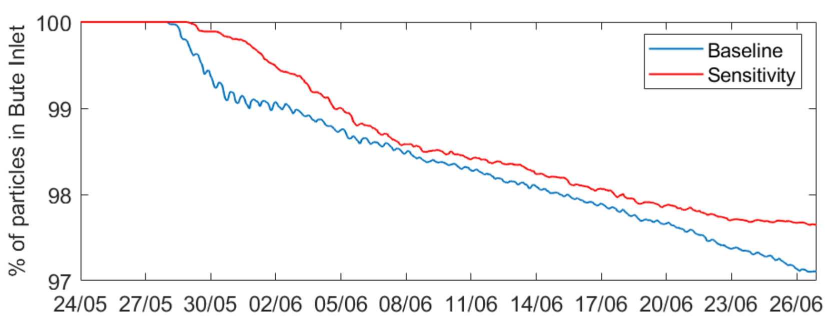


Fig R1.8. Time series of total percentage of virtual particles inside of Bute Inlet in both simulations. The number of particles and their initial location is the same in both cases. Final values are 97.1 and 97.6% for the baseline and sensitivity simulations, respectively.

Regarding R1's presumption: As described in the methods section, the initial temperature and salinity profiles of the sensitivity simulation in Bute Inlet were constant below the main pycnocline; the constant values were selected as the coolest and saltiest observations in the deepest third of the water column. Therefore, the densities below 250 m are not exactly the same between simulations, but they are very close (and basically zero below 400 m. This can be seen in the second row of the new Fig A1 (Fig R1.6), since the red and cyan dots are really close to each other, but not exactly on top of each other. However, we argue that the differences are small enough and away from the focus of this work. The reasoning behind the setup of our sensitivity experiment was to avoid the introduction of strange vertical gradients from the homogenization of just a layer of the water column (so we homogenized the whole water column below the pycnocline).

L 240 responds? I do not understand this sentence.

This sentence will be rewritten, given that we no longer consider the sensitivity experiment to represent "standard summer conditions". The original sentence should have read "shows" instead of "responds to".

L242 give depth range

Will do (it's ~5 – 280m)

L 245 Is it relevant to mention deep water renewals here?

We agree that this information should be moved to the discussion (it does not belong to the summary/conclusions)

L 249 Do you think/mean that the findings from Bute inlet is universal? Would it not depend on the stratification outside of the fjord? What are the "mechanisms" that you refer to?

There are two separate aspects to consider regarding the universality of our Bute Inlet results. First, there is the presence of Arctic outflow events (also known as gap or katabatic winds) that can create a subsurface water mass in winter. The presence of these wind events depends on the location, geography, and topography of the fjord (i.e., a fjord must be connected to the continental plateau to experience these wind events). Several inlets experience Arctic outflow events in the west of Canada (Pickard 1961) and katabatic winds have been studied in other regions such as Alaska (Ladd & Cheng, 2016), southeast Greenland (Oltmanns et al., 2014; Spall et al., 2017), and Antarctica (Forsch et al, 2021). The second aspect when considering the universality of our Bute Inlet results is the geometry of the fjord itself (i.e., length, depth, width). The freshwater forcing in these fjords implies S=0 at the head and some oceanic S at the mouth (~33-35); thus, the horizontal pressure gradient mostly depends on the length of the inlet, such that long inlets tend to have slow circulation below the surface outflow. We think that the findings from Bute Inlet (regarding the slow circulation that allows for subsurface features to stay in place for many months) could be representative of deep, long fjords in the mid-latitudes that experience katabatic winds or winter deep-mixing events. We will add these thoughts into the revised manuscript.

We argue that while the stratification outside the fjord might play a role in the details of the circulation, the key driver is the freshwater forcing leading to the estuarine circulation. As long as there is a freshwater source large enough to create a strong estuarine circulation in a long, deep fjord, then a subsurface Tmin feature will be able to persist (if such a water mass develops during the winter) from spring to the following autumn.

We will clarify that by "mechanisms", we mean the slow circulation in a deep, long fjord, that allows for the persistence of a subsurface water mass. A positive feedback would provide a secondary mechanism: the presence of a subsurface Tmin leads to an even slower circulation in the fjord at those depths, which contributes to the decreased advection of the cold waters and, in a small degree, the persistence of the subsurface water mass in the fjord.

