Reviewer 2: Ilker Fer

The manuscript presents an analysis of openly available datasets, including hydrographic profiles from ice-tethered profilers, wind data from ERA5 reanalysis, and remotely-sensed sea-ice velocity and concentration, and surface geostrophic currents. The study site is a subregion (central Canada Basin, CCB) of the Beaufort Sea gyre region. The methodological framework builds largely on previous studies using similar datasets from this region and is generally well referenced. The novel aspect of the work lies in its examination of the evolution of mixed-layer properties and the relative roles of surface stress and convective forcing. In addition, the authors identify and quantify periods of elevated surface stress and assess their contribution to mixed-layer evolution.

The manuscript is well written, and the figures are both informative and visually effective. The figures support the narrative well, and the narrative supports the results and conclusions. The manuscript will make an important contribution to the literature after some revisions.

Overall, I would argue that the manuscript is a better fit for Ocean Science than for The Cryosphere. I am probably biased because I serve as an editor for Ocean Science. Following the editor's suggestions, the authors have introduced several statements intended to emphasize the relevance of the work to The Cryosphere. However, I do not find these additions particularly convincing. The authors could consider a transfer to Ocean Science.

We welcome this suggestion, but are unsure how such a transfer might work at this stage of the review process. We would defer to the Editor on this.

Because I refer below to some of my own work, I prefer to sign this review and leave the question of relevance of these studies to the authors.

I find the bulk-mixed layer approach highly simplistic and associated with considerable uncertainty, although I also recognize that it may be adequate for this manuscript. I would encourage the authors to expand their discussion of these uncertainties and, more importantly, to provide sensitivity analyses for key empirical parameters. For example, it would be useful to assess how the results change when the coefficients m0 and C as well as the drag coefficient and Ekman used in the surface stress calculations (and therefore friction velocity), are varied by a factor of two. Even such simple experiments would provide a clearer picture of the robustness of the conclusions.

We now provide a more detailed discussion of the sensitivity of the bulk calculations to changes in coefficients. See also our response to the first reviewer with respect to our added discussion on the range of possible drag coefficients.

The structure of the manuscript could also be improved. At present, descriptions of datasets and methods are distributed across several sections. For example, Section 2 focuses on surface stress but also contains much of the data and methods description, while sea-ice thickness observations from the BGOS moorings and the mixed-layer depth estimates are introduced later in Section 4. I suggest consolidating the descriptions of datasets and methods

into dedicated sections. Process-specific calculations, such as mixed-layer entrainment estimates, could remain within the results sections where they are discussed.

Our first iteration of this paper followed this approach, but we found it was too disjointed to lay it out that way. The flow was much smoother when we began with the surface stress data/methods and analysis, then brought in the ocean data once we had a clear picture of the surface ocean stress forcing. Therefore, we prefer to keep the overall structure like this, but we hope our earlier call out to the complete datasets to be used is helpful. We have also added an additional sentence (at the end of the first paragraph of section 4) to guide the reader as follows:

“This requires continuous observations of the upper ocean (profiles of temperature and salinity through the mixed layer resolving the seasonal mixed-layer evolution) as well as observations of ice thickness evolution, and we describe these in the next subsection.”

Finally, I find the literature review somewhat biased toward studies conducted in the Pacific sector of the Arctic Ocean. Several relevant studies from other regions and perspectives appear to be missing. For example, one of the central conclusions of this manuscript regarding the relative importance of convection versus entrainment from below the mixed layer is closely related to one of the key points listed in my 2017 paper, referenced below. A broader review of the existing literature would help place the present results into a more balanced context.

Thank you, this is helpful. We have now added this paper and several others related to studies in the Eurasian Arctic, as detailed in other responses.

My specific comments follow.

Kind regards,

Ilker Fer, University of Bergen

Specific comments (in order of appearance)

Abstract- “HSEs… in the CCB …have increased in frequency over 2003–2023» This is only one event increase, if I understood the text correctly.

We agree with the reviewer and have removed the clause "and have increased in frequency over 2003–2023" from the Abstract. We clarify in Section 3 that while the total number of HSEs per year shows a statistically significant increase during the 2003–2019 sub-period (from ~5 to ~9 events/yr), the trend over the full 2003–2023 record is modest. We retain the time-integrated stress calculation during HSEs.

