The MS presents the study of dynamics of intermediate layers in about 730-m deep fjord - Bute Inlet, a mainland fjord in British Columbia, using short-period calculations with a numerical model that was validated by observational data. The used FVCOM finite-element model has variable mesh size from 13 to about 1000 m. The baseline 1-month model run in summer, simulating subsurface temperature minimum due to adopted initial conditions, is complemented by another experiment where the subsurface cold layer was removed from the initial conditions for temperature and salinity. Comparison of the two numerical experiments revealed that layered circulation depends on the initial stratification – usual three-layer flow is replaced to a four-layer one, when cold subsurface layer of Arctic origin is found in the region. The results are this way interesting and worth of publishing.

Reading further, I was not always able to find justifications for the interesting statements.

We appreciate the thoughtful comments by Reviewer 2 (R2), which will help strengthen our manuscript. We respond to each comment below.

A. "Persistence" is an interesting interpretation that could be discussed, but two one-month model studies do not allow its quantitative evaluation; therefore, this term should be avoided in the title. Two times of this term in the abstract is also not justified. I recommend reformulation of the MS title. Also, the abstract could be rewritten, since about half of it is general introduction not directly connected to the conducted studies.

We plan to reformulate the focus of the manuscript and improve some explanations, such that the connection between our one-month simulations and the persistence of the subsurface cold water mass is better explained. In particular, we want to make it clearer in the text (and title) that we are not attempting to prove the persistence of the subsurface feature in 2019; the presence of the cold and oxygenated waters from March to October was demonstrated with observations by Jackson et al (2023). Our manuscript does not intend to show that the water mass persisted, but tries to shed light on 'why' it did. Both simulations showed an overall sluggish circulation in this deep inlet, though even slower when the subsurface water mass was present. The slow circulation and long transit times underneath the surface estuarine outflow (calculated to be over 100 days in the baseline experiment at the depth of the temperature minimum; this calculation will be added to the manuscript) are the principal reason why the subsurface feature can remain in place until deep mixing starts in the fall (the latter suggested by Jackson et al 2023 and others).

In light of all of the above, we will be changing the title of the manuscript to "Fjord circulation permits persistence of subsurface water mass in a deep mid-latitude inlet" and will adapt the abstract as well (we agree with R2's comment regarding the excess of general introduction and will fix that).

B. Instead of sufficiently long maximally realistic simulation study with variable forcing and boundary conditions, that could describe formation, evolution and decay of intermittent cold subsurface layer, the authors have adopted a simplified approach where open boundary conditions were kept unchanged for a one-month run with modified initial conditions. The authors should carefully justify: (a) Why one-month simulation is appropriate for a process of seasonal duration // from February 2019 to fall, L45-46 //. (b) Why the "sensibility" experiment with altered initial conditions but unchanged forcing is physically feasible. Perhaps it is useful to make an alternative full simulation for the period of missing cold sub-surface layer, when three-layer flow is evident.

(a) As mentioned in the previous response, we are not aiming to simulate the whole process of formation/permanence/destruction of the subsurface water mass. While we understand the reviewer's desire to see a February to October simulation, we note two things. First, we cannot start the simulations earlier, because the high resolution (1km) atmospheric model that provides the surface boundary forcing starts on May 24 2019 (hence, the start date of our model simulations). Second, the deep (~700m), narrow (~2km) inlet is a challenging environment to model in a sigma-coordinate framework; if we had enough observations to do data assimilation, we could likely fix some issues that tend to deteriorate longer simulations. Nevertheless, the simulated summer month allows us to explore the main features of Bute Inlet's circulation and we would not expect large changes further into the summer. Furthermore, we would like to highlight that our one-month simulations show the summer mean circulation at a time-scale similar to many observational studies (e.g. Baker and Pond, 1995; Gillibrand et al, JPO, 1995; Stacey and Gratton, JPO, 2001). Therefore, we argue that the information provided by these simulations is valuable.

(b) The sensitivity experiment aims to show the May/June conditions if the winter Arctic outflow event (or any other winter deep-mixing event) had not occurred. The winter event clearly affected the temperature and salinity of our May initial conditions; however, it did not affect the atmospheric and boundary forcing in May/June. In other words, there is no clear mechanism to explain why/how the winter event would have modified the atmospheric conditions all the way into the summer. Furthermore, the Arctic outflow event depends largely in the local topography, such that it affected Bute Inlet but not other nearby inlets (e.g. Toba Inlet; see Jackson et al 2023). Therefore, there is no clear connection between the conditions at our open boundaries (Johnstone Strait and Strait of Georgia) and the winter event. Therefore, we argue that it is justified to keep the same atmospheric and boundary forcing as in the baseline experiment, while only changing the initial conditions. We do note that it is unclear how the winter event may affect the river forcing in May/June, so that keeping the same river forcing is an assumption and source of uncertainty.