References:

Ladd, C. and Cheng, W. (2016). Gap winds and their effects on regional oceanography Part I: Cross Sound, Alaska. Deep-Sea Research II, 132, 41–53. Doi: 10.1016/j.dsr2.2015.08.006

Oltmanns, M., Straneo, F., Moore, G. W. K., & Mernild, S. H. (2014). Stong downslope wind events in Ammassalik, Southeast Greenland. Journal of Climate, 27(3), 977–993. Doi: 10.1175/JCLI-D-13-00067.1

Pickard, G. L. (1961). Oceanographic features of inlets in the British Columbia mainland coast. Journal of the Fisheries Research Board of Canada, 18(6), 908–999. Doi: 10.1139/f61-062

Spall, M. A., Jackson, R. H., and Straneo, F. (2017). Katabatic wind-driven exchange in fjords. Journal of Geophysical Research: Oceans, 122(10), 8246–8262. Doi: 10.1002/2017JC013026

Fig 1

Lon/lat are exchanged

Consider using color to show the resolution and move panel (a) to the appendix

Why do the two maps appear different – is the aspect ratio not the same?

Consider including a length scale in (b) to help the reader.

What about showing bathymetry rather than resolution?

We have fixed lat/lon labels and coloured the model grid according to the resolution – thank you R1 for the suggestion. We increased the size of the figure and ensured that the aspect ratio is the same in both maps. We decided to keep panel (a) in this figure, given that it now shows model resolution and an improved inset that better indicates the location of the study area (Reviewer 2's suggestion).

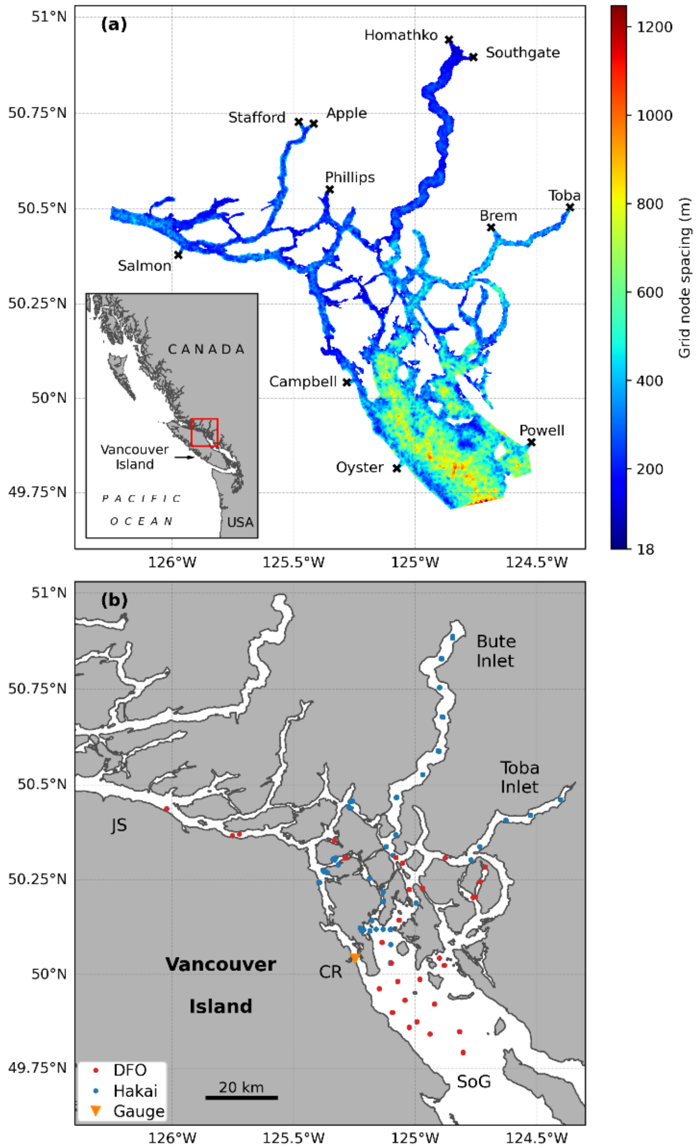


Fig R1.9. New version of Figure 1.

Fig 2.