Abstract- the last statement referring to a “positive feedback” is not adequately supported by the discussion in the main text and does not appear among the findings summarized elsewhere in the manuscript. I therefore recommend either removing this statement from the abstract or expanding the discussion section to explicitly address and support this proposed feedback mechanism..

We have removed the text in question.

Li 10- “energy-balance entrainment parameterization” could be more accurate; this is a vertically integrated bulk mixed layer param based on …

We have changed this abstract text to read “one-dimensional bulk mixed-layer model”.

Li 45- your review is biased to the Pacific sector of Arctic in general. Here, e.g., could consider: Graham, R.M. et al. Winter storms accelerate the demise of sea ice in the Atlantic sector of the Arctic Ocean. Sci Rep 9, 9222 (2019). https://doi.org/10.1038/s41598-019-45574-5

Thank you, we have now cited this in Section 2.3 as follows: A fundamental challenge is that sea ice cover moderates the transfer of wind momentum into the ocean such that an intense cyclone may not lead to significant momentum transfer if extensive sea ice is not responsive to wind forcing, and direct evaluation of the surface stress magnitude is necessary (see e.g., Graham et al., 2019).

See also: Fer, I., et al. (2017, One-dimensional evolution of the upper water column in the Atlantic sector of the Arctic Ocean in winter, J. Geophys. Res. Oceans, 122, doi:10.1002/2016JC012431). One of their key points is “Increase in mixed-layer salinity from freezing (10%) is significantly less than that due to entrainment (90%)”, comparable to one of your conclusions.

Thank you, this is useful context. We have added the following text to Section 4:

“Our results confirm previous observational and modeling studies in the Canada Basin (e.g., Toole et al., 2010; Peralta-Ferriz & Woodgate, 2015; Cole et al., 2017; Zhong et al., 2025) demonstrating that surface momentum transfer plays a larger role in seasonal mixed-layer deepening than convective brine rejection. However, our finding that wind-driven entrainment dominates mixed-layer deepening may be more applicable in general to the strongly-stratified Canada Basin. Consistent with this interpretation, Peralta-Ferriz and Woodgate (2015) identified the Beaufort Gyre and Western Arctic as exceptions to the convective deepening characteristic of the Eurasian Arctic. In the Eurasian sector, weaker halocline stratification allows for convection from sea-ice growth and brine rejection to drive a larger fraction of winter mixed-layer deepening (e.g., Peralta-Ferriz & Woodgate, 2015; Fer et al., 2017; Meyer et al., 2017). However, it remains the case that sea-ice growth plays a small role in seasonal mixed-layer salinification in both basins; Fer et al. (2017) demonstrated in the Eurasian sector that entrainment from below accounts for ~90\% of mixed-layer salinity increases during winter, with sea-ice freezing contributing only ~10\%.”

Surface stress calculation, eq. 1-3 and drag coefficients: quadratic stress parameterization, rotation of uEk and under-ice drag coefficients all become suspect when there are significant buoyancy fluxes, such as from melting. See Reifenberg et al (2025, Turbulence Observations below Drifting Sea Ice: TKE Production and Dissipation in the Meltwater-Influenced Boundary Layer, J. Phys. Oceanogr., 55, 451-470, https://doi.org/10.1175/JPO-D-24-0102.1), particularly

their discussion on drag coefficient, and also summary point 2 of their main finding, which can be linked to your findings.

Thank you for pointing out this important caveat and directing us to the citation.

We have added this text to the Methods:

"We also emphasize that quadratic drag parameterizations,as well as constant Ekman turning angles, assume neutral boundary-layer stratification. In the presence of strong surface meltwater release, shallow freshwater lenses ($1\text{--}2\text{ m}$) suppress shear turbulence and reduce effective drag coefficients (Fer et al., 2022; Reifenberg et al., 2025). Consequently, while quadratic stress formulations carry greater physical uncertainty during peak summer melt, they are more appropriate during the fall and winter freeze-up periods that form the primary focus of our analysis."

We have added this text to the Discussion in Section 4.3:

“An additional source of uncertainty is that the mechanical stress term (w_e^τ) in the bulk mixed-layer formulation assumes a constant drag coefficient (C_{Di}). However, high-resolution microstructure profiling demonstrates that strong, stabilizing surface buoyancy fluxes ($B >0$) from sea-ice melt form shallow freshwater lenses (1-2~m) that suppress under-ice turbulence and alter the friction velocity (u_*) and drag coefficient (Fer et al., 2022; Reifenberg et al., 2025). By neglecting this buoyancy-driven damping, we are likely overestimating the effective mechanical stress transferred to the mixed layer during peak melt.”