All of this information will be better explained/added into the methods section of the manuscript. Lastly, while addressing the comments by both reviewers, we looked into a few available temperature profiles in Bute Inlet (including those presented by McNeill 1974 and Pickard 1961) and realized that all showed some kind of temperature minimum in May/June, even if very small; therefore, we will make clear in the manuscript that our sensitivity experiment does not necessarily represent a year without an Arctic outflow event, but rather a situation where no deep winter mixing occurred.

Below we show a proposed modification of the text:

To represent the summer conditions in the absence of strong deep winter mixing the previous winter (e.g., by an Arctic outflow wind event), a sensitivity experiment was performed by removing the temperature minimum feature in Bute Inlet from the initial conditions. All other initial conditions and forcings (e.g., atmospheric and open boundaries) remained unchanged, given that the winter deep-mixing event would only affect summer conditions in the fjord (e.g., summer open boundary conditions in the Strait of Georgia and Johnstone Strait would not be affected by the outflow winter event in Bute Inlet). It is unclear how the winter event might have affected the summer river discharge; we kept this forcing unchanged to focus on the role of the initial conditions, acknowledging this assumption is a source of uncertainty.

C. The paper could reproduce and/or elaborate the observational background of Arctic outflow, the main headline of the MS, and its response in the Bute Inlet, in order to support the present modelling study. There are general papers by Jackson et al. (2022) and (2023) referenced, but meteorological and oceanographic anatomy of the modelled period would be nice to be read from this paper.

We appreciate R2's point of view, but we believe that modelling the evolution of the subsurface water mass from formation to destruction is beyond the scope of this manuscript. We hope that the new proposed title and change of focus make the goals of the manuscript clearer. Furthermore, for the reasons detailed before (i.e., lack of atmospheric forcing data), we would be unable to model the formation and initial stages of the subsurface water mass (February to late May 2019).

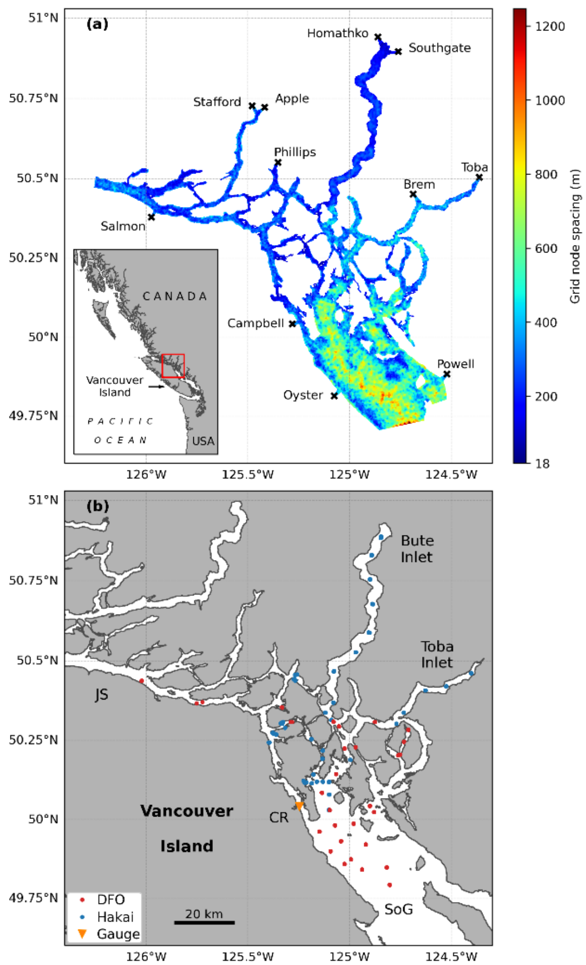
Minor remarks

The terms "baseline" and "sensitivity" are commonly used in other meanings than here, please consider reformulation.

We found this perspective very interesting, since we believe that both terms are currently being used within the standards of the discipline (ocean modelling). We are open to change them if the editor or R2 have specific suggestions. For now, we would keep them as is, since we prefer these terms than the more generic "Experiment 1" and "Experiment 2" type of nomenclature.

1 does not reflect the location of study area in wider geographical context, it was not easy to find it e.g., from Google Maps.

We have improved figure 1 (including some suggestions from R1). It is now larger and highlights more geographical features, like "USA", "CANADA", and "Pacific Ocean". Please see below



L191: "strong winter mixing event" is introduced, but it remains uncovered (see also conclusions L243 and L247).

The strong winter mixing event that led to the subsurface water mass was described by Jackson et al (2023); as mentioned before, the formation of this subsurface feature is not the focus of this paper. Nevertheless, we plan to improve our description of this event and the creation of the subsurface feature (as described by Jackson et al 2023), such that it is clearer to the reader what conclusions are derived from our analysis (and which ones belong to the existing literature).

L247: "Our study highlights how a fjord's circulation can be changed for the whole year by an extreme wind mixing event in winter." Where this statement comes from?

We agree with R2 that this statement was not accurate, since this is not something we really showed. We will remove this statement and, overall, make sure that statements in the discussion/conclusions are properly backed up by our analysis.

I propose revision of the MS.