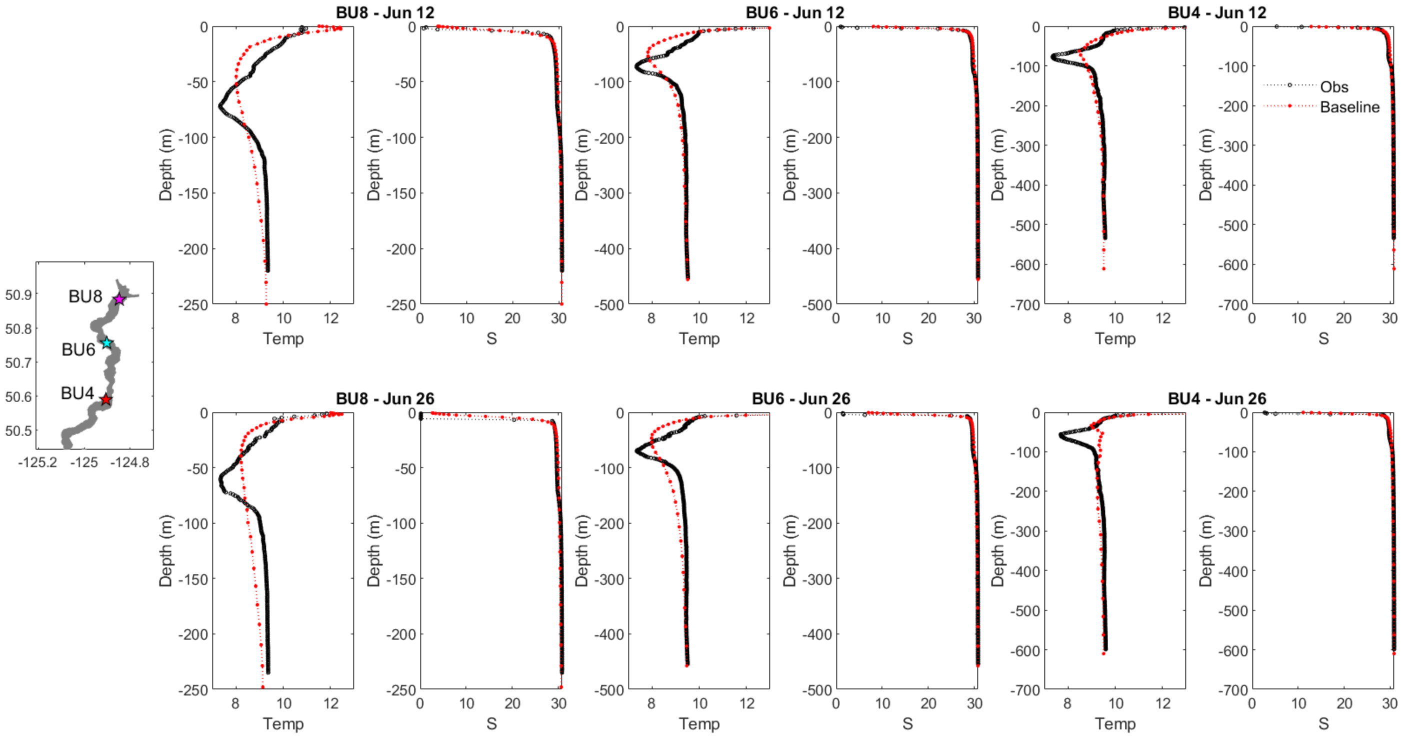
See comment above about the usefulness of these metrics – move to appendix

As discussed in our previous response, we believe that the model evaluation belongs to the main text, while adding information of the evaluation in the upper 100 m

Fig 3.

Plot only three profiles – and let us know where each one is from. Especially for modelled temperature they are different. Are these differences there initially, or are they “produced” within the model.

As discussed above (response to L158), we agree with R1 that the modelled T profiles are not perfect matches to the observations and we are committed to not overstate in the manuscript the performance of the model. Below we show the figure requested by R1; we could replace our current Fig 3 by a figure similar to this one, but we argue that the current Fig 3 is informative as is – we just need to improve the main text to make sure that the performance of the model is not exaggerated.



R1.10: Observations (black) vs Modeled (red) profiles of T and S in three locations (see inset). Top panels show June 12 and bottom panels, June 26.

Consider “cutting” the profiles, so that you show the upper layer with a different x-axis than the deeper waters. Using the large scale needed for the upper layer, means that changes in the lower layer are not shown. One alternative could be to include a row showing also initial conditions in the same way. The observed structure in salinity/density above about 100m but below the surface layer appears to be missing in the model. Is this feature not there initially, or do they disappear during the run.

We appreciate R1’s insightful comment. Figure R1.11 below shows the modelled vs observed profiles below 10 m, such that subsurface features are shown better. As R1 observed, the salinity and density structure associated to the Tmin layer are also over-mixed (the features exist in the initial conditions, as seen in Fig R1.6). Interestingly, we note that the observed T and S features at the Tmin water mass compensate each other in density, such that the observed feature in the density profile is not as pronounced as in either T or S. Thus, the model is able to better represent the density gradients than either T or S. Furthermore, the model properly represents the pycnocline (even if a bit weaker at the depths of Tmin), which is important for the density-driven circulation in the fjord.

We will discuss the model's limitations to properly represent all features related to the Tmin (temperature, salinity and density) and plan to either "cut" the profiles to highlight the differences at depth or to add Fig R1.11 to the manuscript.