Fig 1. Mention the source of data (ERA5)

We have added to the caption: "Data sources are outlined in Section 2.1."

Eq2. Please mention why you include the Ekman velocity in the reference velocity

We have added a clarifying sentence after equation (2) to make this clear, and also to make clear that this formulation follows the established methodology of Yang (2006) and Meneghello et al. (2018).

"where $\boldsymbol{u}_i$ is the sea-ice drift velocity, $\boldsymbol{u}_g$ is the surface geostrophic velocity, and $\boldsymbol{u}_e$ is the surface Ekman velocity. The Ekman velocity ($\boldsymbol{u}_e$) is included alongside geostrophic flow ($\boldsymbol{u}_g$) so that $\boldsymbol{u}_{\text{rel}}$ reflects the true velocity difference between the sea ice and ocean flow ($\boldsymbol{u}_{\text{ocean}} = \boldsymbol{u}_g + \boldsymbol{u}_e$), accounting for ageostrophic boundary layer motion generated by surface momentum transfer (Yang, 2006; Meneghello et al., 2018)."

Ekman depth of 20 m seems arbitrary as this will be dependent on the surface stress

We adopted 20~m following the established methodology of Yang (2006), Meneghello et al. (2018), and Liu & Yu (2025) for the following reasons:

Direct under-ice velocity profile measurements from Ice-Tethered Profilers (ITPs) in the Canada Basin show that winter Ekman layer depths typically range between 15 m and 30 m, with 20 m serving as a representative regional climatological mean (Cole et al., 2017).

In our iterative calculation of surface ocean stress and relative velocity, the Ekman layer depth scales the surface Ekman velocity. Because this velocity is small (typically $< 0.01\text{ m s}^{-1}$) relative to sea-ice drift and geostrophic currents, the surface stress calculation is insensitive to the exact choice of Ekman layer depth within realistic bounds.

To clarify this, we have added a sentence after equation (3):

"Following Yang (2006), Meneghello et al. (2018), and Liu and Yu (2025), a representative Ekman depth of $D_e = 20\text{ m}$ is used, consistent with estimates from under-ice observations in the Canada Basin (15–30~m; Cole et al., 2017). Choice of D_e within this realistic range has a minimal effect on the surface stress calculation, as Ekman velocity ($\boldsymbol{u}_e$) is small relative to ice and geostrophic velocities."

Li 130- type on McPhee

Thanks, we changed Mcphee to McPhee.

Fig 3. I think you show the PDF and 80th percentile of stress using all individual daily estimates (not spatially averaged over CCB). You could clarify this in the caption.

Yes, that is correct. We have added “using all individual daily estimates” to the caption text.

Li 173. You could consider a comment related to the free-drift ratio

Thank you for this helpful suggestion. We have added the following text to explicitly incorporate this:

“The normalized ratio $\frac{|u_i|}{|u_{10}|}$ can be compared directly to the canonical free-drift ratio ($\sim$~0.02) in which internal ice stresses are negligible and momentum transfer is governed primarily by a wind and ocean drag balance (e.g., McPhee, 1980; Fer et al., 2022). While winter drift ratios in the Canada Basin have historically remained well below 2\% due to internal ice stresses (e.g., McPhee, 1980; Cole et al., 2017), the multi-decadal increase in $\frac{|u_i|}{|u_{10}|}$ highlights a trend to more frequent free-drift conditions during freeze-up and winter seasons.”

Lines 180-200, comparisons to Martin et al. results. If I recall correctly, it is a pan-Arctic study. Please clarify if you’re comparing CCB results to an Arctic average. This may also have a consequence on the discrepancy (line 195) you attribute to different analysis periods.

Good point to note this. We have added at the end of this paragraph: “Further, Martin et al. (2014) is a pan-Arctic study, so regional differences may also play a role.”

Li 221: if I understand correctly the increase in frequency of HSEs is from 6 to 7 per year. I am not sure whether this one extra event is significant (both in terms of number and also of impact). Also, I cannot follow how one additional event can increase the mean surface stress from 0.11 N/m2 by 0.54 N/m2 per year, which appears difficult to interpret, as the increase is approximately five times larger than the reported mean annual value. The text refers to a “time-integrated surface stress,” but this is not reflected in the units provided. I suspect that I may be misunderstanding the quantity being reported, which suggests that additional clarification is needed.

Thank you for pointing this out. We have corrected the text so that this unit includes "days".

Our updated text in section 3.2 is as follows:

“To quantify the cumulative momentum delivered by these events, we compute the annual time-integrated surface stress during HSEs as $\int_{\text{HSE}} |\boldsymbol{\tau}| \, dt = \sum_{\text{HSE days}} |\boldsymbol{\tau}| \, \Delta t$ (in units of $\text{N m}^{-2} \text{ days}$; Figure 6c). With an average event duration of $\sim 4.0 \text{ days}$ and a mean event stress of $\sim 0.11 \text{ N m}^{-2}$, the annual cumulative stress integrated across all HSE days ranges between ~ 1.5 and $4.0 \text{ N m}^{-2} \text{ days}$ per year. Over 2003–2023, this annual cumulative stress delivered during HSEs exhibits a linear increase of $0.54 \pm 0.45 \text{ N m}^{-2} \text{ days decade}^{-1}$ (Figure 7c), driven by shifts in total annual HSE days rather than changes in the peak intensity of individual storms.”

We have revised the caption of the relevant figure (and units) accordingly.

Li 240: the definition of buoyancy frequency needs more care.

We have added to this as follows: "Buoyancy frequency ($N^2 = −(g/\rho_0)d\rho/dz$, where g is the gravitational acceleration) is computed from the vertical density gradient $d\rho/dz$ in the upper ..." ρ_0 has been defined earlier (after equation (1)). We have removed the definition of g where it appears later in the manuscript.

Is upper 50-m too restrictive? Profiles suggest N2max may be deeper

Thank you for this. We have corrected the text in Section 2 to state that maximum buoyancy frequency (N^2) is computed over the upper 60~m of the water column for each profile, consistent with Figure 10. The mixed layer was never deeper than 50 m (See Figure 8e).

Li 263: please double check if this statement is correct: “the mixed layer has exhibited a shoaling tendency, which coincides with a decrease in freshwater … content”. If MLD is shoaling, I would have expected an increase in freshwater content.

Thank you, yes, this expectation (i.e., that shoaling accompanies an increase in freshwater content) holds in a 1-D local framework. We have edited the text to convey more clearly that this co-occurrence of mixed-layer shoaling and decreasing freshwater content likely reflects a gyre-scale relaxation rather than 1D local buoyancy input. The added text is:

"Following 2019, the mixed layer exhibits an overall shoaling trend ($\sim -0.4\text{ m yr}^{-1}$), which coincides with a basin-wide decrease in upper-ocean freshwater content (Timmermans et al. 2025; Zhong et al., 2025). Note that this co-occurrence does not imply a local 1D causal link (where surface freshening shoals the mixed layer), but rather reflects changes in the large-scale Beaufort Gyre circulation, in which reduced Ekman downwelling leads to a net reduction in accumulated freshwater."

Eq.5 and li 303 : According to the equation, if B>0 then ice is growing (i.e., brine release and convection). This contrasts with the sign of the second term in Eq.4. Please explicitly mention the sign convention where you write "… B can be positive (destabilizing buoyancy flux from ice growth?) or negative (stabilizing buoyancy flux from ice melting?)". This is also in conflict with where you write "If B > 0, C is set to 1, while for B < 0, C is set to 0.05 (the release of dense brine leads to convective mixing)". It should be the other way around, I think. Please carefully check through. Are the calculations correct?

You are correct, and we're sorry for the confusion. Equation 5 was missing a minus sign. We have fixed this and can confirm that this was only a typo and does not influence any of our calculations or figures, which were already computed correctly. For clarity, we have now explicitly defined the sign convention for B with the following text:

"Under this convention, ice growth ($dh_i/dt > 0$) implies $B < 0$; brine rejection generates a destabilizing buoyancy flux that drives convective mixed-layer deepening ($w_e^B > 0$). Following Farmer (1975) and Stigebrandt (1981, 1985), penetrative convection in the absence of mean shear converts a fraction $C = 0.05$ of potential energy release into entrainment at the halocline base. In this convective regime, $C = 0.05$ represents penetrative convection in the absence of mean shear, which Stigebrandt (1985) adopted as a slight increase over Farmer's (1975) observational average of ≈ 0.036 to parameterize the fraction of potential energy converted into entrainment. Ice melt ($dh_i/dt < 0$) implies $B > 0$; freshwater input supplies a stabilizing buoyancy flux that opposes wind mixing and drives mixed-layer shoaling ($w_e^B < 0$), for which $C = 1.0$ (Stigebrandt, 1985)."