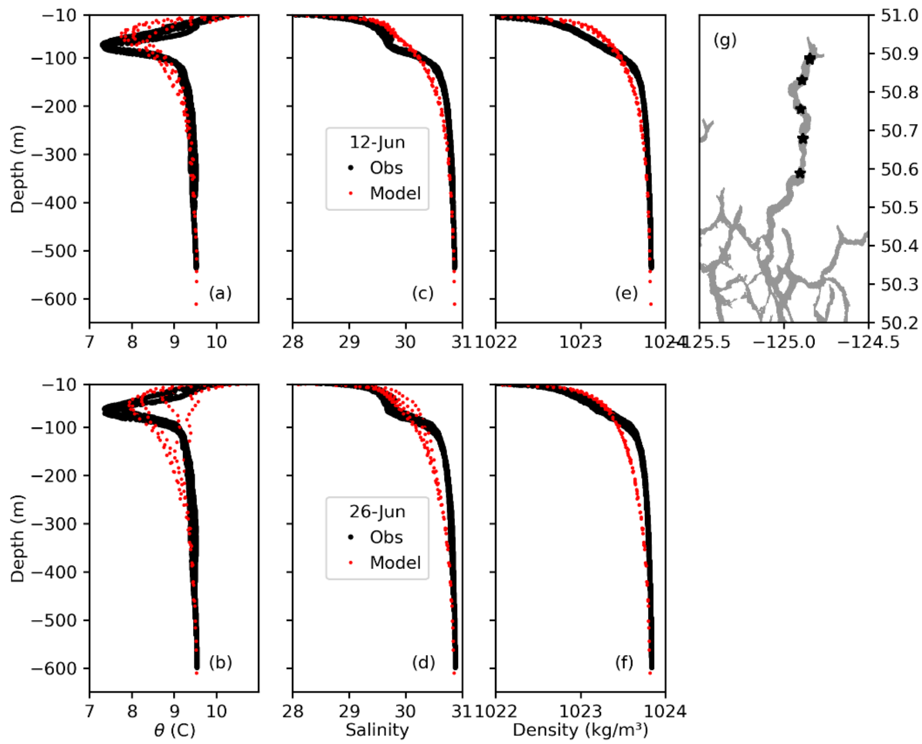


Fig R1.11: equivalent to Fig 3 but starting from -10 m; thus, the x-axes are more appropriate to see the differences between model and observations at depth

Fig 4

What happens at about 70 km – and why is this not commented in the ms? How do you explain the velocities below sill depth? Consider helping your readers see the four layers.

As shown in Fig R1.4, we will highlight the V~0 by means of a horizontal dashed line in this figure. We have discussed how we will improve the wording regarding the flow below the sill depth (see response to L173 above). Around 70 km we see the effect of tidal mixing over the sill – the region of Discovery Islands (characterized by a complex network of narrow channels and deep fjords) has strong tidal currents (Foreman et al, 2012; Foreman et al, 2015). We will add this information to the manuscript.

Figure 4 and 5 basically shows the same thing, right? Maybe you only need one of them?

While the two figures show the same information, we argue that both figures are valuable. Fig 4 allows to clearly see the circulation patterns, including the small horizontal variability (the latter is not seen in Fig 5). In contrast, Fig 5 allows to easily compare the strength of the circulation in the different layers, particularly with the new X axis limits, as suggested by R1 in the next comment (see new proposed figure in Fig R1.5).

Fig 5

For clarity, use a velocity scale suitable for the lower layers – and only give the upper layer outflow velocity as numbers?

Thank you for this suggestion, which we have implemented – please see Fig R1.5

Fig 6

a)Use smaller dots. Not sure this figure is necessary?

We have already reduced the size of the dots. We argue that this panel is useful to identify the cold layer as a distinct water mass.

b-c) I think you need to include panels showing delta ro/delta N2 from the initial conditions for this figure to be meaningful. And would it not be better to (instead or in addition) compare the changes in density/N2 between the start and the end of the run (In the "end of the run" you'd likely have to average over some sensible period, but I think one could use a number less than 29 days?)

We respectfully disagree with R1. These panels focus on how both simulations differ in terms of their mean density and mean stratification. These are crucial points that we will emphasize and clarify in the main text. In particular, the main point is that the circulation is density driven, such that the reduction of the horizontal density gradient (ie, the surface density increases more in the baseline simulation near the head than it increases near the mouth of the fjord) leads to a slowdown of the overall mean circulation. Please note that the 29-day averaging is key to remove the tidal effect from the mean.

Table 1

Move to appendix

As discussed in previous responses, we believe that the model evaluation belongs to the main text (potentially adding information on the evaluation of the upper 100m). We can certainly move the table to the appendix, but not its discussion. We are happy to follow the editor's instructions on whether it is better to move the table to the appendix.