For the convective case, C=0.05 is a representation of penetrative convection in the absence of mean shear (I think this should be better mentioned in the text). Stigebrandt uses 0.05, slightly increasing Farmer (1975)'s average value.

We now mention this in the revised text included in our previous response.

The statements: "During the ice growth season (October through May), wBe remains weakly positive and small» and "…while buoyancy-driven entrainment contributes only 7% (Figure

11a). Hence, surface momentum transfer plays a larger role..” may be sensitive to the choice of m0 and C.

Thanks for this comment. We have now added the following text:

"To assess sensitivity to empirical parameters, we varied m_0 ($0.3\text{--}1.25$) and C ($0.02\text{--}0.10$) across published ranges \citep{farmer_penetrative_1975,stigebrandt_model_1981,stigebrandt_model_1985}. Because entrainment rates in (4) scale linearly with both coefficients, these bounds define the full range of model sensitivity. Across all combinations, momentum stress remains the primary driver of winter deepening; even under a conservative lower bound for the wind-stirring efficiency factor ($m_0 = 0.3$) paired with an upper bound for convective entrainment ($C = 0.10$), buoyancy-driven entrainment contributes less than 10\% of seasonal winter deepening, confirming that mechanical stress dominance is a defining feature of the strongly stratified Canada Basin."

Li 296: “m0 is a constant (empirical) mixing efficiency” It is not a mixing efficiency but an efficiency factor that scales the wind stirring effect. Please clarify.

Thank you, we have revised as follows:

“...where $m_0 = 0.6$ is an empirical wind-stirring efficiency factor (Stigebrandt, 1981, 1985) that scales turbulent kinetic energy generation from surface stress…”

m0 coefficient is tuned to 0.6 to simulate the climatological mean annual cycle of mixed layer T/S in the Baltic Sea. In his own application to the Arctic Ocean mixed layer, Stigebrandt (1981) applied m0 = 1.25. This roughly doubles your stress-based term. How are we going to deal with this uncertainty? It needs perhaps a recalculation and sufficient discussion.

Thank you for pointing this out. We have clarified the text in Section 2 and added text in Section 4.3 as outlined above. We clarify that $C = 0.05$ represents the fraction of potential energy generated by gravitational instability used for penetrative convection in the absence of mean shear, based on Farmer (1975) and Stigebrandt (1981).

Li 336: “Our estimates of shoaling as a result of ice melt are typically larger (~3 m per month on average) than observed decreases in mixed-layer thickness in the melt season”… The stress part of your simplified bulk-mixed layer approach (Eq.4), will probably not be accurate when there are large stabilizing buoyancy fluxes. For example, measurements from the MOSAiC drift showed buoyancy effects in ocean-ice drag coefficient estimates [Fer et al JGR (2022), Upper-ocean turbulence structure and ocean-ice drag coefficient estimates using an ascending microstructure profiler during the MOSAiC drift. https://doi.org/10.1029/2022JC018751]

Thank you for pointing out this important caveat.

We have expanded our discussion in Section 4 to explicitly highlight these physical uncertainties and cite Fer et al. (2022) and Reifenberg et al. (2025) as follows:

"An additional source of uncertainty is that the mechanical stress term (w_e^τ) in the bulk mixed-layer formulation assumes a constant drag coefficient (C_{Di}). However, high-resolution microstructure profiling demonstrates that strong, stabilizing surface buoyancy fluxes ($B >0$) from sea-ice melt form shallow freshwater lenses (1--2~\unit{m}) that suppress under-ice turbulence and alter the friction velocity (u_*) and drag coefficient \citep{fer_upperocean_2022,reifenberg_turbulence_2025}. By neglecting this buoyancy-driven damping, we are likely overestimating the effective mechanical stress transferred to the mixed layer during peak melt."